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AN

# ESSAY

ON THE DISEASE CALLED

# YELLOW FEVER;

WITH OBSERVATIONS CONCERNING

## FEBRILE CONTAGION, TYPHUS FEVER, DYSENTERY, AND THE PLAGUE,

PARTLY DELIVERED AS THE

## Gulstonian Lectures,

BEFORE THE

## COLLEGE OF PHYSICIANS,

IN THE YEARS 1806 AND 1807.


*BY EDWARD NATHANIEL BANCROFT, M. D.*

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS, PHYSICIAN TO THE ARMY, AND LATE PHYSICIAN TO ST. GEORGE'S HOSPITAL.

AND REPUBLISHED, WITH NOTES,

*BY JOHN B. DAVIDGE, A. M. M. D.*

AND PROFESSOR OF ANATOMY IN THE UNIVERSITY OF MARYLAND.


"L' ignorance d'une vérité en physique peut nous cacher la cause d'un phénomène naturel ; mais l'erreur établie au lieu de la vérité arrête les progrès de la science, et substitue des songes et des chimères aux faits et à la nature.—Il est des erreurs et des vérités qui touchent les hommes des plus près que les autres, et ce sont surtout celles qui regardent la conservation de son individu.—*Fontana sur les Poisons et sur les Corps Animal,* tome i, page 97, 4to.


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*District of Maryland, Sct.*

BE IT REMEMBERED, That on the seventh day of October, in the forty-fifth year of the Independence of the United States of America, JOSEPH CUSHING and JOSEPH JEWETT, of the said District, have deposited in this office the title of a Book, the right whereof they claim as proprietors, in the words following, to wit:

[SEAL.]

"An Essay on the disease called Yellow Fever; with observations concerning "Febrile Contagion, Typhus Fever, Dysentery, and the Plague, partly delivered "as the Gulstonian Lectures, before the College of Physicians, in the years 1806 "and 1807. By Edward Nathaniel Bancroft, M. D. Fellow of the Royal College "of Physicians, Physician to the Army, and late Physician to St. George's Hospital; "and republished, with Notes, by John B. Davidge, A. M. M. D. and Professor of "Anatomy in the University of Maryland.

"L'ignorance d'une vérité en Physique peut nous cacher la cause d'une phéno-"mène naturel; mais l'erreur etablee au lieu de la vérité an ête les progres de la "science, et substitue des songes et des chiméres aux faits, et a la nature il est des "erreurs et des vèrités qui touchent les hommes des plus presque les autres, et ce "sont surtout celles qui regardent la conservation de son individu. Fontana sur les "poisons et sur les Corps Animal, tome i. page 97, 4to.

In conformity to an act of the Congress of the United States, entitled, "An act for the encouragement of learning, by securing the copies of maps, charts, and books, to the authors and proprietors of such copies, during the times therein mentioned."—And also to the act entitled, "An act supplementary to and to amend the act entitled, "An act for the encouragement of learning, by securing the copies of maps, charts, and books, to the authors and proprietors of such copies during the times therein mentioned, and extending the benefits thereof to the arts of designing, engraving, and etching, historical and other prints."

PHILIP MOORE,
*Clerk of the District of Maryland.*

# INTRODUCTION.

Whatever obstructs the progress of science, or throws a shade over the research of philosophy, is a subject of fair and legitimate criticism. Truth, physical, moral, or political, is the common property of society; and every member of the literary whole, may, according to taste and ability, enlarge its bounds, or promote its interests. Where motive is ingenuous, it is commendable; and manner, though awkward, may be pardonable. But before we write, we should think; and before we publish, we should at least understand the nature, if not the extent of the subject, on which we are about to admonish the world. The publick, however ready to learn, is impatient of unprofitable intrusion. Vanity may invite derision, but knowledge alone communicates information.

The man who conceives that the use of writing consists in the multiplication of books, mistakes the adulation of private vanity for the approval of publick sentiment. It is the pleasure or the caprice of the many to write, the good fortune of the few to convey knowledge.

In the reiteration of what has been said, is not always to be found the illustration of what is valuable.

The promotion of science, the encouragement of arts, the amusement of the curious, or the polish of the social, appears to be the chief, if not only object for which the pen is properly used. And every writer who undertakes to maintain, with success and understanding, a discussion, or to arrive at any satisfactory conclusion, through a process of reasoning, must in the first place settle his principle and define his object.

Dr. Hosack, by undertaking to write a nosology, *practical* he terms it, necessarily pledges himself to throw additional light on what is already before the profession, or to give something new. Merely to repeat what has been uttered, were to fatigue himself, and disappoint his reader.

The learned author, in his preface, offers the most ample opportunity for us to believe that nosology, the science of disease, is not a branch of natural history; that it is a thing indeterminate in form, and mutable in principle. For he says, that "in the details, however, of the synopsis now to be submitted, it will be readily perceived, that I have been more solicitous to convey a *distinct enumeration* of the *characteristick* or *pathognomonick symptoms of diseases*, and to form *those associations* which are connected with their cure, than to observe the rigid rules exacted by the the naturalist in the formation of the genera and species."

* See Pref. p. viii.

The rigid rules exacted by the naturalist, in the formation of genera and species, can only be such as an honest and faithful history of the distinctive characters may suggest, by which animals, vegetables, and minerals, can be formed into genera and species. Any other rules would be absurd and beside the subject. And these rules can be derived from no other source than nature herself, in her varied forms.

The formation of genera and species, pre-supposes distinctive characters; and to be distinctive, the characters must be regular, otherwise they could not constitute rules to the naturalist, nor would the naturalist be intelligible to his reader, when he might speak on his genera and species.

The writer says, " that I have been more solicitous to convey a *distinct enumeration of the characteristick or pathognomonick symptoms of diseases*," &c.

The writer's object clearly appears to be a *distinct enumeration* of those symptoms which are characteristick of diseases, and such as will enable him to " form those *associations*" which are connected with their cure; and no doubt to be intelligible to his readers, was a part of his purpose. And yet he tells us, that he is not to be expected to observe the rigid rules exacted by the naturalist.

To the naturalist there is no other rule than that which is derived from characteristick or distinctive phenomena, nor indeed can there be. The ingenious nosologist assures us, that his object is to convey a *distinct* *enumeration of the characteristick or pathognomonick symptoms of diseases*, &c. Then by what rule does the author attempt to convey a distinct enumeration of pathognomonick symptoms? If not by the rigid rule exacted by the naturalist, by what other rule does he attempt to form a distinct enumeration or association of symptoms?

As the author admits that diseases have their distinctive or pathognomonick signs, and indeed such a concession is implied in the very attempt distinctly to enumerate the symptoms, in what does the nosologist differ from the naturalist? Each is bound, by the ordinary rules of history and narrative, to give an ingenuous statement of those characters that are indicative of the genera and species, or, in other words, proper to their subjects.

If a *distinct enumeration* (what is not essentially regular, cannot be distinctly enumerated,) can be given of *pathognomonick symptoms of diseases*, in the very admission itself, disease is allowed to be fixed and determinate in its nature; and of necessity, disease is a part of natural history. It is not presumed that disease is the production and intention of art, although it may be, at times, the result of casualty.

By what the writer admits, it is conceded, that nosology is a science; and that there are other sciences, denominated natural sciences, or branches of natural history. It is also suggested by the necessary import of the assumption, that the sciences of nature, in object and principle, are fixed; but that the science of nosology or disease, is variable and indeterminate.

The end of the following pages is to show that nosology is a science; that it is, radically and fundamentally, fixed and immutable in its nature and character; and that it is a physical or natural science, attended by as few marked or sensible variations as any other branch of natural history equally extensive.

Were disease mutable in its fundamental laws, it could exhibit no general or uniform phenomena. The doctrine of disease would become a mere object of speculation. In it there would be nothing regular or formal on which the understanding could exercise its powers, much less could any gentleman, possessed of intellectual sanity, undertake to give to the world a distinct enumeration of those signs which should be pathognomonick, and by which future generations are to distinguish one kind of disease from another.

"It is not, indeed, to be contended," says Mr. Good, "that the distinctive signs of diseases are as *constant* and *determinate* as many of the distinctive signs that occur in zoology and botany; and so complicated is the *animal machinery*, so *perpetually alterable* and *altered* by *climate, idiosyncrasies, and the many accidental circumstances* by which *life is diversified*, that the general rule must admit of a variety of exceptions, and is here, perhaps, rather than any where else, established by such exceptions."

Dr. Hosack's pref. p. viii.

With the opinion, certainly not argument, of this classical writer before his eye, the author of the nosology undertakes to convey a *distinct enumeration* of the *characteristick* or *pathognomonick symptoms of diseases; constant* or *determinate* signs.

If the signs be not constant or determinate, will ingenuity itself explain now a distinct enumeration of symptoms characteristick and pathognomonick of diseases can be given?

To say that signs are not determinate, and yet are characteristick of diseases, is to lay the rational mind under a serious contribution, and awaken sentiments of compassion.

"That the distinctive signs of diseases are not as constant as the distinctive signs that occur in zoology and botany; and so complicated is the animal machinery, so perpetually alterable and altered by habit, climate, &c. &c." is one of those bold and gratuitous assertions which forces us to appeal to the observations and records of naturalists, in regard to animal life. Alterations in animal life, to be entitled to consideration, must mean *fundamental* and *radical.* If animal machinery be fundamentally alterable and altered, what preserves the races of animals distinct? How is it that the horse has not degenerated into the monkey, and man into the ass?

Upon what principle will learned gentlemen explain how the signs in zoology are constant and distinctive, and yet the *animal machinery* so alterable and so altered as to call for exceptions to general rules?

Either the animal machinery is not alterable or altered, radically, or the signs of the animal economy cannot be distinctive. What is not constant, is not distinctive. And what is altered, is not constant. Does animal machinery refer to the human animal only? Is the brute an animal? Or does zoology treat of animals? And if the reference, by the learned writer, be to man only, is the human more alterable than the brute animal? Or either, than vegetables?

It is below the dignity of science, and insulting to the human understanding, to bring such follies before the publick eye. Mr Good may be very classical, in the opinion of the gentlemen of New-York, but certainly, he is neither very intelligible, nor philosophical.

"It is true," says Dr. Young, "that we must not expect the same rigid accuracy in *medicine*, that may be obtained in some of the departments of natural history, since, in fact, many of the distinctions which are required in a nosological method, are rather established for the sake of practical convenience," &c. &c.

Nothing can be more unintelligible and inexcusable, than this confusion of nosology and general medicine. The subject before the reader, and that to which his attention is called by Dr. Hosack, is the science of nosology—the science of disease. The science of medicine involves more extensive considerations.

Nosology is a discourse on disease, as it is cognizable by its diagnosticks or more characteristick signs. In other words, it is the science of pathognomonicks. This appears to be the acceptation and import of the term, by all medical philosophers, from the earliest periods in which the science has been cultivated.

Human nosology, then, is that discourse which treats of the diseases, in their sensible characters, to which the human body is liable.

Disease is, essentially, in nature, in laws and phenomena, the same, whether art interfere or not. The intention of art is not to produce, but remove disease. Hence it follows, that the science of disease must be viewed apart from that art which human ingenuity and skill employ for the removal of disease. No two things can be more distinct. The small-pox occurring in the savage, at the most remote distance from professional aid, is the same in signs and nature, as that which takes place in civilized life. In the eye of science, the disease, and the art resorted to for the removal of the disease, are mutually opposed in fact and nature, however intimately, in the views and practice of the profession, they may be associated.

The science of medicine is general, that of nosology particular. Nosology refers to nature, incumbered and disordered in her functions; art to human skill, addressing itself as well as it may to the removal of disease and restoration of health.

Art, in its address to the removal of disease, may vary; and vary it will, according to experience, to education, to intellectual ability. Different intellects, viewing the same phenomena, will make different deductions, as those intellects may be vigorous or feeble; educated or ignorant; liberal or prejudiced. But to detain the reader longer in pointing out the incongruity and absurdity of extending our views to general practical medicine, in a discourse professedly on nosology, were to offend his good sense. What the writer can mean by a *practical nosology*, I must leave to his own interpretation.

That nosology furnishes the signs from which the rules of practice are deduced is true, for it furnishes the only means by which the professional man can form any idea whatever of the particular condition of the body against which art directs its force. But yet it constitutes no part of the therapeutrick scheme, and hence can never be, to my understanding, practical. But I leave the solution to the ingenuity of the author.

Nosology, or the science of disease, implies three things: 1st. There necessarily must be a subject, a body to be diseased: 2dly. There must be an agent or a cause: And 3dly, there must be diagnostick signs, or disease could not be recognised.

Although nosology has chiefly for its object a consideration of the phenomena, yet the subject and the agent are involved in the general idea of disease. And either the human body cannot be a subject of nature, or the agents are not productions of nature, or the phenomena can take place without a subject or an agent; otherwise I expect to show that the science of disease is a science of nature.

That science is said to be a natural science, the scope, object, and tendency of which are, to treat of those subjects and agencies that strictly are physical. The business of natural history, is to record the things and operations of nature.

A science is termed a natural science, in contradistinction to the science of the mind; to the science of general or particular state polity; to the mathematical or demonstrative science; to the tactical or maritimal science. Tacticks and navigation, however, are rather arts than sciences. To the above enumeration might be added the forensick and medical sciences; as far as the latter has a practical interpretation, however, it is generally styled the "*art* of healing" Although in a more general acceptation, the medical science is denominated the *science* of healing; but not so properly. For assuredly, as relates to practice, medicine is an art, purely tentative; a matter of experiment solely.

It is conceded on all hands, that if it can be established that the science of disease is a branch of natural history, it follows, of course, that in character and diagnostick, it is fixed and determinate.

The laws of animal life, on which depend the structure and economy of the animal body; by which, at first, the whole organization was laid out, and by which it is reproduced and sustained, are natural objects; objects of which the science of nature delights to discourse. And it is in these principles of life that the susceptibilities reside, on which the morbid agents act. Can natural history treat more properly of any subject than of the structure, the life, in its sensible phenomena, and of the susceptibilities of the human body?

This structure, this life, and these susceptibilities, are intimately concerned in disease. And surely, so far, disease is a subject of a branch of natural history. And, so far, must be as unchangeable as the principles of life themselves.

But the principles of life on which morbid agents exert their forces and powers, are not more the subjects of nature than the agents to the operation of which are finally to be traced the disease. Nor can those agents exert their influence, except through properties natural and intrinsick to themselves as agents. We cannot conceive a fundamental change in the properties of an agent, but with a radical alteration of the thing itself. And upon such alteration it ceases to be an agent.

Granting that the subject and agent are the objects of natural history, I propound the question, what can the result or disease be? Can a subject be acted on otherwise than through its own capacities? Or an agent act by other instruments than qualities intrinsick to itself? Is not the disease immediately and necessarily consequential on such operation? But still we are told that the science of disease is not a science of nature; that the phenomena of disease are not the proper objects of natural history! If not the legitimate objects of natural history, of what history are they the proper objects? Diseases have existed from time immemorial, and we have histories of diseases. Shall we denominate such histories medical histories?

Medical histories are complex, not simple; and in strict propriety, are more annals of the efforts of human skill and art for the removal of disease, than of disease itself. Had there been no medical profession, the pen of the historian would have transmitted down to us the narrative of disease.

Nature, in her general scheme, is uniform; otherwise ruin would invade the universe. But one part of nature may fall into collision with another part, and the regularity and uniformity of particular laws be disturbed. In lifeless nature, we style such disturbance and irregularity disorder, or disarray. In enlivened nature, we denominate such disturbance of function, or alteration of structure, disease. And although disease be not a part of the natural healthy functions of the body, yet it must be viewed as the natural result of agents acting on these functions.

A column of electrick fluid passes from a cloud electrified plus, to one electrified minus; or a column descends from a cloud to the earth; the atmosphere is greatly rarified; a sudden evaporation from the cloud is produced; and by this sudden evaporation, the temperature of the cloud is depressed, and a portion of the water of the cloud is rapidly changed, by the sudden and great loss of heat, from a vaporous to a solid state. Irregular masses of ice are formed, which, partly from the resistance of the air down through which they are precipitated, and partly from laws proper to themselves, constitute hail, or congealed bodies. These, by their own superior gravity, and the impulse received from the violence of the wind, in their descent, injure and destroy trees, brutes, and men.

Here, from a simple exchange between the clouds, and a descent to the earth, of an electrick column, we perceive the most disastrous consequences. The contusions in some, and the deaths of other animals, are equally with the prostration of the forest, subjects of natural history. And it is natural science, that by its lamp, conducts curious inquiry from the last effect to the first cause, explaining to the eye of enlightened intelligence, this awful but grand phenomenon. How simple the philosophy of the formation of hail, during the days of the highest temperature—a sudden evaporation.*

The marsh effluvium, wherever found, and however produced, *never from animal matter*, is an agent brought into existence by a certain combination of natural atoms by laws proper to these atoms. Or it is an agent from some simple source and indecomposable. Its nature is inscrutable. But, compound or simple, it is furnished from nature's stores.

This effluvium is poisonous to human life, and produces disease. And to whatever period of the history of its operations the eye of observation is directed, the

* I wrote an essay on this interesting phenomenon between ten and fifteen years ago, for the Maryland Society; in which I attempted to prove that the formation of hail in hot weather was wholly attributable to a sudden passage of electrick fluid from cloud, to cloud, or to some other body.

same effects will be perceived under the same circumstances. Nor in the opinion of the medical philosopher is there any fact more certain, or better established, than that men living within its range, will be effected with diseases of the spleen and liver; with diarrhea, intermittent, remittent, and yellow fevers.

How is this fact established, so as to be made a thing of uniform belief? Is it from individual observation, or the general history of the effects of this poison? Surely, from general and acknowledged history.

This effluvium, whether in the East Indies or West Indies, in Greece or Italy, France or America, is the same; the human body and economy are in principle and general attribute the same. Thus it occurs, that wherever the poison is evolved in given quantities, we see the swollen spleen, diseased liver, bilious colick, morbid secretions of bile, with all their consequences; sick stomach, diarrhea, dysenterick phenomena; intermittent, remittent, and yellow fever; and these modifications under various incidental circumstances.

The variety in the effects of this effluvium on the human body, is not greater than in the conditions under which water is found; sometimes of ice, or snow, or frost, or fluid, or vapour, the productions of incident or circumstance. But yet water modified, as it may be, is the same, and an object of natural history. The effects of the marsh poison are equally uniform. In the same intensity, and under the same circumstance, and on the same excitability, the phenomena will be without variety, and is equally a subject of natural science.

For a physician to say that disease, in principle and nature, is alterable, is to inform the world that he is totally ignorant of the science and history of disease, and of what he undertakes to speak.

We at times, it is true, have a new disease to come in upon us. And what is this, but a discovery that our previous scheme of science did not embrace a knowledge of all the possible morbid effects, or causes adequate to the production of disease? Or indeed, that some new agent or combination had come into operation. But such would be from the store-house of nature, or her plastick, creative power. But this is not change; it is only an addition to the old stock, numerous enough already!

That one and the same disease can arise from this cause to-day, and that cause to-morrow, is unphilosophick and absurd in the extreme, and in the face of all the facts of history, and analogies of nature. As I have advanced, I believe, somewhere else, there is nothing better settled in science, than that no simple effect can be produced by any two or more causes distinct in their nature. Every simple effect must be referred to some simple cause, aided as it may be, by incident and casualty.

What I have said of marsh effluvium, I might, with equal propriety, repeat of the various animal, vegetable, and mineral poisons. They are all from the great workshop of nature, and all their effects on the human body, necessarily consecutive of their attributes.

I might here refer to other irregularities and disorders in the physical world; the tornado, the earthquake, the water-spout, &c. &c. and ask the question—are they more the productions of natural changes and combinations than the plague, the small-pox, the influenza, the canine madness, the mumps, &c.? Or are the former better known than the latter, in their sensible phenomena or ruinous effects? But the one is said to be a branch of natural history; the other is a branch of what science? Is the tornado, the earth-quake, the water-spout, more constant and distinctive in their effects, than the small-pox, siphilis, the canine madness, &c. &c., in their diagnostick symptoms? The former and the latter both vary in degree.

The learned professor has given to the world a nosology; but to what end or purpose, if disease be not regular in its sensible signs, the professor has, in so laboured a manner, spread himself before the world, I cannot conceive. If the *animal machinery*, the human structure, be *so alterable and altered*, and the symptoms of disease so irregular and inconstant as to make the science of disease a matter of speculation, what benefit or advantage can accrue to the profession from any efforts at distinct enumerations of character?

The diseases of the subsequent year may not be known by the phenomena of the precedent, and those of New-York may never appear in Baltimore, Philadelphia, or Lexington. Where, and what then are the advantages derived from spending a great deal of money, and wasting much more valuable time, by attendance on lectures in New-York, for two or three seasons? If diseases do appear, and they will not fail to annoy the world, they may manifest themselves under new forms. How unavailing our knowledge and our reading! Former realities have become unreal, and our science vanishes like the dream of a feverous moment. Of all men in the world, the nosologist should be the last to utter a word against uniformity and regularity of diagnostick and pathognomonick in disease.

The able and ingenious professor offers to the world a nosology, and in the very preface assures his reader, that the literary performance to which his attention is invited, is entitled to but little consideration. For if diagnosticks are not constant and determinate, it may be insisted that the most laboured writing is vain, and the most sedulous perusal equally vain. But the Doctor declares his to be a "practical nosology." I would much rather see a *scientifick* nosology; a nosology founded on nature and accurate observation. In my estimation, those distinctions, in any nosological method, which are established for the sake of practical convenience, are slightly to be valued. Nosology cannot be supposed to take its character and form from practical convenience. On the other hand, practice must always derive its lessons from that order of sensible characteristick signs which nature presents, and, by the aid of which alone, we can form any rational idea of the condition of parts or the whole body.

Were a physician inadequate to discriminate a remittent fever from a small-pox, or a condition of measles from a pleurisy, or an apoplexy from an hydrophobia; or were a surgeon unable to distinguish a wen from a cancer, or a varix from an aneurism, he would provoke the contempt of every man who had ever looked into a medical book. But yet diagnosticks are not determinate!

I ask again, if the science of disease or nosology be not a branch of which natural history treats, will the learning of Dr. Young, or Mr. Good, or any other learned gentleman, inform the profession to what history it may be referred ?

# ADVERTISEMENT.

In the year 1806, I was appointed to deliver the Lectures founded by Dr. Theodore Gulston; and having chosen the Yellow Fever as my subject, and prepared myself to discharge this duty, I found it would be impossible to read what I had written, within the time allowed for these lectures; and considerable parts of it were, therefore, separated, and laid aside. These have been since restored, and some facts have been added, which had not then occurred. I have, moreover, subjoined three chapters on Typhus, Dysentery, and the Plague, which seemed fit to be connected or contrasted with the Yellow Fever, or with each other. I have also added eight Appendices, to confirm or illustrate particular positions, contained in the Essay on

Yellow Fever: and as only the first two parts of this Essay were printed, when I determined to embark for Jamaica, I requested my father to superintend the remainder of the volume.

There are no diseases which affect mankind so extensively, or produce so much mortality, as some of those which occupy this volume; and as very opposite opinions are entertained, respecting their origin and contagious power, real or supposed; and as this opposition of opinions is disreputable both to the science and professors of medicine, and incompatible with the best interests of mankind, I have endeavoured to remove all doubt and obscurity from these important questions, and place them beyond the reach of future controversy. How far this endeavour will prove successful, time must discover. But in regard to the Yellow Fever, I am persuaded that I shall have done more than enough to convince every candid and judicious reader, that it possesses no contagious power; and that a considerable part of the facts and arguments which I have employed for this purpose, might have been spared, had I not been also anxious to convince, even the *most prejudiced*, on this subject.

I know, indeed, that many of these facts, and arguments, have been already noticed; some by one, and others by different writers; but as in these ways, they have failed to produce that general conviction, which is most desirable, I have thought I might render an important service to mankind, by *so* collecting and arranging the proofs, and reasons connected with this question, as would best fit them to elucidate, and support each other, even if I had not been able to add any thing *new*, and valuable from my own stock.

I know, also, that I have been anticipated in several of my most important conclusions; but as I was led to them, rather by own train of reasoning, than by deference for the opinion of others, however respectable, I may in regard to these, say with Montaigne, "Que la verité, et la raison sont communes à un chacun, et ne sont pas plus a qui les a dites premierement, qu'a celui qui les a dites apres." I have, however, commonly mentioned those writers, by whom conclusions similar to mine were first advanced, (so far, at least, as they are known to me) for the purpose of giving them all due credit, and also to confirm my own opinions, by their authority; and, in my quotations, I have, with two or three unavoidable exceptions, always referred to the particular pages of the several works whence my extracts were taken ; so that my readers may be able to ascertain the accuracy with which they have been made.

## AN ESSAY

ON THE

# *DISEASE CALLED YELLOW FEVER, &c.*

## PART I.

### APPELLATIONS OF THE YELLOW FEVER.

THE Fever which is to form the chief subject of my inquiry, has been called Typhus icterodes, by Sauvages, and Typhus cum flavedine cutis, by Cullen; it is generally known, in this country, by the name of Yellow Fever; among the French, by the names of Maladie de Siam, and Fièvre Matelotte; and among the Spaniards, by those of Vomito Prieto, and, according to Coreal and Ulloa, Chapetonada.

The title bestowed by Sauvages is improper, because the Fever in question (independently of any discolouration of the skin) does not accord with his own definition of Typhus, and also because, as will be proved hereafter, it is not connected, at least not generally, with any morbid state either of the liver or of the bile, nor with any permanent obstruction to the passage of that fluid into the duodenum,—causes to which icterus, or jaundice, is commonly referred. Dr. Cullen may have been, in some degree, aware of the latter objection; and he may, on that account, have been induced to lay aside the epithet of "Icterodes," used by Sauvages, and to substitute for it the words "cum flavedine cutis," in his designation of the disorder: but even these, as well as the English epithet of "Yellow" Fever, are objectionable, because they draw the character of the Fever from a symptom which is very often wanting, even in the most severe cases of the disease, and which certainly is not always of high importance when it does occur, very many persons having recovered after being remarkably yellow.

The term of "Vomito prieto," or "black vomit," is likewise objectionable, because this symptom does not occur in a very large proportion of the persons who are attacked by the Fever now spoken of; it is, moreover, certain, that neither* the yellowness of the skin, nor the discharge† of a dark-coloured matter by vomiting, nor even the existence of both these symptoms‡ in the same patient, is peculiar to that disease.

* Dr Lind mentions (at pages 188, 198, &c. of his Essay on the health of Seamen) the existence of a fever at Portsmouth, on board of several ships of war lately returned from Louisbourg and Quebec, "which went commonly under the denomination of the *Yellow* Fever, from the sick often becoming yellow." This was clearly a different disease from the Yellow Fever of hot climates.

† See page 30, of "Surgical Observations on the constitutional origin and treatment of local Diseases," by John Abernethy, Esq for an instance of black vomit in another disorder.

‡ One instance will suffice to show, that those two symptoms are sometimes combined in other diseases. Dr. William Stark, respectable for his accuracy, and zealous pursuit of knowledge, in his valuable Clinical and Anatomical Observations, ("the materials of which," as he tells us, "were collected at a great hospital," which it is well known was St. George's Hospital, London, where it will not be suspected that cases of Yellow Fever were to be found) relates, at ch 1, §. 2, the case of a man who died from "inflammation of the smaller intestines, with effusion of blood," and whose body he examined after death. From this case the following lines are extracted: "A man aged thirty, unknowing of any cause, was one evening suddenly seized with retching and vomiting, which were frequent, day and night, ever after." "His skin became yellow on the fourth day, and what he vomited was observed, on the eighth, to be of a coffee-colour." "He died on the thirteenth day."

Several writers on the Yellow Fever, who have been sensible of the impropriety of the above appellations, have adopted others, taken from among the names given to fevers by the ancient Greek physicians. In the works attributed to Hippocrates, mention is made of violent febrile disorders, which sometimes proved fatal on the fourth day, and even sooner, and were attended with incessant vomiting, sometimes of black matters, yellowness of skin, and other affections so similar to those which are frequently observed in the Yellow Fever, that I am disposed to believe that they could be no other disease. These fevers are described under various names, such as *καῦσος καυσώδης τύφος, ἴκτερος λειπυρίη, φρενῖτις, νόσημα παχύ &c. but as the descriptions are either very loose or very brief, and as each of these names is also applied to other very different diseases, it is evident that no precise meaning can be affixed to them, and that confusion would result from employing any one of them to designate the Yellow Fever.

It is unquestionably of great moment, both for the advancement of science, and the prevention of dangerous errors in practice, that disorders should not be arranged under improper titles: and of all titles, none can be more improper, or tend more to embarrass inexperienced practitioners, than such as are borrowed from certain symptoms, which, like the yellowness of skin, and the black vomit, besides being wanting in the great majority of patients, do not, in general, make their appearance, when present, till towards the close of the disease. I hope, therefore, that the disorder, of which I am

* For καῦσος, see Lib. de judicat page 54, line 54 of Fœsius's Hippocrates, Geneva, 1657; and Lib. 1, de Morb. Vulg. Sect. 7, p. 954. D.: for καυσώδης, Lib. de Affect. p. 519, l. 21: for τύφος, Lib. de intern. Affect. p. 553, l. 6.: for ἴκτερος. Lib. de Morbis, p. 490, l. 6; and Lib. de intern. Affect. p 551, l. 8: for λειπυρίη. Lib de Morb. p. 467, l. 10: for φρενῖτις, Lib. de Affect. p. 518, l. 21. and for νόσημα παχύ, Lib. de intern. Affect. p. 559, l. 14.

to treat, will, ere long, receive a more appropriate name than any yet bestowed upon it; but I must leave to others the task of proposing one, and content myself with that of Yellow Fever, since it is, at present, in general use. I beg, however, to observe, that if it be allowable to apply to a disease, affecting the whole system, a name which designates only one of its symptoms, the terms of causus, or of ardent Fever, seem to be the most eligible of all the appellations that have been borrowed from the ancients, and given to the Fever here treated of; because the excessive heat of the skin, and indeed of the whole body, is a symptom which occurs not only very generally, but also very early in the disease, and requires particular notice for the successful treatment of the malady: so that either of the above names, especially the latter, is well adapted to direct the attention of the practitioner, however inexperienced, to an important point of practice, and is not liable to mislead him in other respects, or to occasion uncertainty or delay as to the management of the patient in the first stages of the Fever. It appears to have been upon similar grounds that Dr. Towne, M. Poissonnier Desperrieres, Dr. Moseley, and Dr. Fowle, have applied the one or the other, or both, of those names to the Yellow Fever.

Before I proceed to describe the appearance of this disease, I shall briefly notice the indiscriminate mode in which the term Yellow Fever has been employed; a subject not unimportant in itself, since it is probably owing to a want of precision in the use of that term, that so much ambiguity has prevailed, even to the present time, concerning the nature of the disease. The truth is, that in those countries in which the temperature of the atmosphere is usually heated, during certain seasons, to 85°, or more of Fahrenheit's thermometer, in the day time, all febrile affections, however produced, have a tendency, in consequence of the action of heat on the human body, to assume the violent and dangerous form which is generally considered as characteristic of the Yellow Fever. Thus, the fevers which sometimes originate from intoxication, and other excesses, from taking cold, or from fatiguing exertions of body, while exposed to the sun, or strong agitations of mind, are, in those hot seasons liable to be accompanied with the same severe and fatal symptoms which occur in the terrible Fever that occasionally attacks a great part of the population of certain towns situated in warm latitudes; and from this resemblance, the term Yellow Fever has been equally applied to Fevers, which are strictly sporadic, and comparatively rare, and to Fevers truly Epidemic.* But, although the same symptoms be observed in the Sporadic, and the Epidemic Yellow Fever, the course of these is not exactly similar, and their causes are totally different. The Sporadic Fevers are always, I believe, of the continued type, and are brought on by certain affections of the body and mind, operating only upon a few individuals; whereas the Epidemic Fever almost always manifests a disposition to remit, unless the speedy death or recovery of the patient precludes a second paroxysm; and it arises from causes of a general nature, such as are capable of operating on a considerable number of persons at the same time. The causes of the Sporadic Yellow Fever are so well known, that it is in the power of almost every individual to preserve himself from its attack; but those of the Epidemic Fever are still involved in so much obscurity, and placed so little within our power, that neither human ingenuity, nor patriotick zeal, with their most persevering efforts, have as yet been able to hinder its appearance, or perhaps materially to check its ravages. This is, therefore, the most formidable and interesting to mankind; and it will, for this reason, form the chief object of my present in-

* By the term Epidemic, I understand simply a disease prevailing in an extraordinary degree, without any reference to contagion, which I do not consider as being essential to an Epidemic.

quiry. A great part of the subsequent observations, however, concerning the symptoms and the treatment of the Yellow Fever, will, I think, be found equally applicable to the Sporadic and to the Epidemic disease. Both have, indeed, been described by so many writers of great eminence, that it will, I hope, not be deemed improper in me to abstain from giving a minute account of all the various symptoms of this Fever in its several forms, and to confine myself to those only which are of most importance, as indicating the nature of the disease, and the parts principally affected by it, together with the proper means of obviating, or removing those affections.

## SYMPTOMS.

The progress and violence of the Yellow Fever differ greatly, according to the force of its cause, the vigour and excitability of the patient, and the season of the year. When it prevails epidemically in hot climates, and attacks young and robust men, lately arrived from temperate regions, the disorder commonly appears in its most aggravated form. In this the patient first complains of lassitude, restlessness, slight sensations of cold and nausea, which symptoms are soon succeeded by strong arterial action, intense heat, flushing of the face, redness of the eyes, great pain and throbbing in the head and in the eye-balls, uneasiness and pain in the stomach, oppression of the præcordia, a white fur on the tongue, and a dry, parched skin, with a quick, full, tense, and generally strong pulse, though it is sometimes oppressed and irregular. These symptoms are speedily accompanied by frequent efforts to vomit, especially after swallowing food or drink, with discharges, first of such matters as the stomach happens to contain, and afterwards of considerable quantities of bile appearing first yellow, and then green,* sometimes tinged with blood, but, in the progress of the disorder, with matters of darker colours ; an increase of pain, heat, and soreness at the præcordia, also occurs, with constant wakefulness, and, frequently, with delirium, more or less violent. This paroxysm, or exacerbation, which has been called the inflammatory, or the febrile stage, generally lasts thirty-six hours, but is sometimes protracted for seventy-two hours, and even longer, probably in consequence of either general or local inflammation, (particularly in the brain or stomach) or of irregularity in the circulation, which are known to prolong the paroxysms in Fevers of Type.

A remission then occurs, in which many of the symptoms

* The green colour of the bile, which is very frequently brought up by vomiting, has been attributed, by some writers, to a morbid condition, or action of the liver, or gall-bladder. It would probably, however, be more just to attribute it to a spontaneous, and, as it may be called, chemical change, which the bile itself undergoes in the duodenum and stomach, while these viscera, being debilitated by disease, or by the operation of certain medicines, and particularly of emetics, no longer possess the power of preventing the matter which they contain from acting chemically on each other, and forming decompositions and new combinations. This power of the stomach, which has been called by Dr. Fordyce its *governing* power, will be particularly noticed, at page 13, &c. in treating of the symptoms which denote a putrescent state of the fluids. It is well known, that the fæces of infants, although yellow when voided, (which is the proper colour of the bile mixed with them) frequently become green after some time ; and, in addition to this, I may adduce the following fact from page 206, of Dr. Heberden's Commentarii de morborum historiâ & curatione. " Cujusdam icterici urina saturrime fusca, paucas horas servata, viridem colorem induit ; quemadmodum interdum fit in vomitu bilis flavæ, cujus color post aliquam moram in viridem mutari solet." As acids, when added to bile, change its colour from yellow to green, it is, probably, from a mixture with acescent matters that the bile becomes green within the intestines : and this is the opinion which the late Dr. John Hunter also entertained on the subject.—See page 129 of his Observations on the diseases of the Army in Jamaica.

subside, so as often to induce a belief that the Fever is at an end, and recovery about to take place. Frequently, however, the foundations of irreparable injury to the brain or stomach have already been laid in the former paroxysm, and in such cases the remission is short and imperfect. During these remissions the pulse often returns apparently to the condition of health, the skin feels cool and moist, and the intellect, if previously disturbed, sometimes becomes clear; sometimes, however, the patient remains in a quiet and stupid state, a symptom generally denoting great danger. Another sign of danger, as denoting a very morbid condition of the stomach, is the renewal of the efforts to vomit when pressure is made on that organ, or food is swallowed. After a certain interval, this remitting stage is succeeded by another, which may be called the second paroxysm, and which probably would appear as a renêwed exacerbation, if the violent effects of the first had not almost exhausted the patient's excitability, and in conjunction with the extreme depression of strength which usually attends inflammation of the brain or stomach, rendered him nearly unsusceptible of those morbid actions which are necessary for that purpose. In this latter stage, then, instead of great febrile heat, and strong arterial action, the warmth of the body, and the frequency and strength of the pulse, are often less than when the patient was in health; but frequently the pain and heat in the stomach become excruciating, with incessant strainings to vomit, which, in most of the fatal cases, are followed by hiccough, and repeated discharges of matters resembling turbid coffee, more or less diluted, or the grounds of coffee, and also by evacuations of similar dark matters from the bowels. Here it is to be observed, that when these symptoms occur, (indicating a violent affection of the stomach and bowels,) the patient is, in general, sufficiently in possession of his intellects to know those about him, and to give distinct answers to questions made to him, although his excessive weakness often renders him incapable of mental exertion, and his inability even to raise his head, may induce the appearance of coma. In those cases, however, in which the brain has suffered greater injury than the stomach, the retching and the black vomit, just described, do not so commonly occur, but instead of them, low muttering, or coma, with convulsions of the muscles of the face, and the other parts of the body, supervene. About this time, also, the tongue and teeth are covered with a dark brown fur; yellowness of skin and petechiæ make their appearance; the urine, when passed has a putrid smell and dark colour;* the fæces likewise be-

* Of the many authors who have treated of putrefaction in fever, the late Dr. George Fordyce seems, in his investigation of that subject, to have approached most nearly to the truth. His reasoning thereupon, which will be found at length at page 71 of the first part of his third Dissertation on Fever, and at page 151 of his Treatise on Digestion, may be summed up as follows:

The Creator, as one of the means of destroying and removing the remains of the successive races of animals and vegetables as they become extinct, which otherwise must have accumulated on the surface of the globe so as to afford no space for new generations, has ordained that dead animal and vegetable matter should be subject to certain processes called fermentations, and terminating in putrefaction. Now every circumstance which would cause a rapid putrefaction to take place in the dead body of a man, is constantly applied to the living body; viz. a warmth of nearly 100 degrees of Fahrenheit's thermometer, the contact of atmospheric air, and motion; and although no chemical circumstance has been discovered in the body of a living man that is capable of hindering such putrefaction from taking place, nevertheless the body of a living man has no appearance of putrefaction: and hence he concludes, necessarily, that there is in the life, independently of all other circumstances, a power of preventing putrefaction. Thus endowed, the living body is able to preserve, from the fermentations abovementioned, not only its own solids and fluids, but also, when in health, the extraneous matters, as aliment, which it contains; and thus, when the living power of the stomach is strong, the food thrown into that viscus will be perfectly digested; but, when the organs of digestion are weak or disordered, or when food is given to an animal which is not adapted to its organs of di-come most offensively putrid ; hæmorrhages sometimes take place from the nostrils, gums, and various other internal surfaces ; there is, in some patients, a suppression of urine, in others an involuntary discharge of it, and of the fæces : the pulse becomes feeble and intermits ; the breathing is laborious, portions of the skin assume a livid colour, the extremities grow cold ; and life is gradually extinguished.

This is a general outline of the Yellow Fever, when it ap-

gestion, a greater or less portion of that food is not governed by the stomach, but runs into the fermentations which would arise, if it had been in the same chemical circumstances, out of the body. It seems to be upon the same principle (viz. the defect of the *governing power*, as Dr. Fordyce has not inaptly styled it, of the organs, or vessels, of the body over the substances they contain) that we are to explain why, in certain morbid conditions, the fæces, the discharges from the vagina and from wounds or ulcers, and also pus, in the lungs, or other parts, the urine, and even the blood, which has been itself supposed to possess a living power, become putrescent. "The putrescency of the blood," (says Dr. Fordyce, at page 285 of the 1st volume of the Transactions of a Society for the improvement of Medical and Chirurgical knowledge) "is the effect of the depression of strength ; for it happens only when there is great depression of strength ; and when such depression arises in any other case, the same progress towards putrefaction is always observed." It is a confirmation of the above opinion, that, in most of those cases of injury to the spine, or disease of the bladder, in which this viscus loses the power of contraction, it also soon loses its governing power, and the urine is voided in a putrescent state, like that voided in severe cases of fever.

Two distinguished modern physiologists have stated it as their belief, that the stomach and intestines had, under certain circumstances, a power of secreting air ; but even their authority has not removed the objections to that opinion which appeared to me to exist ; and the generation of the air in question will, perhaps, be more satisfactorily explained by supposing that, under such circumstances, the governing power of those viscera is, in a certain degree, impaired, in consequence of which chemical decompositions of the matters contained in them begin to take place, and air is thereby evolved. It is not necessary to remind the reader of the volumes of various gazes which can be extricated by chemical agencies, from even small masses of most of the substances usually existing in the alimentary canal.

pears in its most violent form ; and in this form it sometimes proceeds with so much rapidity as to destroy the patient on the third or fourth day, or even sooner. But the disorder frequently appears in a milder form at first ; the course being protracted into several paroxysms, shorter at first, and followed by more distinct remissions, but afterwards increasing in violence and in duration, when the disease terminates fatally. In these cases, death usually happens between the seventh day and the fifteenth.

The features, which are the most remarkable in this disease, are the affections of the head, of the stomach, and of the skin ; and I therefore propose to offer some observations on each of them. There is great reason to believe, both from the symptoms, and from the frequent examinations which have been made after death, that most of those, who die of the Yellow Fever, are destroyed in consequence of some irreparable injury having occurred either in the brain, or in the stomach. It seldom happens, however, that these organs are both mortally injured in the same subject ; more commonly, one of them only is dangerously affected. I do not here mean to affirm, that there have been cases of the Yellow Fever, in which a fatal affection of the head had supervened, without any disorder of the stomach ; or cases in which the stomach was much diseased, while the brain continued in a sound or healthy state ;—for I believe, on the contrary, that these organs jointly suffer more or less in every case and species of Fever ; but, according to my experience, and that of several very respectable practitioners, with whom I have conversed on the subject, those patients in the Yellow Fever, who die from an affection of the head, generally perish early in the disease, and with less vomiting, especially of blackish, or dark-coloured matters ; whereas, on the other hand, those in whom this last symptom greatly predominates, are usually found to have their mental faculties clear, though often much weakened ;

and they seldom expire before the end of the fourth, or the beginning of the fifth day.

## DISSECTIONS.

When the bodies of patients, in whom the affection of the head formed the principal feature of the disorder, have been inspected after death, the integuments of the brain have generally been found more or less inflamed, especially near the temporal bones; the vessels of the dura mater, and of the pia mater, were not unfrequently observed to be very turgid with blood, which moreover was sometimes extravasated. Effusions of a watery fluid also have occasionally been seen over the surface of the brain, or in vesicles between the pia mater and the tunica arachnoidea. In some cases, the integuments have been so firmly attached to each other, and to the brain, that, in attempting to raise, or separate them, a part of the substance of the brain has been torn up. The volume of the brain is often increased, and the substance of it is, in some instances, more firm than usual; when cut, the vessels distributed through it, have been so distended with blood, that the medullary part has immediately become thickly spotted with red points, owing to the oozing of blood from the divided vessels; and it was not rare to find that some of those vessels had been ruptured, and that blood had escaped into the substance of the brain. The ventricles have usually contained water, frequently of a yellow colour, and were, in some cases, quite filled with it. The plexus choroides has often been loaded with blood.

Such is the disorganization which careful dissectors have uniformly detected, in a greater or less degree, in those cases of the Yellow Fever in which the predominant symptoms indicated a severe affection of the head. It accounts sufficiently for the occurrence of those symptoms, and for the fatality which so often attends them, as well as for the derangement of mind, the loss of memory, the impaired state of the sight, and other senses, and the extreme feebleness of the limbs, which are the frequent consequences of this disorder in those who escape with life; and from which they sometimes recover very slowly.

Those cases of the Yellow Fever, in which the stomach is principally affected, are next to be considered. This organ appears to be the most universal and important of all the viscera, and that which is the most indispensably necessary to animal life. Some animals are organised with so much simplicity as to have no visible brain, heart, or lungs; other animals, with cold blood, which are endowed with these organs, will live, and even move, for a considerable time after being deprived of them: but no animal is formed without a stomach, nor would any one be capable of supporting life, perhaps for even a very short time, after that viscus was destroyed.— There is, indeed, no other animating viscus discoverable in Zoophytes, Hydatides, Polypi, &c. though they are all capable of muscular motion and self-propagation. In the more perfect warm-blooded animals, which possess other vital organs besides the stomach, this last always sustains a distinguished part in the general system, by its important functions and associations. When it is in its healthy state, every other part of the body feels its salutary and invigorating influence; and when, on the other hand, it languishes or suffers, they all participate in its derangement. Ardent spirits, Opium, Æther, &c. when swallowed, produce powerful effects in every part of the body, long before they can have passed beyond the stomach; and a few ounces of strong Laurel-Water* occa-

* Fontana, sur les poisons, vol. ii. p. 125. "Si on donne cette eau (de Laurier-Cerise) en grande quantité aux animaux, ils meurent presque dans l'instant sans convulsions, toutes les parties de leurs corps étant relâchées, & dans l'affaisement.

sion almost instant death, upon reaching that viscus, without any struggle or appearance of re-action. But this intimate and extensive connexion of the stomach with other parts of the body, and its great irritability, subject it also to be injured and disorder by every thing which occasions injury and disorder to them. Thus it exhibits marks of inflammation in animals killed by the bites of venomous serpents, and of rabid animals, and by other poisons externally applied; and it is always, in some degree, affected by attacks of Fever, in which loss of appetite, aversion from food, and inability to digest it, occur with not less certainty than any other febrile symptom. But the Yellow Fever is almost invariably attended by marks of disorder in the stomach, much more decided than these,—I mean nausea, followed by frequent vomiting, even in the early part of the disease, and by the discharge of considerable quantities of bile,—a discharge which appears, in these cases, to be caused solely by the efforts to vomit, in the same manner as in sea-sickness, and other affections, in which the evacuation of bile, and even the increased secretion of that fluid, (which violent vomitings are believed to occasion) are merely a consequence,* and not a cause, of the disorder. These indi-

* Dr. G. Fordyce has properly noticed, in his valuable Dissertation on Simple Fever, (page 94,) the error of a common opinion, viz.—that "redundancy of bile constitutes an essential part of the attack of Fever," which opinion has been formed, because bile is "conspicuous from its colour, taste, and smell; while the gastric, pancreatic, and other juices, as they are not very conspicuous for their sensible qualities, have not been taken into the account." "If the pancreatic juice had been blue, (continues he,) and had any particular taste or smell, and the bile had been colourless, insipid, and inodorous, or as much so as the pancreatic juice now is; in that case, whatever has been said of the redundancy of bile, as being an essential part of the attack of Fever, would have been said of the pancreatic juice.—*See also the Note, at page 467, of Dr. Clarke's Observations on the Diseases of Long Voyages.* The idea of an increased secretion of bile in the Yellow Fever, or

cations of a morbid affection of the stomach in the Yellow Fever, are accompanied with others, affording similar evidence of the state of that organ, particularly excessive thirst, acute pain, and burning heat in the stomach, with so great a degree of tension and soreness over the epigastric region, that external pressure cannot be endured. As the disease advances, these symptoms become more violent, the strainings to vomit are incessant; and when the termination is fatal, hiccough and the black vomit usually supervene.

When the bodies of those who have died with the preceding symptoms have been dissected, the stomach has, in every instance, as far as either my knowledge or my information extends, exhibited very evident signs of inflammation. In some cases, almost the whole inner surface was inflamed; very often portions of the villous coat were abraded, and not unfrequently observed floating among the contents of that viscus. Marks of inflammation, but less violent than these, have also been often seen in the small intestines, especially near the pylorus. The inflammation observed on these viscera, seems to be of the kind denominated erythematic, which is found to affect the villous coat of the intestines more frequently than any other. This kind of inflammation is apt to spread wherever there is a continuity of membrane or of structure; and as such continuity exists through the alimentary canal, the viscera, nearest to the stomach, must be liable to participate in the inflammatory affection of the latter.

### THE BLACK VOMIT.

As erroneous opinions have been entertained concerning both the origin and nature of what is called the Black Vomit, it

in persons living in warm climates, is opposed by Dr John Hunter, upon grounds which appear to be just, at page 128 and 316, of his Observations on the Diseases of the Army in Jamaica.

may be proper to state, in this place, some facts and observations relating to it.

From the time of Galen, bile was supposed to be the principal agent in producing intermittents; and as soon as we became acquainted with the Remitting Fever of the West Indies, it was generally ascribed to that cause. Dr. Towne, who, in 1726, published an account of the Yellow Fever, under the name of "Febris Ardens Biliosa," considered it as a truly bilious disorder; so did Hillary, who called it a "Putrid Bilious Fever." This idea has been since very generally adopted, and the matters which constitute what is termed Black Vomit have, accordingly, been deemed, by many persons, to be the product of a morbid secretion of the Liver, or else of a morbid change in the bile. That neither of these, however, is the case, has been clearly ascertained, by late examinations made upon the bodies of great numbers of persons who have died of the Yellow Fever, particularly by Dr. P. S. Physick,* in Philadelphia. In these dissections, the Liver was very rarely found diseased, and even when in that state, the disease seems almost always to have been of a chronic nature. The alteration from the usual appearance of this organ most frequently observed, was a greater or less degree of paleness. The stomach, however, was constantly diseased, if the symptoms, which I have lately mentioned as denoting inflammation in that organ, had previously existed. The matter of Black vomit (which is commonly found in the stomach, but is also found in the intestines when they have partaken of the inflammation) was never discovered in the Gall-Bladder, the Liver, or any other viscus or cavity. The stomach has sometimes been loaded with this black matter,

* For more satisfactory information to the reader, the paper of Dr. Physick, which is alluded to, is given in the Appendix, (No. 1.) and there is also added the substance of another memoir concerning the Black Vomit, written by Dr. Isaac Cathrall, of Philadelphia.

while, in the same subject, not only the Liver was free from disease, but the bile in the Gall-Bladder was in its natural healthy state; and this has been seen in several cases, in which a contraction of the pylorus had completely obstructed the passage from the duodenum into the stomach. The matter of Black Vomit is, besides, essentially different from bile; it differs from it in colour; for, however dark the bile may appear in its most concentrated state, it always displays a yellowish, or greenish yellow tinge, when spread on a white surface, or when diluted; and this is never observed with the matter of Black Vomit. It has also been found that an addition of bile to the latter altered its nature so much as to give it an appearance different from that which it had before; nor could the Black Vomit be imitated by any mixture of various proportions of dark-coloured bile, with the fluids found in the stomach. It differs likewise, most decidedly, in taste; the Black Vomit being always insipid, when freed from other foreign matters, whereas bile can never, by any means, be deprived of its intense biterness. In many cases, portions of the inner surface of the stomach have been covered with a coat of thick blackish matter, and upon removing this coat, the parts beneath it, and no other, were found inflamed. The substance thus obtained, was exactly similar to that of Black Vomit, and, like it, incapable of being made to adhere again, when applied to the same or to any other parts. Hence there is reason to believe, that the matter forming this black covering, must have been derived from the vessels of the inflamed part, especially as it could not of itself have produced the inflammation, since it is so perfectly bland, that it has been frequently dropped into the eye without exciting any greater sensation than pure water. Neither can it be supposed, that it could have come from the gall-bladder, or from the biliary ducts, (in which, as I have remarked, nothing like it has ever been found) or from any part of the alimentary canal, and have attached itself afterwards to the stomach, in the manner in which it is seen adhering,—first, because it has no adhesive quality when it has been once detached from the surface on which it was found;—and, secondly, because, in some subjects, no such matter was found loose in the stomach or intestines, although a very great part of the inner surface of the stomach was, at the same time, covered with a coat of it. At those spots, moreover, where the villous coat had been abraded, the extremities of arteries have been frequently seen filled with this dark-coloured matter; and collections of the same matter have even been discovered immediately under the villous coat,—a situation to which it is impossible that any foreign matters, which had passed into the stomach, could have found access. To these facts, which render it incredible that the liver has any share in producing the matter of Black Vomit, I may add that this vomiting of dark matters never accompanies *Hepatitis*, or any other affection of the liver; and that it has seldom appeared, so far as I can learn, in any case of the Yellow Fever, in which there was not reason to believe that the stomach was, or had been inflamed. It may, therefore, be concluded, that when this symptom occurs in the above disorder, it is usually a consequence of inflammation in that viscus.

Some authors, who had adopted a similar conclusion, were inclined to consider the matter of Black Vomit as a particular morbid secretion, by the inflamed vessels or glands of the stomach; but this opinion, if I understand their meaning, does not accord with fact; for it is to be supposed that the villous coat, and the glands beneath it, are the only parts of the stomach by which secretion can be performed; and proofs have have just been adduced, that the matter in question may be formed without the co-operation of that membrane, or the glands connected with it, since it has been found in the extremities of arteries, and lying beneath the villous coat itself.

The least objectionable, and, in all probability, the true explanation of the most common formation of this substance, and of the phænomena attending it, seems to be, that it is merely blood which has been effused from some of the small arteries, ruptured in consequence of the separation of certain portions of the villous coat, and has coagulated within the general cavity of the stomach, or on the surface over which it was effused; and having been afterwards detached, and triturated by the violent and frequent contractions of that organ, in the efforts to vomit, has had its appearance as a coagulum of blood altered, and its colour* darkened by the gastric juice, or by some chemical decomposition, either

* Dr. Henry Warren, at page 39, of his Treatise concerning the Malignant Fever in Barbadoes, (1740,) says, "I ought here to observe, that the fatal black stools and vomitings are vulgarly supposed to be only large quantities of black bile, or choler; which false notion seems to be owing to that fixed unhappy prejudice that the fever is purely bilious. But let any one only dip in a bit of white linen cloth, he will soon be undeceived, and convinced that scarce any thing but mortified blood is then voided, for the cloth will appear tinged of a deep bloody red, or purple, of which I have made many experiments."

Dr. Cullen, treating of Hæmatemesis, (See Art. 1017 of his first lines of the Practice of Physic,) mentions the "black and grumous appearance" of the blood thrown out from the mouth, as one of the signs by which "the blood may be certainly known to proceed from the stomach;" and in Art. 1029, Treating of Melæna, or Morbus niger, which consists "in an evacuation either by vomiting or by stool, and sometimes in both ways, of a black and grumous blood," he says, "it is highly probable that what gave occasion to the notion of an atrabilis among the ancients, was truly the appearance of the blood poured into the alimentary canal in the manner I have mentioned; which appearance we know *the blood always puts on, when it has stagnated there for any length of time.*" Blood acquires a dark colour also, though in general more slowly, when it happens to be effused in other parts of the body; and the brownish tinge, which the fluid in Hydothorax and ascites frequently exhibits, seems to be derived from a similar change of colour induced in the red globules of blood that had been effused into the thorax and abdomen, together with serum and coagulable lymph.

spontaneous, or produced by the action of the air, or other matters contained in the stomach.

That blood is really poured out into the stomach in the manner just stated, will scarcely be doubted, many respectable* authors having affirmed, from their own observation, that blood, sometimes red, as if very recently effused, and often grumous, is frequently vomited in the Yellow Fever. It seems, indeed, impossible that any portion of the villous coat could be destroyed, or separated from the other coats, without occasioning a rupture of the arteries which conveyed blood to it, or that an effusion of blood should not immediately take place from the ruptured arteries, and continue until some of the blood thus poured out had coagulated, either within their cavities, or over the adjoining surface, so as to prevent the effusion of more blood. This is the usual operation of nature in all other hæmorrhages, and, we may presume, that it must unavoidably be performed in the case before us.

* Thus Dr. Rush, at p. 46, Edinb. edit. of his account of the bilious Yellow Fever in Philadelphia, in 1793, says, "there was frequently discharged from the stomach, in the close of the disease, a large quantity of grumous blood, which exhibited a dark colour on its outside, and which, I believe, was frequently mistaken for what is commonly known by the name of the black vomiting;" which last Dr. Rush had conceived to be "bile in a highly acrid state."

On the same subject, one of the latest of the authors alluded to, M. I N, Berthe, Professor in the School of Medicine, at Montpellier, (who was joined with two other Medical Professors of that University, Mess P. Lafabrie and V. Broussonet, in a commission sent by the French Government into Spain, in 1800, to observe and report upon the Yellow Fever, then prevalent in many parts of that country, and who has since published the result of the information collected by that commission, under the title of "Précis Historique de la Maladie qui a régné dans l'Andalousie in 1800," printed at Paris in 1802) has stated at p. 87, as follows, viz.: "On observait également à cette époque le hoquet, le vomissement noir: les malades rejetaient tantôt du sang, et tantôt de l'atrabile; et plus souvent ces deux matières mêlées et en même temps fétides."

There is another mode by which the vomiting of black matters in the Yellow Fever has been explained. Sir John Pringle, treating of this symptom, at page 197, of his Observations on the diseases of the Army, states his belief that the blood, "by oozing into the stomach, gives that blackish cast to what is then thrown up;" and he attributes this oozing to the blood's being "here so much *resolved*." Dr. John Hunter entertained a similar opinion; "the blood," says he, "being frequently in a *dissolved* state, is forced into the stomach and thrown up, forming, what is called by the Spaniards, the black vomit." See page 64 of his Observations on the diseases of the Army in Jamaica. Dr. Blane, however, apparently with greater justice, thinks that "this happens, more probably, from *a relaxation of the vessels on the surface of the alimentary canal*, than from a *dissolved* state of the blood." "The black matter," adds he, "that is vomited, and the black colour of the fæces and urine in the last and hopeless state of this disease, seem to be owing to the propensity to hæmorrhage in the internal surfaces." See page 410-1, of his Observations on the Diseases of Seamen. It has been ascertained by Sauvages, and other physicians, that in Melœna, blood in various gradations of change, from a red fluid to a matter resembling the grounds of coffee, has been sometimes vomited by patients, whose stomachs were found, on dissection, free from any abrasion or rupture of the blood-vessels; and it is also known, that a vomiting of similar dark-coloured matters has occasionally happened suddenly to women in labour, especially in cases of rupture of the uterus, without being preceded by a vomiting of any other matters, and in patients, the coats of whose stomachs were observed to be perfectly sound upon examination after death. A relaxation of the vessels on the surface of the stomach, (which may be, and perhaps often is, accompanied with some degree of inflammation) appears to be the cause to which the effusion of blood in such instances ought to be referred: and if such relaxation may take place in the affections just mentioned, it is to be presumed, that it may also take place in a disease of so debilitating a nature as the Yellow Fever, which is sometimes seen to occasion hæmorrhages from many other internal surfaces. On these grounds, therefore, it seems not improbable, that a relaxation of the vessels of the stomach had existed in most, or all of those cases of this fever, in which the black vomit occurred with little or no previous vomiting, or in which the coats of the stomach were entire. I must, however, remark, that in the accounts of dissections of patients dead of the Yellow Fever, which have come to my knowledge, I have found the cases just described, to bear only a very small proportion to the number of those in which injury had been done to that viscus; and I am, therefore, inclined to believe, that the black vomit is much less frequently the consequence of a relaxation of vessels, than of a separation of some portions of the internal coats of the stomach.

I must not omit to mention among the appearances on dissection, that the lungs and pleura have sometimes been found to have undergone some degree of inflammation during the course of the Yellow Fever; but, although the sufferings of the patient must have been greatly increased from such an affection, it does not appear that the inflammation was often so considerable as to warrant the supposition that it had caused his death.

## AFFECTIONS OF THE SKIN.

The connexion between the skin and the alimentary canal is well known, and, perhaps, results, in a considerable degree, from the identity of their membranes. This connexion may contribute to some of the appearances observed on the surface of the body in Yellow Fever, which generally begins with more moderate sensations of cold than other Fevers, but soon produces strong arterial action, during which the skin becomes excessively dry and parched, with an intensely burning or pungent heat. Sweats are in this stage a very rare occurrence, and when they do appear, no relief is afforded by them. A feeling of general soreness of the skin also takes place in many patients. But the most remarkable symptom affecting the skin in this Fever, is a yellow suffusion; which, though far from being a constant symptom, occurs often enough to have given occasion for the name by which the disorder is now commonly distinguished. The yellowness begins in a few cases, within the first forty-eight hours; sometimes on the third day, and frequently not until the fourth or fifth. It is, indeed, sometimes observed but a few minutes before, or a little after death. I believe that in many instances it might, with attention, be first discovered on the eyes; but it is commonly first observed on the cheeks, extending towards the temples, and about the angles of the nose and mouth; about the lower jaw and on the neck, along the course of the jugular veins, whence it afterwards spreads in stripes and patches along the breast and back downwards, so as at last to become universal in some patients, though in others it remains partial. The yellowness is sometimes of a dingy or brownish hue, sometimes of a pale lemon, and at others of a full orange colour. When the yellowness appears only in patches or spots, and of a dingy or brownish hue, these are frequently intermixed with other spots of a florid red, or a purple, or livid colour.

A considerable difference of opinion has subsisted respecting the cause of this symptom; some physicians having ascribed it to serum, which has been rendered yellow either by a colliquation, or dissolution of the red globules, or by a peculiar action of the vessels, and afterwards effused under the cuticle: and some to an *error loci* of the globular part of the blood, which, as they conceive, might occasion yellowness, by getting into the smaller order of vessels, in consequence of their great debility and relaxation, or into the cellular membrane; as happens after Ecchymosis, from external contusion; in which the skin, though at first livid, becomes yellow, when a part of the red globules is removed by absorption or otherwise. The late Dr. George Fordyce, in his fourth dissertation on Fever, (page 74) attributes the yellowness in question to the sebaceous matter, which he supposes to be then secreted more copiously by the sebaceous glands of the skin; he contends that, "the colour is very different from that which takes place in Jaundice," supposing, erroneously, that "the secretion from the kidneys has not that deep yellowish brown, nor that thick sediment, which have almost always been seen in those persons, in whom bile has got into the blood." Another author again, the late Dr. John Hunter, (whose authority on this subject is entitled to greater weight, from his experience in the Yellow Fever, and who had found, as every other person must have done, who has really made the trial, that in this disease, "the urine is of a very deep colour, and stains linen rags yellow, like that of a person in the Jaundice," See page 72 of his Observations on the diseases of the Army in Jamaica) believed it "probable that the inflammation in the coats of the duodenum and stomach, and the violent contractions they suffer from repeated vomiting and straining, may produce a spasm of the gall ducts, sufficient to interrupt the course of the bile;" (See page 157 of the same work) the consequence of which was, as he states at pages 135 and 137, that the bile was "absorbed and carried by the lymphatic vessels into the general mass of circulating fluids," and thus became "the cause of the yellowness." And lastly, Dr. William Saunders thinks that, in the more aggravated species of the Yellow Fever, this symptom "depends rather upon a particular state of the lymph in the cellular substance of the parts, than upon the absorption of bile into the circulating mass:" (See page 104, of his valuable treatise on the structure, œconomy, and diseases of the liver, 3d edition); but that in "the ordinary," i. e. "the Endemic Fever of the West Indies," "the Jaundice seems to depend upon a redundant secretion," the quantity of bile being, perhaps, so very considerable in this disorder, as he states farther at page 233, "that though the greatest part of it escapes into the primæ viæ, the whole may not readily find a passage; and the surcharge thus occasioned, may give rise to regurgitation and absorption."

I was once inclined to adopt one of the latter opinions, because it seemed difficult to conceive by what other means, or from what other cause bile should be introduced into the blood vessels in this Fever so as to render the skin yellow. That fluid is naturally intended to perform all its offices in the alimentary canal, and to be all conveyed thither: and it is only when its passage through the ductus communis choledochus into the duodenum is obstructed, that nature is believed to have provided means for its escape into the blood vessels, in order to obviate the mischief which might result from an excessive accumulation of bile in the gall-bladder, and biliary ducts. But no such obstruction appears to exist in the Yellow Fever; the alvine fæces being commonly dark-coloured, so as to demonstrate the admixture of bile; and the quantities of that fluid which are discharged from the stomach during the whole course of the disease, being such as to obviate all suspicion of an obstruction in the duct, and all probability of an accumulation of bile in the liver, or gall-bladder. These, however, and other objections against the existence of bile in the blood, as a cause of the yellowness in question, were overcome in my mind by further consideration and inquiries; and I am now disposed to think that, with perhaps one partial exception to be hereafter mentioned, the yellowness of the skin, which is frequent in this Fever, is derived from the bile.

In the action of vomiting, the abdominal muscles contract strongly, while the diaphragm is forcibly drawn downwards: by these motions the liver, from its situation immediately beneath the diaphragm, and its large bulk and inelasticity, suffers a certain degree of compression. When this compression is moderate and gradual, as it appears to be in most cases of ordinary vomiting, it is probable that some portion of the bile contained in the biliary ducts is thereby propelled into the duodenum, whence it passes into the stomach, and is thence throw up with other matters. When, however, there has been very frequent and violent vomiting for some length of time, the stomach, diaphragm, and abdominal muscles, are apt to become irritable to an extreme degree, so that, at each effort of the former to discharge its contents, the latter (whose power, as Mr. John Hunter has observed, at page 158 of his work on the Animal Œconomy, is "often capable of forcing the bowels themselves out of the abdomen, producing rupture") will frequently be thrown instantaneously into strong spasmodic contractions, and the liver together with the gall-bladder, will be, as it were, suddenly caught and tightly squeezed in a powerful press, the necessary consequence of which pressure seems to be, that all the fluids contained in that viscus will be driven towards both extremeties, backwards as well as forwards, in those vessels which are not provided with valves to prevent their retrograde motion. Under such circumstance it can scarcely be doubted, (after the experiments of* Haller, confirmed by others, demonstrating the facility with which fluids may pass from the biliary ducts through the pori biliarii into the hepatic veins) that the bile will be forced to regurgitate in this manner, and pass from those ducts into the vena cava, at

* "Altera certissima anastomosis est ex ductibus biliariis in venam cavam, quam cl. ante me viri viderunt, & mea experimenta confirmant. Elem. Phisiolog. Corporis humani. Tom 6. p. 509. 4to.

"Baron Haller observes, that a subtile injection thrown in by the hepatic duct will escape readily by the hepatic veins. This is a fact; and I know from experiment, that water injected in the same direction, will return by the veins in a full stream, though very little force is used. From the facility with which water takes this retrograde course, a probability arises that, if from any cause the natural direction of the bile be obstructed, it will readily obey the same (retrograde) direction." Dr. Wm. Saunders, Treatise on the Structure, &c. of the Liver, page 108.

each violent compression of the liver; and that by continued and strong spasmodic contractions of the before-mentioned muscles in vomiting, a considerable quantity of bile may be carried into the circulation, and a yellow suffusion,* exactly resembling Jaundice, be, even very speedily, produced. It is in this manner that we must account for the universal yellowness of the skin which even in the time of Galen, has been observed to follow the bite of the viper,† when the poison was of

* As the liability of the liver to such compression depends on circumstances varying in different individuals, and principally on its situation, form, and size, and also on the suddenness and force by which the surrounding muscles happen to contract upon it, we can hence understand why Jaundice may be more readily produced in one person than in another, although the symptoms might seem to be equally violent in both. We may likewise hence understand why infants, in whom the liver is, proportionably, much larger than in adults, and who are subject to numerous indispositions inducing strong convulsive contractions of the diaphragm and abdominal muscles, should be so frequently affected by Jaundice; the cause of which affection, however, appears to have been overlooked, and mistaken by Haller, (Elementa Phisiologiæ corporis humani Tom. 6. page 590) for curdy matters obstructing the common duct, and by others for viscid bile imparted therein, or other gratuitous suppositions. The only authors within my knowledge who have expressed a belief, that the bile might be driven into the blood vessels by the violent action of the diaphragm and abdominal muscles, and thus occasion Jaundice, are, Haller (in his work entitled, " Hermanni Boerhaave Prælectiones academicæ," § 348,) and Van Swieten (in his " Commentaria in H. Boerhaave Aphorismos Sect. 631 and 950); these very learned Physicians, however, do not appear to have thought, that the violent action of those muscles simply could produce a Jaundice, for they suppose the pre-existence of gall-stones, or of some other obstruction in the cystic or common duct, by which the excessive vomiting that precedes the Jaundice in such cases, is always produced as a salutary effort of nature: but this supposition does not accord with the well-ascertained facts, that gall-stones may obstruct the common duct, and produce Jaundice, without exciting any vomiting, even when they are attended with excruciating pain, and that Jaundice frequently occurs, although a free passage of the bile into the duodenum exists.

† Fontana was led to suppose, that the yellowness of the skin in persons bitten by vipers is derived from the bile, which is carried into the circulation in consequence of the common duct being closed by some crisping or irritation in the duodenum, arising from convulsive vomitings; but he offered no

sufficient force to produce the usual symptom of convulsive vomiting, and for the similar effects that have likewise been observed to ensue from the bites of some other venomous animals. In these cases a deep yellow colour of the skin has been observed within even a single hour after the accident, (as Dr. Mead affirms, at page 9 of his mechanical account of poisons, 3d edition) and consequently too soon to have been the result of any thing but a regurgitation of bile as before explained : for in regard to absorption, it is scarcely credible that the lymphatick vessels of the liver could ever, under any circumstances, take up and convey into the circulation such a quantity of bile as would suffice to produce Jaundice in that short period of time : neither is it readily to be perceived why, or how, in persons bitten by vipers, or in those labouring under certain affections to be presently mentioned, in whom a yellowness of

opinion concerning the means by which the bile is conveyed thither. "Convenons donc." says he, at page 68, vol. 1, of his work sur les poisons, "que si les sujets attaqués par le venin deviennent jaunes, il faut que la cause qui produit cet effet ait intercepté le cours de la bile, apres qu'elle est separée dans le foie, sans avoir auparavant nui en rien à cette secretion Je croirois volontiers qu'elle ne se répand ainsi dans la masse des humeurs, que parce que son cours est intercepté dans le canal cholédoque avant qu'elle se dégorge dans le duodénum Les convulsions de l'estomac et des intestins, qu'éprouvent ceux qui out été mordus par la vipère, peuvent très bien irriter, et crisper le duodénum, et boucher ainsi cet orifice. Ne nous étonnons pas non plus de voir la même jaunisse se manifester chez ceux qui ont pris d'autres poisons, puisqu'ils éprouvent aussi de semblables convulsions, un tiraillement douleureux dans le creux de l'estomac, des vomissemens bilieux et convulsifs, une contraction autour de l'ombilic et d'autres accidens dans le bas ventre." Fontana probably borrowed his gratuitous notion of a *crisping* of the duodenum from Morgagni, (de Sedibus and Causis morborum. Epist. 37. n. 35.) ; and Dr John Hunter appears to have taken his idea of "a spasm of the gall-ducts," as mentioned at page 36, from Fontana, combining with it, that of the bile being absorbed by the lymphatic vessels of the liver, and employing both to explain the occurrence of Jaundice in the Yellow Fever. The bite of the rattle-snake does not cause excessive and convulsive vomiting as that of the viper ; and it seems reasonable to suppose that the poisons of different snakes may be endowed, each with peculiar properties, and produce different effects.

the skin is speedily induced, a particular system of vessels in a particular viscus, i. e. the lymphatics in the liver, should have their functions at once changed, and should on a sudden be excited to absord bile copiously, and this while there is a ready exit for the passage of that fluid into the intestine, as is manifested by the matters discharged in such cases from the stomach and rectum.* In the same way yellowness of the skin, to

* It may yet be doubted whether Jaundice be produced by *absorption* of the bile in any instance, even in a complete obstruction of the common duct by a gall-stone, although this is the mode in which it is now by most persons supposed to be always produced in that case. A justly eminent physician relates an experiment made by himself, which, as he thinks, "evinces that the absorbents take up the bile from the interior part of the liver, and convey it by the thoracic duct into the mass of blood." He tied the hepatic duct in a living dog, and two hours after, the animal being strangled, he examined the parts. "On inspection," says he, "it appeared that the absorbents had been very active, for they were very much distended with a fluid of a bilious colour, and their course, which was very conspicuous, could be traced with the greatest ease to the thoracic duct, the contents of which seemed only moderately bilious. The bilious colour was, in a great measure, *concealed* by the *red* particles of blood, which had been extravasated by the injury, taken up by the absorbents, and conveyed into that canal It is *probable*, however, that the bile was only just entering the blood vessels, as on a very careful inspection of the eye, the tunica conjunctiva did not betray the *slightest* appearance of Jaundice" From this experiment the author draws the following conclusion. "It seems then, that, during the space of two hours, the secretion of the liver had been sufficient in quantity to distend its ducts,—to stimulate the absorbents to relieve that distension, and to allow of a small portion of their contents to be conveyed into the blood vessels"

This conclusion, however, notwithstanding my great deference for the opinions of this author, does not appear to me sufficiently supported by the preceding facts. It is, indeed, mentioned that the contents of the thoracic duct were "bilious;" but they are admitted to have been "only moderately" so; it is moreover acknowledged, that "the bilious colour was, in a great measure, concealed by the red particles of blood;" and as it was by *the eye* alone, that the result of this experiment was determined, I think that a slight yellow tinge, which confessedly was almost "concealed by red," is not of itself of sufficient evidence to decide a doubtful point of great physiological, as well as pathological, importance. The presence of bile in the a remarkable degree, attended with yellow sweats (which, as well as the urine, gave to linen a yellow tinge, have been sometimes produced by the excessive vomitings, and violent spasms, which ensue from eating some species of mushrooms in Europe, and certain poisonous fishes* in the East and West

thoracic duct, would probably have been determined with greater certainty by tasting the matters contained therein.

When the circumstances attending an obstruction of the common duct, by a gall-stone or other cause, are fairly considered, it seems highly probable that some regurgitation of the bile must happen, which, if continued for a sufficient length of time, may produce Jaundice: for, in such a case of obstruction, the bile will still be secreted as copiously as before, and must accumulate in the ducts, and distend the liver considerably beyond its usual size; and it does not seem possible, that in this state the latter can avoid suffering almost perpetually some degree of compression by the mere actions of the muscles in respiration; nor probable that, when thus turgid, it could be compressed without some portion of the redundant bile being each time driven into the hepatic veins. Besides these, other stronger actions of the thoracic and abdominal muscles are likely to be very frequently excited by the general uneasiness, or local pains, accompanying this morbid condition of the liver, as in sighing, coughing, &c. which may also force bile into the hepatic veins. In these ways regurgitation appears to be of itself adequate to the production of Jaundice; and if it be so, to recur to the doctrine of absorption, as another source of that disease, without any decisive proof that the bile is really absorbed, would only multiply causes without necessity.

* Several authors of undoubted credit, as Kæmpfer, Frezier, Sloane, Catesby, Ulloa, Osbeck, Forster, &c. have given accounts of various species of fishes found in the seas of warm climates, which frequently act as poisons upon those who have eaten them. The morbid effects produced by such food have been described by Dr Robert Thomas, now of Salisbury, but who formerly resided nine years in the West Indies, (at page 586 of his "Modern Practice of Physic, 3d edition); and among these, besides "severe vomiting and purging," he stages the following "In the advanced state of the disease, I observed that the whole surface of the body acquired a deep yellow hue as in Jaundice, and that the urine was likewise tinged of the same colour Even the perspiration gave a deep yellow tinge to the linen These appearances took place, in a very high degree, in one or two cases, but more particularly in my own, as I was so unfortunate as once to experience the deleterious effects of a poisonous rock fish," (perca marina)

As the fishes in question are poisonous at some times and situations and

Indies, and also from swallowing a violent dose of arsenic, or other poisonous substances. And if the vomitings and spasms, arising from these causes, are found in a multitude of instances, to produce general yellowness of skin, with such excretions, by urine and sweat, as manifest the presence of bile, we may surely infer that the severe vomitings, which occur in the Yellow Fever, may produce the like effects, and that they also may cause the introduction of bile into the blood-vessels, and thus induce the yellow suffusion of the skin under our consideration. In like manner, temporary jaundice is sometimes found to arise from Spasmodic colic,* Hysterics, and, as Haller,† Lind, and other highly respectable physicians have declared, strong passions of the mind.

The exception to the yellow suffusion being generally derived from the bile, to which I lately alluded, refers to those cases in which the yellowness of skin occurs partially, i. e. in patches or spots, previously to, or shortly after, death, or in which the patches are of an obscure or dingy hue, and intermixed

not at others, it is not improbable that their poison is acquired by feeding on certain noxious submarine plants or other substances. They are said to be rendered innocent by being laid for some hours in salt.

* Dr. William Saunders, whose opinions concerning the origin of the yellowness of the skin in the two supposed species of Yellow Fever have been lately stated, attributes the Jaundice, that sometimes follows the affections here mentioned, to the same cause which Fontana believed to produce the yellow suffusion in persons bitten by vipers. "When Jaundice has arisen from very acrid emetics, or griping purgatives, or colic, or hysteria, the resistance to the free passage of the bile, is either at the very extremity of the ductus communis, or during its oblique course through the substance of the duodenum, at which part it is liable to compression from the muscular action of that intestine."—See page 243, of his Treatise on the Liver.

† Nascitur enim icterus, et bilis adeo retrograda in sanguinem refluit plurimas ob causas, quarum aliquæ vix corporeæ sunt, ut ex ira." "Huc mæror profundior, terror," &c.—Haller Elem. Physiol. Corp. Human. Tom vi. p. 592.

"A violent fit of anger or grief will immediately produce a Jaundice."—Lind. page 177, on his Essay on the Diseases of Europeans in Hot Climates.

with petechiæ; the yellowness may, perhaps, in these instances, be produced by a cause similar to that which produces the yellowness that follows Ecchymosis; and this cause is probably connected with that particular state of the blood, and of the vessels, which occurs in the worst cases of the disorder, and gives rise to Hæmorrhages from various parts of the body, external and internal. It seems admitted by all practitioners, that these yellow patches on the skin indicate extreme danger; but a general yellow suffusion, such as I suppose to arise from bile, which is forced into the blood vessel by the temporary compression of the liver, is, according to my own experience, and that of many practitioners with whom I have conversed, a symptom of little real importance in the Yellow Fever; the bile so introduced being in all probability, incapable of doing any more considerable mischief than it is observed to do in those cases of Jaundice, which have succeeded to a* colic, or a strong hysteric fit, &c. It has, indeed, been associated with extreme danger in the Yellow Fever, by most writers on that disease, but only, as I believe, because the excessive vomiting, which had produced it, had also produced other more destructive effects.

## DIAGNOSIS.

Having thus stated, and endeavoured to account for, the principal symptoms of the Yellow Fever, I shall conclude this part

* When a Jaundice is produced by the affections here mentioned, it will gradually disappear without any aid from medicine; if the suffusion be slight, it will vanish in a few days; but if deep, and approaching to an orange colour, it will generally require from four to six weeks for its removal. The kidneys seem to be the principal means by which the bile, and most other unnecessary ingredients, are extracted from the circulating mass, a portion of bile, corresponding with the quantity thereof existing in the blood-vessels, being voided in every discharge of urine. An infinite number of remedies are recorded, by which the Jaundice is stated, and was believed, to have been cured; and the above explanation may serve to point out the real degree of their respective virtues, as well as their modes of operation in such cases.

of my subject with noticing some of the diagnostics, by which it may be distinguished from the distemper properly called the Plague, and from that fever which is now known, in this country, by the name of Typhus,—two diseases with which it has, by some writers, been, even lately, assimilated and confounded.

The Yellow Fever prevails only in those countries, and in those seasons, in which the heat is, or has recently been so great as would destroy, or stop the progress of, the Plague; and it is for this reason that the latter disease has never been known to exist in intertropical countries, the temperature of which, however, is eminently suited to the existence of the Yellow Fever. The latter disease is not accompanied with the glandular and cutaneous affections, called Buboes and Carbuncles; some of which, especially the former, always accompany the Plague; for although, patients are sometimes cut off by the latter disease, before Buboes appear above the surface of the adjoining parts, their germs may nevertheless, as I believe, be always felt, after death, in the glands near the groin or axilla. It is true, indeed, that the parotid glands are occasionally affected in Yellow Fever; but this is not a common affection, and it differs greatly from the glandular tumours which occur in the Plague. The Yellow Fever is moreover, always attended by a violent febrile paroxysm:—this is essential to its character; but it is admitted by several writers on the Plague and I have myself witnessed the fact, that persons have been attacked by the latter disease without having the least febrile affection,—an occurrence which has also been observed in the Small-Pox, in the Scarlet-Fever, and in the Measles. Finally, Blacks are very rarely seized with the yellow Fever, and when seized, they are much less violently affected by it than Whites living under the same circumstances; but I had occasion to observe, in Egypt, that Blacks were not at all less susceptible of the Plague than Whites, and that they died of it in a far greater proportion.

Yellow Fever differs from Typhus in the following circumstances, viz. it prevails, as I have already mentioned, only during, or immediately after, very hot seasons, in which Typhus is soon extinguished; and it is, in its turn, completely extinguished upon the accession of cold weather, in which Typhus is commonly most prevalent; it attacks most readily and most violently the young and robust, over whom Typhus is allowed to have the least power;—it begins with much greater exertions of the living power than Typhus,—is attended with many different symptoms, and terminates much sooner;—it is, besides, disposed to remit, and it frequently changes into a regular remittent, and sometimes even into an intermittent fever, which true Typhus is never observed to do. There are some other very important circumstances in which the three diseases differ from each other, but these are reserved for another place.

### PROGNOSIS.

Having already stated all that I had to submit in regard to the prognosis in the Yellow Fever, I must beg permission to refer the reader, who desires farther information upon that subject, to the treatises of former authors.

### TREATMENT.

In offering some observations concerning the cure of the Yellow Fever, it is not my intention to recommend any particular indiscriminating mode of treating the disease, in its several forms and varieties, being persuaded that none which I could devise would be found adequate to all cases of the disorder; but my sole aim will be to point out the general principles by which, as I conceive, the most urgent symptoms may be re-relieved, and the violence and fatality of the fever lessened.

### BLEEDING.

The remedy which first presents itself to our notice is bleeding, as being proper only in an early stage of the disease.

Concerning this evacuation, the most opposite opinions have been delivered, some considering it as an indispensable remedy, and others alleging that nearly all who were bled had died. The number of persons who have survived, after copious bleeding, in this disease, among whom I may be included, are a sufficient proof that this evacuation is not necessarily fatal; and, therefore, we can only account for this contrariety of opinion, by supposing that, where bleeding has proved hurtful, some important mistake must have been made as to the necessity of that evacuation, or as to the quantity of blood required to be drawn.

It has already been observed, that the Yellow Fever, especially the violent forms of it, seldom occur among any other persons than strangers recently arrived from temperate climates, the greater part of whom will commonly be found to be young, robust, and vigorous.—Hence we might be led, *a priori*, to believe, that these persons would be most liable to that inflammatory disposition, which is well known to be a very frequent concomitant of the intermittent and remittent fevers common in Europe; but we can have no hesitation in regarding the Yellow Fever as a disorder frequently, in its first stage, accompanied with a very considerable degree of general inflammation, (a degree which is, perhaps, greater than occurs in any other kind of fever) if we attend to the leading symptoms which are visible at the commencement of the disease,—I mean the hard, full, and strong pulse,—the distressing sense of universal distension, the red, starting, watery eye, and the parched skin. Dissections, moreover, of persons, who were victims of this disease, have very generally exhibited signs of considerable inflammation in various organs, and especially in the head and stomach. Now experience has clearly demonstrated that general inflammation always increases the duration of the paroxysm, whenever it supervenes in a fever of an intermittent or a remittent type, (as the Yellow Fever is) without being removed, and that it likewise augments the severity of all the febrile symptoms; the consequence of which is either that the patient is often destroyed during the paroxysm, though he might otherwise have survived; or, at least, that extreme weakness, with all those symptoms called putrid,* which are its usual effects, is more speedily induced. To avoid, therefore, the mischiefs arising from such superadded violence, no means appear to me so certain or beneficial as bleeding; but, that it may prove advantageous, it ought to be performed copiously; and from a large orifice, as early as possible after general inflammatory action is perceived; it being sufficiently ascertained that such action is more speedily and completely subdued by taking away a large quantity of blood at once, in this manner, than by a larger evacuation at two or more bleedings; and that, although the patient may be much debilitated at first by the former, his strength will, in the end, be less exhausted than by the latter. Those physicians, who have found the greatest benefit from this remedy in the Yellow Fever, insist most strongly upon the necessity of bleeding early, (as within twenty-four hours, and even twelve if possible, from the attack) to the amount of twenty-four or thirty ounces in the more violent cases; but in mentioning these quantities, it is not my intention to recommend that all patients should indiscriminately be bled to such an extent: the necessity of this evacuation, and the quantity in which it is to be performed, can only be indicated by the vigour of the patient's constitution, and the presence of inflammatory symptoms, and their degrees of violence and previous duration;—and doubtless, in some patients, bleeding may be superfluous, or detrimental.

* It is not uncommon, in hot climates, for the symptoms denoting putridity, to supervene within two or three days after the commencement of fever; and this has led several systematic medical writers, the greater part of whom have never been out of this island, to believe, and assert, that the fevers of hot climates are usually *putrid*. and very seldom inflammatory. These writers, however, seem either not to have been aware of the violence, and exhausting nature of the symptoms, which precede the appearances of putridity, or not to have been acquainted with the true causes of those appearances.

## COLD WATER.

One of the least tolerable among the earlier sufferings of the patient in this disorder, is a sensation of burning heat through the whole body, which is far from being imaginary, as his general temperature frequently rises four degrees, or more, of Farenheit's thermometer, above the natural standard. Happily we have a remedy for this most uneasy and formidable symptom, in the external use of cold water,—the safe and efficacious operation of which has been very ably explained by the late Dr. Currie. The modesty and generous delicacy of this estimable man, have led him to do injustice to himself, that he might perform what he thought an act of justice to the supposed discovery of a contemporary writer, from whose practice, in a distant country, as reported to him, he had first derived the idea of employing cold bathing in fevers. In doing this, however, he could not have been fully acquainted with the claims which several physicians, ancient as well as modern, had to acknowledgments of the same nature, for having recommended, or mentioned, the external application of cold, by bathing, or otherwise, in febrile disorders; and especially Hippocrates,* who, in several parts of his works, has given

* A multitude of instances might be cited from the works of this great physician, shewing the extensive use which he made of cold and of warmth, especially in external applications, towards the cure or relief of general or local affections; but I shall content myself with referring to only a few such instances, observing that his practice appears to have been grounded upon the general principle of restoring the due temperature of parts, by cooling those which he conceived to be too much heated, and of comforting, by warmth, those which were either too cold or debilitated;—a principle which, after some experience, I am inclined to consider as the best practical rule upon this subject that he could have followed. And Hippocrates had, in this respect, so judiciously adapted his practice to the suggestions of nature, as to have discovered that heating and cooling applications might be usefully employed, at the same time, with the same patient, for the purpose of correcting the deficiencies and excesses, of heat in different parts. Some pas-

particular directions for the employment of cold water, which are almost as judicious as Dr. Currie's. If, however, as Malpighi says concerning Harvey, "in arts and sciences he is properly to be deemed the discoverer, who, by a proper investigation, unravels nature's perplexities, and calls in reason

sages, it is true, may be found, which seem to contradict the above-mentioned principle; but these are comparatively few, and might justly, perhaps, be included among the interpolations with which the writings, commonly ascribed to Hippocrates, are, on good grounds, believed to abound.—See the treatment directed in the disorder termed Distension of the Lungs from Inflammation, in the third book de Morbis, (page 489, line 29 to 53, of the edition of the Works of Hippocrates, by Fœsius's, printed at Geneva, in 1657;) in Ilei, also in the third book de Morbis, (page 491, line 34 to 46;) in Causus,in the book de Affectionibus, (page 518, line 41 to 50;) in Tertians and Quartans, (page 520, line 48;) in Typhus, in the book de Internis Affectionibus, (page 553, line 25 to 38;) in Ulcers, in the book de Liquidorum Usu, (page 426, line 45 to 47.) See also the Cases, in the fifth book of Epidemics, of the woman in Larissa, ill of Puerperal Fever, cured by water very cold, (page 1144. F.) and of another woman, who, being in appearance dead, was recovered by throwing thirty amphoræ of cold water over her, (page 1153. B. C.) Hippocrates had likewise successfully employed effusions of cold water in Tetanus and Opisthotonus,—(See the third book de Morbis, page 491, line 30 to 33, and the book de Liquidorum Usu, page 427, line 34, the latter of which two passages is repeated in the fifth book of his Aphorisms, 21.) and even in the Gout, as appears from the twenty-fifth Aphorism of the last-mentioned book, and from the book de Affectionibus, (page 524, line 23, 24.)

It is not improbable that Hippocrates borrowed his modes of using cold water from similar uses, which he might have observed in the course of his travels through rude nations, among whom that natural and simple remedy for excess of heat was likely to be much employed, especially in Fevers. The two following passages will prove the existence of a like practice among very unenlightened people in other and opposite parts of the world.

The first is taken from a manuscript letter, (preserved in the library of the British Museum, and marked in Ascough's catalogue, 4432. 71.) which was written by Dr. Oliver Coult, at Calcutta, to Dr. Mead, and is dated the 25th of November, 1718. "I am credibly told, (says Dr. Coult) that, upon the coast of Sumatra, Pegu, and Siam, the natives, in fevers of all kinds, whether continued, intermitting, or eruptive, also in diarrhœas and slow dysenteries, wash frequently in the rivers, which are very cool, in the season of rains," (from June to November.)

and experience to support, and facts to confirm, what he asserts," then will Dr. Currie doubtless be esteemed the discoverer of this remedy.

It is only when the heat of the body is above the healthy standard, that cold water should be applied externally; and the patient's feelings will sometimes best direct how long and how frequently the application should be made; but we ought always to recollect that, if he should become chilled by it, not only mischief may be caused by driving a considerable quantity of blood from the surface to the internal parts, especially the brain, but also a violent re-action of the system might be produced, which could scarcely fail of protracting and aggravating the paroxysm.

As the usual modes of applying cold water to the surface of the body, viz. by placing the patient in a bathing-tub, filled with water, or pouring water over him, or washing him with wet sponges, while sitting on a stool, may sometimes cause serious disturbance and fatigue to him, and are often attended with difficulty or inconvenience enough to deter both patient and attendants from persisting in their use for a sufficient

The second will be found in a well-written tract, composed by a Mr. Bourgeois, Secretary of the Board of Agriculture at the Cape in St. Domingo, in 1755, entitled "Mémoire sur les maladies les plus communes à Saint Domingue; leurs remédes," &c. and contained in a volume of "Voyages interessans dans différentes colonies Françaises, Espagnoles, Anglaises, &c. à Paris, chez Jean-Françoise Bastien, 1788." "L'habitude des Nègres qui veulent guérir des fièvres est de se jeter dans l'eau la plus froide, de s'y baigner, & de se mettre sur la tête des herbes fraîches qu'ils arrachent au fond des ravines ou des rivières. J'en ai vu l'essai sur des blancs, qui convenaient que cela leur ôtait l'ardeur de la fièvre, que le mal de tête cessait presque aussi-tôt, & qu'ils se sentaient soulagés. Plusieurs m'ont même dit en avoir été guéris. Ces herbes se changent d'instant en instant, and se retirent toujours aussi chaudes que si on les eût fait bouillir: elles procurent de fortes transpirations, & débarassent surtout la tête. J'ai éprouvé ce remède sur moi-même. Mais pourquoi douterait-on de son efficacité? qu'on se rappelle ce que rapporte Chardin, de la manière dont la fièvre se guérit en quelques lieux de l'Orient, où l'on ne connaît d'autre cure, que de se faire jeter sur le corps des seaux de l'eau la plus fraîche."—P. 483.

length of time, a safe and useful substitute may be procured, by covering the patient, as he lies in bed, with a single sheet wetted* with cold water, which, by evaporation, will gradually reduce the temperature of his body to a proper standard.

The addition proposed by two or three writers of distilled spirit,—as rum,—to the water which is employed as a bath, would, indeed, contribute to cool the patient more speedily; but it may be disagreeable, if not injurious, to the patient, to inhale the spirituous vapours; and it is probable that the process of cooling by water only in the management just mentioned will be sufficiently quick and effectual in most cases.

Besides the external application of cold water, there is another use of it, I mean the drinking of it in small quantities frequently, which, as I have good reason to believe from personal observation, will be found of great efficacy, in moderating the excessive heat of the body, as well as the violence of general febrile action; also in disposing the skin to perspire gently; and in preventing inflammation of the stomach, or diminishing and removing it after it has been excited. The utility of frequent draughts of the coldest water in the cure of ardent fevers, and likewise in various other inflammatory disorders, was very generally known among the ancient† physicians, as is evident

* The mode here proposed of carrying off the superabundant heat of the body in fever, may be used in this country during warm weather; but, during cold weather, if the patient's room be not too much heated, he may, in general, be sufficiently cooled, by merely diminishing the quantity of his bed-covering.

† It is true that these ancients differed among themselves about the proper time of administering cold drinks in fevers;—Celcus, Galen, and most of the other Roman and Greek physicians, deeming it dangerous to administer them before the fourth day, or before those appearances, which they regarded as the signs of concoction, had taken place; while the Arabian Physicians gave cold drinks in the beginning, without waiting for such signs; the latter practice, however, has been proved by more modern experience, to be safe as well as advantageous; we cannot, therefore, wonder that some sanguine physicians should have pushed it to an extreme, and employed it alone in the treatment of febrile disorders, as in the diæta aquea of certain Italian Physicians.

from the writings of most of those whose works have been handed down to us; and it has been so fully established by the experience of many of the most considerable medical writers on the Continent during the last two or three centuries, especially in Italy and Spain, that it is a matter of no less surprise than regret, that this beneficial remedy should have been so little employed by British* and American physicians in the cure of the Yellow Fever. It is scarcely necessary to add, that in places where ice or snow is preserved during the hot season, water-ices, made with acidulous fruits, will be found a safe and very pleasant mode of diminishing febrile heat.

## PURGATIVES.

THE state of the primæ viæ likewise demands early attention. Costiveness frequently precedes, and generally accompanies, the Yellow Fever; and as an accumulation of fæcal matters usually produces morbid irritability in the whole intestinal canal, but more especially in the stomach, and aggravates other symptoms, it is highly expedient to employ a cathartic without delay. The medicine, which should be given for this end, ought, for reasons which I shall immediately explain, to be such as will not offend or irritate the stomach by its bulk or quality; and unless there be considerable determination to the head, the dose ought not to be very powerful, lest the patient should be too much reduced by excessive evacuations, and a

* Dr. John Williams, of Jamaica, and Dr. Rush, are two of the few exceptions I have met with to this remark.—"Large draughts of cold water, (says Dr. Williams) or other cool liquors, have occasioned profuse sweats, when all the sudorificks in the shop would not have had the same effect." He adds, "I have often observed that those persons who had this (the Bilious or Yellow) Fever on board of the vessels in the harbour, who seldom drank any thing *but cold water*, no beds to lie on, or clothes to cover them, with a free admission of air, frequently recovered"—See pages 16 and 27 of "Essays on the Bilious Fever, containing the different opinions of those eminent physicians, John Williams, and Parker Bennet, of Jamaica." London, 1752.

Dr. Currie has made some useful observations on cold drinks in the 11th chapter of vol. i. of his Medical Reports on the Effects of Cold Water.

prolongation of the paroxysm, or a diarrhœa be the consequence. Calomel, with Scammony, Jalap, Gamboge, and similar purgatives, will best answer the above purpose; and it will be proper to repeat them as may be requisite, in order to procure two evacuations daily during the continuance of the fever.

## EMETICS.

Emetics have been recommended in the begining of this disease by some, but reprobated, and as I think, very justly, by other writers, in no respect inferior to the former, either in discernment or experience. My reasons for condemning the use of emetics are,—first, that they commonly fail in their principal object of removing nausea, which is very apt to continue, and even in a greater degree than before; for this symptom rarely proceeds from any load of undigested food,*

* It is, perhaps, only for the purpose of removing such undigested food, and thereby preventing the injurious effects which its continuance in the stomach would occasion, that vomiting can be beneficially employed in the Yellow Fever; and, in cases of this description, the above purpose may be sufficiently attained, if not by draughts of tepid water alone, to aid the stomach in discharging its offending contents, at least by a moderate dose of Ipecacuanha, which is a more certain emetic than any of the preparations of Antimony in use, and is also preferable to the latter for other reasons presently to be explained.

The practice of giving emetics in the beginning of fevers, has probably been rendered more general by the opinion first advanced, as I believe, by Sir John Pringle, (See page 290, of his Observations on the Diseases of the Army) and afterwards adopted and maintained by Dr. James Lind, who, in his Dissertation on Fevers and Infection, chap. 2. says, that "if a person be seized with chills or sickness, after examining a prisoner, visiting a prison, or being in a crowded Court of Judicature, where prisoners, suspected of infection, have been tried, a vomit taken immediately seldom or never fails to prevent the future mischief;"—(See page 346 and also 248 and 257, of his Essay on Preserving the Health of Seamen, second edition;) but I am persuaded, by numerous facts which have fallen under my observation, to be stated in another part, that in all the instances adduced by Dr. Lind to support his opinion, no one of the persons, whom he supposed to have been infected, and to have been preserved in this mode from fever, was really infected; and I am likewise persuaded by other facts, also to be stated, that when a person has imbibed a dose of contagion sufficient to produce fever, a vomit will not only not prevent, but, on the contrary, assist its production.

or bile, or phlegm in the stomach, but seems rather to proceed from other causes, such as sympathy with the morbid state of the brain, or of the surface of the body, or else from an inflammatory affection of the coats of the stomach itself; and these are causes which emetics have but little power to remove. Secondly, the patient cannot vomit without making violent efforts, which will exhaust his strength, increase the circulation, and propel a large quantity of blood into the head where it may occasion the most serious mischief. Thirdly, there is a peculiar tendency in a warm temperature, to render the stomach and intestines relaxed, irritable, and liable to inflammation; hence the great prevalence of Cholera Morbus and Dysentery, in all countries, towards the end of Summer and in Autumn; and this natural effect of heat is, in no disease, more perceptible than in the Yellow Fever, in which a disposition to vomit is usually a very early symptom, and one of the most difficult to allay, as well as one of the most fatal if not allayed; for what is properly understood by the term of the Black Vomit, rarely occurs except as the sequel to frequent vomitings, nor can we be surprised at the remarkable frequency of this disposition to vomit, since we learn, from very numerous dissections, that the stomach is more or less inflamed in most of those who have died of the Yellow Fever. Instead, therefore, of prescribing emetics in this disorder, it soon became my chief anxiety, while attending the sick in military hospitals, in the West Indies, to calm the irritation of the stomach by every possible means; and I had full employment in this occupation; for the greater part of my patients, in the Yellow Fever, were persons to whom emetics had already been administered before they were sent into the hospitals. The mode which proved most successful towards effecting this intention, when patients, with constant vomiting, came under my care, was to give small doses of Opium, as half a grain, at intervals, at first of half an hour, and afterwards of one or two hours; to procure sufficient alvine evacuations, where the bowels had been torpid, by clysters, and also by combining moderate doses of the more powerful purgatives, as Calomel, Scammony, Jalap, &c. with the Opium, such evacuations being highly useful towards checking the vomiting, by promoting the natural propulsory action of the stomach and intestines; to apply a large blister or sinapism over the epigastric region, and to forbid the patient from swallowing food of any kind, liquid or solid, as the presence of even a very small quantity in the stomach always renewed the strainings to vomit. The patient was, however, directed to rinse his mouth frequently with lemonade, or some other pleasant and acidulated liquid.

When this treatment had been persisted in for eight, ten, or twelve hours, I generally found that the vomiting had subsided, and that the patient was able to retain a little food, which I then allowed him to take, at first in small portions, as a tea spoonful or two, and gradually* in larger: and I have the satisfaction of knowing, that very many persons were enabled to take sufficient nourishment, and in the quantity of half a pint or more at once, within a day or two after this simple plan of treatment had been adopted, and that they finally recovered; when it seemed highly probable that they would have been carried off in the same space of time, if, according to the mode which some authors have advised, and many practitioners have pursued, I had kept the stomach in a perpetual state of irritation, by forcing the patient, who had rejected one potion, immediately to swallow another, perhaps possessing even a more stimulating quality than the former.†

* I have very often found that patients, in the condition here described, were able to retain, and relished, small quantities of spruce beer, cooled as much as possible, when almost every thing else disgusted them, or was rejected by the stomach.

† Although Dr. Cullen and Dr. George Fordyce, two of our greatest modern teachers of medicine, have been partial to the use of emetics, and have recommended them in the commencement of Fevers, the weight of their recommendation, so far as it regards the treatment of the Yellow Fever, is considerably lessened by recollecting, that neither of them was personally acquainted with any but the Fevers of this country, which are much less violent in their symptoms, and less rapid or dangerous in their course, than the Fevers of hot climates, and in which it is certain, that emetics are

Though opium, as I have found, given in the manner above-mentioned, may be of great service towards putting a stop to

given with greater safety than in the latter: yet the following passages from their works will show, that both these experienced physicians were aware of the bad effects which emetics are capable of producing.

Cullen. First lines of the Practice of Physic, paragraph CLXXVIII. "It is seldom that vomiting is found to produce a final solution of Fevers; and, after they are once formed, it is commonly necessary to repeat the vomiting several times; but this is attended with inconvenience, and sometimes with disadvantage.—The exercise of vomiting is often a debilitating power; and therefore, when the vomiting does not remove the atony and spasm very entirely, it may give occasion to their recurring with greater force."

Fordyce. Third Dissertation on Fever, second part, page 73. "It happens sometimes, when an emetic is employed, that, with every precaution, the sickness will continue, and the patient shall pass a restless and distressing night, more so than would probably happen if no emetic had been exhibited."

Id. Fourth Dissertation on Fever, page 80. "Preparations of antimony, ipecacuanha, and other medicines, which produce symptoms similar to those which take place in the ordinary crisis of Fever, and especially Dr. James's powder, have frequently been employed in this very violent disease (the Yellow Fever). The patient's stomach very soon becomes so extremely irritable, that any dose of such medicines which might be expected to be at all efficacious, has produced vomiting; which, when it takes place in *any great* degree, *has hardly ever been got over, but has destroyed the patient.*"

Sir John Pringle was also aware of the disadvantages of emetics in the advanced state of Fever. See page 308 of his Observations on the diseases of the Army.

Some useful instruction concerning the injuries that may be caused by emetics, and particularly by antimonial ones, in the treatment of Fevers, is to be derived from the account given by M. Le Cat, M. D. (and published in the 49th vol. of the Philosophical Transactions, part 1, page 49) of a "malignant Fever that raged at Rouen, in the winter of 1753–4," where "the havock it made gave them the reputation throughout Europe of having the plague." This was the contagious Fever to which Dr. Cullen has applied the name of Typhus; a disorder, in a great measure, peculiar to the British Isles, and but little known to French physicians; for which reason it appears to have been very unsuccessfully treated by them whenever it has been introduced into Brest, or other ports or towns in France by English prisoners of war. The treatment pursued by M. Le Cat on the above occasion was, "after a bleeding or two," a vomit, the formula of which was "four grains of emetic tartar dissolved in a quart of water, the fourth part of which is given at a time; after this had worked either by vomit or stool, another fourth was taken, and so on, till the patient was supposed to have vomited or purged enough." This remedy sometimes produced "a small flux of five or six stools a day," and is thereby said to have effected a cure, as it might do in slight cases; "but when this success did not follow, the patient was again bled, first in the arm, then in the foot, and every two or three days there was given some cassia, quickened by an excessive vomiting, much caution is nevertheless required in the use of it; for if it be given freely, and in a larger quantity than is necessary for quieting the stomach, delirium and coma may be brought on, affections not less to be dreaded than that which the opium was intended to remove. And, indeed, it will be found of the greatest consequence, throughout the disease, to pay unremitting attention to the state of the brain, and to moderate, as far as possible, every action which threatens mischief to that most important organ.

If, therefore, the patient should, after having been sufficiently bled, complain of very severe pain in the head, or be delirious, or comatose, it would be proper to support him in bed, so that his head may be raised, to apply a blister at the nape of the neck, or between his shoulders, and to keep cloths wetted with cold water wrapped round his head: and if these should not have procured the desired relief, to have the head shaved, and fix a large blister over it, by which

emetic, in a decoction of tamarinds." From vomitings and purgings thus reiterated, the reader will not be surprised at the following appearances having been discovered in the stomach and intestines on examining the bodies of "many" of those who died, viz. "In some, part of the villous coat of the stomach, and of the small guts was inflamed, and the rest of these organs were filled with an eruption of the miliary crystalline kind, except that it was larger." "In others, a strong inflammation had seized the whole stomach, and a small portion of the œsophagus, but the intestines were free."

In those cases where the delirium had continued long and violent, we found either ulceratiou on the stomach, or its villous coat separated, together with a great inflammation, and even some gangrenous spots on the other coats of that organ."

"The manner of recovery from this disease, adds M. le Cat, deserves a place in the history of it. There were but few who recovered of it in the usual way, that is to say, who only wanted the restoration of their strength, exhausted as well by sickness as by the medicines. Almost all of them, even those who had it in the first and second degree, (the mildest degrees) still felt some remains of the symptoms of the disease;" "others who escaped the mortality of this dangerous poison, carried about with them for several months and still feel, its terrible effects."

M. Le Cat appears to have had no suspicion that the above appearances, and slow or imperfect recoveries, were the consequences of his mode of treatment; for, in mentioning that treatment, he styles it "the most successful," which it might have been, compared with other modes then in use; but to have (erroneously) considered them as characteristics of an unusual and peculiar distemper.

sleep is frequently soon induced, and severe pain in the head greatly mitigated or removed, after other usual means have failed.

## SUDORIFICS.

SUDORIFICS have, as well as emetics, been frequently commended, and employed in the treatment of the Yellow Fever: I cannot, however, join in this commendation, for the following reasons; 1st. They do not seem to be at all necessary, because a natural perspiration will readily ensue as soon as the excess of heat above the standard of health has been removed, which can be accomplished with certainty by the proper application of cold water to the surface of the body. 2dly. The means by which a perspiration is to be excited are not altogether innocent. Small doses of such sudorific medicines as, when given in large quantities, prove emetic, tend to increase that disposition to vomit, from which, as I have just mentioned, the greatest danger is always to be apprehended; and of this class of sudorifics none are so detrimental in the Fevers of *hot* climates as preparations of antimony, because, aided by the natural operation of heat, they usually leave behind them an extreme degree of irritability in the primæ viæ, which but too often resists all our endeavours to appease it. Again, if the sudorifics be composed of medicines of the above class, combined with opium, as Pulvis Ipecacuanhæ compositus, some share of that irritability will still be produced, though less, indeed, than in the former case; and the first effects of this combination will generally be a morbid increase in the heat of the body, and a greater determination of blood to the head, which may soon be followed by delirium or coma. Moreover, medicines of this description do not always succeed in causing perspiration; and if they fail in this respect, they will probably produce other effects very injurious to the patient, such as aggravating the existing symptoms, and lengthening the paroxysm.

### PERUVIAN BARK AND CORDIALS.

By the means above-mentioned, joined with mild febrifuge remedies, as saline draughts in an effervescent state, and by such others as will mitigate the sufferings of the patient, we may confidently hope, either that the termination of the first paroxysm will also terminate the disease, or at least, that the brain and the stomach will have been so far protected as to obviate the dangerous consequences of the succeeding stages; and in this expectation we may begin, as soon as the febrile commotion subsides, to administer the Peruvian Bark, in such form of preparation, and in such quantity, as will best suit the state of the stomach, in order* to restore that organ to its proper functions, to strengthen the system, and to prevent any return of the Fever. It should, however, be a rule not to give bark in this disorder till the period just mentioned; for if it be given when there is a parched skin, a hard pulse, a dry tongue, great heat and pain at the stomach, or delirium, it will generally be found to increase and prolong these symptoms.

When the patient begins to sink in strength, he should be supported by combining aromatics with the bark, and also by wine more or less diluted, or even brandy, which is often more palatable and more easily retained by the stomach than wine; and these cordials should afterwards be continued, or increased according to the state of his stomach, and the degree of his

* Dr. John Clarke, after a review of the practice in Fevers, as contained in the journals of the Surgeons of the different ships in the service of the East India Company, between the years 1770 and 1785, has given the result thereof in the following words, at page 468 of his valuable work on the diseases of long voyages. "Upon the whole of the evidence it appears that, when Fevers of any consequence prevailed in the ships, either at sea, or at the different stations in India, mortality was almost invariably the consequence of bleeding, and the continued use of purgatives and antimonials. That under a cordial regimen, and moderate evacuations, succeeded even by a late use of the bark, many recovered; and that under the early, liberal, and continued use of this medicine, not one instance of death is recorded."

debility. If the extremities grow cold, they should be warmed by artificial means.

### MERCURY.

It will doubtless be expected that I should speak of salivation by Mercury in the Yellow Fever, which some writers have so highly extolled; and, indeed, I should have done it earlier, if the results of my former experience, or a satisfactory explanation of its mode of operation, had relieved me from the doubts and difficulties which I have felt on this subject.

Dr. Henry Warren, in his "treatise concerning the Malignant fever in Barbadoes," (for so he called the Yellow Fever) first printed in 1740, after condemning what he designates as "a very odd, and unwarrantable practice which had obtained for many years among several of the plantation practitioners in that island, of giving calomel in Inflammatory Fevers," says, (at page 36) "I have yet never heard of mercury being given in this malady, (Yellow Fever) and I hope I never shall; as, no doubt, it would here act an uncommon mischievous part." It was not then foreseen that, in the year 1793, a medical practitioner in Grenada would be found boldly administering mercury in this very disease, even so as to excite copious salivation, because he supposed, erroneously,* that "the liver was the most diseased part," and had heard of two Surgeons of the army who, some years before, gave calomel with success in the Yellow Remittent Fever, *not indeed to salivate*, but to produce evacuations *by stool;* and also, because he thought that "it was, at all events, better to try a doubtful one, than remedies of no efficacy." Although I should not have thought these motives sufficient to warrant an innovation so extraordinary, yet if it has really preserved even a few of the lives said

* See pages 351 and 423 of Vol. 1, of Dr. Chisholm's Essay on the Malignant Pestilential Fever, &c.

to have been saved thereby, I shall most readily excuse the acknowledged "temerity" of that new practice, and sincerely rejoice at the practitioner's good fortune in thus stumbling upon an efficacious remedy for a disease commonly productive of so much mortality. I must, however, acknowledge, that I am not yet convinced of the supposed benefits of this new practice; for, should it even be true, as is pretended, that the patients labouring under Yellow Fever, in whom a salivation can be excited, generally recover, I do not perceive that we could thence fairly infer, that their recovery was effected by the salivation. It is well known that, in many cases of that disorder, more than 500, and, in some, more than 1000, grains of calomel have been given internally to a single patient without producing any sensible effect on the salivary glands, or even on the intestines; and although, to explain this inactivity of the mercury, it has been supposed, that in such cases the absorbents alone were in fault by not taking up the mercury, this explanation cannot be admitted, because the intestines have commonly been as little excited by the calomel thus introduced as the salivary glands; and it seems, therefore, probable that a general torpor, or defect of excitability, and of vital energy, existed in such patients,* and that the mercury proved inefficacious in them only, because they had already made considerable approaches towards the condition of a dead body, in which it is obvious that no quantity of that medicine, however large, could exercise a stimulant power. If this reasoning be just, there will be room to suspect at least, if not to conclude, that, when patients die of Yellow Fever, after all attempts to excite salivation in them have failed, their deaths

* Dr. Chisholm, after mentioning (at page 429 of Vol. 1, of his Essay) that there are some habits which, *under the influence of disease*, resist the action of mercury even when more than 2000 grains have been given, while there are others in which salivation is excited by less than ten grains, adds, "hence it may not be irrational to conclude, that the susceptibility of, or resistance to, the action of mercury in habits, in which *the morbid action* of the cause of the Malignant Pestilential, and Yellow Remittent Fevers *has already taken place*, are *in the direct ratio of their excitability*;" a conclusion tha is in conformity with the explanation which I have above given.

have resulted, not from the want of any good effect which salivation may be thought capable of producing, but because the condition of their living, or sensorial power, and of the functions depending thereon, had already become so morbid as to render their recovery impossible; and, on the other hand, that where persons have recovered from the Yellow Fever, after having been salivated, their recovery was not occasioned by the salivation, but was the consequence of such a condition of the powers of life, and of the functions connected therewith, as induced a mitigation of the disorder, for the same reason, and perhaps, cæteris paribus, in the same degree, as it favoured the operation of mercury upon such persons; and therefore that, although recovery has not unfrequently followed, or accompanied salivation, the latter was not a cause of the former. There is, indeed, no source of error more common or productive, than that of supposing an event which closely follows another to have been occasioned by it; and it may be doubted, whether a great number of the advocates for mercurial salivation in the Yellow Fever have any other, or better, foundation for their conviction of its efficacy.

Besides the uncertainties in the operation of mercury, which depend on the different conditions, or degrees of the general excitability, there are others, arising from certain constitutional peculiarities not well understood, which give occasion to excessive salivations by the taking of only a few grains of calomel; and this effect constitutes a serious objection to the use of mercury, in a disease so rapid and dangerous, without manifest necessity.

In order, however, to attain the truth upon this important subject, it is not sufficient for us to discover, that recovery generally follows salivation in Yellow Fever, though even this is contradicted by many very respectable authorities; but we must ascertain whether those practitioners who excite salivation in as many of their patients as may be susceptible of it, under that disorder, do in fact lose a smaller proportion of them, than those who purposely abstain from all endeavours to produce that discharge: and on this point I must declare that, after some experience, assisted by no ordinary portion of inquiry and information, I have not been able to discover that the salivators were more successful than the others. And, if not more successful, their practice has certainly been hurtful; because, in most of the persons who have recovered, the (perhaps useless) salivation had retarded the convalescence, and produced very troublesome affections of the tongue, mouth, and throat, with other ill consequences; as is well known and acknowledged, even by its advocates. Dr. Chisholm (at page 357, of vol. i. of his Essay,) warmly acknowledges his "obligations to Dr. Rush for supporting in a masterly manner," and "pursuing the mercurial mode of treatment," and expresses both "admiration and respect" for his "fortitude" in doing so. But Dr. Rush, notwithstanding this support and this fortitude, has candidly stated, that "in the City Hospital, (of Philadelphia) where bleeding was sparingly used and where the physicians depended chiefly upon salivation, *more than one-half died* of all the patients who were admitted."—(See page 128, of vol. v. of his Medical Inquiries and Observations.)—But great as this mortality was, it fell vastly short of that which occurred in a detachment of the Royal Artillery, placed under the care of Dr. Chisholm, when of twenty-seven recruits for that corps, who arrived in Grenada, in July, 1793, twenty-six were seized with the Fever, and of these,* twenty-one died before the middle of August ensuing, that is, in six weeks. (See page 133, of vol. i. of Dr. Chisholm's Essay.)—Upon the subject of this occurrence, Dr. John Hunter has ob-

* Among the cases given by Dr. Chisholm, in his first Appendix, the four which are numbered 9, 13, 14, and 15, (See pages 371, 383, 386, and 387, of vol. ii.) seem to be the cases of four out of the five survivors of the twenty-six men in question; and it is remarkable that in the two first of those cases very little mercury was administered, but the Peruvian Bark, with some opium, wine, &c was chiefly relied upon for the cure; and that in the two last, *no mercury whatever was given.* The twenty-five cases, therefore, before us, and their results, appear by no means to correspond with Dr. Chisholm's high commendation of, or professed confidence in, the mercurial treatment.

served, (at page 328, of his work on the Diseases of the Army in Jamaica,) that although "Dr. Chisholm had satisfied himself of the great virtues of Mercury, at least four months before, yet this is a mortality never exceeded in any Fever."*

To one who is sincerely desirous of discovering and adhering to the truth, it is extremely difficult to reconcile, or account for, the very opposite testimonies given on this subject; and the doing it would moreover be too invidious for me to attempt it. This, however, appears certain, that the good effects of the mercurial treatment have been greatly exaggerated by persons who either were deceived, or were willing to deceive others; that many persons have died of the Fever in question, although mercury, administered externally or internally, had produced a copious salivary discharge; and that, in very many others who have recovered, this discharge did not begin† until after a solution, or a great mitigation, of the disease had evidently taken place, which solution, or mitigation, therefore, could not have been the effect of salivation.— I cannot, with an eminent and respectable physician,‡ who treats of this practice, "aver that, although I have been called in to attend many under such circumstances, *not one* survived,

* Dr. Chisholm, who was likewise attacked by the same Fever in the same year, was himself more fortunate; for though he had recourse, at first, to the same remedy, he became convinced of its inefficacy, in his own case, early enough to call for the advice of Dr. William Munro, then of Grenada, and now of Demerary, under whose care he recovered, but, as I have been well informed, by a very different mode of treatment.

Dr. Chisholm, at page 237–9, of his second volume, mentions the great success of the mercurial treatment in the hands of his former partner in Grenada, Mr. W. Campbell; yet, as I have been assured from respectable quarters, this gentleman refused to take Calomel, when he was afterwards attacked by the Yellow Fever, of which he died.

† "Mercury *seldom* salivated until the fever intermitted or declined. I saw several cases in which the salivation came on during the intermission, and went off during its exacerbation; and many in which there was no salivation until the morbid action had ceased altogether in the blood-vessels, by the solution of the fever."—See Dr. Rush's Account of the Bilious Yellow Fever of Philadelphia, in 1794, in the 4th vol. of his Medical Inquiries and Observations, page 94

‡ See the "Essay on the Yellow Fever of Jamaica, by David Grant, M.D." page 51.

and that they became more victims to the mercury than even to the Fever;" but I can aver, that I had not a few opportunities of observing the effects of mercury given in this disease, while I served, in 1796 and 1797, as physician to the army, under Sir Ralph Abercrombie, in the West Indies; and that I saw nothing, which, to my understanding, could afford a proper encouragement to continue the mercurial practice; and this, I have great reason to believe, may be said of most of the other physicians and medical officers, who were then, or have been subsequently, employed with the British forces in the West Indies; and therefore, though I have adopted no invincible, nor, as I hope, unreasonable, prejudice on the subject, I cannot venture to recommend the use of mercury to excite salivation in Yellow Fever, without farther evidence of its utility. At the same time, I consider the use of it, in this disorder, as a purgative, to be highly beneficial.

Of mercurial frictions, which have been largely employed in the Yellow Fever, it may also be observed, that they do not seem likely to prove altogether innocent in those cases in which they may happen to do no good; for, besides the salivation which they may produce, when the patient lives long enough, and which is to be added to the number of his sufferings, already sufficiently abundant, the very act of rubbing-in the mercury tends greatly to disturb his body and mind, when his only wish is to remain unmolested; while the covering a large portion of the skin with a greasy ointment produces a considerable accumulation of heat therein, by which the general heat of the body, and with it many of the other febrile symptoms, will be increased.

END OF PART FIRST.

# PART SECOND.

Having given some account of the symptoms and treatment of the Yellow Fever, it seems expedient, as far as possible, to ascertain its cause: but in attempting to do this, I find the natural course and progress of my inquiry obstructed by certain doctrines, taught by authorities highly respectable, and almost generally adopted, though, as I think, without sufficient evidence; and I am, therefore, induced to enter upon a previous examination of these doctrines, in order to remove the obstruction occasioned by them, and clear a path which will, I hope, lead towards the truth.

To render this examination the more methodical and satisfactory, I beg leave to propose, for discussion, two problems, to which the doctrines in question seem to be referable, viz.—First, Are *all* fevers naturally contagious, or capable of exciting fever in other persons not predisposed thereto?

Secondly, Can a fever, strictly contagious, be *generated* by an accumulation of filth, or of putrifying, or putrid matters, or by the crowding of healthy persons into confined, or ill-ventilated, and unclean places?

The affirmative of the first of these problems has been asserted by Dr. Cleghorn,* Dr. Robert Hamilton,† Dr. John Clarke,‡ and more especially by the late Dr. George Fordyce. The last of these authors, in his Dissertation on Simple Fever,

* Observations on the Epidemical diseases of Minorca, third edition, page 132, &c.

† Observations on Marsh Remittent Fever, page 39, &c.

‡ Observations on the Diseases which prevail in Long Voyages to Hot Countries, vol. i. page 151, &c.

(page 113 and 114, second edit.) endeavours to maintain, that "by repeated experience, it is now known that, although it very frequently happens that a man coming near another afflicted with fever, is not afterwards affected with the disease, yet of any number of men, one-half of whom go near a person ill of this disease, and the other half do not go near a person so diseased, a greater number of the former will be affected with fever, than of the latter, in a short period afterwards. In some instances, the proportion is not very different; in others, the author has known seven out of nine, who went near a person afflicted with fever, seized with the disease, in the space of three weeks afterwards. There is, therefore, (adds he) a perfect ground, from experience, for believing, that coming near a person afflicted with fever is a cause of the disease."

This general indiscriminating assertion, if it were true, could only prove that some fevers are contagious,—not that all are so.—But the assertion is manifestly founded upon a supposed probability, or presumption, that such effects would result from the causes here described; for no one can believe, that an actual experiment was ever made by selecting a certain number of persons, and sending one-half of them into close communication with a febrile patient, and afterwards contrasting what happened to these, with the condition of those who were not allowed to approach any person labouring under fever. Nor would a single experiment afford any conviction on this subject, for reasons too obvious to require explanation. Much also would depend on the *species* of fever to which the individuals in question are supposed to have been exposed, which is not mentioned by Dr. Fordyce. Few persons, if any, doubt of the contagious quality of what is called Jail Fever, and few believe that intermittent fevers possess that quality.

The same author, at page 116, inculcates that "a peculiar matter is probably generated in the body of a man in fever, which, being carried by the atmosphere, and applied to some part of the body of a person in health, causes a fever to take place in him." This matter having no sensible properties, "its existence (says he) is only known by its effect in producing the disease." And in the next page he asserts, that "this infectious matter is produced by *all fevers whatever*;" but immediately adds, that, "as far as he knows, no person has been seized with fever, in consequence of coming near another person afflicted with it, where the fever consisted of one paroxysm only;" and he thereby, in effect, admits that he has gone beyond his knowledge, and contradicted his own uniform experience, in asserting that "this infectious matter is produced by all fevers whatever." And probably, if he had examined facts with sufficient accuracy and caution, he would have discovered that other fevers, beside those of one paroxysm, had occurred, which were not known to have ever re-produced fever in any other person.

In the same page, Dr. Fordyce declares, "that Intermittent Fevers produce this matter, or, in other words, are infectious;" and that he "knows this from his own observation, as well as that of others."

Here again the author seems to have hazarded an assertion, for which it is hardly possible that he could have had any certain foundation, because it is now known, as will be more fully stated hereafter, that the ordinary, and perhaps sole cause of intermittents, i. e. Marsh* miasmata, may remain inactive for several months after having been imbibed by a person in health, and, finally, produce fever, notwithstanding such long inactivity; and therefore, although Dr. Fordyce may have known persons to be attacked by intermittents, after having communicated with others labouring under that disorder, he could never have been certain that they had not, within the preceding six or eight months been exposed to these

* I beg to state, in this place, that, in joining the epithet marsh, or marshy, to the terms miasmata, exhalations, effluvia, &c. and in considering these as a cause of fever, I do not mean to intimate that such miasmata, &c. are emitted solely by marshes, (it being certain that they frequently arise from soils in a different state,) but only to designate the quality of those vapours, which are eminently the product of marshy grounds.

miasmasta, existing, as they do, in a variety of unsuspected places. He seems, indeed, to have been less confident in regard to these than in regard to other fevers, for he immediately subjoins the following concession, viz.—"But intermitting fevers are not nearly so apt to produce it, (the contagious matter,) or at least, to propagate it, as continued fevers; and the more violent the continued fever is in its febrile symptoms, the greater quantity of infectious matter is produced."

This general assertion of the contagious nature of all fevers without exception, is so important in the conclusions deducible from it, and so much at variance with the general experience of mankind, more especially in regard to those fevers, which so frequently follow any considerable exposure to the exhalations of marshy, or damp, soils during, or soon after, very hot seasons, that I feel it incumbent on me to contest this assertion; and in doing so, I beg leave to observe, that the kinds of evidence, which would be sufficient to produce conviction in a Court of Judicature respecting the ordinary transactions of our lives, would often prove fallacious in regard to medical facts, and especially those which relate to the existence and effects of contagion;—the former being cognizable by some of our senses, we are enabled to ascertain and testify the truth concerning them; but this rarely happens in regard to the latter; of which our belief frequently depends upon supposed causes and effects, whose existence and relations are not capable of being either seen, heard, or felt; and yet men will frequently imagine they have seen, heard, or felt, all that is necessary to warrant their belief; and will, in such cases, even assert what appears to them to be true as confidently as if their judgment had not been, in any degree liable to error.

Hence the works of medical writers abound with *supposed* facts, which are now known to have been more or less fallacious; and to this source of error, all proofs of the existence of contagion are particularly liable; because the matter, of which it consists, is not distinguishable by any of our senses; and we can, therefore, only presume its existence and agency by certain effects or events, which may be *suspected*, but can hardly ever be absolutely *proved*, to have resulted from it.—Aware of this difficulty, Dr. Haygarth has very properly made a distinction between facts adduced to shew the existence of contagion, from the circumstance of certain persons having been attacked by a particular disease, which last he names *affirmative* proofs; and other facts shewing the non-existence of contagion, from a number of persons having escaped the disorder, who had been fully exposed to the action of effluvia, supposed to be infected; these he calls *negative* proofs. "Observation, or experiment," says he, "can determine with much greater certainty what *does not*, than what *does*, give infection;" whence he justly concludes, that the negative proof is capable of being established by incomparably stronger evidence than the affirmative, and is therefore, in all cases, much better entitled to credit."*

Indeed, Dr. Fordyce maintains, page 110, of his first Dissertation, that, "in treating of Fevers, nothing is to be admitted as a cause, the knowledge of the action of which does not depend upon experiment;" and he observes, at page 112, that, "of the number of causes to which fever has been ascribed by the practitioners who have treated of this disease, few will bear the test of any strict inquiry:"—an observation which will I think, hereafter appear applicable to more than one of the causes of fever, which have been supposed by Dr. Fordyce himself, notwithstanding the great merit of his writings in many other respects. If contagion be a quality naturally belonging to all sorts of fevers, without distinction, they ought all to manifest this property in circumstances favourable to, and upon persons susceptible of, its action. That there are fevers, however, which do not manifest this property, or quality, in circumstances highly favourable to its operation, and upon individuals who must have been fully exposed to, as

* See his letter to Professor Waterhouse, at page 296, of his "Plan for exterminating the Small Pox."

well as susceptible of, its impressions, if such contagion had existed, may be demonstrated by hundreds, and probably by thousands, of well-authenticated facts, capable of infinitely overbalancing the supposed evidence derived from occurrences, in most of which it was easy to mistake, for the effects of personal contagion, those produced by morbid causes existing in the atmosphere, and derived from very different sources.

It will, however, be sufficient to adduce a few only of these facts at present, especially as I shall have occasion hereafter to mention a variety of others, of similar import, though for a different purpose.

Dr. James Lind, in his Essay on the Diseases incidental to Europeans in Hot Climates, (p. 27, fifth edition,) states the following fact,—viz.

"In the month of *August*, 1758, Admiral Broderick, in the Prince ship of war, anchored in the Bay of Oristane, (in Sardinia) where twenty-seven of his men, *sent ashore* on duty, were seized with the epidemical distemper of this island; *twelve* of them *in particular*, who had *slept on shore*, were brought on board delirious; *all* of them laboured under a low fever, attended with great oppression on the breast, and at the pit of the stomach,—a constant retching, and sometimes a vomiting of bile, upon which a delirium often ensued. Those fevers changed into Double Tertians, and afterwards terminated in obstinate Quartan Agues. It is worthy of remark, that in this ship, which lay only two miles distant from the land, *none were taken ill but such as had been on shore*, of whom seven died."

The same respectable author, at page 221, mentions another similar fact in the following terms,—viz.

"In a voyage to the Coast of Guinea, performed in the year 1766, by the Phœnix ship of war, of forty guns, the officers and ship's company were perfectly healthy, till, on their return home, they touched at the Island of St. Thomas.—Here the captain, unfortunately, went on shore, to spend a few days in a house belonging to the Portuguese governor of that island. This happened during the rainy, or sickly, season. In the same house were lodged the captain's brother, the surgeon, some midshipmen, and the captain's servants.—But in a few days after their being on shore, the captain, his brother the surgeon, and every one, to the number of seven, who had slept in that house, were taken ill; and all of them died except one, who returned to England in a very ill state of health.—The ship lay at anchor there twenty-seven days, during which time three midshipmen, five men, and a boy, remained on shore, for twelve nights, to guard the water casks, under pretence that the islanders would steal them; all of whom were likewise taken ill, and two of them only escaped with life.—At that island, *only those who slept on shore were taken ill; no other man* of the ship's company was seized with any distemper during their stay there. Even during the whole voyage, if we except these unfortunate persons, only one man died, and he was killed by an accidental blow upon the head.—None of those who slept on shore escaped the sickness, and of them only three survived it." And, at page 225, he adds, "In the year following, the Phœnix made another voyage to the Coast of Guinea, and happened again to touch at this island in the sickly season, where she lost eight men out of ten, who had imprudently remained all night on shore. At the same time, *the rest* of the ship's company continued in perfect health, who, after spending the greatest part of the day on shore, always returned to their ship before night. On board the Hound sloop, then in company with her, only one man died during the whole voyage; the officers having been particularly careful not to permit any of the people to continue all night on shore in that place. This man was cut off by an obstinate intermitting fever, with which he had been first seized at Sheerness."

The same author, in his "Essay on the most effectual means of preserving the health of Seamen," had previously observed, at page 57, "that the fever of the Island of St. Thomas, is, to a proverb, in that part of the world, deemed the most malignant and fatal species of any African or American Fever;"* and consequently, if Dr. Fordyce's doctrine were true, that all fevers are more contagious according as their symptoms are more violent, this fever ought to have been communicated to great numbers on board the Phœnix, whose intercourse with the sick in that ship must have been sufficiently near and frequent. The same want of contagion has, however, attended this fever on other, and, so far as I know, on all other occasions.

Dr. Trotter, late physician to the Royal Navy says, (See Medicina Nautica, vol. i. page 456,) "In a voyage down the Coast of Guinea, in the Assistance, in the year 1762, we had scarcely a man indisposed. We wooded and watered at the Island of St. Thomas, and with a view to expedition, a tent was erected on shore, in which the people employed on these services were lodged during the night. On the middle passage, every man who slept on shore died, and the rest of the ship's company remained remarkably healthy."

Of a similar nature are the facts which occurred in regard to the Ponsborne and Nottingham East Indiamen, at the Comora Islands; (See Medical Observations and Inquiries, vol. iv. page 156,) at one of these islands. viz. Mohilla, a great part of the crew of the former ship, after sleeping on shore in August, 1765, were attacked by a violent fever, which, in a few weeks, proved fatal to more than *seventy* of them; and, on the 16th of July of the following year, the Nottingham having anchored to the leeward of Johanna, (another of the Comora Islands,) and a considerable part of her crew having been sent, and allowed to sleep, on shore, they were attacked, soon after the ship had put to sea, by a severe remitting fever, of which

* Of the Island of St. Thomas, Dr. Robertson, Physician to Greenwich Hospital, observes, page 32, of his Meteorological and Physical Observations, &c. 4to. that "the town is built on the leeward-most part of the island, which is not at all cleared of the woods, nor the marsh drained; the consequence of which is, it is generally peopled from Portugal every *second* year, it proves so fatal to Europeans." For another proof of the danger of sleeping on shore on that island, see pages 33 and 98 of the same work.

several died. Of this fever, Dr. Badenoch, then surgeon of the Nottingham, observes, "*it infected only those that slept on shore*, and having gone through them, the fever ceased." And *this* he adds, "was *likewise the case with those on board the Ponsborne*, in regard to the Bilious Fever, which prevailed in that ship, at the island of Mohilla."

A similar occurrence is related by Dr. John Clark, in the first volume of his Observations on the Diseases which prevail in Long Voyages to Hot Countries, page 124; after describing the low place, "covered with impenetrable mangroves," at North Island, near the Streights of Sunda, where most of the East India ships take in wood and water for their homeward voyage, he adds, that "a Danish ship, in 1768, anchored at this Island, and sent twelve of her people on shore to fill water, where they only remained two nights. *Every one* of them were seized with a fever, of which *none recovered*; but although the ship went out to sea, *none*, except the twelve who slept on shore, were attacked with the complaint." Here again was a fever so violent as to kill every one in whom it was excited, and from a cause so powerful as to affect every one who was exposed to it, which, notwithstanding, did not reproduce itself in a single instance.

Many facts and occurrences, of a similar nature, might easily be added to the preceding; but they are all rendered superfluous by the notorious and unfortunate events, which have recently happened among the British officers and soldiers employed in Zealand, among whom, though near thirty thousand of them were attacked by fever, which proved fatal to nearly one-sixth of the whole number of sick, I have not been able, after much inquiry, to discover a single case, in which there has been reason to suppose, that any one person caught the fever from another. But, on the contrary, it appears to be the unanimous opinion of the army physicians employed on that service, (with most of whom I have conversed on the subject,) and also of the other medical officers, best qualified to judge of such matters, that no contagious quality accompanied the fever in question, either upon the island of Walcheren, or among the sick removed to this country. And I shall hereafter adduce the most convincing evidence, that in fact, these fevers were not of a contagious nature; and we may, therefore, consider ourselves as abundantly warranted in concluding, that *all* fevers are not endowed with a contagious quality, which conclusion is all that I propose to establish at present.

Problem II. Can a Fever, strictly contagious be generated by an accumulation of filth, or of putrefying, or putrid, matters, or by the crowding of healthy persons into confined, or ill-ventilated, and unclean places?

Most writers on the subject of Contagious Fever have either inculcated or believed, that it might be generated,—first by an accumulation of those disgusting matters, commonly denominated filth;—secondly, by the offensive vapours emitted by corrupting dead bodies, or by other matters in a putrid state;—and, thirdly, by crowding persons, even when healthy, in ill-ventilated and unclean places.

I have no desire to weaken any of the prejudices which tend to promote cleanliness in civilized nations, any further than is absolutely necessary for the manifestation of truth, on a question of great importance to mankind; and I flatter myself that we shall all find within ourselves sufficient motives to remove or avoid filthiness, even when convinced that it does not produce contagious fever. Whence the belief of its doing so was derived, I am unable to explain; but it has probably been confirmed by the frequent co-incidence of such fever, with nastiness and offensive smells in the dwellings of indigent people. There is, however, no necessary, or natural connexion between the former and the latter.

Dr. Fordyce asserts, (first Dissertation, page 115,) that he has known persons to be ill of the most infectious fevers, and to communicate fevers to others by infection, when there was no peculiar smell nor taste, nor any thing perceptible to the senses in the atmosphere surrounding them; and similar assertions have been made by Dr. Lind, and others, which I believe, are in conformity with the experience of all physicians. I know, indeed, that masses of animal and vegetable matters, and especially the former, while undergoing putrefaction, or other modes of decomposition, as in privies, &c. may give out vapours so condensed and noxious as to cause asphyxia, and sometimes almost immediate death, to those by whom they are inspired. But such mischiefs have no relation to fever; nor are those who recover from them, afterwards affected, in consequence thereof, by any febrile disorder. This is also true of the dangerous, and often fatal, effects produced by the fumes of charcoal, and the mephitism of mines, long-neglected wells, &c. which are not known to have ever produced fever; these do not however, properly relate to our present inquiry.

Every thing which I have been able to discover, or ascertain, respecting the nature and properties of contagion, induces me to consider each of its several species as a peculiar morbid quality, or power, imparted to certain animal secretions, in consequence of some particular, though unknown, actions excited in the living body, when actually disordered, by the very same species of contagion previously, and in like manner, elaborated in another body, whilst labouring under a similar disorder from a similar cause; and therefore, though we are unacquainted with the origin of any one species of contagion, yet, considering the properties manifested by all, ever since they have been known to exist, we may conclude, that being thus produced, exclusively by, and within, the living body, each is capable of exciting, in other living bodies, the same morbid action, or disease, which occasioned its own production, and of thus maintaining and propagating itself indefinitely; and consequently, that though contagion be a morbid and morbific secretion or production, it is also a natural one, wholly, *inimitable*, either by *accident* or art. If this be true, it must follow that, though noxious vapours should result from those fortuitous, and ever varying, collections of unclean or putrefying matters commonly denominated filth, which, as in the instance of marsh effluvia, may produce diseases, including fever, yet the diseases so produced will be incapable of exciting similar diseases in other persons, and will, therefore be destitute of the most essential property of contagion.

Indeed, if it were true that vegetable or animal matters, while decomposing or putrefying, could de novo generate contagion properly so called, the species or varieties of contagion ought necessarily to have become as numerous and various as the matters so decomposing, and also as various as their relative proportions;—every dunghill, every collection of rubbish and filth, ought to be capable of generating the cause of a new disease, and that disease ought to be capable of reproducing itself in other persons; and human existence, with such additions to the other dangers which surround us, ought to have become the most precarious, transient, and deplorable, of all the works of creation.

No person, who is even moderately acquainted with the subject, can believe that a disorder resembling Small Pox, (for instance,) and possessed of the same properties, could be created by any accidental collection, or even by the most artificial and scientific combination, of either organic or inorganic matters, not impregnated by the specific contagion of that disease. On the contrary, we have the strongest reason to believe that neither human ingenuity, nor any co-operation of natural means, could even alter the nature of variolous contagion; and that, in fact, it has continued, without any lasting change in its properties, ever since that unknown æra when its morbid action was first exerted upon mankind; though, having been successively transmitted through the bodies of, perhaps, several hundred thousands of individuals of different colours and temperaments, many of them probably contaminated at the same time by scrophula, syphilis, cancer, or other morbid taints, or infections, (and this, during the prevalence of numerous epidemical or pestilent diseases,) these ought, if any thing could, to have produced every degree of deterioration, of which the original virus was susceptible, and some permanent varieties, at least in this species of contagion.* We know, however, that its specific properties are invariably the same, and that the differences which are observed in its effects, depend upon causes connected with the individuals to whom it is respectively applied; the disease, cæteris paribus, proving no worse, when communicated by one dying of it in the most confluent and malignant form than it would have been,† if communicated by one recovering from the mildest product of inoculation. And we have similar reasons for believing that Measles, Chicken Pox, and other specific contagions, are equally permanent and unalterable. If then the powers of life, and the organs by which

* Dr. Adams, physician to the Small Pox Hospital, observes, at page 21 of his work on morbid poisons, in 4to. that, from the great affinity, or analogy between the variolous and vaccine contagions, "we might even expect that the characters of the two might be altered, by applying both at the same time, and also that the phenomena of one might imitate the phenomena of the other, in such a manner as to render the distinction between them doubtful. It is, therefore, a matter of surprise, that the distinction should be so regularly observed, and the laws which separate other morbid poisons be so rarely infringed."

He also remarks, at page 398, that "Small Pox and Cow Pox, contrary to the law of all morbid poisons, which are different in their nature, will proceed together in the same person without the smallest interruption of each other's course. If inserted nearly at the same time in the same person, each proceeds in the same course as if they were in two distinct subjects; if inserted nearly in the same spot, the two form *one common areola*, but the vesications are distinct, and each preserves its own character, till that of Small Pox becomes purulent from suppuration," &c. In this case, he adds, "you may take Small Pox matter from the pustule, which, by the adhesive inflammation, will remain distinct from, though seated in part of the Vaccine Vesicle; and from the other parts of the Vesicle you may take vaccine matter, and each will perpetuate its respective morbid poison."

Thus we find that, by the simultaneous association of two infections, so nearly alike, that the action of the one renders the body insusceptible to the action of the other, the energies of the constitution cannot produce even an intermediate, or hybrid contagion.

† At page 10, vol. i. of the Transactions of a Society for improving Medical and Chirurgical knowledge, Dr. G. Fordyce says, "I have the greatest reason to believe, that it is not of the smallest consequence, (in inoculation) whether the matter be of the mild or the confluent kind. I never knew of an instance of any other disease being communicated by inoculation of the Small Pox."

these contagions are successively renewed and perpetuated, cannot even *alter* the qualities or effects of the latter, by any of the changes which may be supposed to have taken place in their actions and in the fluids of the human body, from a variety of morbid, and other, causes excited in so many different individuals, it is credible, that putrefaction, which is but a natural separation of organised matters, previously held together only by animal or vegetable life, should be capable of *generating* a *new* contagion? Such matters spontaneously decomposing, and returning to their natural inorganic and harmless combinations, necessarily obey their respective chemical attractions; and the products resulting from this sort of obedience are as certain and constant as the formation of Sea-salt, by combining soda with muriatic acid. There is no chance, therefore, nor even possibility, of thus generating any thing so wonderful, and so immutable, as contagion, which, resembling animals and vegetables in the faculty of propagating itself, must, like them, have been the original work of our common Creator, and must have been continued in existence by the energies of a living principle, exerted successively in the different bodies, through which it has been transmitted from one generation to another. As well might we revive the for-ever exploded doctrine of equivocal generation, and believe, as formerly, that insects, reptiles, &c. are the offsprings of mere corruption, as to believe that a substance so analogous to them, in that most mysterious and essential function of self-propagation, could originate from that cause, or form any operation of chemical agencies alone.

As this reasoning, however, may not of itself produce general conviction, especially on strongly prejudiced minds, let us recur to matters of fact, and let these decide whether, in reality, any, and more especially a febrile, contagion has been produced by putrefaction. In former ages, when ignorance and credulity, which always accompany each other, were prevalent, many surprising aad alarming stories were reported, and believed, of widely-spreading diseases produced by this cause, and more especially by the putrefaction of animal substances. Fortunately, the truth, or falsehood, of such reports may be easily ascertained by facts within our own knowledge; for, as the same causes, cæteris paribus, must always produce similar effects, we have a right to expect that, if putrefying carcasses, fish, &c. were ever able to generate contagious, or other, fevers, they should still be able to do it, especially when collected in the largest masses, and when the impressions, to be made by their effluvia, are assisted by the most favourable circumstances.

Many writers of celebrity, and among them the great Lord Bacon, have thought that no effluvia were so infectious, and pernicious to mankind, as those which issue from putrefying *human* bodies; and, although a century and a half has elapsed since Diemerbroeck* attempted to convince physicians that, at least, such effluvia could not produce the Plague, yet the old opinion has kept its ground; and it is still believed, that, in their milder state, they may cause putrid fevers, and in their more concentrated state, a true pestilence. There are facts, however, on a large scale, which completely decide this question:—two of these deserve particular notice.—The first relates to the exhumations made in the church-yard of *St. Eloi*, at Dunkirk, in the year 1783; and the other to those made three years afterwards, in the church-yard of the Saints Innocens, at Paris. As the undertakings and results were similar in both instances, I shall, to avoid repetition, here describe only the latter, which I have preferred, because the corpses here taken up were much more numerous than at Dunkirk, and probably constituted the greatest mass of putrefying animal matter, of which we have any accurate information. The church-yard of the Saints Innocens, at Paris, situated in one of the most populous quarters of the city, had been made the depository of so many bodies, that, although its area enclosed more

* Tractatus de Peste, Lib. I. Cap. viii. p. 41

than 1700 square toises, or near two acres, yet the soil had been raised by them eight or ten feet higher than the level of the adjoining streets; and upon the most moderate calculation, considerably more than six hundred thousand bodies had been buried in it, during the last six centuries; previous to which date, it was already a very ancient burial ground.* Numerous complaints having been made concerning the offensive smells, which arose from this spot, and sometimes penetrated into the adjoining houses,† and the public mind being greatly alarmed, it was at last determined to forbid all future burials there, and to remove so much of the superstratum as would reduce the surface to the level of the streets. This work was undertaken in 1786, under the superintendance of M. Thouret, a physician of eminence in Paris, and in two years he accomplished the removal of that superstratum, almost the whole of which was impregnated, or *infected*, as M. Thouret styles it, with the remains of carcasses, and of quantities of filth and ordure, thrown upon it from the adjoining houses.

* In less than 30 years, more than 90,000 corpses had been deposited here by the last grave-digger. The poor inhabitants were buried in coffins made of very thin deal boards, and were regularly stowed as closely as possible, upon and beside each other, in large pits about thirty feet deep, and capable of receiving each from twelve to fifteen hundred coffins. These pits were gradually filled with coffins, and then covered over with earth about one foot in depth, and the bodies left to putrefy. But as the same space was commonly wanted in fifteen or twenty years for other bodies, this mass of animal corruption was then dug up, and a like number of recent corpses deposited in the same pit; and this operation was successively repeated through nearly the whole extent of the church-yard, from generation to generation, until the earth itself had been so completely supersaturated with human putrefaction, as to have no longer any action, or decomposing influence, upon bodies buried therein.

† According to a Mémoire on this subject, read at the Royal Academy of Sciences, by M. Cadet de Vaux, in the year 1781, " Le méphitisme qui s'étoit dégagé d'une des fosses voisines du cimetière, avoit infecté toutes les caves: on comparoit aux *poisons* les plus subtils, à ceux, dont les Sauvages imprégnent leur flèches meurtrières, la terrible activité de cette émanation. Les murs baignés de l'humidité dont elle les pénétroit, pouvoit communiquer, disoit on, par le seul attouchement, les accidens les plus redoutables."——See " Mémoires de la Societé Royale de Médecine," tom. viii. p. 242; also Annales de Chimie, tom. v. p. 158.

"The exhumations," says this gentleman, (in the narrative of them, which he published in the Journal de Physique, for 1791, page 253) "were principally executed during the Winter, but a considerable part of them was also carried on during the *greatest heats* of Summer. They were begun with every possible care, and with every known precaution; but they were afterwards continued, almost for the *whole* period of the operations, without employing, it may be said, *any precaution whatever;* yet no danger manifested itself in the whole course of our labours,—no accident occurred to disturb the public tranquillity."* This account is authentic,—and was read before the Royal Academy of Sciences at Paris. It is moreover confirmed by the report of M. Fourcroy, who was joined in this commission with M. Thouret for certain chemical objects, which report was also read at the Academy, and is printed in the sixth volume of the Annales de Chimie. If this result from taking up nearly twenty thousand bodies, in different stages of putrefaction, be insufficient alone for my purpose, there is another almost equally conclusive in its nature and extent.

It is well known that M. Berthe, Professor in the School of Medicine, at Montpellier, and two of his colleagues in that University, were sent, by the government of France, into Spain, to examine, and report upon, the nature of the Yellow Fever, which had proved so fatal in several towns of Andalusia, in 1800. M. Berthe has published the report of the

* It does not appear, after the fullest inquiry, that any febrile disorder was ever produced by this immense mass of corruption, during the removals made in 1786, &c. or while it was suffered to remain as a burying ground. The grave diggers were, indeed, sometimes thrown down suddenly, and, for a time, deprived of sense and motion, (as in what is termed *Asphyxia*,) by the concentrated vapours which escaped, upon accidentally breaking open, by their spades, the abdominal viscera of bodies, in an early stage of putrefaction. These vapours also, in a more diffused state, are said to have sometimes produced nausea, loss of appetite, and, in a course of years, paleness of countenance, debility, tremors, &c. But fever of any kind, and much less *contagious fever*, does not appear to have been noticed, as resulting from the offensive, or putrid matters of this church yard, either to the grave diggers, or to the neighbouring inhabitants.——See Annales de Chimie, tom. v. p. 154, &c.

commission, of which he was a member, and in it has mentioned that, being at Seville only a few months after the epidemic had ceased, he frequently visited the burying places just without the city, in which the victims of the fever had been interred; that, in these excursions, he was accompanied by the French Consul at that city, and had occasion to converse much with the guards stationed at these places, and with the grave-diggers still employed in them: and he states, that, besides these, many thousands of the inhabitants of Seville also came thither, some from curiosity, and others in processions, to testify their sorrow and respect for their departed friends. In one of these grounds, south-westward of the city, ten thousand bodies had been buried; in two others seven or eight thousand; and in that of Triana about four thousand.

"The heats of the Spring," says M. Berthe, (which, I need not observe, are considerable at Seville) "were, at this time, beginning to be felt, and the ground of these burial places, being clayey, was already cracked into wide and deep crevices, through which a fœtid odour was exhaled, the result of the decomposition which was going on among these heaps of bodies."*

Filled with alarm at the calamities which might be produced by such masses of putrefaction, M. Berthe, and his colleagues, represented these supposed dangers to the Spanish government; and then went to Cadiz, where they found the churches more or less filled with putrid emanations from the same cause: but as they did not discover that these supposed fomites of infection were productive of any mischief, their fears concerning them seem at length to have subsided completely; for, in their reply to the President and Members of the Board of Health, who had requested a statement of their opinions, they expressly declare their belief, that "if the Yellow Fever could be produced by the effluvia arising from

* See page 28 of "Précis Historique de la Maladie qui a regné dans l'Andalousie, en 1800, par I. N. Berthe, Professeur de l'école de Médecine de Montpellier," &c.

putrefying bodies, it was evident that such a misfortune must already have taken place, through the imperfect manner in which the tombs and vaults, pointed out by them, had been closed,—a defect which they had observed even in the churches that they were most frequented."* Thus it appears that the putrid emanations from the bodies of many thousand persons, who had recently died of the Yellow Fever, did not, and therefore could not, produce that disorder.

To the preceding facts I may add another, which is related by a man whose veracity is as little to be questioned, as his exalted philanthropy,—I mean John Howard, in his work on Lazarettos, page 25.

" The governor, at the French Hospital at Smyrna, told me, (says Mr. Howard) that, in the last dreadful plague there, his house was rendered almost intolerable by an offensive scent, especially if he opened any of those windows which looked toward the great burying ground, where numbers were left, every day, unburied; but that it had no effect on the health of himself or his family. An opulent merchant, in this city, adds he, likewise told me that he and his family had felt the same inconvenience without any bad consequences."

If the exhalations from piles of bodies destroyed by the plague itself, and corrupting in the open air, were thus incapable of generating the contagion either of fever or of plague, even during the prevalence of a pestilential constitution of the atmosphere, (if any state of the atmosphere ever deserved that title) it may, I think, be safely affirmed, that there are no circumstances under which putrid animal matter can be supposed ever to produce febrile contagion.

I have now before me a great number of similar facts, well authenticated; but those which I have just stated will, probably, suffice to convince most of my readers, that if putrefying animal matters are not completely harmless, they are, at least, innocent of the charge of producing *contagious fevers;*

* See page 331 of M. Berthe's work.

and, therefore, I shall content myself with referring those who may desire further evidence on this point, to Appendix, No. 2, where they will, I believe, find rather a redundancy, than a deficiency, of such proofs.

Whatever variety of sentiment may have been entertained with respect to the supposed generation of infection by filth, or by putrefying bodies, it appears to have long been an universal opinion, at least among those who have admitted the existence of any infectious fevers, that, to use the words of Dr. Cullen, (first lines of the Practice of Physic, Sec. lxxxi.) "the effluvia constantly arising from the human body, if long retained in the same place, without being diffused in the atmosphere, acquire a singular virulence; and in that state, being applied to the bodies of men, they become the cause of a fever, which is highly contagious."

This opinion is become so familiar, that few persons hesitate to adopt it, however difficult it be to comprehend by what means these effluvia can acquire such contagious properties.*

* Dr. Chisholm, in his Essay on the Malignant Pestilential Fever, &c. page 281, of vol. i. includes, among the causes of that disease, "the product of animal substances of every description, deprived of life, and in a state of putrefaction, which, exhaling azote and oxygene chemically combined, and diffusing through the atmosphere to a certain extent the basis of pestilential infection, are equally capable of producing contagious and pestilential diseases." The same author has again recently delivered this doctrine in several parts of his letter to Dr. Haygarth, (8vo. 1809,) particularly at page 133, where he mentions, as a "*most important fact* in medical physics, that the vapour, or exhalation, arising from animal matter, accumulated in a putrid state, and rendered stationary by the neglect of ventilation, is universally the cause of the fever of infection,—the *Typhus*, which annually diminishes the population of these cities," &c. "The same causes, (he adds,) probably gave origin to the *Plague*." Very soon after this, however, (i. e. in October last,) Dr. Chisholm, with laudable candour, thought proper, in a great degree, to retract this doctrine, by asserting,

"That the effluvia from dead animal bodies, passing through the natural process of putrefaction, and unrestrainedly diffused through the atmosphere, is injurious to living animal bodies exposed to their action, no more than inasmuch as their fœtor is offensive to the olfactory nerves; that, when confined to a very limited space, and their principles, instead of entering into new combinations, are concentered, and in that state applied to, or received into, the bodies of living animals, these effluvia may act as a poison, producing in the living animal frame fever perhaps, but *incommunicable*, or incapable of propagation by contagion; or instant death by a sud-

We can all understand that if a person under an infectious disorder be confined in an ill ventilated room, the infectious effluvia, when they are of a powerful nature, will, with the other emanations from his body, be gradually accumulated, and the atmosphere of that room may thus, at length, become loaded with infection; but, that the emanations from a person who was not ill of an *infectious* disease, should ever undergo so remarkable a change in their nature, as from being innoxious, to acquire not only the power of producing a disease, but a contagious disease, capable of regenerating itself in other persons, seems to me incredible.

I have already remarked, particularly in regard to the Small Pox, that the human body has no power even to alter, in the slightest degree, a contagion already existing therein; and that it must be infinitely more difficult for it to generate one entirely *new*. If it were otherwise, with what certainty would it not be effected in a variety of places, which are entirely exempted from it. Take, for instance, those in which the natives of Kamstchatka dwell constantly during seven months of the year, and which are called yourts; these are sunk seven or eight feet below the surface of the ground, and are covered with a thatched roof, in the form of a truncated cone, open at the top; they consist of one small apartment, which usually contains six families with their utensils, and stock of provisions for the winter, the chief part of which is dried fish almost putrefied.

If the combination of personal nastiness, with the most

den exhaustion of the living principle."—See his paper in the Edinburgh Medical and Surgical Journal, Oct. 1810, page 389.

But though Dr. Chisholm has so far returned towards what I believe to be the path of truth, he still adheres to the commonly received opinion of the generation of *contagious* fever, by *crowding*, and *deficient ventilation*. "The cause, in fact, (says he,) of *Typhus*, is, I believe, an undefined change in the atmospheric air, brought about by its confinement in a very limited space, and incapacity, in a great degree, of renewal, and the respiration of an effluvia, (effluvium) emanating from the persons inhabiting the wretched close dwellings in which the fever is found."—See Note to page 391, of the Edinburgh Medical and Physical Journal, Oct. 1810.

putrid smells and foul air, were capable of creating the contagion of fever, every yourt would necessarily be a fomes of infection. "Here they eat, drink, and sleep, crowded promiscuously together; and satisfy all the calls of nature without modesty or restraint, and never complain of the noxious air that prevails in these habitations.* Yet, instead of being generally attacked by contagious fevers every winter, they seem to enjoy as good health during this season of confinement as any other people; and fevers are not even mentioned in the list of diseases, which that respectable traveller, M. Lessep, either observed, or heard of, as existing among them.

The people of the island of Oonalaska, also, "inhabit jourts, or subterraneous dwellings, each common to many families, in which they live in horrible filthiness;"—(Pennant's Arctic Zoology, vol. i. page cliv.) and the Samoiedes live in subterraneous dwellings, equally filthy, for almost nine months in the year, who yet are reported by travellers to be strong, active, and healthy. In addition to all this filth, crowding, and want of ventilation, the *food* of these people may be considered as little better than putrefaction itself. Mr. Pennant, describing that of the natives of Kamstchatka, says, "their ambrosial repast is the Huigal, or fish flung into a pit until it is *quite rotten*, when it is served up in a state of carrion, and with a stench that is insupportable to every nose but that of a Kamschatkan." But these people, notwithstanding, are seldom attacked by any other disease than scurvy, for which they seem to possess a remedy in the Allium Ursinum, or Wild Garlic, and in the Pinus Cembra.†

The Greenlanders and Esquimaux appear, by the accounts of those celebrated navigators, Davis, Frobisher, Baffin, Henry Ellis, &c. as well as of Bishop Egede and Crantz, to live, during the greater part of the year, in very close, ill-

* See Lessep's Travels, page 230, &c. also Pennant's Arctic Zoology, vol. i. page cxxxii. also Voyage to the Pacific Ocean, &c. vol. iii. page 374.

† Arctic Zoology, vol. i. page cxix. also Lessep's Travels, page 90.

ventilated, and crowded habitations, (without chimneys) which, notwithstanding the great severity of the cold, they keep extremely warm by their numbers and breath, assisted by a single burning lamp in each, and by excluding fresh air so completely, that any other people would think themselves in danger of being suffocated by the offensive vapours continually exhaling from the lungs and bodies of the inhabitants, and which involve them as a thick fog; and yet fever of any kind is a rare disease among these people, though, like those of Kamstchatka, &c. they are much disposed to scurvy.

Dr. Matthew Guthrie, Physician at St. Petersburgh, in a letter to Dr. Priestley, on the Antiseptic Regimen of the Natives of Russia, inserted in the sixty-eighth volume of the Philosophical Transactions, mentions, at page 623, that " the Russian boor lives in a wooden house," " caulked with moss, so as to be snug and *close*. It is furnished with an oven, which answers the triple purpose of heating the house, dressing the victuals, and supporting, on its flat top, the greasy mattrass on which he and his wife lie."

" During the long severe winter season, the cold prevents them from airing this habitation, so that the air cannot be very pure, considering that four, five, or six people eat and sleep in one room, and undergo, during the night, a *most stewing* process from the heat and closeness of their situation, insomuch that they have the appearance of being dipped in water, and raise a steam and smell in the room, not offensive to themselves, but *scarcely supportable* to the person whom curiosity may lead thither."

" Now, if it be considered that this *human effluvium* must adhere to every thing in the room, especially to the sheepskins, or mattrass on which they sleep, the moss in the walls, &c. and that the apartment is *never ventilated for six months at least*; at the same time that these people are living upon salt-fish," &c. " and the whole time without fresh vegetables," &c. " If it be a fact that they are, in spite of all these predisposing causes, *strangers to putrid disease*, it will sufficiently justify my first assertion, that the regimen, nature has dictated to these people, is highly antiseptic."

Dr. Guthrie had, in a preceding part of this letter, stated that, notwithstanding this mode of life, "the Russian boor enjoys a state of health that astonishes an inhabitant of a country where the dreadful consequences are so well known of bad air within, excessive cold without, joined to a want of fresh vegetables for a length of time."

Dr. Guthrie has stated these facts principally to shew the supposed beneficial effects of the Russian drink called Quass, &c.; but I am entitled to avail myself of them for the purpose of demonstrating, that long confinement in close unventilated houses, without chimneys, in an atmosphere replete with human effluvia, and in very cold weather, when Typhus or Contagious Fever is commonly most prevalent, does not produce that disease, it being, as will hereafter appear, unknown in that part of the world.

Dr. Charles de Mertens, an eminent physician who had resided many years in Russia, writes, in a letter from Vienna, dated January 14, 1778, and printed in the same volume of the Philosophical Transactions, page 661, &c. that "the common people (at *Moscow*) live in small wooden houses, generally very low, in which they *crowd* together both night and day, during three parts of the year, on account of the great cold. There is little air in the room, the windows of which are very small. Here they *stew* together in humidity and nastiness; for except the bath, which they use once a week, they are extremely nasty." These people, he observes, enjoyed, notwithstanding, a much better state of health than the higher classes of society at Moscow, who were frequently attacked severely by scurvy.*

* From the description given by Dr. Orræus, (Politiæ Petropolitanæ Medicus,) the habitations of the Russian and Polish peasants seem, at least, to equal those of the poor of any other country, in accumulated filth and foul human effluvia. His words are as follow, viz.—

"Eccui ignota es pauperculorum vivendi ratio? Degunt cumulatim in domun-

Having stated these facts in regard to the supposed effects of crowding human beings in small unventilated habitations, in northern countries, let us see what effects result from similar causes in the warmer regions. And here the African slave ships most obviously present themselves for examination. Until within a few years these vessels notoriously conveyed human beings across the Atlantic in a state of closer compression, and in an atmosphere more offensively impregnated with human exhalations, excretions, and excrement, than could probably be found in any other place of confinement. "The poor wretches (says Dr. Lind, in his treatise on the jail distemper) are crowded together below the deck, as close as they possibly can lie, with only a small separation between the men and women; every night they are shut up under close hatches, in a sultry climate, barred down with iron to prevent an insurrection;" "and though some have been suffocated by the close confinement, or foul air, though they are subject to the flux, and suffer from a change of climate, yet an infection is scarce known among them; or if an *accidental* Fever, occurring from the change of climate, should become infectious, it is generally much more mild than in the opposite situation,"—i. e. that of ragged felons under transportation. It will be here observed, that Dr. Lind, influenced as he was by the commonly-received opinions, mentions an infection (meaning of Fever) as being "*scarce known,*" in the slave ships, instead of asserting, as he might have done

culis depressis, angustis, humidiusculis; esculenta & potulenta sua, partim jam corrupta & fermentantia in iisdem, vaporibus empyreumaticis obnubilati, apparant; quisquilias raro everrunt; illuvies varias, negligentius quaquaversum profundunt; ut alias immundities ex infantibus & propriis excretionibus provenientes, taceam. Mephitidi hinc productæ assueti, de renovando aere vix cogitant. Uti Jassiæ, sic etiam in Polonia, ubique fere inter Judæos pauperiores, sordide omnino & arcte, uti notum est, viventes, prima pestis quasi incubatio fiebat. Medici, qui Moscuæ, in officinam pannorum, ad examinandum infectos, repetitis vicibus mittebantur, de fœtore in habitatiunculis operariorum, cui vix per aliquot minuta perferendo pares essent, conquerebantur."—Vide "Gustavi Orræi, M. D. Descriptio Pestis quæ anno MDCCLXX in Jassia, & MDCCLXXI in Moscua grassata est. Petropoli, 1784."—Page 51.

with truth, that it is *never known:* for after very extensive inquiries, I am fully convinced that Fever of any kind rarely occurs on board these vessels, and *contagious Fever never;* though great mortality has frequently happened from other diseases, and more especially from Dysentery.* Dr. Trotter, who was formerly surgeon to a slave ship, after noticing what I have just stated from Dr. Lind, adds, "The confinement of so many wretched creatures in a *small* space, deservedly attracted the animadversion of a physician investigating the sources and progress of contagion. But *Contagious Fevers* we find are not their diseases." See Medicina Nautica, vol. i. p. 184.†

I could readily accumulate proofs in confirmation of the preceding statement, concerning slave ships; but the truth in regard to it is now so generally known and acknowledged, that they must be unnecessary; and there certainly is nothing in the constitutions of Negroes, which exempts them from Typhus or Contagious Fever; on the contrary, they have been found as susceptible of it as Whites, and considerable numbers of them, who were sent from this country, and from Nova Scotia, to the new colony of Sierra Leone, died of it on their passage thither, as will be more fully related in another place.

An instance of the crowding of Europeans on ship board, which approaches very nearly to that of Negroes in slave ships, may be found in the "Narrative of the deportation to Cayenne," of J. J. Job Aimé, and one hundred and ninety-two other persons, on board the Decade frigate, in conse-

* See Lind on preserving the health of Seamen, page 317, 318, Second edition.

† Dr. Garden, in a letter to the Rev. Stephen Hales, D. D dated Charlestown, South Carolina, March 24, 1756, after mentioning the Guinea slave ships arriving there, adds, "I have often gone to visit those vessels on their first arrival, in order to make a report of their state of health to the governor and council; but I never yet was on board one, that did not smell most offensive and noisome: what from filth, putrid air, putrid dysenteries, (which is their common disorder) it is a wonder that any escape with life." See Dr. Hale's Treatise on Ventilators, second part, page 95

quence of the revolution (in France) of the 18th Fructidor, (Sept. 4th,) 1797, written by himself, and printed for J. Wright, Piccadilly, 1800. In this narrative the writer says, (page 78) "we were placed in the between-decks, before the fore-mast and main-mast, occupying nearly one-fourth of the superficies of the vessel, having about four feet and a half in height, and receiving no light but by the scuttles; that is to say, by two openings of three feet square."—"Partitions had been made in this part of the between-decks," &c. "In this place, the door of which was locked, were crowded and squeezed together 193 individuals, mostly aged and infirm. We lay in two rows one over the other, forming as it were, two stories, in hammocks of coarse cloth, and extremely narrow." "Those above could not raise their heads without hitting those above; neither could any of us make the smallest motion without disturbing his neighbours; for we all touched each other, and, not having the least spare room, formed, as it were, but one mass." "And that nothing might be wanting to increase the horror of our situation, as we were not permitted to go out for fourteen hours together," (i. e. from 6, P. M. until 7½ A. M.) "and sometimes more, tubs had been placed in the midst of us, where we might satisfy the indispensible wants of nature; and to get to these sorry receptacles, we were obliged to creep, on our bellies, beneath the hammocks. How insupportable then must have been the infection of such a close confined place, which was already poisoned by our own exhalations! Indeed, the air, which passed from this hole, was so hot and fœtid, that the centinels, placed at the hatchways as our guard, demanded that the time of their duty, at so dangerous a post, might be shortened."

In addition to this morbid atmosphere, the exiles, most of whom had been "accustomed to the elegance of life," were condemned to subsist upon the coarsest, the most disgusting, half-putrefied food, in the taking of which, says Aimé, "we resembled a flock of animals who eat their food out of one common trough," and were, besides, made "a subject of mirth," by "the officer who superintended the distribution of our meals," which were also too scanty to satisfy the cravings of hunger,—(page 82 to 85.) They were also condemned to endure the greatest and most offensive personal filth, swarming with lice, &c. "If it be recollected, (says the author) that we were obliged to sleep in our clothes, and when it is known that several of us had not taken off our lesser garments during the voyage, it may be easily conceived that it was not our linen alone, into which these horrible vermin had introduced themselves."—(Page 81.) "Our blood, it is true, was not shed, (says he,) but there was not one of us, who would not have a thousand times preferred a speedy death, to the miserable state in which we existed."—(Page 85.) But though they were kept in this state during ninety-six days, and, to use the words of Aimé, "there was every reason to expect, that one half of us would have been the victims of such inhuman treatment; nevertheless, astonishing as it must appear, under these circumstances, *not one of us perished.*"—(Page 85.) They were, indeed, as might well be supposed, attacked by scurvy and other disorders, some of which are called fevers, though the latter appear to have been so slight, and of so short a continuance, as hardly to deserve that name; but certainly nothing like contagious fever existed among them, or could have existed in such circumstances without extensive mischief. Indeed there was only one person lost during the voyage, and he (a sailor) accidentally fell overboard. And yet here was every thing likely to generate febrile contagion, (if it could be generated by crowding, want of ventilation, filthy clothing, and unwholesome, corrupting food, together with anxiety and dejection of mind, &c.) to a much greater extent than in any gaol within Great Britain.

In Dr. Lind's Essay on preserving the Health of Seamen, page 195, I also find the following statement, viz.

"During the month of October, (1759) the squadron arrived from the West Indies, after the reduction of Guadaloupe, so over-run with the scurvy, that, when in the channel, ten or a dozen persons usually died of it every day. Out of three hundred and fifty scorbutic patients, who were sent ashore from those ships, there was *not one who had a fever*. This I mention, (says Dr. Lind,) for the sake of the following remark: The surgeon of the Panther" (of sixty-four guns) "told me, that forty of her men had died of the scurvy in their passage home; and, during that time, there were usually ninety patients in the sick apartment. The place appropriated for the sick, was in the *bay* of the ship, (which Dr. Lind calls "the most damp and unwholesome part of a ship," page 133) "and had no pipe from the ventilator, nor any scuttles cut through its sides, for the admission of the fresh air. A number of patients, thus closely crowded together, rendered the place so disagreeable and suffocating, that the sick were in a manner stifled or want of air. The surgeon, when visiting, could scarcely breathe in it, or remain for any length of time, without being obliged to have recourse often to the fresh air upon deck, and sometimes to spirit of hartshorn, or to a glass of wine, for his immediate relief. He observed, that both the virulence and mortality of the scurvy were heightened by the unventilated air of the place, in which the sick, for several weeks, had been confined; yet, out of above an hundred patients, sent to the hospital by this surgeon, not one was remarked to have any symptom of contagion generated in that apartment."

Another fact, which deserves mentioning, relates to the prisoners taken out of the memorable Spanish galeon, captured, by Commodore Anson, in the Centurion, on the 20th of June, 1742, and is recorded at pages 492 to 496, 15th edition, of the Account of the Commodore's Voyage round the World, published by Mr. Richard Walter, who had accompanied him as his chaplain.

"The galeon had five hundred and fifty men at the beginning of the action," of whom "sixty-seven were killed, and eighty-four wounded." All the prisoners were "sent on board the Centurion before night, except such as were thought the most proper to be retained to assist in navigating the galeon." The prisoners were "placed, all but the officers and the wounded, in the hold, where to give them as much air as possible, two hatchways were left open." "The sufferings of the poor prisoners, though impossible to be alleviated, were much to be commiserated; for the weather was extremely hot, the stench of the hold loathsome beyond all conception, and their allowance of water but just sufficient to keep them alive. All this considered, it was wonderful that not a man of them died during their long confinement," (from June the 20th to July the 28th,) "except three of the wounded, who expired the same night they were taken."

An additional proof of the like import may be derived from the dreadful catastrophe, in the black-hole, at Calcutta, on the 20th of June, 1756, in which, out of one hundred and forty-six persons, one hundred and twenty-three perished by suffocation. And a further reason with me for noticing it is, to correct the misrepresentations thereof, which I have heard and seen; for it has been asserted, that the twenty-three survivors were afterwards seized with Typhus Fever, as, indeed, they ought to have been, if crowding, with an accumulation of human effluvia, and want of ventilation, could produce it. But whoever will read the narrative of this occurrence, given by Mr. Holwell, the chief officer of the British factory at Calcutta, (which none of the medical writers I allude to seem to have perused, or, at least, not with due attention,) will, I am sure, be convinced, from all the subsequent events, that not one of the survivors in question was attacked by any disease which could, with propriety, be called a fever. It was impossible indeed for men, who had undergone such extraordinary sufferings, and had preserved their existence with so much difficulty, not to feel exhausted and indisposed, when they were released from their dungeon the next morning; and it seems that, within forty-eight hours, every one had a considerable eruption of boils over his body, which was probably caused by the excessively profuse perspiration, which each of them had undergone, and is not a rare consequence of very copious sweating. But, as I shall demonstrate in the Appendix, No. 3, a fever did not ensue in a single individual among them; and, therefore, no febrile contagion was generated, even in an atmosphere rendered pernicious to life, and not only loaded with effluvia perspired from the living body, but also with the most offensive smells from those who expired in the course of the night, and whose bodies had fallen into rapid putrefaction, as soon as life was extinguished.

The Lord Chancellor Bacon seems to have been strongly impressed with a belief of the existence of contagion in prisons, and of its being, at least, greatly *augmented*, if not *generated*, by filth and deficient ventilation. "The most pernicious infection, (says he,) next to the Plague, is the smell of the Jail, where prisoners have been long, close, and nastily kept; whereof we have had, in our time, experience twice or thrice, when both the judges that sat upon the Jail, and numbers of those who attended the business, or were present, sickened upon it, and died."*—(In Sylva Sylvarum, Cent. 10, Num. 914.)

* Mr. Anthony Wood, in his "History and Antiquities of the University of Oxford," published by John Gutch, M. A. Oxford, 1796, after mentioning the Black Assize in that city as one of the "Mortalities," which Lord Bacon must have contemplated in the passage just quoted, adds, "where the other happened I am not certain; however, that the like was at Cambridge, at the Assize kept in the Castle there, in the time of Lent, 13th of Henry VIII. Ann. Dom. 1521-2, is evident; for the justices there, and all the gentlemen, bailives, and all resorting thither, took such an infection, that many of them died; and almost all that were present fell desperately sick, and narrowly escaped with their lives."—Vol. ii. page 188, &c. This seems to have been the earliest instance of what, perhaps, may be considered as jail infection, communicated in a Court of Justice, of which any information has been transmitted to us; but Lord Bacon could not, with propriety, have mentioned it, as occurring in his time; it having happened forty years before his birth.

One of the instances here alluded to, doubtless, was that of the memorable *Black Assize* at Oxford, in the month of July, 1577, which I shall more particularly notice in the Appendix No. 4. The other instance seems to have been that mentioned by Holinshed, as occurring at Exeter, during the Assizes there, in March, 1586, of which a further account will be found in the same Appendix.

From that time I can discover no instance of any remarkable mortality or sickness, supposed to have been produced by Jail infection, until the year 1730, (an interval of one hundred and fifty-three years,) when, at the Lent Assizes, some prisoners, who had been removed from Ilchester Jail, to take their trials at *Taunton,* were believed to have infected a part of the Court, and produced a contagious disease, of which the Chief Baron Pengally, with some of his officers and servants, and Sir James Shepard, Knight, and Serjeant at Law, died afterwards, at Blandford, in Dorsetshire. John Pigot, Esq. High Sheriff of Somersetshire, also died, as was supposed, of the same disease, which spread considerably at Taunton, and proved fatal to several hundreds.—(See Gentleman's Magazine, for May, 1750.)

Twelve years after, viz. in April, 1742, according to Dr. Huxham, (de Aëre, &c. vol. ii. p. 82,) a fever, which he calls putrid, contagious, and highly pestilential, ("febris putrida, contagiosa ac pestifera valdè,") appeared at, and in the neighbourhood of Launceston, occasioning great mortality there. This fever, he adds, was generated in the prisons, and widely disseminated by means of the County Assize,—("genita hæc in carceribus febris et per comitia provincialia disseminata longe latéque.")*

* At page 83, Dr. Huxham makes this addition, viz.—" Perfrequens est utique generatio febris pestilentis in angustis immundisque carceribus ; etiam ipse aer conclusus in fodinis, speluncis, puteis, tandem evadit exitialis admodum idque longe citius, si accedunt quoque plurima animalium effluvia, quæ et ipsa porro magis magisque in horas violenta fiunt, brevique pestifera maximè."—(Here he refers to Lancisi de repentinis mortibus, L. i. C. 6) " Atmosphæra stagnans, frequentia hominum polluta, mox valdè rancet & ad respirationem inepta est prorsùs ; imo aquæ dulcis balneum

The next remarkable occurrence of this sort happened at the Sessions of the Old Bailey, in the spring of 1750, which proved fatal to the Lord Mayor, and two of the Judges, with several eminent and other persons, who, as was asserted, and is now generally believed, were infected by the contagion of Jail Fever brought into the Court from Newgate. With how little reason or truth this assertion was made, I shall endeavour, by a minute examination of facts, to ascertain, and demonstrate, in the Appendix, No. 4. And this task I have the more readily undertaken, by reason of the very important conclusions respecting febrile contagion, which have been, as I think, erroneously deduced from the melancholy events in question.

It was in consequence of, and immediately after, this memorable transaction, (viz. in May, 1750,) that Sir John Pringle published his "Observations on the nature and cure of Hospital and Jail Fevers," from pages four and five of which the following extract is made, viz.

"The hospitals of an army, when crowded with sick, or when the distempers are of a *putrid* kind, or at any time when the air is confined, especially in hot weather, produce a fever of a malignant nature, always accounted fatal. I have observed the same sort of fever to take its rise in crowded barracks, and in transport ships, when filled beyond a due number, and detained long by contrary winds, or when the men were kept at sea, under close hatches, in stormy weather."

"The cause seems plainly to arise from a corruption of the air, pent up, and deprived of its elastic parts by the respiration of a multitude; or more particularly vitiated with the perspirable matter, which, as it is the most volatile part of the humours, it is also the most putrescent."

As soon as I became acquainted with this fever in the hospitals abroad, I suspected it to be the same with what is called

sorde cutaneâ fœdatum putrescit atque putet brevissimè. Nec mirum est hoc utique, quandoquidem a quolibet adulto homine unciæ 40 feri rancidi vaporis quotidie exhalant."

here the Jail Distemper, which I had never seen; and was confirmed in my opinion, by having an opportunity of comparing them, which was furnished by the following accident."

Here the author relates the means by which two hundred men, of Brigadier Houghton's regiment, were, in 1746, attacked by a "fever, which came directly by *contagion from the true Jail Distemper*," communicated in a manner which he describes: and these men, he adds, "being under my care, I had the best opportunity of examining the distemper, which I found differed in nothing from the usual Hospital Fever, in either symptoms, violence, or cure." And on this foundation he proceeded to "consider the two diseases as *one*," and to describe them accordingly; having, as he observes, "met with no author who has treated them in so clear and full a manner, as to enable a physician either to *know*, or cure them."—Page 7.

When this was written, external putrefaction was believed to produce highly morbid and malignant effects upon, and within, the living human body; and both Dr. Huxham and Sir John Pringle, prepossessed by this belief, were thereby, probably, induced to promulgate their doctrines and opinions on this subject with less consideration, and more confidence, than men of their superior talents and understanding, would otherwise have done. Indeed, Sir John Pringle (and probably Dr. Huxham) was ignorant of an important fact, which, if known, might have altered his opinion on this subject; for he was manifestly convinced, that warm, or hot weather, would promote the activity and force of Jail contagion, (as, in truth, it ought to do, were, that contagion generated by filth, putrefaction, and deficient ventilation;) and in the publication just mentioned, he expresses his belief, that the fever, supposed to have been recently produced by infection from Newgate, would "be, in a great measure, confined to those who were present at the trial, especially *if the weather continued moderately cool*;" not suspecting, what is now ascertained, that the contagion of Typhus, or Jail Fever, is always rendered most virulent and morbific by severe frost,* which, by increasing the density and purity of the air, renders ventilation least necessary, and completely arrests the progress and influence of putrefaction and of its products; while, on the contrary, this contagion is soon enfeebled, dissipated, and destroyed by hot weather, in which putrefaction proceeds most rapidly, and crowding with deficient ventilation is most hurtful.

The opinions, however, of these celebrated physicians are now generally prevalent in this country, and more especially in regard to prisons, which are considered as eminently the parent as well as the fomes of the contagion of Typhus Fever. That this fever often exists in them cannot be denied; but this circumstance can afford no evidence of its having been generated therein, any more than the multiplication of vermin in such places could demonstrate the spontaneous generation of these, and other insects, by the nastiness which favours the deposition, and hatching of their eggs. It must, indeed, be impossible to adduce any sufficient affirmative proof on this subject; for, as the contagion of Jail Fever, though commonly inactive during the hotter part of the summer, always exists in this country; and, as it frequently remains dormant in the human body several months after being received therein, the breaking out of this fever in a prison can never afford any evidence of its having been generated, where it first appears. For, even if the person first attacked should have been so long imprisoned, as to make it incredible that he was infected previously to his imprisonment, there must always have been so many ways and means, by which the contagion might have been introduced from without, (e. g. by infected persons, gar-

* The benevolent John Howard, in his work on Prisons, (page 467,) observes, that "the Goal Distemper is always observed to reign more in our prisons during winter than summer; contrary, I presume, (adds he,) to the nature of other *putrid* diseases." Similar, but stronger, testimonials will hereafter be adduced.

ments, bedding, &c.) that its having been so introduced will always be much more probable than the spontaneous generation of contagion; an operation, or process, of which we have no example, and which, if it really took place, would to me seem miraculous.

I have already proved, that crowding, filth, and deficient ventilation, do not, in a variety of other situations, produce any thing like contagious fever; and I might fairly conclude, therefore, that these causes would not be more efficacious or noxious in jails, than they are found to be in the places already mentioned. But least any persons should imagine that there may be some circumstances in a prison peculiarly suited to the generation of what is called Jail Fever, I will, in regard to this particular, undertake what I have already performed in regard to the putrefaction of animals, &c. and instead of requiring affirmative proofs from those who assert the generation of febrile contagion by such causes, will take upon myself to refute these unsupported assertions by decisive *negative* evidence: and for this purpose I will resort to the observations and testimony of Mr. Howard, than whom no man ever took more pains to ascertain the truth concerning prisons, or stated it with more exactness and candour; and the result of all that he either heard or saw is, that the Jail Distemper is not known in the prisons abroad.

In his work on Prisons, he informs us, (page 125,) that on conversing with Dr. Tissot, at Lausanne, the latter expressed his surprise at our Jail Distemper; said, "I should not find it in Switzerland;" and added, that "he had not heard of its being any where but in England." "I did not," continues Mr. Howard, (as the Doctor said,) "find the Jail Fever in Switzerland."

In regard to the prisons at Venice, Mr. Howard says, (page 106,) of the same work, "the chief prison is near the Doge's Palace, and it is one of the strongest I ever saw.—There were between three and four hundred prisoners, many of them confined in *loathsome* and dark cells for life; executions here being very rare. There was no fever or prevailing disorder in this *close* prison."

At page 117 of the same work, Mr. Howard, describing the great prison of Naples, La Vicaria, says, "it contained, when I was there, according to the gaoler's account, nine hundred and eighty prisoners. In about eight large rooms, communicating with one another, there were five hundred and forty sickly objects, who had access to a court, surrounded by buildings so high as to prevent the circulation of air. In seven *close offensive* rooms, were thirty-one prisoners almost without clothes, on account of the great heat; and in six dirty rooms, communicating with one another, were fifty women." Here he adds the following note, viz. "In visiting the prisons of Italy, I observed, that in general great attention was paid to the sick; but I could not avoid remarking, that too little care was taken to *prevent* sickness. From the heat of the climate, one might imagine the *Jail Fever* would be very likely to prevail; *but I did not find it in any of the prisons*."

Sir John Pringle, in a discourse delivered by him to the Royal Society, as their president, the 30th of November, 1776, says, "the late Dr. Mounsey, (F. R. S.) who had lived long in Russia, and been *Archiater* under two successive sovereigns, acquainted me that, happening to be at Moscow, when he perused my observations on the *Jail and Hospital Fever*, then lately published, (1750,) he had been induced to compare what he read in that treatise, with what he should see in the several *prisons* of that large city. But to his surprise, after *visiting them all*, and finding them *full of malefactors*, (for the late Empress at that time suffered none, who were convicted of capital crimes, to be put to death;) he could discover *no fever among them*, nor learn that any acute distemper, peculiar to jails, had *ever* been known there. He observed, that some of these places of confinement had a yard into which the prisoners were allowed to come for the air; but that *there were* *others without* that advantage, *yet not sickly.*" "He concluded with saying, that, upon his return to St. Petersburg, he had made *the same enquiry* there, and *with the same result.*"*

After adverting to this part of Sir John Pringle's discourse, Mr. Howard, in his Account of the State of the Prisons in England and Wales, (page 94,) adds, "in this ancient capital of Russia, (Moscow,) I found no trace of any such prisons, or dungeons, as were common formerly in the castles of England, and in several foreign countries." "That cruel mode of confinement, in many of our prisons, has been, and still is, a principal cause of the Jail Fever; no symptoms of which fever did I see in Moscow, or any part of Russia.† He had, however, previously described (at pages 87, 88, 92, 93 and 94,) prisons and hospitals in Russia, which he found in a very apt state for generating febrile contagion, according to the generally received opinion on this subject; they being very foul and close. Near the end of his work on Prisons, (viz. at page 467,) Mr. Howard brings the result of his observations and enquiries, concerning the cause of the Jail

* See Dr. Kippis's Edition of Sir John Pringle's Six Discourses, &c. 8vo. page 168.

† This has been confirmed by the Reverend William Coxe, M. A. &c. who, in his Account of the Prisons and Hospitals of Russia, &c. (page 25,) says, "I made particular enquiries whether there have been any signs of Jail Fever, or Epidemical Distemper, ever discovered among the prisoners in Russia, but could not hear of the least tendency to such disorders." This fortunate exemption certainly cannot be ascribed to any peculiar advantage in the construction of the Russian prisons, or any superiority of cleanliness, because the late Empress Catharine, in the answers which she dictated to her secretary, and sent to Mr. Coxe on that subject, declares, that "there has been hitherto no general plan for the construction of prisons, nor rules for their distribution and situation."—And that "there is *no more regulation for the cleanliness of the prisons* than for their construction and situation. By an *abuse* (she adds) favourable to the prisoners, they are, in many places, permitted to go to the baths."—But, she thinks, "it is probable, that the *cold alone* prevents epidemical disorders.—Travels into Poland, Russia, &c. vol iii. page 133, 8vo. Cold, however, is now certainly known not to produce any such effect, in regard to the contagion of Jail or Typhus Fever, which, as has been already stated, (at page 124, &c.) is equally unknown in the habitations of the Russian peasants.

Fever, to this pointed conclusion. "If it were asked, (says he,) what is the cause of the Jail Fever? It would, in general, be readily replied, the want of fresh air and cleanliness: but as I have found, in some prisons abroad, *cells* and *dungeons* as *offensive* and *dirty* as any I have observed in this country, *where*, however, this distemper was *unknown*, I am obliged to look out for some *additional cause* for its production." Mr. Howard's further experience, in his subsequent tour over a great part of Europe, and into Turkey, in (1785, 6, and 7,) being in conformity with his preceding statement, he repeated it in the same words, in his work on Lazarettos.—Page 231.

This "*additional cause*," which Mr. Howard thought it necessary to look for, in order to explain the production of Jail Fever, can be no other than the *contagion* thereof, which, however prevalent in this kingdom, has no existence in most other countries, and where it does not exist, there is good reason to conclude that the true Jail or Typhus Fever never occurs, though other fevers have been frequently mistaken for it; this is, doubtless the reason why all those accumulations of filth, in close crowded places, do not occasion febrile contagion in prisons abroad, though in this country, where that disorder always exists, they contribute greatly to its retention, concentration, and virulence.

The frequent intercourse between the subjects of Great Britain and those of France, by reciprocal captures at sea, has been a cause of introducing the Typhus Fever into the ships, and among the seamen of the latter. But, as Paris is at a considerable distance from the sea coast, there is good reason to believe that this fever has been rarely, if ever, known in that metropolis; and that, when it has occasionally existed at any sea-ports, or in the interior parts of France, (as at Rouen, see page 71, note,) it has, in general, been originally derived from British prisoners. And it was, doubtless, for the reason just given, that an eminent and justly-celebrated physician, Professor Sauvages, of Montpellier, when he admitted the Jail Fever (which he denominates Typhus Carcerum) into his *Nosologia Methodica*, relied solely on the authorities of two English physicians, Huxham and Pringle, adopting exclusively their descriptions of the disease, which he has mentioned in several parts of his valuable work, but always with references to the same English physicians only, which probably he would not have done, if, with his very extensive reading, he had found any other sufficient authority for the existence of this fever, (which, indeed, he does not appear to have ever seen,) and for its characteristic symptoms.

A further proof of the rarity of this fever, in the interior of France, seems to present itself in the ninth volume of the Mémoires of the "Societé Royale de Médecine," of Paris, in which it appears that this society, in November, 1790, presented to the National Assembly, a plan of a new Medical Constitution in France,—(page 102;) and at the fifth section of the second part of this plan, which relates to the "fonctions du Mèdecin dans les Dépots de Mendicité ou Maisons de travail, et dans les prisons," the following observation is made, viz. "On *sait* que faute de propreté et de soins, et par l'entassement des hommes, ou le mauvais traitement des malades, les prisons ou dépots ont souvent été le foyer d'Epidémies redoutables. C'est *surtout* en *Angleterre* qu'on en a éprouvé les *funestes effêts;* c'est là qu'on a vu la plus expansive des contagions s'élancer de ces maisons pour infecter au loin les flottes par la presse; les armées par les recrues faites dans les Bridewells, (ou maisons de correction;) les villes et les campagnes par les Sessions des Comtés, & les possessions Anglaises dans les îles par la transportation des criminels." I ought here to observe, that four or five years before this plan was presented to the French National Assembly, Messieurs Tenon and Coulomb, two of the commissaries, nominated by the Royal Academy of Sciences, for matters relating to hospitals, had been sent by the French government to England, to obtain information on that subject; and were here most favourably received, and made acquainted with every thing likely to render their mission beneficial. And, among other acquisitions of knowledge, they were informed of, and persuaded to adopt, the opinions prevalent in this country, respecting the supposed generation of febrile contagion by crowding, filth, and insufficient ventilation, which opinions appear to have greatly influenced the Royal Medical Society of Paris, in that part of their plan which has been just cited. But those who have had opportunities of seeing and comparing the conditions of the poor, as well as of the streets, houses, prisons, and hospitals, of Paris and of London, must be convinced, that if the causes just mentioned, had been sufficient to produce the supposed effect, Typhus Fever would have prevailed in the former, at least as often, and as long ago, as in the latter. And that it could not have been proper or justifiable in this society, to select and represent England as the country in which, *above all others*, the pernicious effects of contagion so produced, had been oftenest, and most fatally manifested.

That there may, however, be no doubt on the subject of this comparison, I shall extract, and place in Appendix No. 5, certain parts of a large volume in quarto, entitled "Mémoires sur les Hopitaux de Paris, par M. Tenon, Professeur Royale de Pathologie, &c. imprimés par ordre du Roi," (1788;) by which it will appear that the Hotel Dieu, of Paris, is not only the largest, but the most *crowded and filthy hospital on earth;* that a single building thereof, called "Batiment Meridional," generally contains two thousand six hundred and twenty-seven patients, crammed together, from four to six in each bed, with every circumstance and degree of nastiness, and deficient ventilation, so that if such causes could have generated contagious fever, it must have been *there* generated, nearly two centuries ago; and being once generated by them, it must, from their continual aggravation, have been constantly maintained, and spread to a greater extent, and with increasing virulence.

Believing, as I do, that any additional evidence on this subject would be superfluous, I shall content myself with observing, that the respectable Dr. James Lind, though he had allowed himself to adopt and maintain the common opinion, that Contagious Fever might be generated by the means which have been so often mentioned, has, notwithstanding, upon several occasions, stated facts, which, with proper attention, might have led him to a different conclusion. In his chapter on the Jail Distemper, at page 315, of his Essay on preserving the Health of Seamen, (second edition.) are the following paragraphs, viz.

"The *origin* of the jail infection is a point, at present, entirely unknown. No person has given us the least satisfactory account how or where it is generated. It does not seem to originate in air, and there are many prisons abounding with filth and impurities, *perfectly free* from it.

"In ships also, an infection is generally imported from the land, and many that have been long in a very dirty condition, at sea, bring their men quite healthy into the harbours.—Indeed, I have always observed, that the most healthy ships were such as arrived from a long foreign voyage, the scurvy being the chief, and almost the only complaint among them. Whereas ships of war, especially when fitted out in the Thames, even in times of peace, very often received this infection from London."

And, finally, at page 227 of the same volume, we find this assertion, viz. "I never heard of any ship which, after having been carefully and properly smoked, did not immediately become healthy: And if, afterwards, they turned sickly, it was *easy* to trace that sickness, from other infected ships, jails, and the like places." Certainly this would not have been the case, if it were possible that contagion should be generated, *de novo*, as has been supposed, by the causes in question.

From the preceding facts and considerations, I think it may be safely inferred, that filth, crowding, putrid human effluvia, and deficient ventilation, though favourable to the retention and accumulation of febrile contagion, where Typhus

Fever exists, or has existed, and consequently, to its activity, do not of themselves either generate, or enable the human body to generate, that contagion; and that fevers are not contagious, nor liable to become so, unless produced by contagion.

Assisted by these inferences, I shall next proceed to ascertain, as far as I may be able, the causes of the Yellow Fever, together with the truth or fallacy of those reasonings which ascribe either its origin or propagation to contagion.

END OF PART SECOND.

# PART THIRD.

## OF THE CAUSES OF THE YELLOW FEVER.

THE Creator of the world, for purposes which it is our duty to respect as wise and good, has so constituted the surface of the earth, that, in a great part of it, the soil, when moistened and assisted by suitable degrees of solar heat, is naturally disposed to produce certain vapours or exhalations, technically denominated *marsh miasmata,* and possessing a specific power of exciting fever in the human body, which fever, though most frequently intermitting or remitting, is a great cause of mortality, especially in hot climates.

This important truth is now so well ascertained, and so generally admitted, that many proofs in support of it will scarcely be deemed necessary. Several remarkable instances of fevers produced by this cause, have been already stated between pages 82 and 88 of this volume, and to these it may suffice to add the following.

Dr. John Hunter, in his Observations on the Diseases of the Army in Jamaica, informs us, that the place in Kingston Harbour in that island, at which "the ships of war take in their water, being wet and swampy, it commonly happens that the men employed in filling the water-casks are taken sick, either at the time, or a few days after; and there are examples where, out of sixty or seventy men sent on that duty not one has escaped a fever.

Dr. Blane also, in his interesting "Observations on the Diseases of Seamen," alluding to the same service, at the same place in Jamaica, says, (p. 92.) that "it was the practice of many ships of war) to leave the water-casks on shore all night, with men to watch them, and as there is a land-wind in the night, which blows over some ponds and marshes, there were hardly any men employed on that duty who were not seized with a fever of a very bad sort, of which a great many died." Afterwards, at p. 392, when treating of "the bilious remitting fever," the same author observes, that it "may generally be traced to the air of woods and marshes; and in our fleet hardly any men were attacked with it, but those who were employed in the duties of wooding and watering."

Dr. Lind, speaking of the unfortunate attempt to make a settlement at the Island of Balambangan, near Borneo, where scarcely one in ten of those sent thirther survived the first six months, says, "from October till April, during the north-east monsoon, the wind comes from the sea, and the settlement is perfectly healthy; but from April till October, during the south-west monsoon, the wind blows over the marshes, both of this Island and Borneo, and produces fevers of the most malignant nature, which frequently cut off the stoutest men in twelve or fourteen hours." See his Essay on the Diseases incidental to Europeans in Hot Climates, p. 99, 5th Edition.

And finally,—Nicholas Fontana, who went out as surgeon to an Italian East India ship, in November, 1776, in which service he continued five years, and afterwards published some judicious observations concerning the diseases of Europeans in hot climates in the Italian language, informs us, that "the ship having arrived at the Bay del Agoa, on the Eastern Coast of Africa, in March, 1777, some tents were pitched along the bank of the river Spirito Santo, which is low and swampy, to accommodate the sick of the scurvy, and those who were émployed in wooding and watering, and that, of forty-seven sailors who had slept on shore, there was not one who escaped a violent fever, which proved fatal to twenty of their number. —"Observazioni intorno alle mallattie che attaccano gli Europi ne' climi caldi." P. 11. also p. 76.

If more proofs of the specific power of marsh miasmata to produce fever should be desired, they may be found in the treatise "De noxiis paludum effluvis, corumque remediis," by "Jo. Maria Lancisius Archiater Sanct$^{mi.}$ Patris Clementis XI.;" and in the works of Sir John Pringle, Dr. Lind, Dr. John Clarke, and several other medical writers of eminence, as well as of undoubted credit.

Assuming, then, that these miasmata are a most powerful and frequent cause of fever, it seems expedient to enquire concerning their origin, nature, and constituent principles.

The exhalations from marshy grounds may be presumed to consist, either of pure aqueous vapour alone, or of this vapour, combined or intermixed with other vapours, or particles resulting or extricated from some of the various matters, which naturally constitute the soil, or have been superadded to it; and we ought, therefore, if possible, to ascertain whether the noxious effects of these exhalations are produced by pure water only, either dissolved or diffused in the atmosphere, as some respectable authors have asserted, or whether they are solely or principally occasioned by any other matters extricated from the earth?

There are two modes or forms in which water may exist in the atmosphere; one is that of a complete dissolution by the air, so as to be rendered invisible to the eye, and sometimes insensible even to the nicest hygrometers; the other is that of very small globules, commonly perceptible to the eye, and disturbing the transparency of the atmosphere, as in what is called mist or fog.

If pure aqueous vapour in the former of these states were really a cause of fever, we should uniformly discover that sailors are, and have been, more liable to that disorder on the ocean than when on shore, or in harbour; since it may safely be affirmed that the atmosphere at sea is more saturated with aqueous vapour, than it can be on shore, because a much greater evaporation must necessarily take place from a vast expansion of water, than ever occurs from an equal surface of land, not covered, or not nearly covered, by water. It is, however, notorious, that if vessels are not sent to sea in an improper condition, their crews are generally much more healthy on the wide ocean than in any other situation. Dr. Lind, in his Essay on Preserving the Health of Seamen, (p. 218,) states, as a general proposition, confirmed by long experience, "that persons at sea are less subject to fevers than those at land." Dr. Blane, also, in his work on the Diseases of Seamen, says, (p. 252,) "The air at sea in those climates, (West Indies) as well as *every where else*, is extremely pure and wholesome, and there is no where that seamen are more healthy or comfortable." He had previously made a similar, and, in regard to "violent fevers of hot climates," a more *pointed* assertion at p. 204.

To these testimonies may be joined that of Dr. John Hunter, who (at p. 14 of his Observations on the Diseases of the Army in Jamaica states,) that "*simple* moisture is harmless, at least as far as relates to the production of fevers, of which the two last mentioned places (Fort Augusta and Port Royal) may likewise be given as examples, for they are nearly surrounded with water on all sides." He adds, "It is true the air is perfectly clear, yet it must be loaded with moisture in consequence of the great heat of the sun acting upon the water." And finally, not to tire my readers with superfluous testimonies, I shall content myself with adding that of Dr. Gillespie, who, in his Observations on the Diseases of his Majesty's Squadron on the Leeward Island Station, between 1794 and 1796, states, (page 20) as the general result of his experience, that "a ship of war is rarely affected with a sickly crew *at sea*, in the West Indies, and as rarely continues a fortnight in *port* without some of the seamen being attacked with fevers and fluxes."

It seems probable, however, that the morbid influence, which was attributed by the late Dr. George Fordyce, to *pure water*, in exciting fever, was principally intended to be understood of aqueous vapour *merely diffused* through the atmosphere, in the form of *mist* or *fog;* and as his Dissertations on Fever, which promulgate or assert this doctrine, are amongst the most valuable medical works produced in this country, it may be expedient to examine this part of the subject more minutely, in order, if his opinions respecting it should be erroneous, to obviate that extensive adoption of them, which his high authority might otherwise obtain, upon a question of great importance.

The following are Dr. Fordyce's reasonings and statement on this subject, at page 146 of his Dissertation on Simple Fever, viz. "A man going into water of a moderate temperature, and remaining in it for some time, has not been found more frequently afterwards affected with fever, than after standing, walking, or any other indifferent circumstance. It is certainly, therefore, not the application of the water to the body that gives occasion to the disease; but if the air has particles of water floating in it, and a man has continued for some time in such an air, fever has ensued much more frequently than when he had lived in a dry air." Hence the author is led to conclude, that moisture must be a cause of fever; and that he may persuade his readers to adopt the same conclusion, he assumes the following unsatisfactory proposition, p. 151. "If those, who contend that the application of water suspended in the atmosphere, in the form of moisture, does not produce fever, were to live a year or two in Batavia, they would be convinced, by fatal experience, that men living in a moist atmosphere are more frequently affected with fever than in a dry one." Surely, if Dr. Fordyce's opinion on this subject were just, those who thought differently from him might have been made sensible of their error by less inconvenient and dangerous means than a voyage to, and residence at, Batavia. The author then proceeds as follows:—"Moisture in the air produces more fevers, the warmer the atmosphere; but moisture produces fever in all temperatures. The Dutch have endeavoured to make the country of Batavia resemble Holland in the immense number of its canals. The consequent moisture of the atmosphere is very great in both places; but although fevers, therefore, frequently occur in Holland, they bear no comparison in number to those which happen in Batavia, where the fatality, owing to the moisture and heat of the climate, is so great, that it is wonderful any person should even approach that settlement but from the absolute impossibility of otherwise obtaining water or food." This proof of the effects of moisture I cannot but consider as entirely gratuitous, because, to use the words of Sir George Staunton,* (whose description agrees with those given by Captain Cook and other respectable navigators) the settlers at Batavia live "in the midst of swamps and stagnated pools; from whence they are every morning saluted with 'a congregation of foul and pestilential vapours, whenever the sea breeze sets in and blows over this morass;" and, before Dr. Fordyce could have been warranted in ascribing the violent fevers which are so common in that settlement to moisture *alone*, he ought to have proved, that in such a situation no other causes existed by which they could have been produced.†

* An authentic Account of the Embassy to China. Vol. i. p. 242.

† Dr. Horsefield, an American physician, who now is, or lately was, employed in travelling over the Island of Java with the sanction of the government of Batavia, observes, in an account of his voyage to that island, in the year 1800, (published in Dr. Cox's Medical Museum, vol. i p. 75, &c.) that "it is impossible for the imagination to conceive a situation more favourable to the production of marsh miasmata than that of Batavia. "If," adds he, "human industry and ingenuity should be exerted in planning and constructing an elaboratory for the production of pestilential vapours, a situation exactly resembling that of Batavia and its environs would be the result." But even here, Dr. Horsefield states the rainy season to be "comparatively healthy," to those who have it their power to avoid immediate "exposure to rain;" contrary to

The author next allows, that "fevers more frequently arise, when the moisture is evaporated from a marshy country, or from stagnating water, than when it proceeds from the sea, large lakes, or rivers confined within their banks, and running with a rapid stream. "This," says he, "has given occasion to suppose, that some other vapours proceed from marshes besides water, and produce the disease." But as such a notion must clash with his own hypothesis concerning simple moisture, he brings forward a second proof, to show that fevers have been produced by moisture, when it has arisen from the earth in a state of purity, that is, not impregnated with any of the matters contained in the soil which are undergoing the process of decomposition. "It certainly happens often," he says in page 154, "that a considerable degree of putrefaction takes place in marshy grounds, and more especially in warm climates; but it is by no means to be concluded, that moisture in the atmosphere always produces fever in consequence of putrefaction. Putrefaction can only take place in animal or vegetable substances. If water, therefore, not impregnated with either, should be in such a situation as to produce moisture in the atmosphere, no putrefaction can take place; therefore, if fevers ensue, they are certainly in consequence of moisture, not putrefaction. Many instances of this may be brought, as in the war which took place in Flanders, between 1710 and 1711, an army encamped upon *a pure sand,* in which water was found in digging less than a

that which must have been the case had the doctrine of Dr. Fordyce been true,—and Dr. Horsefield assigns as the reason of this greater healthiness during the rains, that the rivers and canals are then "plentifully supplied with water, which flows through them with considerable rapidity, and most of the lower marshy situations are entirely inundated with water." But in July, August, and September, these waters become nearly evaporated; and "the quantities of marsh miasmata *now* produced, are not only inconceivably greater than at other times, but the diseases produced by them are much more malignant and intractable in their nature." And this (which, if Dr. Fordyce's opinion were just, ought to be the season of health) becomes, to use Dr. Horsefield's own words, "the *season* of *death* and *destruction,* in which the hospitals and church-yards are filled."

foot deep, and occasioned a great moisture in the air, which produced in a few days numbers of fevers, although the army was perfectly healthy before, and no more fevers were produced on shifting their ground."

This last instance, apparently more decisive than the former, is, however, of a very questionable nature in several respects. It would be injustice towards Dr. Fordyce, to suppose that he could have stated this as a fact, if he had not been persuaded that it had really happened, and exactly as he has related it; but, unfortunately, he neither mentions the spot where it occurred, nor the author by whom it was related, and who might have been greatly misinformed or deceived as to the circumstances; and every one will agree in this, that no fact which is to serve as the foundation of an important doctrine, can have any claim to be received into a philosophical discussion, unless it be fully attested. Now, with regard to the alleged purity of the sand, on which the camp was pitched, I may observe, that it is extremely difficult, not to say impossible, to find any soil that does not contain some portion of vegetable and animal matters; and that, even if these had not previously existed in the sand, they would have been immediately supplied by the army encamped thereon, and being assisted by the moisture abounding there, would have very soon afforded vapours, differing greatly from those of pure water.

But supposing the sand on which this encampment was made to have been perfectly pure, still there may have been marshy ground at a small distance, whose exhalations might have caused the fevers in question; and it is not assuming too much to say, that, in a flat country like Flanders, wherever any ground is so low and wet that water is found at less than a foot beneath the surface, the surrounding land is likely to be very marshy. Moreover, as the fevers broke out among the troops only "a few days" after they had encamped on the sand, there is more than a possibility that they were caused, not by the unhealthiness of their actual position, but by that of the station which the army had recently quitted; and one need only read the description which Sir John Pringle has faithfully given in his work on the Diseases of the Army of that part of the Netherlands in which military operations have been mostly carried on in modern times, in order to be convinced, that a very large portion of the surface of that country consists of marshy ground.

Such are the objections which occur to this second instance, as it stands in the Dissertation on Simple Fever; and with these I should have dismissed the consideration of it, as being of too doubtful a nature to deserve much notice, if Dr. Fordyce had not adduced a similar fact in support of the same doctrine, after a lapse of eight or nine years, which I shall quote in his own words, from page 63 of his "Fourth Dissertation on Fever," viz. "The author has shown, in a former dissertation, that moisture, by dissolving in the air, or by evaporation, is one powerful cause of fever; that it is often the cause of intermittents, as well as of the other diseases which have been above enumerated," (dysentery, continued or remitting fever, and irregular semitertians, under which last title he includes the disorder now known by the name of the Yellow Fever) "without any putrefaction taking place, is certain, from several instances. These diseases have been produced in countries where the water was found at only a foot or two under the surface of the earth, whence the moisture has arisen and contaminated the air, so as to occasion these diseases, while the soil has been perfectly dry,* and there has not been the least appearance of putrefaction, the country being clear from woods. In this case it could be nothing but the moisture† that produced the disease. One instance of this occurs in the encampment of the English army in the war about the year 1745, in a sandy plain in Flanders. Another

* Either the soil was not *perfectly dry*, or so much moisture *did not rise through it*, as to produce a morbid *contamination* of the air.

† This is rather an hasty assertion, since it is obvious, that soils, upon which not a single tree is growing, may nevertheless contain putrefying animal and vegetable substances in considerable quantity.

in a region of Peru, where water is every where to be found at about seventeen inches below the surface of the earth, though the country itself is barren for the want of water, and uninhabitable from the number of dysenteries and semitertians which take place in it."

The only difference between these two encampments on a plain of sand, is that of the dates, concerning which a mistake might easily have been made by one who has often stated facts very loosely; and for this reason I am inclined to believe, that Dr. Fordyce alluded to the same fact, while he was writing each of the passages I have quoted. With this idea I naturally recurred to Sir John Pringle's excellent Medical History of the War, between 1742 and 1748, where I expected to find the account of this encampment; but neither in this work, nor in any other I have met with, relating to the transactions at this period, have I been more successful in discovering the object of my inquiry, than I had been before in the many searches which I had made after the instance said to have happened in the former war.* There are, however, certain passages in Sir John Pringle's book, which appear in some measure to correspond with Dr. Fordyce's statement, and they are, perhaps, the instances which the latter author had in his mind on both occasions. Sir John Pringle, describing the face of the northern part of Dutch Brabant, says, it† "is nearly as flat as any ground of the Netherlands, the only inequalities being some sand-hills and insensible risings, which give the advantage of a few feet in height to some of

* Among other endeavours to ascertain the circumstances which Dr. Fordyce ought to have stated, in regard to his supposed facts, I applied to Dr. Wells, who had edited the latter part of his work on Fevers, hoping that, either from the papers of Dr. Fordyce, or from their conversations, Dr. Wells might be enabled to supply the desired information. The latter gentleman, however, in a letter dated the 30th of April, 1806, declared himself unable to do this; politely offering, at the same time, to "make inquiry among others of Dr. Fordyce's friends, and, should it be successful, to communicate the result." No such communication having since been received I must necessarily conclude, that the promised inquiry has proved fruitless.

† Observations on the Diseases of the Army. Page 62, 7th Edition.

the villages. The soil is a barren sand, and so little water is seen, that, at first sight, the country might seem to be dry and healthful. But this appearance is deceitful, for water is every where to be found at the depth of two or three feet; and in proportion to its distance from the surface, the inhabitants are free from diseases." After this the author mentions that, during the summer of 1748, the troops which were cantoned in different towns and villages became very sickly, and that "the sickness was much greater near Breda and Bois-le-duc than at Eyndhoven, which lay at a greater distance *from the inundations, and from other marshy grounds.*" The following inference comes next, which seems to me to have no necessary connexion with what immediately proceeds, and to be not only erroneous, but liable to considerable misinterpretation, viz. "The moisture, therefore, in most of the cantonments arose principally from the subterraneous water which exhaled through the sand." Let us now examine if this account can be fairly construed into a proof that pure moisture was the cause of this sickness."* "On the 12th of May, 1748, the army left Hillenraet, near Roermond, and in a few days came to Nistleroy," (or Nesterle, which is situated in the centre, as it were, of the North of Brabant) "where they encamped for the last time;" and, "on the ninth of July, the camp broke up, and the troops went into cantonments;" the war being then at an end. During this interval† "some seasonable rains, with thunder and lightning, seemed to prevent any sultry heats; the ground besides was dry, and the camp airy, *so that the sickness was inconsiderable as long as the troops kept the field.*" This is the only encampment which I can discover to have been made on a sandy ground in the course of the war; but, surely, this cannot be cited as the example in which "numbers of fevers were in a few days produced among troops previously healthy, in consequence of their being encamped on a pure sand, where water was found at less

* Page 60. † Page 61.

than a foot deep." Nor does the account given by Sir John Pringle, of the health of the army subsequently to this encampment, at all coincide with Dr. Fordyce's statement, that "no more fevers were produced on shifting their ground:" on the contrary, so many fevers broke out, that* "the troops had scarce been a month in the cantonments when the returns of the whole sick were increased by two thousand, and afterwards they rose considerably higher." Besides the army,† "the peasants were great sufferers," from these epidemic fevers. "This country," says the author, "had not known so much distress for a number of years, as two such causes" (of disease) "had not occurred, I mean the *drying up* of the inundations," which had been made about the fortified towns, since the commencement of the war, "with a hot and close summer and autumn." I shall not here inquire what effects might have resulted from the operation of these causes, because the inquiry more properly belongs to another part of this work, where it will probably appear, that two such causes as these were fully capable of producing the fevers in question: but having shown, from the best authority, that the health of the army was comparatively good during the only encampment which is recorded to have been made in the course of the war between 1742 and 1748, "on a sandy plain, where water was found at the depth of two or three feet:" and having also shown, that the sickness which ensued shortly after the camp was broken up, among the several divisions of the army, cannot, with any justice, be ascribed to the transpiration of simple moisture through a pure sand, since it is even stated by Sir John Pringle himself, who seems to have been an advocate likewise for the noxious properties of moisture, to have been proportioned to the distances of the different cantonments "from the inundations and other marshy grounds." I shall now examine the validity of the third instance, concerning the unhealthiness of "a region in Peru," which Dr.

* Page 66. † Page 67.

Fordyce has annexed to his second account of the encampment on a plain of sand.

The air of improbability which accompanies his description of the *region* in question, (which certainly ought to have been designated by its proper name) will immediately be perceived by the reader. "Water," says he, "is every where to be found at about seventeen inches below the surface of the earth.—The country itself is barren for want of water;" yet so much moisture transpires through the ground, as to render the spot "*uninhabitable.*" Surely, if so much aqueous vapour passed through the earth as to communicate to the atmosphere a morbid excess of humidity, the soil itself could not have been so extremely dry as, from that very circumstance, to be rendered barren; warm dry earth would, undoubtedly, absorb some portion of the aqueous vapour in question during its passage; and we may safely infer, that, so long as the soil remained *dry*, the vapour which arose from the subterraneous water must have been too little for its saturation, and consequently, insufficient to load the air with a noxious degree of moisture. But neither this objection, nor such of the objections advanced against the preceding instance, as are applicable to the present case, deterred me from endeavouring to discover if any part of Peru answered the description given by Dr. Fordyce of this particular region: and since the author, who never was out of Great Britain, has, on this occasion also, neglected to mention whence he derived his information, and none of his surviving friends are able (so far as I can learn) to supply the omission, I had recourse to the best accounts hitherto published of that most singular and interesting country, (Peru,) particularly to those of Don Antonio Ulloa, who, to the rank of Lieutenant-General in the Spanish Navy, and Naval Commandant in Peru, added scientifick attainments sufficient to procure his adoption into most of the learned societies in Europe. This distinguished author has given a narrative of his voyage with Don George Juan and the French Academicians, who were sent by the Governments of Spain and France to South America, in 1735; and he afterwards published some observations on Peru and other parts of America, in Spanish, under the title of "Noticias Americanas," of which a translation in French, was printed at Paris, in 1787, with the title of "Memoires Philosophiques, Historiques, Physiques, concernant L'Amerique, &c." It is more especially from the latter* of these works, that the following account of what Dr. Fordyce would probably, call "the regions" of Peru, is extracted. The western coast of South America, adjoining the Great Pacific Ocean, consists of *low* land, which forms a kind of zone along the shores of that ocean, varying in breadth from eight to twenty leagues, and extending from 7° or 8° of North Latitude, to 27° or 28° South of the Equator, the whole of which bears the name of "*Valles.*" "Au point ou finissent ces plats pays commencent les Cordilleres," an immense chain of mountains, which runs southward almost to the Streights of Magellan, and occupies at its base a breadth of from thirty to fifty leagues. Upon this great mass of mountains are found large habitable tracts, called Sierras, to distinguish them from the low land on the coast; and we learn that these tracts, which Ulloa has named, "La Partie haute Habitée," are at an elevation of 4536 varas, (of Castile, or 12,451 English feet) "au dessus des terreins qui avoisinent immédiatement à la mer." "On voit par là, que cette partie de l'Amérique a une bande de terrein sensiblement plus élevée, que toutes les autres contrées habitées du globe." "Il y a dans la partie haute habitée, des royaumes très etendus, des provinces fort peuplées; il s'y voit aussi de vastes contrées désertes." This high ground, however, serves but as a base for an higher range of mountains: "les cimes des montagnes qui s'élèvent sur cette même plaine élevée,† ont plus de 6,600

* And from pages 22 23, 24, 28, 29, 37, 222, 244, of the first volume.

† The summit of Chimboraçon is stated to be 19,595 feet above the ocean.

varas de haut (18,117 feet;) elles surpassent donc les autres, de 2,063 varas (5,664 feet.)" These surperincumbent mountains, far exceeding all others on the surface of the globe, and eternally covered with snow, are, of course, uninhabited, and therefore cannot be the objects of our present consideration: neither is it the "Partie haute Habitée," the object of it, since according to Ulloa, "on n'y voit ni fievres intermittentes, ni putrides." Besides these parts, there are ravines, extending in different directions through the *Sierras*, which he thus describes. "Dans la partie élevée, la terre est entrecoupée de vastes profondeurs, qu'on y appelle *Quebradas*." "Le fond sert de lit aux eaux qui y coulent, et tiennent presque toujours le milieu. Ces eaux suivent les detours et les deviations du terrein latéral—& continuent ainsi leurs cours dans ces profondeurs entre les montagnes, & arrivent enfin dans la partie basse du terrein, d'ou elles se rendent à la mer; mais la masse d'eau qu'elles forment dans cette seconde partie a peu de profondeur, & semble n'être répandue que sur la surface du sol." These Quebradas, as Ulloa believes, have been gradually worn by the torrents, which have descended for a long succession of ages from the heights; they vary in their depth and breadth, the perpendicular depth of some of those chasms being 1,769 varas, (4,855 feet) or even more, and their width sometimes exceeding two leagues, so that "Elles ont assez de surface pour devenir le local de nombre d'habitations fort peuplees, qui en tirent tous les produits nécessaries à la vie;" and their soil is moreover sufficiently rich and fertile to permit an extensive cultivation of the sugar cane. It is true, that intermittent fevers of a dangerous kind are occasionally seen in the Quebradas, (from causes which produce them in other countries;) these spots, however, are neither uninhabited nor barren, but exactly the reverse, and, therefore, we cannot consider them as the unhealthy region which Dr. Fordyce meant: nor does there appear, from the accounts I have met with, to be any other part of Peru to which he could have alluded, except the country lying between the sea and the foot of the Cordilleras. Upon this low country of the "Valles" it is remarkable that no formal rain ever falls; but there are wetting fogs, called "garuas," during what is there named winter; and as "le sable domine dans les terreins bas, même à des distances assez considérables," one is at first led to suppose, that the soil must here be barren for want of water; but it will be immediately perceived, that this is far from being the case. During about one half of the year, viz. from July to January, which is called winter in the low lands of Peru, the ground receives an ample supply of moisture for all the purposes of the most luxuriant vegetation, by a contrivance of nature, no less singular than bountiful: while this season continues, the low country is constantly covered with a thick fog, through which the sun is scarcely ever able to penetrate; and this fog, although not sufficiently damp to wet one's clothes, yet is moist enough *"pour pénétrer la terre, pour fertiliser le sol le plus aride et le plus stérile de sa superficie, parceque le soleil ne peut la dissecher." During the rest of the year, which is the rainy season in the high lands, and is there termed the winter, the earth is likewise supplied with moisture, by means of artificial irrigations, which appear to have been in general use among the Peruvians, long before the discovery of America by Columbus. At this time numerous streams are pouring through the Quebradas into the low country, where many of them lose a part of their waters in irrigating the land. "Comme on y a le degré de chaleur requis, (says Ulloa) il né s'agit plus que d'y faire des petits canaux pour conduire l'eau ou il est nécessaire; ainsi de terreins stériles on en fait des campagnes, dont la fertilité ne le cède pas aux terres les plus grasses." We must also take notice that these streams do not all discharge themselves into the sea: some of them which happen to flow into situations

* Voyage Historique de l'Amérique, par Don Geo. Juan & Don Ant de Ulloa; Tom. i. p. 454.

so low, that "les terreins n'ont pas assez de pente pour leur écoulement," are there arrested in their course; and at these places the ground becomes swampy; it is from this cause that "on voit dans les terreins bas quelques étendues de terre fangeuse." After this description of the low country, we cannot, certainly, be surprised at learning, that intermittent fevers occur frequently among its inhabitants: we find, however, that even these fevers do not correspond with the account which Dr. Fordyce has given, either of the nature, or of the cause of them; for Ulloa says, that "Dans la partie basse, ces fièvres ne sont point dangéreuses, quoique longues et très fatiguantes:" and it is unnecessary to observe, that fevers of this mild nature are not likely to depopulate a district; and, in regard to their cause, it could not be asserted, with truth, even if the country had been rendered uninhabitable by fevers, that these were produced by pure moisture, since, although sand may predominate in the low lands, as Ulloa states, yet the soil is not exclusively composed of sand, and it cannot be imagined, that pure aqueous vapour could alone be exhaled from a tract of country, of which a considerable part consists of land which is not only cultivated, but rendered extremely fertile by copious irrigation; and in which there also appear to be some extensive patches of marshy ground. Hence we must be convinced, that Dr. Fordyce has been as little correct in referring us to Peru, as in sending us to Batavia, in search of the noxious effects of simple moisture.

The deference which I conceived to be due to the opinions of this author, especially when they seemed to be corroborated, so far as regards the tendency of moisture to produce fever, by those of Sir John Pringle, and some other respectable physicians,* and the importance of the subject itself,

* Even Dr. Lind, overlooking many facts of a contrary import, stated by himself, appears to have adopted an opinion similar to that which I am now controverting. In his work on "the Diseases incidental to Europeans in Hot Climates."—(5th edition) we find, at page 9, this passage, viz. "In my Essay on preserving Seamen I have

have drawn me into a long examination of the proofs adduced by the former writer, in order to point out their insufficiency. It may, however, here be mentioned, that Dr. Fordyce, although he entertains no doubt concerning the validity of his proofs, nevertheless finds great difficulty in accounting for the alledged effects of moisture; and, towards the close of his discussion* of this subject, after repeating an observation already quoted, viz. that since water is innocent when applied to the body in a mass, as during immersion, but causes fever when applied in the form of small particles floating in the air, "it cannot be the mere application of the particles of the water that produces the disease;" he says, "it must, therefore, be something that they apply to the body which occasions it;"—but "what this may be," he confesses, "is not very clear." He afterwards throws out an idea, that, "as the evaporation of water produces cold, moisture may only be a means of suddenly applying cold to the body," and that the *cold* so produced may be the cause of the fever; aware, however, of some objections to this idea, especially, as he had previously stated, that *heat* rendered the moisture more noxious as a cause of fever, he concludes with saying, "but this the author leaves to future experiment and discussion." From expressions so full of uncertainty, and, I may say, of contradiction, it is evident that he was by no means satisfied as to the soundness of his favourite doctrine; and his doubts on the subject would have been still greater, if he had recollected a very interesting fact, related in page 57 of Sir John Pringle's "Observations on the Diseases of the Army," (from which Dr. Fordyce, as I have already said, seems to have intended to borrow his second instance, relative to the encampment on a plain of sand) which is so demonstrative of

said, that a malignant fever of the remitting kind, most frequently a double tertian, is the *genuine produce of heat and moisture*, is the autumnal fever of all hot countries, and is the epidemic disease between the tropics: to which I may add, that it is also the disease most fatal to Europeans in hot climates."

* Dissertation on Simple Fever. P. 156.

the opposite effects of pure moisture, contrasted with those of marsh effluvia, that no apology can be necessary for introducing it here.

According to Sir John's statement, four battalions, of about seven hundred men each, were stationed in the two islands of Walcheren and South Beveland, during the very hot summer of 1747. These islands are from one to two miles asunder, and form part of Zealand, which province (p. 2,) "is not only low and watery, but surrounded with the oozy beaches of the Eastern and Western Scheld, and the most marshy parts of the country," (the United Provinces and Dutch Brabant along the Maes) "so that almost every wind, except from the sea, adds to its native moisture and unwholesome exhalations." Under such unfavourable circumstances of situation and of season, the troops, "both in the field and in quarters, became so very sickly, that, at the height of the epidemic, some of those corps had but one hundred men fit for duty, and the Royals in particular, at the end of the campaign, had but four men who had never sickened. But Commodore Mitchel's squadron, which lay all this time at anchor in the channel between South Beveland and the Island of Walcheren, in both which places the epidemic prevailed, was neither afflicted with the fever nor the flux; but amidst all that sickness enjoyed perfect health; a proof," as the author justly observes, "that the air of the marshes was dissipated, or corrected, before it could reach them." This account, had it been known to Dr. Fordyce, or remembered by him, would, probably, have weakened his confidence in the power of moisture to produce fever; for he would, I presume, have readily acknowledged, that the atmosphere could not have been *less* moist (and he might have found, that it was more moist) where the squadron lay at anchor, surrounded as it was by water, than in those islands; and, consequently, that, if simple moisture had been the real cause of the fevers which were so prevalent on shore, the sailors must have suffered at least equal sickness with the soldiers.* But his confidence therein, would, perhaps, have ceased altogether, if he had attended to the well-established fact, of which it cannot be supposed that this author was ignorant, that the occurrence of marsh fevers may, with certainty, be prevented, by laying the marshy grounds under water; an operation which certainly would not diminish the humidity of the atmosphere. But proofs still more convincing, if possible, than the two just mentioned, may be offered in refutation of Dr. Fordyce's doctrine. Indeed, we know that the air which passes over, or is incumbent upon marshes, during winter, in this climate, is generally harmless in regard to the production of intermitting or remitting fevers, although it is then commonly more replete with moisture, than in summer. And, moreover, persons who live on *peat-bogs* or moors, are, at all seasons, for reasons to be explained hereafter, completely exempt from the fevers to which the inhabitants of marshy grounds are subject; although it cannot be pretended, that less moisture is evaporated from the surface of the former, than from that of the latter. Again, every one can recollect

* A similar exemption from the fevers raging epidemically in the Islands of Walcheren and South Beveland, occurred in regard to the people on board of the British ships belonging to the late expedition against Zealand. This fact, concerning which I have received numerous corroborating testimonies, from respectable officers employed on that service, has been officially declared in the report made to the Secretary at War from Middleburgh, the 10th of October, 1809, by Dr. Blane, Dr. Lempriere, (physician to the Army) and another medical officer, who were sent by his Majesty's government to Walcheren, for the purpose of investigating the nature and causes of the malady prevailing among the troops in that island, wherein they state, that they had "ascertained that the crews of the vessels stationed in the very narrow channel, only a few yards from the land, between Beveland and Walcheren, have continued perfectly healthy during the whole campaign; thus decidedly proving that the noxious exhalation is nearly confined to its original source." See the "Military Papers relating to the Expedition to the Scheldt, presented by his Majesty's command to both Houses of Parliament, February, 1810." Marked E. page 110. Certainly the crews of the ships in question, (between Beveland and Walcheren) who continued in health, must have been exposed to at least as much moisture, as the soldiers labouring under fever on shore.

a multitude of instances in which persons have been exposed for hours together in the heavy mists which are frequent in this climate during the winter, without having been afterwards attacked by fever. But the remarkable healthiness of the men employed in the Newfoundland fisheries, where, as it is well known, they are generally enveloped in the dampest fogs for several months together, affords the least ambiguous proof, within my knowledge, that the atmosphere, when loaded with pure moisture only, has no greater power of causing fever, than it has when in any usual state of dryness. "It is difficult," says that celebrated astronomer, Mr. Cassini, in the account he has given of his voyage to Newfoundland, "for one who was never there, to form an idea of the life the fishermen lead at the Great Bank. It must be no less powerful a motive than the thirst after gain, which can prevail upon these poor wretches to spend six months between the sky and water, in a climate where they are almost always excluded from the sight of the sun, and constantly breathing so thick a fog, that they can hardly see from one end of the ship to the other." Page 125.* In such an atmosphere then, if any where, we might expect to find the effects of moisture on the human body, manifested and exemplified in the most decisive manner; but fevers, or other severe disorders, are so little to be included among those effects, that Dr. Lind, after mentioning "the surprisingly healthy state of the ship's companies, who annually visit the Banks of Newfoundland," adds, "it is a constant observation, that the men belonging to the

* The waters which issue from the Gulph of Mexico, forming what is commonly called the Gulph Stream, flow with considerable rapidity near the Banks of Newfoundland, bringing with them a temperature of from six to ten or twelve degrees warmer than that of the super-incumbent atmosphere, and of the sea itself in that part of the ocean, according to the season of the year. This superior heat in the Gulph Stream, aided by its motion, produces a copious evaporation of aqueous particles from the surface, which are immediately condensed by the coldness of the air, so as to produce those fogs which, during summer, prevail on the Newfoundland station, to a greater excess, probably, than in any other part of the globe, unless it be in the "*valles*" of Peru.

Newfoundland fleet, return every autumn to England with *much more healthy and robust* constitutions than they left it."*

From the preceding remarks I deduce these conclusions, viz. that pure water existing under any form in the atmosphere, does not cause fever; therefore, that marsh exhalations would be innocent, if they consisted merely of simple moisture; and finally, that, since these exhalations do produce fevers, they must contain some other matter than moisture, which imparts to them the noxious qualities they possess. This leads me to inquire what this matter may be.

The substances which compose the soil of marshes differ little, if at all, from those which are found in other soils, and, according to the most recent investigations of chymists, they seem to be the following, viz: calcareous, siliceous, and argillaceous earths; sometimes magnesia, oxide of iron, and vegetable and animal matters, in various proportions, with a few saline compounds, (often in quantities so small as not to be easily detected) and water; but they differ as to their relative quantities; the proportions of the water and of the vegetable and animal matters, compared with the other ingredients, being much greater in marshy than in dry soils. If these various substances be classed according to their respective kingdoms, we shall readily perceive that none of those which belong to the mineral kingdom, can constitute the vapours which arise from marshes, because none of them is able

* See Dr. Lind's Essay on the Diseases incidental to Europeans in hot climates. Pages 30, 31. Since the above was sent to the press, the author being at Falmouth, on his way to Jamaica, went to one of the copper mines at St. Dye, about ten miles distant, into which he descended with several gentlemen, his fellow passengers, more than 120 fathoms, and remained there three hours, in an atmosphere so *overloaded with moisture* that the clothes, with which they had been supplied for the descent at the mine, were soon made wet, as was every thing which they had occasion to touch. But this humid air did not produce the slightest injury to the health of any of them; nor could they discover, after having made very particular inquiries, that the workmen in this and the other mines were more liable to fevers than persons otherwise employed above ground; though it was stated, that pulmonary affections were more frequent among them, probably from causes which do not relate to this subject.

to assume an æriform state, at least in any temperature to which the atmosphere, or the earth are ever naturally heated; and for this reason, as well as for many others, which are sufficiently obvious, it is plain that the noxious effects of marsh exhalations, cannot be produced by the mineral substances contained in the soil. With regard to the other matters, I mean those which belong to the animal or the vegetable kingdoms, we know that in suitable circumstances of temperature, air, and moisture, all organized bodies begin to decompose as soon as the principle of vitality is destroyed, and that, in the decomposition of them, a large portion of their constituent parts is converted into æriform fluids of different kinds, such as those which are at present known by the names of nitrogen, hydrogen, carbonic acid, hydro-carbonated, phosphorated, sulphurated, and other gazes. It is therefore evident, that in a marsh, where myriads of plants and animals are constantly perishing, and where the presence of water causes them afterwards to undergo a variety of decompositions and of new combinations, an abundance of vapours must necessarily be disengaged from those decaying substances, and arise from the surface of the earth, along with the moisture which is evaporated at the same time. Hence it appears, that the atmosphere of marshes must necessarily contain, besides common air and moisture, a quantity of vapours extricated from vegetable and animal matters during their decomposition; and since the fevers caused by marsh effluvia do not proceed from the action either of pure atmospheric air, or of pure moisture, on the human body, it follows, that they can only be produced by that of the vapours last mentioned. Several ingenious persons have endeavoured to analyze the air of marshes; but as their experiments are very imperfect, and the results of them, in some respects, contradictory to each other, we are yet without any decisive or satisfactory information on the subject.* When science shall be more ad-

* Air collected immediately over, and close upon, the surface of marshes, has commonly been found to contain hydrogen and carbonic acid gaz in considerable propor-

vanced, all the *chemical* ingredients of marsh exhalations may, perhaps, be discovered; but as most, or all, of the airs or gazes hitherto known, have been respired either singly, or variously combined, by persons who have submitted to the experiments of what has been called Pneumatic Medicine, or by others engaged in chemical pursuits or manufactures, without any of these persons having been attacked by fevers afterwards, (although some of those airs have produced other injurious and even fatal effects;) it is very possible that the property of causing fever does not belong to any one of those gazes in particular, but rather to several of them collectively, or, perhaps to some peculiar miasm emitted at the same time with the gazes, which it may be as impossible to detect by any tests, however ingenious, as it is to detect the contagions of the small-pox, measles, typhus fever, &c. when existing in the common air. We may never even discover whether the vapours of marshes derive their property of causing fever from

tions, with a great deficiency of oxygen gaz. That this should be the case might well be expected, considering what the constituent parts of vegetables and of water are, so far as we have been able to discover them, and how they must naturally act upon each other, when undergoing spontaneous decomposition. Animals are supposed to consist of principles similar to those of vegetables, with the addition of nitrogen, (or azote) and of sulphur and phosphorus, in different states. A few, indeed, of the vegetables are composed of nearly the same matters as animals. Ammonia, of which nitrogen is a constituent part, has been supposed, particularly by Van Mons, to have the property of correcting or meliorating air, when it abounds with carbonic acid gaz. If this supposition be well founded, will it enable us to understand why the vapour of *animal matters only*, when they are decomposing or putrefying, does not excite fever in mankind as that of vegetables appears to do?

Dr. William Currie, of Philadelphia, seems to believe, "that the unwholesomeness of low and moist situations in the summer and autumnal months is not owing to any invisible miasmata, or noxious effluvia, which issue from the soil, and lurk in the air, but to a deficiency of the oxygenous portion of the atmosphere in such situations, in consequence of vegetable and animal putrefaction, in conjunction with the exhausting and debilitating heat of the days, and the sedative power of the cold and damp air of the night." (American Philosophical Transactions, vol. iv. p. 128.) But if the mere abstraction of a part of the ordinary proportion of oxygen in the atmosphere could occasion intermittents or remittents, these fevers ought to be produced by every crowded assembly, and in a multitude of situations, where no such effect has been observed o suspected.

vegetable matters alone, or from animal, or from the mixture of both: indeed, the discovery, could it be made, might prove of little utility towards explaining the effects of marsh exhalations; but at present it seems probable that this property is derived from vegetable matters exclusively; because some decomposing plants, particularly hemp and flax, during their preparation by steeping in water, and the indigo plant laid in heaps (after the colour has been extracted) to form manure, have often been accused, and in several instances with great apparent justice, of occasioning dangerous fevers among persons living near them;* while, in regard to animal matters,

* Some difference of opinion has formerly existed among medical writers, concerning the morbid effects produced by the steeping and partial decomposition of hemp and flax to fit them for subsequent operations. Lancisi, however, after having considered all the known facts respecting this question, thinks them easily reconcilable by admitting that this operation is harmless, if performed in streams of *running* water, ("nihil obesse lini maceratione in aquis fluentibus;") but noxious in stagnant shallow water, and confined situations; ("contra vero ejusmodi macerationem *pestilentem* esse constat, ubi palustres desident aquæ, ventique silent;") and he gives the history of an epidemic fever, commonly intermittent or remittent, and often resembling the tertiana lethargica of *Torti*, which, for several summers, infested and almost depopulated the ancient town of Urbs Vetus, in an elevated and salubrious part of Etruria; and which was occasioned by ponds or stagnant waters, in the lower part of the town, in which hemp and flax were macerated; (in quibus linum & cannabis macerabantur") but this being prohibited in 1705, the fevers did not afterwards recur. See Lancisius de noxiis paludum effluviis, pages 32, 242, and 254. I was also informed at Naples, that in several places near that city, and particularly in some beyond the Grotto of Posilippo, the sleeping in houses contiguous to ditches, in which hemp or flax were steeping, had been almost constantly followed by fever.

Equally injurious effects have been ascribed to the preparation of indigo, both in the East and West Indies, by several writers; and, according to the best information, which I have obtained on the subject from well-informed gentlemen, who had been largely concerned in the manufacture of that article, these are chiefly occasioned by the exhalations, arising from vast heaps of the indigo plant, which are negligently formed (after the colouring principle has been extracted) near the works and houses of the labourers, and there left to decompose and become manure, which is of an excellent quality after two or three years. These heaps, wetted from time to time by heavy rains, and afterwards heated by the powerful rays of a vertical sun, emit very copiously, vapors, or miasmata, resembling in their effects those of marshes, for those persons who live near to, and especially on the leeward side of, these fermenting vegetable masses, are commonly attacked by fevers, chiefly remittents, and similar to those which prevail in swampy situations. And, according to my information, the numerous facts already mentioned, seem to prove that, however putrid they may become, their effluvia do not excite fever of any kind; and, in regard to the *mixture* of putrefying animal and vegetable matters, we have daily proofs that vapours may arise from them, for example, from large dunghills, without sensibly affecting the health of people who live close to them, or who are enveloped for hours together in their fumes, while working upon them.

As it appears, from these observations, that the noxious ingredients existing in marsh vapours can only be yielded by vegetable or animal* matters during their decomposition; this conclusion leads us naturally to suppose, that the marshes best adapted to emit powerful miasmata must be those in which the proportion of vegetable or animal substances is greatest, and in which their decomposition will be the most rapid and complete. The circumstances, therefore, which favour such decomposition, deserve particular notice, as favouring the production of miasmata in an equal degree.

Animal and vegetable substances require for their spontaneous decomposition, moisture, the contact of air, and certain degrees of warmth. With regard to the first of these agents, (moisture) it is so necessary, that there is nothing more efficacious in preventing such substances from putrefying, even for centuries, than the total deprivation of it. It may, therefore, be affirmed, that moisture is essential to

connexion of these fevers with the heaps of fermenting indigo plants is now so well understood and believed in that part of the world, that the more intelligent indigo-makers no longer permit such heaps to be formed near their works, or the habitations of their workmen, but cause them to be placed at considerable distances, and to the leeward thereof, and thus preserve their labourers in health.

* In this and other places I mention animal substances as concurring with the vegetable in producing marsh miasmata, because there are, probably no grounds whence these miasmata arise, which do not contain some dead insects and reptiles, with other animal matters; and I cannot venture to assert, that these have no share in producing the morbid exhalations in question; though, for the lately-given reasons, I am disposed to believe, that they are wholly formed by the mutual decompositions of vegetables and of water; and that animal matters, when in considerable proportions, may even have an opposite, or correcting effect.

putrefaction, and, consequently, that no miasmata can be formed, in a soil which is perfectly dry. Accordingly, it is found on the west coast of Africa, and in some of the West Indian islands, which are liable to long droughts, as Barbadoes, and more particularly Antigua, that marsh fevers occur very seldom in those dry seasons; but that they become very prevalent whenever these droughts are suddenly terminated by frequent rains. But neither will putrefaction take place without the presence of air, moisture alone being insufficient for that process; thus substances, which would have been readily decomposed in the open air, have been preserved uncorrupted for ages, while immersed in water, and thereby, in a great degree, secluded from the air; for although it be true that air or oxygen exists naturally in water, yet it exists in a quantity which is often too small for any, but very slow, putrefaction, at least in certain substances.

Examples of this fact are not, indeed, frequent with animals, probably because they naturally contain more air, and therefore require less for their decomposition than vegetables; but it is very certain, that many kinds of timber have remained under water for a great lapse of time in a perfectly sound state. Hence we may perceive, also, that the formation of miasmata, instead of being assisted, will be greatly impeded by a superfluity of water, (dividing and separating the matters to be decomposed, and obstructing the access of air to them) and that it will be most abundant in that soil which contains *no more moisture* than is really necessary for a complete decomposition of the vegetable and animal matters existing therein. An attention to this *important* truth will enable us to understand why, in some countries, frequent and heavy rains render marsh fevers prevalent, while in others, the deprivation of rain for two or three months produces equally morbid effects. Dr. Lind was fully convinced of these *similar* results from such *seemingly opposite* causes in different countries; and, at page 43 of his volume on preserving the Health of Europeans in Hot Climates, (5th edition) he appears to have thought it difficult to assign satisfactory reasons for them. What I have just mentioned respecting the Western Coast of Africa and the Islands of Barbadoes, Antigua, &c. will serve to illustrate and prove the *morbid* effect of much rain in dry situations; and for instances of equally morbid consequences in opposite situations, from the *want* of rain, we need only refer to certain countries between the tropics, which being naturally very low, are mostly overflown during the rainy seasons, in which their inhabitants are commonly healthy; fevers being rarely seen among them until the prevalence of dry weather has so far caused the water to evaporate from the ground as to leave the surface uncovered in many places. This notoriously happens in the Dutch and French Colonies on the Coast of Guiana; I mean Surinam, Berbice, Demerary, and Essequebo, as well as at Cayenne, and the adjoining settlements on the Continent, where marsh fevers only prevail in the latter part of the dry seasons. The like causes produce or augment the noxious influence of marsh miasmata at Fort Royal, and its neighbourhood in Martinico; and, to use the words of Dr. Gillespie, "with greater effect when the rivers are *low* by the continuance of dry weather, and when the tides, which never rise more than one foot, are weak.—This," he adds, "seems to account for the generation of remittent and intermittent fevers more powerfully in *dry* than in *wet* weather, as is the case here." Observations on Diseases, &c. on the Leeward Island Station, &c. p. 24. Dr. James Clarke also, when treating of the bilious remittent fever in Dominica, observes, that "when there was much rain in the months of May and June, and *dry* sultry weather prevailed in the following months of July and August, this fever raged much among the troops and strangers." Treatise on the Yellow Fever, &c. p. 75.

This is also the case in a great part of St. Domingo, as has been observed both by French and British writers. Among the former, M. Gilbert, who was chief physician (medecin en chef) to the army sent, in 1802, under General Le Clerc, to reduce that island, ascribes the aggravated violence and prevalence of the yellow and other marsh fevers, which, in a few months, nearly destroyed that army, to "les effets d'une *secheresse* extraordinaire, et d'une chaleur dévoranté." See p. 4 of his "Histoire Medicale de l'Armée Françoise à St. Dominguée." He afterwards refers, at p. 69, to the work of M. Poupé Desportes, who died at St. Domingo in 1748, and who, after having attentively observed the Yellow Fever in that island for fourteen years, found "qu'elle a été toujours d'autant plus *cruelle*, que les années ont été plus seches."

Baglivi informs us, (Opera Omnia, p. 157, 158,) that the marsh fevers, arising from damp situations in and about Rome, were greatly aggravated by the like causes; and he, therefore, adds "mirum non videatur si consulibus L. Valerio Potito & M. Manlio, Pestilentia orta sit in agro Romano, *ob siccitates & nimios solis calores*, teste Livio, Lib. V." That which Livy here, and in some other places, has denominated Pestilence, was, probably, no other than a violent epidemical marsh fever, differing, perhaps, a little in degree (only) from what is now called Yellow fever.

These facts suggest, and enable us to understand, the expediency of sometimes inundating a marsh, during the heat of summer, when its exhalations prove noxious to the inhabitants of a neighbouring town; it having been always found, that so long as marshes are completely overflowed, the vapours arising therefrom are innoxious, and that they only become injurious when so much of the water has been evaporated as to expose the surface of the soil to the air.* For these reasons

* Of this Sir John Pringle gives a decisive proof, at p. 62 of his Observations on the Diseases of the Army, 7th Edit. viz. The country round Breda had been inundated at the commencement of the war, for military purposes; but early in the summer of 1748, the preliminaries of peace having been signed, the water was let off, and the grounds, which had been covered by it, were, by this operation, made bare and exposed to the sun's rays, so that "a dangerous epidemic fever, of the remittent kind," soon "raged at Breda and the neighbouring villages. The States of Holland, being made sensible of this, gave orders to let in the water again, and keep it up till winter." An expedient which produced the desired effect, as it has done on other similar occasions.

it appears very probable that any piece of ground, in a hot climate, which contains a portion of fresh, or undecomposed, vegetable matters, and which, by being low and flat, with a suitable substratum, or intermixture of clay, is adapted to the retention of moisture, would, if supplied with only a moderate quantity of water, be soon in fit condition for emitting very concentrated miasmata; and it seems very probable, also, not only that the quantity of water necessary for this effect might not be sufficient to convert the ground into what is commonly called a marsh, but that it might even be so small as to escape common observation.*

Here, however, it is necessary to observe, that, under some circumstances, the earth may contain very large quantities of vegetable matters, and an abundance of moisture, and yet not be in a state to give out vapours capable of causing fever, although the temperature should be such as to permit the formation of them.—This is the case with peat-bogs or moors, the inhabitants of which, as I have before remarked, are exempt from those intermittent fevers, which are so frequent over marshy districts.—These bogs possess, in a remarkable degree, the power of preserving substances from putrefying, it being well ascertained, that not only plants and trees, but even human bodies with their clothing, when completely immersed in the peat-soil, will scarcely undergo any change during a long course of years; and it is probably owing to this peculiar property that they do not exhale, and, perhaps, do not generate, miasmata similar to those which arise from marshes.—For proofs of this exemption of the inhabitants of peat-bogs from intermitting fevers, see Appendix, No. 6.

* Dr. M'Lean, in his volume on "The Diseases of the Army in St Domingo," at page 25, says, "It must be admitted, that fatal miasmata arise where there are no very certain appearances of a marshy soil. The Mole and St. Marks, (St. Domingo) do not appear surrounded with marshes, and yet the fever *reigns* in both these places with *great activity*."

Some chemists, who have made experiments with a view to discover the nature of peat, are of opinion, that its antiseptic powers are derived from that vegetable principle to which the name of Tannin has been recently given. It appears, however, that a certain proportion of iron is always found in peat; and as this metal, when either dissolved or oxydated, is capable both of preventing putrefaction and of tanning, or converting skins into leather, (a fact not generally known at present) there is room to suspect that chemists may have attributed to that imperfectly-understood principle, tannin, those properties which, in reality, belong to the oxide of iron alone.*

Heat is the last of the agents requisite to the formation of miasmata, which we are to notice. Putrefaction, it is known, is wholly suspended in a freezing temperature, and proceeds very slowly, while the mercury in Fahrenheit's thermometer continues below 45°; but, in proportion as the mercury rises above this degree, putrefaction takes place more readily, and proceeds with greater activity, being most rapid and complete in a temperature of about 100°; every addition of heat, however, beyond 100°, seems to check that process.

Hence we perceive how much more copiously the miasmata given out by vegetable and animal substances during their decomposition must arise from marshy grounds in hot than in cool weather; moreover, a warm temperature is suited, in a remarkable degree, to the growth and multiplication of plants and animals—and thus it yields a plentiful supply of materials from which miasmata are formed; it is not surprising, therefore, that we should uniformly find the exhalations of marshes to be most powerful when the seasons are hottest, or that the

* I ought here to observe, that the antiseptic power of these bogs, (whatever it may be) is not sufficient to preserve animal and vegetable matters from decomposition, in a very *high* temperature; and it is, perhaps, for this reason that peat-bogs, although frequent in countries in which the summer is warm enough for the production of intermittents, are not known to exist between the tropics, unless, perhaps, in the more elevated parts of Quito and Peru, where the heat is less than in France and Spain.

violence of marsh fevers should always correspond with the heat of the atmosphere at, or some time previous to, their occurrence. Where the surface of the earth remains frozen for a considerable space, as, during the long winters of northern countries, these exhalations have no existence, and marsh fevers never occur, unless it be from miasmata, generated and *imbibed* during the preceding summer and autumn. They begin, however, to make their appearance soon after the return of spring, but at first only in their mildest and most innocent form; that is, as regular intermittents, generally of the tertian type; and they preserve this type while the temperature continues to be moderate. Afterwards, when the weather becomes warmer, these fevers become less regular and more severe: and, in the hottest parts of the summer or autumn, they frequently assume the more aggravated types of double tertians, or of remittents. In this country, however, where the summer is but moderately hot, the last-mentioned types of marsh fever are not usually of a dangerous character; but in Holland, the Netherlands, Germany, &c. where the heats of summer are generally greater than in this climate, marsh fevers are often attended with considerable mortality, as may be seen in the able account which Sir John Pringle has given of them in his work on the Diseases of the Army; and as was recently experienced in the unfortunate expedition to Zealand.

In the still hotter summers or autumns of Spain, Italy, and more southern regions, these remittents are yet more violent,*

* "Principio æstatis febres, ut plurimum tertianæ, non malignæ, corripiunt: adaucto vero æstu, febres continuæ, atque etiam exitiales urgent; longè tamen deteriores evasuræ, et planè pestilentes circá æquinoctium autumnale, præcipuè si pluviæ, nebulæ, rubigines, ventique australes accesserint. Tandem circa hyemale solstitium de pernicie ubique remittunt, &c." Lancisi de noxiis paludum effluviis, pag. 42.

"The epidemic of autumn, and prevailing distemper of this, (Zealand, Brabant and Flanders) and other marshy countries, is a fever of an intermitting nature, commonly of a tertian form, but of a bad kind, which, in the dampest places and worst seasons, appears as a double tertian, a remitting, or even an *ardent* fever. But however these fevers may vary in their appearance, according to the difference of constitutions and

and, as will hereafter be shown, they very frequently appear in the form which is known at present by the name of yellow fever, the most fatal examples of which may commonly be found in the hottest parts of the globe, when other circumstances are favourable to the production of marsh miasmata. Experience, therefore, warrants us to conclude, that in cool temperatures, none but the milder types of these fevers are ever produced, and (as will be abundantly proved hereafter,) that it is only in the hotter, that they occur in their most aggravated and violent forms: and although a considerable share of their increased severity should, as I think, be ascribed to the *direct* operation of heat *upon the human body,* a greater ought, I presume, to be imputed to its powerful chemical agency, in promoting the formation of marsh miasmata, more copiously, and probably with greater morbific powers.

I ought here to notice the well-known influence of clay, in promoting the formation of marsh miasmata, either as a stratum for the soil, in which they are formed, or when mixed with it in a large proportion. The celebrated Linnæus, who, in the early part of his life had travelled over nearly the whole kingdom of Sweden, found, or thought he had found, that intermitting fevers occurred in all those places, where the soil abounded with clay, and *only* in such places. Strongly impressed with this fact, he was led by it to imagine and believe, not that clay contributed to the production of marsh miasmata, by retaining the water necessary for the decomposition of organized matters, (as is probably the case) but that the particles of clay being dissolved in the water, drank by the inhabitants of these places, were conveyed into the blood vessels, and there occasioned fevers, by creating *obstructions;* according to the Bœrhaavian doctrine, then prevalent. In

other circumstances, they are all of a similar nature. For, though in the beginning of the epidemic, when the heat is greatest, they assume a *continued,* or a *remitting* form, yet, by the end of autumn, they usually terminate in regular intermittents." Pringle, Observations on the Diseases of the Army, p. 6.

this belief, he published his "Hypothesis nova de febrium intermittentium causa;" as an inaugural dissertation for the degree of Doctor in Physic, which he took at a later time of his life than was usual. In this dissertation he mentions the "*loca natalia*," or places in which he had found intermittents to exist; and the different degrees of force and frequency in which they occurred, and also those in which these fevers were wholly or almost unknown. By this account they appear to have been prevalent in the Southern and Eastern provinces of Sweden, but not in the Northern, where, with a very few exceptions, they had never been seen; a circumstance which might naturally have led him to take into his consideration the effects of temperature in contributing to their production. Attending, however, only to the supposed *co-existence* of clayey soils, and intermittents, (which a Swedish clergyman, at Philadelphia, had represented as being a fact, in that part of America also,) Linnæus delivered his new hypothesis in these words, "Nostra igitur est sententia quod intime solutæ particulæ argillaceæ, quæ lubricæ sunt, cum aqua simul potæ et cibo mixtæ, sanguinem intrent et tandem ultimis vasis arteriosis resideant, & morbi symptomata creent." In the truth of this hypothesis he was so confident as to advance this assertion, "Vana tamen est omnis cura, ni caveatur simul a *causa data*, scilicet aqua argillacea, quod experientia toties comprobavit." Linnæi Amænitales Academ, Vol. I.

Besides the greater or lesser *aptitude* of particular soils to retain the portions of moisture best suited to the decomposition of organized matters, some differences, both in the quantities and qualities of miasmata generated therein, will probably result from the particular vegetable and animal substances dispersed in them, and from their relative proportions to each other. Lancisi "De noxiis Palud." &c. p. 44, supposes the marsh effluvia of particular places to differ from each other, as well in their *nature* as effects—and Sir John Pringle asserts, "that the putrefaction of animal or vegetable substances, in a dry air, is apt to produce a bad fever of a more *continued* form; whereas putrid effluvia, in a moist atmosphere, have a greater tendency to bring on paroxysms and remissions." (Diseases of the Army, p. 324, 7th edit.) Probably, however, this distinction, if it has any foundation, must principally depend on a greater concentration of the miasmata *extricated* in a dry air, than of those which have been diluted and diffused by the redundant aqueous particles which necessarily accompany great moisture. Whether this greater or lesser concentration constitutes the whole difference between the morbid exhalations of particular places and seasons is a question which I am afraid to answer, because the known facts connected with it are too few to warrant a decision. It is, indeed, probable, as will hereafter be mentioned, that the miasmata of particular towns, (mostly either sea-ports or accessible to shipping) in which the aggravated forms of yellow fever have almost exclusively prevailed in the West Indies, the United States of America, and the southern parts of Europe, differ from the common exhalations of marshes, in *quality* as well as in degrees of concentration; but whether this difference be occasioned merely by the greater heat which, at such times, commonly exists in these towns than in the surrounding country, and which may exalt the powers of such miasmata, by perfecting the decompositions which produce them, or whether it be partly the result of a difference in the organized matters decomposed by that excessive temperature, I am unable to determine.

But, besides the influence of rain in the *formation* of miasmata, it seems to assist afterwards in promoting their *extrication* from the soil, and their diffusion in the atmosphere. Dr. Blane, (p. 261 of his Observations on the Diseases of Seamen,) considers moist air as "a vehicle of noxious exhalations, with which (says he,) it seems to have a greater chemical affinity than dry air." This is, at least, true of carbonic acid gaz, which is, doubtless, one ingredient of these exhalations. There are, moreover, some countries, in which rains may promote the extrication of miasmata without any chemical attraction, merely by loosening and opening the surface of the earth, which, particularly on the coast of Africa, is often overspread by a hard crust in the dry seasons. Dr. Lind, in his work on the Diseases of Europeans in Hot Climates, states this fact (p. 47,) adding, that, "by the continuance of rains this crust is softened, and the long pent-up vapours are set free, which thence become the cause of sickness."

Dr. Henry Warren, also, seems to have observed the effect of rain in assisting the extrication of marsh miasmata, but without suspecting the mode of its operation, which indeed he was not likely to do, because he believed the yellow, or, as he called it, malignant fever, of Barbadoes, to arise solely from contagion. He states, however, at p. 8 of his Treatise, and, as he thinks, "with great certainty, that, at the time this malignity is actually harboured among us, (i. e. in Barbadoes) a continuation of dry and sultry weather has been so far from giving any aggravation to it, that it has rather seemed to repress it, and make it lie more lulled and dormant, until the returning rains, and a moist atmosphere, had set it at liberty to exert its rage." It is difficult to conceive how rain could have produced such an effect, if the disease had been propagated, as Dr. Warren supposed, by personal contagion; and therefore his statement of a fact, at variance with his theory, is the more to be depended upon, because it must have clearly manifested itself to his senses; and it will serve to confirm what I lately mentioned at p. 198 of the salubrity of the air at Barbadoes during the continuance of hot and dry weather.

It has been frequently observed by others, as well as myself, that the prevalence of cold easterly winds, in the spring, has been soon followed by that of intermittents in persons who had been exposed to marsh effluvia. Dr. Lind, at p. 16 of his volume on the Diseases of Hot Climates, mentions this to have happened in a very extraordinary degree, in the years 1763 and 1766; and he appears to think that an easterly wind has some peculiar aptitude for extricating or absorbing marsh miasmata, or, to use his own words, that it "*raises* a copious vapour from water mud, and all marshy, or damp places." I am, however, disposed to believe, for reasons to be hereafter mentioned, that, in this case, the east winds act merely as an *exciting cause* upon persons who had imbibed marsh miasmata during the preceding summer and autumn, and which had remained inactive during the winter.

From this general view of the sources of marsh effluvia, and of the circumstances which are requisite or favourable to their production and extrication, it will be perceived that the former depend entirely on the joint influence of seasons, and of local circumstances:—The seasons, indeed, vary so much, as rarely, if ever, to resemble each other exactly in the course of many years, and are sometimes in one year the very reverse of what they were the year before at the same places; thus of the two winters of 1794-5, and of 1795-6; the former, says Dr. Heberden, in his paper on the influence of cold upon health, (in the philosophical transactions for 1796) was "the coldest, and the latter the warmest, of which any regular account has ever been kept in this country." We may conclude, therefore, that considerable variations, with regard to the formation of miasmata, must annually occur in every place; and we ought to find, not that the health of persons living in or near a swampy or low situation is affected in the same manner every summer or autumn, but, on the contrary, that it is very differently affected in different years, as it is known in reality to be, by all medical practitioners in marshy countries.

Thus it was observed many years ago by Dr. Chalmers, who practised medicine with great reputation for a long time at Charleston, in South Carolina, and paid very minute attention to the climate, that the yellow or putrid bilious fever appeared in that city whenever "the weather was *very* warm and *wet* withal."* This, he states to have happened in the year 1770, when "much rain having fallen throughout the summer," and the weather becoming "so warm that the mercury often rose to the 96th degree of the thermometer, the putrid bilious fever appeared in August, and continued till the month of October following."† The same observation has repeatedly been made, before and since his time, concerning the effects of a very hot and moist season on the health of the inhabitants of Charleston,‡ and when we recollect that the city itself is placed on a low flat point of land, between two rivers, upon which great encroachments have been made by wooden wharves, &c. and that the adjoining country is, also, very low, and in many parts swampy, we shall readily perceive, that a season like this must have been extremely favourable to the formation of miasmata in that situation. But a very different effect resulted from the very hot summer of 1752, described by the same author, vol. 1, p. 22, and in which "the mercury often rose above the 90th degree throughout the months of May, June, July, and August; and, for *twenty successive days*, excepting three in June and July, the temperature of the shaded air varied between the 90th and the 100th degree; and sometimes it must have been 30 warmer in the open sunshine, to which great numbers of people were daily exposed for many hours together."—When the mercury rose to the 97th and 98th° in the shade, the atmosphere seemed

* The ground, upon which the city of Charleston is built, though very low, contains a large proportion of sand, and more frequent falls of rain are, therefore, necessary for the production of marsh effluvia therein, than would otherwise contribute to that effect.

† See his account of the Weather, and Diseases of South Carolina, vol. 1, p. 163.

‡ Dr. Moultrie, of Charleston, in South Carolina, in his valuable "Dissertatio inauguralis de febre maligna biliosa Americæ," printed in 1749, mentions it as having been *there* observed, "*Quò calidior est cœli temperies, eò vehementius febris maligna biliosa grassatur*" And in confirmation of this observation he adduces the following instance, viz. "Anno 1748, in eodem loco (Caroli oppido) febris hæc erupit circa medium mensis Augusti, primâ cujus septimanâ nulla ibi unquam calidior erat, ut mercurius in Fahrenhetii thermometro ad 97°, 97 1-2 et 98 in aëre umbroso ascenderet, et calor hicce cum multis imbribus diú duravit: a cœli temperie in *frigidiorem versâ mitescit, et in intermittentem febrim mutabatur*." Page 8.

in a glow, as if fires were kindled around us: in breathing, the air felt as if it had passed through fire; nor were the nights much less sultry and distressing than the days; refreshing sleep, therefore, was a stranger to our eyes, insomuch that people were, in a manner, worn down with watching, and the excessive heat together." It is not, I believe, easy to conceive that any state of the body could have been more favourable to the occurrence of the yellow fever than the above; yet, says Dr. Chalmers, "a more healthy season had never been known than this, so long as the weather continued *steadily warm and fair.*" The cause of this singular healthiness is, however, easily explained: for during this time, as appears from another part of his work. (i. e. p. 18 and 19) "a *general drought prevailed.*" "The earth was so parched and dry that not the least perspiration appeared on plants, which shrunk and withered; all standing waters were dried up, so that travellers could not find water for themselves or their beasts for a whole day together:" "in several settlements no water could be found by digging ever so deep." While the earth was so completely exhausted of moisture it is obvious that no miasmata would be formed in it.*

Having offered these examples of the opposite effects, which two different seasons have actually produced in the same place it would be superfluous to enter upon a detail of the divers consequences which may result from other varieties of season. Nor is it necessary to present a statement of particular facts

* Dr. John Hunter, in his Observations on the Diseases, &c. of Jamaica, observes, at p. 13, that "the heat of tropical climates, though generally reputed the cause of their unhealthiness, will not alone produce fevers, as is strongly exemplified in those living on board of ship, who remain free from fevers; and, also, in the inhabitants of certain *dry sandy* spots, along the coast, in which the heat is uncommonly great, yet the situations are healthy, as Fort Augusta, Port Royal, and others."

In Egypt, when the British army, at the siege of Alexandria, in the summer of 1801, was encamped on *dry sand*, at a distance from all swamps, with the sea on one side and the Lake Maadie on the other, a fever was rarely, if ever, seen. I observed a similar exemption from fever in the same season at Rosetta, which is at some distance from any swamp.

to shew that the differences of local circumstances are neither less numerous nor less important, towards the generation of noxious exhalations than those of season. It may, therefore, be affirmed, that as the inhabitants of any one place cannot be affected in the same manner in regard to health every summer, or every autumn, so likewise they may be very differently affected, in the same season, from the inhabitants of another place situated within a short distance from themselves. Thus it seems perfectly consistent with the laws of nature that the summer of 1800, which produced the epidemic yellow fever in Cadiz and Malaga, should not have been able to produce it at Gibraltar; and that the summer of 1804 should have been such as to occasion the disease in each of these towns. In another part of this volume I shall endeavour to shew that the above variety in the occurrence of the disorder at those places may justly be ascribed to situation; and will therefore, abstain from any further observations at this time on the influence, which mere locality possesses over health, under different circumstances of weather.

The distance to which the exhalations of marshy grounds may be conveyed from their source, and retain the power of causing the yellow or other marsh fevers, will partly depend on the force of the wind, and partly on the extent of the surface from which they arise, and on their being more or less copiously extricated from that surface. If the wind be very moderate, and blow steadily from the same point, and if the miasmata be abundantly emitted from a very great extent of surface, it seems probable that so large a mass of them as would thus be formed might be conveyed a quarter, and perhaps half a mile, before it became so diluted with atmospheric air, or so dissipated by the wind, as to lose its morbific power: and, it is obvious that such a mass of exhalations, if it were wafted into a town, would be able to produce fever in the majority of the people inhabiting the quarters which it traversed, with as much ease as it would produce fever in an individual only; or, in other words, that it would within a certain extent, as easily cause an *epidemic* as a *sporadic* fever.

In mentioning "a quarter, and perhaps half of a mile," as the greatest distance at which marsh effluvia seem capable of being conveyed, even under the most favourable circumstances, from their source, so as to produce disease. I have confined their morbid influence within much narrower limits than those which are generally described by medical writers; most of whom suppose marshes capable of exciting fever at the distance of several miles. It is, indeed, to be regretted, that observations on this subject, have not been made, and reported with greater care and precision. Sir John Pringle, indeed appears to have thought more justly on this subject, and after describing the epidemic marsh fever which raged in Zealand, both among the inhabitants and the British troops, in the year 1747, he adds, "But Commodore Mitchell's squadron, which lay all this time, at anchor, in the channel between South Beveland and the island of Walcheren, at both which places the epidemic prevailed, was neither afflicted with the fever nor the flux; but amidst all that sickness enjoyed perfect health." (Diseases of the Army, p. 57.) This immunity of the British seamen is, by the author, justly ascribed to their having been out of the reach of that which he calls "the moist and putrid air of the marshes," though the whole width of the channel is, I believe, in general, but little more than *one* mile and therefore the squadron could not, even at midway, be placed at more than half that distance from the grounds whence noxious miasmata arose. Dr. Lind (on preserving the Health of Seamen, p. 69) notices this fact, and makes the following addition to it, viz. "when Commodore Long's squadron, in the months of July and August, 1744, lay off the mouth of the Tiber, it was observed that one or two of the ships, which lay *closest* to the shore, began to be affected by the pernicious vapour from the land, whilst some others, lying further out at sea, at but a very small distance from the former, had not a man sick; at the same time, the Austrian army, under the command of Prince Lobcowitz, suffered so great a sickness, through the proximity of their situation to the marshy country, that they were obliged to decamp."

Dr. Blane, also, observes, that "it is difficult to ascertain how far the influence of vapours from wood and marshes extends, but there is reason to think that it is to a *very small distance.* When ships watered at Rockfort, (Jamaica) they found that if they anchored *close* to the shore, so as to smell the land air, the health of the men was affected, but upon removing two cables' length, no inconvenience was perceived." (Diseases of Seamen, p. 206.)

But the most decisive evidence on this subject has been obtained by the late expedition to Zealand. Drs. Blane, Lempriere, &c. in their report to the Secretary at War, dated Middleburg, October 10, 1809, and printed by order of the House of Commons, assert their "having *ascertained* that the crews of the vessels stationed in the very narrow channel (only a few yards* from the land) between Beveland and Walcheren, have continued perfectly healthy the whole campaign; thus decidedly proving that the noxious exhalation is nearly confined to its original source." Here it should be recollected, that it is stated in the same report, that "the number of sick and convalescents, in the different hospitals, amounted to more than two-thirds of the total force," *at that*

* This expression of "a few yards" is much too indefinite. In conversing on the subject afterwards with Dr. Blane, he appeared only to be certain that the vessels in question, or, at least, many of them, were stationed at *less than a quarter of a mile* from the shore. According to the best information which I have been able to obtain, the ships of war at Flushing were anchored generally at about one quarter of a mile from the shore. Those in the Roompot channel at about three-fourths of a mile from land. It was chiefly in the latter channel, and at about that distance from shore, that the transports having on board the cavalry, (viz. 2d Dragoon Guards, and 9th and 12th Light Dragoons) were stationed. These did not land, and, consequently, did not partake of the sickness. Mr. Webb, Inspector of Hospitals, in his evidence at the House of Commons, asserts, that "the men who remained on board the ships were *extremely healthy.*"

*time*, "notwithstanding about 1500 sick had been already sent home by different conveyances from Walcheren alone."

The general prevalence of health on board the ships of war and transports was also confirmed, on my enquiry at the Transport Office, by Mr. M'Leay, Secretary, and Mr. Houseman, chief clerk of that department. Dr. Blane, also, had the goodness to communicate to me a letter from Captain Hanchett, who commanded the Raven Sloop of War, during the expedition against Zealand, and being wounded, had remained thirteen nights on shore, (for the cure of his wounds) by which he contracted an obstinate intermittent. In his letter, dated Exeter, April 29th, 1801, Captain Hanchett writes as follows: "the Raven, while I commanded her on the late expedition, was more *through the narrow channels* of Zealand, and more *in shore* than any other vessel, of any description, employed there; her station being that of the leading ship of the squadron in shore withal; and after the action of the 3d of August, I went up the narrow pass between Schowen and Goree, (within four miles of Williamstadt) laying not more than a *pistol shot* from that shore, and was the last down upon the retreat. There was, however, *no ague in the ship but mine*, which was, no doubt, occasioned by my wound; and, I believe, there were very few in the other vessels of Commodore Owen's squadron." "I had forgotten to mention that, during the time we were refitting at Ter Veere, the men had leave (to go) on shore, *but never staid the whole night;* and, when laying off Schowen, they went on shore to bathe and watch, under the charge of the commissioned officer of each division, every evening at five o'clock; and after bathing they ran races along the dykes for half an hour, but there was never any appearance of ague except in myself."

The people of Italy have long had frequent and fatal experience of the noxious power of miasmata, (by them denominated *Mal' Aria*) with which Rome, in particular, is greatly infested during and after very hot and dry summers; yet Dr. Lind observes, that the effects of the *Scirocco*, or South east wind, "which passes over the adjacent marshes," have been experienced to extend only to the parts of the city which lay nearest the marshes, occasioning an epidemic fever in these, while the rest of the city was healthy." See his volume on Preserving the Health of Seamen, p. 67.

In like manner *Baglivi* represents the Mal'Aria of Rome, as acting only in particular spots or parts of the city, and asserts as matter of wonder, that the *healthy* are separated from the *unhealthy* spots, *only by very short spaces*;* the former being chiefly on the northern and eastern quarters of the city, *farthest from the river*: for the Piazza, and Porto del Popolo, though on the north, are extremely, unhealthy, by being low and *close* to the Tiber. Indeed, marsh fevers at Rome commonly begin about the Porto del Popolo.

* Aer Romanus Squallidus est & insalubris, non quidem omnibus in locis, sed iis potissimum quæ *deficientibus* ædificiis, pigro atque immoto aere sordescunt; multo magis si Tiberi adhærent, vel, convallium instar, montibus obsepiuntur, aut exhalationibus subjacent quas veteres parietinæ, cryptæ, & antiquorem ædificiorum rudera emittunt. Ex quo patet regionem Circi Maximi, inter Palatinum atque Aventinum sitam, omnemque illum campum qui inter aventinum ac Tiberim, portamque Ostiensem jacet, plane noxium esse & damnabilem." "Quæcunque loca *crebris* ædificiis ambiuntur, atque *editiora* sunt, in *septentrionem* atque *orientem* spectant et *multem* a Tiberi distant, salubriora: Contra, quæ sejuncta sunt & remota a frequentibus tectis, situque sunt humili, ac maximè in convallibus, tum *propiora* Tiberi, in meridiem atque occasum spectantia, minus salubriora judicantur: *Quibus etiam in locis (quod sane mirum) brevissimi intervalli discrimine*, hic aliquantum salubris existimatur aër; illic contra noxius & damnabilis." Baglivi Opera Omnia, p. 157, 158.

The reason why particular spots within the walls of Rome were *destitute*, or almost destitute of houses, seems to be that their (notorious) insalubrity had either destroyed or driven away those who formerly lived thereon, and when the existing houses were decayed, had deterred other persons from rebuilding in thóse situations: and therefore, Baglivi justly mentions these places ("deficientibus ædificiis") as being among the most noxious. It is from a similar motive that the General Committee of Health, of the City of New York, in their report on the means of securing the health of its inhabitants, (dated the 20th of January, 1806) after stating that "various houses, in different parts of the city, have, on *the recurrence of every malignant fever*, proved to be the *principal seats of disease*, and the graves of their tenants," "suggest the propriety of prohibiting the same to be let or occupied as dwelling houses, that they may be converted into warehouses, and that any injury sustained by the proprieors be defrayed by the public." See p. 95 of Documents, relating to the Board of Health, printed at Yew York, 1806.

I was repeatedly told at Rome, in the year 1802, by persons deserving of confidence, that these fevers sometimes prevailed among the inhabitants on one side of a particular street, whilst those on the opposite side entirely escaped their attacks; and this was said to have often happened in a certain portion of the *Corso;* the western side of which was distinctly pointed out to me, as being much more unhealthy than the other.* Professor *Berthe,* in his work on the fever of Andalusia, has mentioned similar facts as occurring in some of the streets and squares of Cadiz, in the year 1800; though he ascribed it not to the very limited action of marsh effluvia, but to that of the contagion, by which he supposed the prevailing fever to have been propagated in that city. And it seems highly probable, that in many cases the miasmata producing yellow fever in sea-port towns of the West Indies, and the United States of America, arise from the soil immediately *around,* and perhaps, sometimes *under* the very houses, wharves, &c. where they are imbibed, by the persons in whom that fever afterwards appears. Accordingly we find that, in New-York, Philadelphia, Baltimore, Norfolk, and Charleston, this fever *always begins,* and often *continues,* exclusively in the *low* streets immediately adjoining to the harbours and wharves of these towns, except in the cases of some individuals, who, after having imbibed the noxious exhalations of the

* Similar facts are stated by Lancisi, particularly in the 3d chapter of his second book, de Nox. Palud. Effl. in which are several instances of marsh fevers prevailing in *particular* parts of Rome, and its vicinage, whilst other parts *closely adjoining* remained healthy. In one of these he says the salubrious districts might be separated from the insalubrious, by a *diagonal* line drawn, "a postica Pontificii Palatii parte, quam *Belvidere* appellant, usque ad emissarium magnæ illius cloacæ, quæ in Tiberim juxta Arcis fossam eo loco aperitur," &c. p. 199. And in the next page he refers to several authors who have attested similar events, particularly Ramazini, De Constit. anni, 1690, who describes a tertian epidemic, which occurred at *Modena,* in that year, but was wholly confined to the *low* parts, in which there was *stagnant* water, and extended no further.—"Non ampliora spatia occupasse." The people in other parts, (which he designates) having never, within their remembrance, been more free from fevers. "Nunquam aliàs à febribus magis securos se vixisse meminerint."

wharves and low streets in question, by residence or employment in or near them, happen to fall sick in other situations.*

After these observations, respecting the distances to which marsh effluvia may be conveyed *horizontally,* without losing their morbific power, it may be proper to inquire how far they are capable of retaining it, when raised *perpendicularly,* or nearly so, from their source. Unfortunately, our stock of facts relating this point, is even more deficient than in regard to the other; though it is sufficient to ascertain that their power of exciting disease, is rapidly diminished at very small distances from the earth.

Dr. Hunter, in his work on the Diseases of the Army in Jamaica, p. 306, says, "the barracks at Spanish Town, consist of two floors, the first upon the ground, the second on the first. The difference in the health of the men on the two floors was so striking as to engage the attention of the assembly of the island, (of Jamaica) and, upon investigation, it appeared that *three* were taken ill on the ground floor, for *one* on the

* Being at the Horse-Guards on the 19th of November, 1810, I saw there Captain M'Koy, of the 21st regiment of foot, who had then just arrived with dispatches from Sicily, and was informed by him that, in July and August, 1808, while his own and another company were quartered at the post of *Venetico,* in a barrack of nearly 100 feet in length, which consisted of one (ground) story, *forty* men of the latter company, occupying one-half of the barrack, were attacked by a violent mal' aria fever, which proved fatal to eleven of them, but did not reach a single man of his own company, occupying the other half of the barrack, though there was no division between the parts or halves occupied by the two companies, nor any perceptible difference in the soil on which the different parts or ends of the barrack stood. Each had, indeed, its own door to pass into and out of the barrack, but both doors opened on the same side;—nor was there any difference in the discipline, diet, or management of the men of the two companies. Venetico is situated between *Melazzo,* and *Messina,* and is supposed to be more elevated than the Rock of Gibraltar, and, at least, 1500 feet above the sea. The barrack stood at the summit of this mountain, upon a rock covered by a *clayey loam,* and the noxious miasmata were, most probably, emitted more copiously from the soil at or about one end of the barrack, than that of the other. though no difference was discoverable therein. Great elevation, in a warm temperature, is no security against noxious vapours, if the elevated spot be covered by earth, containing vegetable matters, and in which *clay* predominates. Very pernicious marsh fevers notoriously prevail in such situations in the East Indies, (where they are called *hill* fevers) and *in* other places within the tropics.

other. The ground floor was not, therefore, used as a barrack afterwards." A similar fact occurred at St. Anne's barracks, in Barbadoes, between the 27th July and 20th of August, 1805, when two hundred and seventy-eight men of the 15th regiment of foot, then very lately arrived from England, were attacked by the Yellow Fever, of whom seventy-seven died. These men chiefly occupied the barrack which runs towards the sea, and is nearly at right angles with the officers' or *stone* barrack, and has "*low wet ground on each side.*"* In this barrack the men on the lower floor were "taken ill in the ratio of *three to one*, of those on the upper floor." This statement I have taken from a report made to Dr. C. Ker, then Inspector of Hospitals in the West Indies, by Mr. Major Carroll, Surgeon to the Forces, under whose care the sick in question were placed; which report was dated Barbadoes, 10th of September, 1805, and a copy of it put into my hands, by the writer, in November, 1806.

Whether similar differences occur in all climates between the ground floor, and the next above, in regard to the influence of marsh miasmata, I cannot determine; I believe that the former are, in this respect, every where, much more unwholesome than the latter. Sir John Pringle, after mentioning the prevalence of intermitting and remitting fevers at Ghent, and still more at Bruges, in the summer and autumn of 1742, adds, "it was then observed that such as lay in the upper stories, were much more healthy, than those who were below in the ground floors, which were all very damp." (Diseases of the Army, p. 13.) The same ill effect upon the *ground* floors was experienced during the late expedition at Walcheren, and, therefore, Drs. Blane, Lempriere, &c. in their report to the Secretary at War, lately mentioned, say,

* Dr. Chisholm, alluding to this part of Barbadoes, observes, that, "the eastern side, where Constitution Hill is situated, and where the king's-house, and an extensive barrack stand, is thought to be affected by *marshy miasm*, from a branch of the sea, which runs a considerable way into the country." Essay on Malignant Pestilential Fever, &c. vol. 2, 160.

"on no account should ground *floors* be used as *sleeping* apartments. The more lofty the buildings the better; for the tenants of the upper stories, not only enjoy the best health, but when taken ill, have the disease in the mildest form; an instance of which came under our observation when we visited Fort Ramakins, and the same is confirmed by the experience of the natives." When the small elevation of a single story (not exceeding twenty feet) from the ground, is found so greatly to diminish the power of noxious exhalations, it might be expected that the tops of hills rising a few hundred feet above the level of the sea, or of the surrounding country, would always be found healthy. Experience has, however, often proved the contrary, particularly on the *Morne-fortuné* at St. Lucie, and on the Hospital, and Richmond Hills, at Grenada, where very great mortality has repeatedly occurred among British soldiers. But in these and similar cases it seems probable that the soil, at or near the tops of these hills, contained matters suited to the formation of marsh miasmata, with sufficient proportions of clay to retain the necessary moisture. There can, indeed, be no doubt, that this is the case of the *Morne-fortuné*, which I observed to be very wet, and, in some degree, swampy. This and Richmond Hill, being at their tops more than seven hundred feet above the level of the sea, could not, I am persuaded, be so greatly affected merely by exhalations from any *low* and damp grounds in their neighbourhood.

After these facts, relating to the heights and distances at which marsh effluvia may be conveyed from their source, so as to produce fever, it may be proper to notice the greater degree of noxious power, which they are supposed to exert, when applied to the body during the night, than when applied in the day. This greater noxious power in the *night*, and especially *during sleep*, has been strongly asserted by Lancisi, de Noxiis paludum effluviis, p. 77, &c.; and he was so perfectly convinced of the fact that he has devoted a particular chapter, (the 21st) to explain the cause. "Cur juxta paludes noctu præsertim indormientes magis quam vigilantes lædantur?" And he begins this chapter by saying, "Nemo Arbitror de facti veritate dubitabit qui diu medicæ arte operam dederit;" and then declares. "Nos certe Romana Nosocomia per æstatem, & autumnum plena videmus miseris agrorum colonis; ac per urbem sæpe dolemus incautos venatores, ac peregrinos, quamquam non longo tempore palustria loca incoluerint, quia tamen *brevem somnum* prope lacunas cæperunt, malignis febribus afflictari." These facts he ascribes partly to an increased susceptibility of our bodies, "quæ in somno ad labem suscipiendam proniora fiunt," and partly to a difference in the condition of the effluvia themselves, "quæ per noctem præcipué deteriora evadunt;" and he explains the cause of this difference in these words, "Quod vero attinet, ad effluviorum pravitatem, certè eadem post solis occasum perniciorior est: quidquid enim per vim solis attenuari, dissiparique continget, eo recedente, concretione gravius efficitur, terræque rursus incumbit, et dormientes infestius adoritur," &c. p. 79. Lancisi refers to several authorities and instances in support of his assertions and opinions, and concludes by admonishing those who, in summer, travel through the Pontine marshes, between Rome and Naples, even without sleeping, *not to do it at night*, as was too often done. to avoid the greater heat of the day.* Similar admonitions are still given at Rome to all strangers, and they are founded on the uniform experience of ages, which has afforded numerous instances of travellers, who, in consequence of their passing these fens during the night, (though the passage requires but six or eight hours) have been attacked with violent and mortal fevers. This is confirmed by Baglivi, in the following words: "Vetus enim Latium desertum fere hodie est, & squa-

* "Neque vero solum dormientibus noxius est per noctem palustris aer; sed etiam iis qui vigilantes per cœnosa loca interfaciunt. Qua de re monitos vellem quotquot vel Neapoli Romam, vel Roma Neapolim contendunt, ut diurnos potius æstus subeant, quam nocturni frigoris voluptate decepti contemeratam ambientis aeris vim excipiant," &c. p. 80.

lidum; Austri flatibus immediate objicitur; & variis ejusdem in locis, insaluberrimus *aer* observatur, ut pote circa *Ostiam*, et *Portum æstivo* præsertim tempore; quo quidem si aliquis in præfatis, aliisque Latii locis *pernoctaverit*, & exinde urbem revertatur, *corripitur statim maligna febri*, quam vulgo ex mutatione aeris dicunt." Opera Omnia, p. 158.

It will be recollected, that in the instance of the Phenix Ship of War, mentioned at p. 96 of this volume, "none of those who *slept* on shore escaped the sickness, and only three of them survived it;" and that, though nearly all the rest of her crew, consisting of 280 men, went, in parties of twenty or thirty, at different times, on shore in the day, and "rambled about the island hunting and shooting"—"bartering for provisions, washing linen," &c. "not one of those who returned to the ship at night was taken ill, or suffered even the slightest indisposition." And that exactly similar effects occurred the following year, with the same ship at the same place, where "she lost eight men out of ten, who had imprudently remained *all night on shore*;" whilst the rest of the ship's company, "who, after spending the greatest part of the day on shore, always returned to their ship before night," "continued in perfect health." In like manner the crew of the Hound Sloop of War, (then in company with the Phenix,) by never sleeping on shore, continued in good health. It may be recollected also, that in the cases of the Ponsborne and Nottingham East Indiamen, (p. 99) those who had slept on shore were exclusively attacked by the fever; and, in particular, that "the carpenter (of the Ponsborne) and his crew, nine in number, by their all *sleeping* on shore, catched the fever and died, except one, who was a negro. The effects were exactly similar in the cases mentioned by Drs. Clark and Trotter, (p. 99 and 100) and in that of Fontana. Dr. Lind has, moreover, in different places, mentioned other instances of similar morbid effects, resulting from exposure to marsh effluvia *by night*, and the like has been done by Dr. Blane, Dr. John Hunter, &c. &c. We have, therefore, reason to believe, not only that the morbid miasmata are condensed or precipitated with the falling dews, by the diminished temperature of the night, and thus *accumulated* near the surface of the earth, but that the body is rendered more accessible to their noxious influence during sleep, by its greater relaxation, and by a suspension of those protecting exertions of the living power, which accompany our wakeful exercises. Perhaps something may also be owing to the fact discovered by Dr. Ingenhouze, that vegetables only emit azote or nitrogen at night, instead of the oxygenous or vital air, which is copiously separated from their leaves when exposed to the sun's rays.

I ought, were it possible, here to ascertain how, and through what channels, the morbid influence of marsh effluvia is applied to, and exerted upon, or within the body, so as to become the cause of fever. But here our knowledge is but little better than complete ignorance. The miasmata, in question, being absolutely imperceptible by any of our senses, can only be known by the disorders which they excite: these, however, are not such as to indicate the manner in which they were produced, nor the passages by which the morbific power has gained admittance. The throat, trachea, bronchiæ, and lungs, obviously present themselves, as the parts through which all æriform and respirable substances might be most readily and naturally introduced within the system, but these parts appear to suffer much less than the stomach, brain, and nervous system, from the impressions of marsh miasmata; and as there is much uncertainty and difficulty attending every explanation which has occurred to me on this subject, I shall abstain from proposing any; and proceed to enquire, how soon these miasmata, after being sufficiently applied to the body, commonly produce fever, and how long they may remain inactive therein, without manifesting any change or effect.

On this subject Dr. Lind says (p. 182 of his work on the Diseases of Europeans in Hot Climates) that "from comparing many instances of people who have *slept* on shore during the sickly season, and in consequence of it, *who alone have been taken ill out of the whole ship's company,* then lying in an open road, it appears that some are *immediately* seized with sickness or delirium, many are not seized with either, till they have been on board two or three days; several have been only slightly indisposed for the first five or six days; and, in a few, the symptoms of indisposition have not appeared before the 10th or 12th day."

The same author (on Preserving the Health of Seamen, p. 78) says, "there are numerous instances of boats' crews having suffered greatly by *sleeping* near the mangroves, with which the sides of rivers are frequently planted in the torrid zone. I have known the *whole* of a boat's crew seized *next* morning with bad fevers." And at p. 81, he mentions a similar fact, communicated by the Surgeon of a Guinea Ship, which, going "up one of the rivers for the sake of trade, it was found very dangerous to *sleep* on shore."—"First the captain, then the mate and two or three seamen were taken ill, *each* of them *the morning after they had lain on shore.*" But such very sudden attacks of fever from marsh effluvia are, I believe, uncommon, even in the hottest climates or worst situations. They, indeed, occur not unfrequently within four or five days; but are much oftener delayed until the 9th, 12th, and 15th days, after exposure to marsh miasmata, even at Batavia, Gambia, St. Thomas's, Mohilla, &c. Dr. Jackson, who, like some others, is disposed to believe that these attacks "take place chiefly at septenary periods," asserts "from his own observations made upon numerous bodies of men,"—and upon healthy soldiers, sent to the concentrated sources of endemic fever," that "among such, fever scarcely ever appeared before the seventh day, commonly not before the fourteenth; and in numerous instances, not till the expiration of six weeks or two months, though the cause of disease, during this time, was ordinarily in great activity." Outline, &c. of Fever, p. 248.

Dr. John Hunter observes, that "after the human body has been exposed to the poison, (of marsh effluvia) sometimes a longer, sometimes a shorter period elapses, before a fever is produced. "Men, (he adds) on the watering service, are not all taken ill at the same time; some fall sick the first or second day, others not till several days, after they have ceased to be exposed to the cause of fever, by returning on board of ship."—"Some have embarked on board the ship in good health, and have been seized after ten or fourteen days with the remittent fever. Examples in this way have come to my knowledge, of the fever appearing three weeks after ceasing to be exposed to the cause of it." Observations on the Diseases of the Army in Jamaica.—He adds, in a note to p. 329, that, "it would be curious and interesting in the history of the remittent fever, to ascertain the interval that *may* take place between exposure to the cause, and the appearance of the disease;" and then mentions the West Suffolk regiment of Militia, called, in 1793, from their own county, (one of the healthiest in England) to *Hilsea* barracks, of which the "low, marshy, and unhealthy situation, has been fatally known to the army, since their first erection." The men were then "all in perfect health;"—but "became very sickly; and twenty-two died of fevers, before they left the barracks," about the end of June following. In July this regiment, with eleven other battalions, was encamped at Waterdown, in the neighbourhood of Tunbridge Wells, and "their sick list soon amounted to 100, out of 500," among which were many with fevers, that "had all the marks of a bad remittent;" some of which terminated fatally—and the deaths in this regiment exceeded the amount of those in *all* the eleven other battalions together, on the *same* ground. But the point of most importance is that some of this regiment "were taken ill of the fever, in the month of *October*, *who had never had it before;*" i. e. nearly four months after their removal from "the *cause* of the disease, at *Hilsea* barracks."

In addition to this, he mentions the case of the 18th Regiment of Foot, in 1783 and 1784, as stated by Mr. Venour, then Surgeon of the regiment, and now Deputy Inspector of Hospitals. By his statement it appears, that the 18th regiment, after having been stationed at *Hilsea* barracks, from the 22d June, 1783, to the 9th October following, was then embarked for Gibraltar, where, though the regiment consisted only of 400 men, the number of agues was increased, by the beginning of May, to 280, including women and children,—of whom a considerable part were then *recently* attacked *for the first time;* and *whilst no agues existed in any other part of the garrison.* On these facts, Dr. Hunter remarks, (p. 334,) that "the 18th regiment of Foot, and the West Suffolk regiment of Militia, after leaving Hilsea barracks, were both in situations where they could not contract fevers, and the regiments encamped with the *latter*, and those in garrison with the *former*, had *no fevers.* There cannot, therefore, be a doubt that *the poison had remained quiescent in their bodies for four, five, and six months.*" When thus poisoned, "getting wet in the open air, proved a strong existing cause of fever, as was observed both in the West Suffolk and 18th regiments." He adds, "ships returning from a warm climate, particularly if they have been in harbour during the unhealthy season, have many of their men taken ill of the remittent fever, *even two or three months* after being at sea; and care should be taken, not to confound this fever with what is called the Jail, Hospital, or Ship fever." P. 335.

Extraordinary as these facts appeared to be, when first made known, the fullest confirmation of them has been since produced by the late expedition to Zealand; in which it has been indisputably ascertained, that considerable numbers, both of officers and soldiers, who were employed on that service, and who escaped the sickness, whilst at Walcheren, and other parts of Zealand, were attacked by intermitting fevers, and some of them as late as *six, seven, eight,* and even *nine months,* after being brought back to this country; though care was taken to place them generally in situations remote from all the known sources of marsh miasmata. This fact is now become notorious; I have seen it verified in the returns of several regiments, and it has been confirmed to me by a great part of the physicians, and by several of the surgeons, both staff and regimental, to whose care the sick of that army were committed in this country.

We may, therefore, now understand, what had before appeared to me very difficult to explain satisfactorily; I mean the cause of those intermittents which occur, more especially in fenny countries, at an early part of the spring and summer; before the soil or atmosphere can be supposed to have, at any time, in that year, acquired sufficient warmth, either to *form*, or *extricate* marsh miasmata, capable of producing fever. Consequently *vernal* intermittents may *now* be considered as resulting from miasmata received into the body during the *preceding summer or autumn*, and (after having remained in a quiescent state during winter) rendered active by some exciting, or proximate, cause of fever in the spring. It may be presumed, however, that in such cases, the original dose, if I may so call it, of marsh miasmata, was but moderate, because its effects would otherwise have been sooner manifested, and this will account for the well known mildness of vernal intermittents, and the facility with which they are generally cured.*

* Dr. Jackson, at p. 60 of his Outline of the History and Cure of Fever, mentions a detachment who were embarked at the Mole, in St. Domingo, for St Marc, and landed on the 3d of June, among which several were taken ill of Yellow Fever, during the passage, and twenty were sent on shore from one ship with that disease, after her arrival at St. Marc, of whom eleven or twelve died, within four days, though they were all apparently in good health when embarked. He also mentions, as a circumstance which "appeared *inexplicable*," that at St. Domingo, "uncommon sickness and mortality took place under every *transportation* of troops, to different posts." It seems probable, however, that, in all these cases, the troops had previously imbibed certain portions of marsh miasmata, which, though quiescent for some time, acquired a morbid activity by sea sickness, getting wet, or other debilitating causes, during their transportation from one port to another. It is even probable, that actual disease was sometimes produced by such causes, in persons whose constitutions might

From all these facts it may be inferred, that by differences either in the quantity or quality of the noxious exhalations of marshes, their operation, as a cause of fever, is liable to great varieties, in regard to its celerity, or the length of time in which disease actually appears; which is sometimes within twenty-four hours, and sometimes not until six, eight, or even nine months have elapsed, and then only when assisted by some accidental or exciting cause. The *longest* periods, so far as I can discover, have occurred exclusively in cold, or temperate climates; the shortest, only in the hotter; and in general there seems to be some foundation for believing, that, *cæteris paribus*, the disease will be most violent in those cases where it appears soonest after the morbid cause has been applied to the body; and that the rapidity of its production, will be in proportion to the quantity and concentration or force of the noxious miasms; differing in this respect from the small-pox, and some other specific contagions, whose morbid influence, together with the mildness or severity of the disease resulting from it, seems to depend *exclusively* upon the *state of the body*, in which it is exerted, and the treatment of the patient in regard to temperature, diet, &c. &c. and not upon either the *quantity* or *quality* of the contagion, producing the disease. The state of the body, indeed, and other circumstances, have a considerable influence in regard to the mildness or severity of the fever from marsh effluvia, but not an *exclusive* one, as they seem to have in regard to the effects, of several, at least, of the specific contagions; e. g. Dr. John Hunter observes, that if persons are exposed to the exhalations of marshes, "when fatigued by hard labour and long fasting, the poison gains admission more readily into the body, and produces *immediately* the worst kind of fever. It is in this way, (he adds,) that soldiers suffer so much on actual service in the West Indies: the few cases of fever which

under more favourable circumstances, have withstood, and finally subdued the poison, without any attack of fever.

proved *fatal in twenty-four hours,* that occurred to me, were all contracted in a similar manner." (Diseases of the Army in Jamaica, &c.)

There is, however, *another condition of the body,* which is of great importance, in regard to the production of yellow fever, and which, therefore, requires a particular investigation; I mean the *cause* of that remarkable *susceptibility* to this disease, which is commonly found in persons who have *just* arrived at places where it occurs, from cold or temperate climates; and of the equally remarkable exemption from it, which is commonly experienced by the *old* inhabitants of hot countries; and which, in the latter, is universally ascribed to their having become seasoned, as it is called; but however familiar this term may be, and of whatever importance its proper signification really is, (since it involves the means of preservation from one of the most dreadful maladies which afflict the human race) it has been long employed either without any precise meaning, or with meanings which are inadmissible. Thus it is often said, that a person is seasoned who has once had the Yellow Fever; but very improperly, because the same individual may have the disorder several times; besides which, many persons become exempt from the fever, and ought, therefore, to be considered as being truly seasoned, without having ever suffered an attack of the disease. It is, also, frequently believed, that one may become seasoned by residing long in those towns in which the Yellow Fever is apt to recur; but the very great numbers of the inhabitants of Philadelphia, New-York, Malaga, Cadiz, Seville, &c. who have been swept off by the distemper, within a few years, are melancholy proofs that an efficacious seasoning is not to be acquired merely by such residence. Nor can it be said, that those who live near marshes are peculiarly seasoned, because, in hot countries, numbers of persons, who live at a distance from marshes, are proof against the Yellow Fever, although they are sometimes attacked with slight remittents, or intermittents.

After some reflection on this interesting subject, the various degrees of susceptibility which are observed in different individuals, or in different places, seem to me capable of explanation on a very simple principle, I mean the effects of temperature on the human frame, which does not appear to have been sufficiently noticed.

The body, whilst in health, is found always to be, with very slight variations, at the temperature of 98 degrees of Fahrenheit's thermometer, and there is good reason to think, that any considerable variation from this point would necessarily produce morbid effects. It seems, therefore, to be of high importance, that the body should be preserved from such deviations; and the author of nature has, accordingly, provided efficacious means for that end:—different opinions are, indeed, entertained concerning these means; and since the later chemical discoveries have been made, it has been generally believed, that, in an atmosphere, the temperature of which is less than 98 degrees, the heat of the human body is maintained at that point, by a process similar to that of combustion, and depending upon a combination of oxygen gaz (taken into the lungs by respiration) with carbon and hydrogen: and that, in an atmosphere heated above 98 degrees, the temperature of the body is kept down at that point by the effect of an evaporation of matters perspired from the skin. There are, however, insurmountable difficulties opposed to this doctrine, but a full statement of them would, in some degree, be foreign to the subject under our consideration.*

* In June, 1794, I controverted this *chymical* doctrine, regarding the production of *animal* heat, at an *act*, or public exercise, in the University of Cambridge, previous to the taking of my first degree in physic. Perhaps a *summary* of some of the facts and arguments, which render that doctrine inadmissible, may be acceptable here.

1st. Animal heat is a *natural* production: and no chemical process like combustion can take place *naturally* in a healthy animal, because the power of life, while it subsists, naturally counteracts and suspends all chemical attractions, or affinities within the body; and even, after death, when this suspension is removed, they spontaneously occasion a *different process*, that of putrefaction.

I will, therefore, at present, only remark, that it is *utterly incredible* that these *opposite processes* should ever be carried on so *accurately in reference to each other*, and be so exactly *balanced*, as invariably to keep the body at the heat of 98 de-

2d. The attractions or affinities subsisting between oxygen, hydrogen, and carbon, even if unrestrained by the living power, would not, so far as our knowledge extends, enable them, by combining to form water, and carbonic acid gaz, with a disengagement of sensible heat, as, according to this doctrine, they are supposed to do, unless they were *actually ignited*, which they never can be in the lungs or blood-vessels of an animal in health.

3d. Were it even possible, that the air naturally inspired by the lungs should combine and afford *heat* in the manner supposed, the heat, so afforded, being necessarily limited and proportioned to the quantity of oxygen inspired, would, at all times, be greatly insufficient for maintaining the human body at its natural temperature, even in the milder parts of Europe ; and, in an atmosphere below the freezing point, it would greatly fall short of the quantity of heat commonly *abstracted* from the body, and carried out of the lungs, by the warm *moist* vapour copiously expired from them. So that in cold, and even in moderate weather, one effect of respiration is to cool the blood, according to the opinion which, during several ages, prevailed on this subject. And, if the quantity of oxygenous gaz, naturally inspired by mankind, be incapable of affording heat, sufficient to maintain their natural temperature, the quantity inspired by animals of the *celaceous* order, must be yet more, and *much more*, insufficient to maintain the heat natural to them, living, as they commonly do, from choice, at the edges of ice, near the poles, and immersed in a fluid, (salt water) so well suited to abstract, and rapidly conduct away their heat, however it be acquired. The degrees of animal heat naturally belonging to the several genera and species of this order, have not been accurately ascertained ;—it is known, however, to exceed that of mankind in some, and probably, does so in all of them. Some of the species of whale, particularly the *Balæna mysticetus,* (the common or larger whalebone whale) are of an enormous size, and from 50 to 100 feet in length ; in a few places they are said to have been found 160 feet long. And, it is ascertained that, in most animals of this order, the passage *for air* through a part of the trachea, is proportionably " very small or contracted ;" and that the pulmonary cells are *smaller* than in quadrupeds, which, says Mr. John Hunter, " may make less air necessary." (See his Observations on the Structure and Economy of Whales, in the Phil. Trans. vol. 77, p. 418, 419.) And as these animals, in addition to the smaller quantity of air taken in their lungs at each inspiration, do not commonly breathe oftener than once in about fifteen minutes, and when frightened, only *once* in about *half an hour*, (See Phil. Trans. No. 387, p. 256.) it must be *absolutely impossible* that oxygen, so *sparingly* received into their lungs, should, upon any principles yet known, afford supplies of heat sufficient to maintain the high natural temperature of these animals, (whose œconomy nearly resembles that of men and quadrupeds) in the *cold water*, by which they are surrounded.

Finally, to avoid unnecessary proofs, and demonstrate beyond the chance of contradiction, that animal heat is not produced by respiration, we need only *recollect*, grees, in all the diversities of temperature that occur in different climates and situations, and, therefore, that this important *conservatory* function must depend on a power more *exalted* in its nature, and more *certain* in its operations; which can be no other than the *power of life*,—a power which, in proportion as it is more vigorous in robust individuals at the prime of life, notoriously enables them to resist the *opposite extremes* of heat and cold, and preserve their bodies at the proper standard, more perfectly, and for a greater length of time, than at a more advanced age. I will not venture to assert that no addition to the heat of the body can be made, either directly or indirectly, by the combination of oxygen with the blood, and I readily admit, that its temperature may be diminished by a copious evaporation from the surface;—but, if either of these causes should co-operate with the living power to a small extent, the one in raising, and the other in lowering, what is called animal heat, it must always be *in* complete *subordination* to the higher principle of which I have been speaking, and to which nature has committed the impor-

(what is well known) that it continues to be produced for some time after respiration has *entirely ceased*, more especially in persons who die by apoplexy, or by suffocation, from fumes of burning charcoal, &c. who are frequently observed to retain their former temperature undiminished for several hours after all breathing has stopped; and in some cases, it has even been raised above the standard of health; e. g. Dr. Clarke, (on Diseases, &c. in Long Voyages, vol. 1, p. 44,) says of a seaman, who died by apoplexy, from a "*coup de soleil*," that, after all the external signs of life had disappeared, when "no motion was to be felt in the thorax, nor any pulsation in the arteries;" when his jaws were locked, his eyes dead, and *staring*, "the heat of his body was *much above* the standard of health, and communicated a burning pungency to the touch; he was bled largely from the arm and jugular; the blood was *very hot*, and it was with difficulty stopped." Indeed, heat continues to be more or less generated in all persons after what is *called death*, so long as they retain any portion of excitability or living power. Were it not for this they would be reduced to the temperature of the surrounding atmosphere, as speedily as another body of the same species and size, which, after being absolutely dead and *cold*, had been artificially heated to the same degree.

The natural heat of vegetables seems to be analogous to that of animals, and equally the product of a living power, independently of any process resembling combustion.

tant charge of preserving the temperature of the body at the standard of health, amidst all the varieties of climate, and of external circumstances. This is a charge which cannot be fulfilled in an atmosphere like that of England, the mean temperature of which may be estimated at 50°, without a considerable expenditure of the living power, in order to generate constantly, at the mean rate of 48° of animal heat: and after the body has been, for a length of time, accustomed to make this exertion, it is easy to perceive that, upon removing into a warm climate, such as that of the West Indies, the general mean temperature of which may be taken at 79° or 80°, very material changes in the functions of the system become absolutely necessary for the preservation of health. But these changes are not to be suddenly effected; and, until the body becomes perfectly accommodated to the heat of this new climate, the whole animal œconomy must be considered as almost in a state of morbid excitement.* It is not this state,

* There is a great analogy between animals and vegetables in regard to their predisposition, from habit, to generate certain portions of heat; and in their susceptibilities from the same cause, of being inordinately excited by a removal to climates or situations, warmer than those to which they have been accustomed, Mr. T. A. Knight, in his "Observations on the Method of Producing New and Early Fruit," (in the Trans. of the Horticultural Society, part 1, p. 30) mentions the following facts: "If two plants of the vine, or other tree of similar habits, or even if obtained from cuttings of the same tree, were placed to vegetate during several successive seasons in very different climates; if the one were planted on the banks of the Rhine, and the other on those of the Nile, each would adapt its habits to the climate in which it was placed; and, if both were, subsequently, brought in early spring into a climate similar to that of Italy, the plant, which had adapted its habits to a cold climate, would instantly vegetate, whilst the other would remain perfectly torpid. Precisely the same thing occurs in the hot-houses of this country, where a plant, accustomed to the temperature of the open air, will vegetate strongly in December, whilst another plant, of the same species, and sprung from a cutting of the same original stock, but habituated to the temperature of a stove, remains apparently lifeless. It appears, therefore, that the powers of vegetable life in plants, habituated to cold climates, are more easily brought into action than in those of hot climates; or, in other words, that the plants of cold climates are most excitable." P. 191. "But, the influence of climate on the heat of plants, will depend less on the aggregate quantity of heat in each climate, than on the *distribution* of it in the *different seasons* of the year. The aggregate temperature of England, and of those parts of the Russian empire, that

(of excitement) however, which alone is productive of fever; since we know that innumerable persons have gone from Europe to the hottest regions of the globe, and have continued there for years, without being attacked by fever, when other causes did not assist in producing that disease. The inhabitants of South Carolina, as I lately mentioned, were exposed to this kind of excitement, in an extreme degree, during a great part of the summer of 1752, and yet had never been more healthy; and other instances of the same import, might, if necessary, be adduced. But, although the simple operation of the warmth of hot climates upon the human body be not the cause of this disease, yet it is chiefly, if not entirely, to the various degrees of that derangement which it occasions in persons not accustomed to warm climates, that I attribute all those varieties of liability to the epidemic Yellow Fever, which are observable in different individuals, from the extreme susceptibility of northern strangers to the almost complete immunity of Creoles, and more especially of African negroes. It may be very difficult to point out the particular means by which heat occasions this extreme susceptibility; and yet it is not difficult to understand, that a morbid cause may be able to produce a much more violent disease, when it

are under the same parallels of latitude, probably does not differ very considerably; but, in the latter, the summers are extremely hot, and the winters intensely cold; and the changes of temperature between the different seasons are sudden and violent In the spring, great degrees of heat suddenly operate on plants which have been long exposed to intense cold, and in which excitability has accumulated during a long period of almost total inaction; and the progress of vegetation is, in consequence, extremely rapid. In the climate of England, the spring, on the contrary, advances with slow and irregular steps, and only very moderate and slowly-increasing degrees of heat act on plants in which the powers of life has scarcely, in any period of the preceding winter, been totally inactive. The crab is a native of both countries, and has adapted alike its habits to both; the Siberian variety, introduced into the climate of England, retains its habits, expands its leaves, and blossoms on the first approach of spring, and vegetates strongly in the same temperature, in which the native crab scarcely shows signs of life; and its fruit acquires a degree of maturity, even in the early part of an unfavourable season, which our native crab is rarely or never seen to attain."

is assisted by the co-operation of so powerful an agent as heat, than it could produce when acting by its own single influence; and it is upon this principle that I shall endeavour to explain the general law, by which the susceptibility to the Yellow Fever is, *cæteris paribus*, regulated. To accomplish this object, it will be necessary to take a concise view of the climates, in which the Yellow Fever has principally raged, and to apply the principle just mentioned to the results, which the experience of several years, in each of them, has afforded.

The variation of temperature, to which the climate of the West Indies is subject in the course of the year, is comprised within a few degrees; for the mercury in the thermometer seldom descends below 70° during the winter months, even in the coolest part of the mornings, and I have rarely found it (in the smaller or windward islands) to rise above 88° in the shade on the warmest days of the summer; though the heat is said to be sometimes a little greater in St. Domingo, and the other large islands. The excitement in the system, which ensues immediately upon a person's coming into so warm a climate from a temperate region, renders him, as I have already said, eminently liable to the Yellow Fever, when exposed to the influence of marsh effluvia: by degrees, however, the excitability producing this commotion abates, and, at the end, sometimes of twelve months, but oftener of a year and half, or two years, he acquires the power of supporting this high temperature, and then becomes almost as insusceptible of the disease, as the natives or old inhabitants; and afterwards retains this happy immunity, so long as he continues in that climate, the great uniformity of which does not, indeed, afford him any means of losing it. He may, likewise, go from the West Indies to those parts of North America, or of Spain, where the Yellow Fever is raging with the utmost fury, with almost a certainty of escaping the disease; and even if he should not preserve his health perfectly in such situations, he will, at the worst, be only seized with a mild remittent or intermittent fever, though living in the midst of those who are dying with the yellow fever in its aggravated forms.

The horrid massacres and pillage of the white proprietors in the French West Indian Islands, by their negro slaves, (in consequence of the principles instilled by the French revolution) had so alarmed the wretched survivors, that great numbers of them fled for safety to different parts of the United States. We were about three thousand French, says Dr. Valentin, (the author of a late Treatise on the Yellow Fever, who was, himself. one of that number,) when we landed at Norfolk and Portsmouth, in the Bay of Chesapeake, in July, 1793, after the dreadful catastrophe at the Cape: some of those who remained in these towns were seized with a remittent. but not one with the yellow fever, although our sudden transition from affluence to absolute want, might have been fully capable of predisposing us to this malady, which a stagnant and heated atmosphere, and a low swampy situation, seemed, of necessity to produce, in the country in which we arrived. (Page 68.)

Many of these refugees were also living at Philadelphia in the same summer of 1793, during which above 4000 of the inhabitants died of the distemper; but (as we are distinctly told by Dr. Rush,) they universally escaped it. In this place, however, it will be proper to remark, that the immunity which Creoles possess, relates chiefly to that variety of yellow fever which is epidemic. I have already mentioned that a fever, attended with all the symptoms which are held characteristic of the yellow fever, may, in hot climates, be brought on by intemperance, great fatigue after being overheated by the sun's rays, sudden diminution of temperature, violent agitations of mind, and other causes, which are known to be capable of exciting fever in all countries: to this sporadic fever Creoles are subject, though in less violent degrees than Europeans; for no length of residence in any climate can be supposed to exempt one from the operation of such causes.*

The climate of the United States of America is very unlike that which prevails between the tropics; for the variation of temperature, within the year, is not in the former limited to 18 or 20 degrees, as in the latter, but extends from 60 to 80 degrees; the heat in summer being often greater than in the West Indies, and the cold in winter being in many parts, much below the freezing point. Nor does the same climate prevail over all the states; for although, during the warmest part of the year, the heat being nearly as great at Philadelphia, New-York, and other towns in the latitude of 40 or 41 degrees north, as at Charleston, in South Carolina, which is seven or eight degrees nearer to the equator, yet the annual mean temperature of the more northern states, is considerably

* Dr. Lempriere does not seem to believe that *these* causes *alone* will produce the fever in question. He says, (Diseases of the Army in Jamaica, vol. ii p. 112) "that how much soever persons are exposed to the sun where marsh miasma does not prevail, the attack of fever does not ensue; and that, whenever there has been an instance of Idiopathic fever, in situations that are deemed healthy, it will be found, upon careful inquiry, that the patient had been exposed to marsh miasma, in an occasional visit to some place where it exists." That this may often be true I believe, but not that it is always so; for, though mere exposure to the sun, without a sudden cooling of the body, or the co-operation of some of the other causes just mentioned, may not be sufficient to produce fever, yet I am fully convinced of its having done so, not unfrequently with such co-operation. But, whether the fever so produced, will be, in all other respects, exactly similar to an Idiopathic fever produced in the way which Dr. Lempriere supposes, and especially, whether it will have an equal tendency to *remit*, is more than I am willing as yet to decide.

Though Dr. Hillary practiced, with the *highest* reputation, in Barbadoes, for a considerable number of years, there is good reason to believe that all the cases of yellow fever which fell within his observation, were merely sporadic, and most of them produced by some of the causes which I have mentioned, as sufficient for that purpose; and it must be principally to such cases that Dr. Lind alludes, at p. 120 of his volume on Preserving Health at Hot Climates, when he says, "I am very sensible that one or two persons may sometimes be seized with the yellow fever, when no other person in the neighbourhood labours under it; and even that, at such a time, its most mortal symptom, the *black vomit*, may attack a person *newly arrived*, without any previous complaint" He afterwards observes, at p 178, that "drunkenness, or any debauch, will often give a fever, which in less than forty eight hours terminates in the death of the patient."

less than that of the southern, because, on the one hand, the season of heat is of shorter duration, and, on the other, the winter is much more severe in the former than in the latter. The cold at Philadelphia, for instance, is so great, that the Delaware "river is frozen from three to nine weeks almost every winter,"* and is yet more intense at New-York, and in most of the towns of New England: but at Charleston, as we are informed, by Dr. Chalmers, "it seldom freezes more than four or five times in that season; and then a *thaw* so soon succeeds, that in the space of ten years the ice may not be strong enough to bear a man." (See an Account of the Weather and Diseases of South Carolina, by Lionel Chalmers, M. D. vol. i. p. 23.)

It is owing to these differences between the same seasons that, while the annual mean temperature of Charleston is estimated at 66 degrees, (according to a Register kept by Dr. Chalmers, for ten years,) that of Philadelphia is reckoned at only 52° 5, and that of New England at from 50 to 48 degrees; and from these differences there will also result certain physical effects in the inhabitants, which claim a particular attention. In the southern states, where the summer begins earlier, and the autumn lasts longer, than in the higher latitudes, the inhabitants have to endure a long continuation of hot weather; and in doing this, they at length acquire, in a considerable degree, the power of supporting heat; of which power they lose but a small portion during the winter, because the frosts being there neither frequent nor of long continuance, their bodies accommodate themselves with ease to the mild temperature of that season, and there are few occasions in the course of it, when those greater efforts are required which the body is obliged to make whenever it has to resist a severe degree of cold, and, by the frequent making of which, it seems to be always rendered less able afterwards to sustain heat. Hence it results, that the southern inhabitants can

* Geography of the United States of America, by Jed. Morse, A. M. 4to. p. 426.

scarcely be affected by the moderate warmth of the ensuing spring, or the gradual increase of temperature, during that season; and that they will be able to support the subsequent heats of the summer, with little or no morbid excitement. But, in the northern states, the comparatively short term of the hot season does not there permit the inhabitants to acquire an equal share of the power of supporting heat with their fellow citizens in the south: and that share of it which they do acquire, is much diminished afterwards, or, perhaps, wholly lost, during the long and rigorous winter of those latitudes, when great and continued exertions of the animal powers become necessary that they may be enabled to sustain so cold a temperature. It is, therefore, obvious, that on the return of the summer, the transition which the inhabitants of the northern states will then have made, from extreme cold to excessive heat, must be the means of causing a much higher degree of excitement in them than will ever be produced in those of the southern states, for the reasons before given, and, on recurring to facts, it appears from them that when the yellow fever has raged over this part of the world, (which seems never to have happened except when summers have been exceedingly hot) the degrees in which the citizens of the different towns of America were liable to the disorder, correspond exactly with the power of supporting heat conferred on them habitually by their respective climates. Accordingly, the citizens of Savannah and Charleston are almost equally exempt from the yellow fever with Creoles, as I had the opportunity of witnessing in both places in the year 1797. But, that this may not seem a mere assertion of mine, I shall beg permission to quote the testimony of Dr. Ramsay, of Charleston. This very respectable physician, after mentioning that the yellow fever had occurred there in seven out of nine of the *last* years, of the *last* century, and had continued in all those years from the month of July to the following November, distinctly says, that, "with very few exceptions (chiefly children,) it exclusively fell on strangers to the air of Charleston."*

The situation of Norfolk, in Virginia, is nearly midway between Charleston and Philadelphia, and its climate corresponds with its situation, being colder than that of the former city, but warmer than that of the latter. The yellow fever has frequently appeared there; and, according to the testimony of Drs. Taylor and Hansford, (residents in that town) it has, at least, "in its malignant form, always originated on the (banks of the) river, or on *low, new-made grounds,* and in houses built on the docks. In all cases, it begins with strangers and new settlers, affecting every one in proportion to his time of residence, and leaving the old inhabitants not wholly exempt, yet proof against its destroying power." "Persons from higher latitudes often fall victims, but with European strangers, the fever was generally uncontrollable." (New-York Medical Repository, vol. iv. p. 205, 6.) This account is strongly confirmed by Doctors Seldon and Whitehead, of the same place, in a well-written statement, which will be introduced hereafter.

With regard to the towns of the more northern states, as Baltimore, Wilmington, Philadelphia, New-York, New-London, Boston, &c. there is ample proof that the inhabitants of each of them become more subject to the disease, as their situation approaches to the north: for, it is but too evident, from

* See Dr. Ramsay's Review of the Improvements, &c. in Medicine, in the eighteenth century, read on the first day of the nineteenth century, before the Medical Society of South Carolina, p. 39. If this testimony, delivered before so many persons acquainted with its truth or falsehood, and never, as I believe, controverted by any one, could need confirmation, I might adduce that of the late governor of that State (Mr. Drayton) who, in his Review of South Carolina, printed at Charleston, in 1802, has made the following statement, at pages 27 and 28. "The Typhus icteroides or putrid bilious, or *Yellow* Fever, is, however, particularly local to Charleston; and is not known to have originated in the country. To the natives, and *long* inhabitants of this city it has not yet been injurious. But those who come from the country, during the autumnal season, or who have not been accustomed to spend the fall months in Charleston, or to foreigners at their first arrival, it is particularly dreadful, and many are those who fall victims to its fatal influence."

the registers of the deaths, reports of the Boards of Health, and other authentic documents, which have been made public, that whenever the epidemic has broken out in those places, the mortality has not been there almost wholly confined to strangers, as in the southern towns; but, on the contrary, that a very large majority of those who had the fever and died, consisted of *fixed residents*. This has been especially the case in the four last mentioned towns, the citizens of which seem to have been almost as readily attacked and carried off by the disorder, as foreigners even from the north of Europe, though persons lately arrived from hotter climates generally escaped it. In like manner, the official account of the yellow fever at Cadiz, in 1800, asserts, that, "persons lately arrived in that city from the West Indies, did not suffer an attack of the epidemic," while those persons who had come "from *Canada*, and other *northern* countries," were very liable to the disease.

Thus, by attending to the usual and necessary effects of heat on the human frame, under different circumstances of climate, it will be perceived, that we obtain a simple explanation of some of the most important general facts with which the occurrence of the Yellow Fever is connected. An additional proof, that the security from that disease is principally derived from the *ability to endure great heat*,* without its

* The natural ability to endure great heat varies not only among different races of men, but also among individuals of the same race, though living constantly in the same situations, e. g. light and red-haired people with very white skins, universally bear cold better, and are more incommoded by heat, than the black-haired with dark skins. But, besides these constitutional differences, there is another, which depends more immediately upon the *established* habit of generating certain degrees or portions of animal heat, for the purpose of keeping the temperature of the body at the healthful standard; and it is this difference *extending generally to all sorts of persons*, that is now more immediately in my contemplation; though its effects are liable to be augmented or diminished by the constitutional peculiarities of individuals, and of the several races among mankind. Probably the power of habit may also, as Lancisi, and others, have believed, extend so far as to diminish the morbid influence of marsh miasmata upon persons who, from their birth, or, for a considerable number of years, have been more or less exposed to their impressions, at least there are facts, or sup-

causing any considerable derangement in the animal œconomy, is, that this security continues only so long as this ability continues: for, if the inhabitants of warm climates remove, for a few years, into cold countries, and afterwards return, they are then found to be liable to the fever. So, likewise, the refugees from the French West-Indian islands, in America, who, as was before observed, *at first* universally escaped the disorder, were frequently attacked by it *after* they had passed three or four winters in that country. Another important proof of the same nature, is the more frequent appearance, and the greater severity of the Yellow Fever, in *populous cities*, than in the country: circumstances which it is natural that we should expect, when we recollect the various causes which contribute to warm the atmosphere in towns, and which do not exist out of them; such, for example, as the absorption of solar heat by, and the reflection of it from the walls of houses, and the pavements of streets, the assemblage of a number of living bodies; the many fires kept up for domestic purposes, and the obstruction to the circulation of the air, occasioned by large collections of houses. By a combination of these causes, the temperature of the atmosphere, in a large town, may be raised to a degree sufficient to produce the Yellow Fever within it, while that of the neighbouring country may not be high enough for its rural existence.† There is an interesting fact connected with this last,

posed facts, which support this belief. In regard to individuals, who, by long residence in cold climates, have become *habituated* to the generation of large portions of animal heat, it must be remembered, that the cold, which forces this habit upon them, will naturally preduce in them a considerable rigidity and strength of fibres, with an inflammatory diathesis, and that, when they remove directly to an intertropical situation, they will commonly carry with them a great *accumulation* of excitability, which, co-operating with the established habit of generating much animal heat there, may readily produce in them a most aggravated and violent form of fever, from a cause or causes which, in the *relaxed* systems, and with the *diminished excitability* of persons who have resided many years in hot climates, would only produce an intermittent or remittent.

† Dr. Caldwell, in his Medical and Physical Memoirs, says, "Philadelphia, like every other large and populous city, possesses a factitious climate of its own, different

which ought not to be omitted. Dr. Ramsay, whom I have before quoted, after mentioning, that persons coming to Charleston from the higher northern latitudes of Europe and America, had been most subject to this disease, and had most rarely survived it, states, that "the inhabitants of the country parts of South Carolina had little better chance of escaping it altogether, if they came into the city, or of recovering, when attacked;"* but, considering that, according to Dr. Chalmers, the atmosphere is always from 10° to 15 hotter in Charleston than in the country, where it frequently freezes pretty hard, whilst, at the same time, no signs of ice appear in town, we shall instantly perceive that the people from the country are susceptible to the fever in the same degree, when compared with those of the town, as persons coming from towns considerably to the north of Charleston; and it is also plain, that the same reasoning is applicable to the country people residing near most other large towns.†

from the climate of the surrounding country." "The summer climate of Philadelphia, and of other large cities, similarly situated, is an artificial torrid zone, in which the thermometer rises from four to six degrees higher than it does at the distance of a few miles in the country."

By a very accurate account of the state and variations of two *Register thermometers* kept, one in London, and the other at Newick Park, in Sussex, in the winter of 1806-7, it appears, that, though the latter place is thirty-five geographical miles south of the former, yet the weather was commonly between four and five degrees colder at Newick Park than it was at the same hours and days in London. This account was given to me by the late Mr. Tiberius Cavallo, whose care and love of truth are well known.

* We are informed by Spanish and other writers, particularly Humboldt, that this also happens at Lavera Cruz, where the Yellow Fever exclusively attacks strangers from more temperate northern climates, or persons coming to town, from *higher* and *cooler* situations in the country; and this is also the case in Jamaica, &c. See Dr. Dancer's Medical Assistant; or, Jamaica Practice of Physic, p. 84.

† The younger *Michaux*, who, under the auspices of the French government, made a second voyage to the United States of America in 1801, tells us that he landed at Charleston, in South Carolina, on the 9th of October in that year, contrary to the advice which was given him, and which was followed by the other passengers, of retiring to Sullivan's Island until the appearance of frost; that he was soon after attacked by the Yellow Fever, which, in that very year, proved fatal to eight-tenths of all the *strangers* in that city, and which had nearly cost him his life. He observes, that this disease varies in the degrees of its intensity every year, and that the inhabitants of

If after all these facts, which are not less in conformity with the *simplest laws* of *nature*, than in harmony with each other, and which appear to me to form a chain of convincing and even decisive evidence, any further proof could be desired, concerning the uniform correspondence of the exemption from the Yellow Fever, with the power of supporting heat, it is afforded by the knowledge of the various degrees in which negroes are susceptible to the disease under different circumstances.

The annual mean temperature of those parts of Africa, which are peopled by the various tribes of negroes, is, perhaps, no where less than 84° upon the coast, and it is, probably, several degrees higher in the interior. This, however, is only to be understood of the temperature in the shade, or in the coolest places, and does not indicate the greatest heat which the natives of Africa can sustain, nor even that which they may be said to bear habitually: for it will be recollected, that they pass a great part of the day in the open air, and as their bodies are generally naked, except about the middle, they are fully exposed to the action of the sun's rays, the direct heat of which has been estimated, and, I believe, without exaggeration, to vary from 120° to 160°, while the thermometer in the shade marks from 85° to 110°. But the bodies of blacks, and especially their *colour*,* are so admirably adapted for supporting, or rather for

Charleston are but little affected by it. But, that those who live in the *higher* parts of that state, at the distance of two or three hundred miles, and who came to Charleston during the four months, in which the Yellow Fever commonly prevails, are as liable to be attacked by it as strangers; and, therefore, (he adds) all intercourse between the country and city is suspended for one-third of the year, excepting that of a few white persons who, from necessity, go to the latter, always taking care, however, not to *sleep* there; and that of negroes bringing provisions, who are but little subject to the disease. See Voyage à L'Ouest des Monts Alleghanys, &c. par F. A. Michaux, M. D. &c. A Paris, 1804. p. 2, 3, 4, and 5. This account accords with the information which I have received from medical and other gentlemen of unquestionable veracity, and well acquainted with Charleston.

* See in the Philosophical Transactions of the Royal Society, for 1804, "An Enquiry Concerning the Nature of Heat," &c. in which the effect of *a black skin*, or of a *black* external surface, in promoting the *cooling* of bodies, is proved by experiment See also Mr. John Leslie's "Experimental Inquiry into the Nature and Propagation of Heat.

resisting, the intense heat in which they are naturally destined to live, that, at all times, their skins feel cool to the touch of an European. Endowed with these physical peculiarities, they are there so little subject to fevers, that the disease does not appear to have a *distinct* and *peculiar name* in any of their languages. Sometimes, indeed, they are affected by slight febrile indispositions; but these as Dr. Winterbottom observes, in his Account of the Native Africans of Sierra Leone, (vol. II. p. 13,) are seldom of longer duration than twenty-four hours, and they are usually the sequel of some debauch, and especially of that excessive intemperance, in which they indulge at the funerals of their friends.*

In the West-Indian Colonies, blacks are still so far exempt from febrile disorders, that even intermittents, the mildest of fevers, seldom occur among those negroes who are employed to labour in the fields, and they do not often even among those who have diminished their power of enduring heat by wearing clothes, and by living with peculiar indulgence in the cooler dwellings, provided for the comfort of Europeans.† Nearly

* Vitruvius seems to have been the first who noticed a difference in regard to the predisposition to fevers, and the power of supporting them with fortitude, between the inhabitants of cold and those of hot climates. He says, Lib. vi. cap 1. "Sub Septentrionibus nutriuntur gentes immanibus corporibus, candidis coloribus, directo capillo & rufo, oculis cæsiis, sanguine multo, quoniam ab humoris plenitate cœlique refrigerationibus sunt confirmati. Qui autem sunt proximi ad axem meridianum subjectique solis cursui, brevioribus corporibus, colore fusco, crispo capillo, oculis nigris, *cruribus invalidis*, sanguine exigus, *solis impetu perficiuntur;* itaque etiam propter sanguinis exiguitatem timidio es sunt ferro resistere, *sed ardores ac febres sufferunt sine timore*, quod nutrita sunt eorum membra cum fervore; itaque corpora quæ nascuntur sub septentrione a *febri sunt timidiora & imbecilla*, sanguinis autem abundantia ferro resistunt sine timore."

† Dr. Mosely, in his Treatise on Tropical Diseases, (3d edit.) p. 146, asserts, that none of the Europeans sent in 1780 on the expedition against St. Juan, "retained their health above *sixteen* days, and not more than three hundred ever returned; and those chiefly in a miserable condition. It was otherwise with the negroes who were employed on this occasion; a very few of them were ill, and the remainder of them returned to Jamaica in as good health as they went from it." He adds, it was the same at the taking of Fort *Omoa* from the Spaniards. "On that expedition half the Europeans who landed died in six weeks. But very few negroes; and not one of two hundred that were *African born*. The Creole negroes did not bear hardships so

the same may be said of the negroes belonging to plantations of rice, in the Floridas, and the State of Georgia, which, it is well known, are almost always made on swampy grounds, because they require to be overflowed at certain times: on the rice plantations, in the Carolinas, however, blacks sometimes have intermittent and remittent fevers, though neither so frequently, nor so severely, as the whites, who reside on the same spots; but they seldom or never have the epidemic Yellow Fever, as Drs. Moultrie, Lining, and Chalmers, of Charleston, have formerly attested; and, as subsequent experience has sufficiently proved. In Virginia, however, and in Maryland, negroes have occasionally been attacked by the latter disorder, and they are found to be still more subject to it in the states which are to the north of these, as Delaware, Pennsylvania, New-York, and New England. Dr. Rush says, that he was led to believe, from various publications on the Yellow Fever, that the blacks would escape it, while it was epidemic at Philadelphia, in 1793: but, as he candidly confesses, it was not long before he was convinced of his error, for it seems that, although the disease was commonly lighter in them than in white people, "yet many of them died with it," at that unfortunate period, and many more of them have also died of it, in that city, during the epidemics of the

well." This accords with Dr. Dancer's account of that expedition; but he adds, that though the Mosquito Indians, who were sent upon it, suffered from fevers, and still more from fluxes, the Indians of Cape Gracios a Dios, who have an admixture of negro blood, suffered less than any other Indians. The great and almost peculiar exemption from fevers, enjoyed by the black *natives* of *Africa*, is also asserted by M. Bourgeois, in his Dissertation " Sur les Maladies de St. Domingue," printed in a volume, entitled "Voyages Interessans Dans Differentes Colonies. " p. 417, where he says, "Les Nègres de la Côte," (d'Afrique) "et les Nègres Créoles font presque en ce genre *deux espèces différentes*; car ceuxci, quoique d'une complexion forte et vigoreuse dont n'approchent point nos Créoles blancs, ont pourtant plus fréquement que les autres *la fievre* et les diverses sortes de maladies auxquelles les blancs paraissent spécialement affectès. Les négres nouveaux, qu'on nous mène d'Afrique, sont d'un tempérament plus dur;" "*Jamais* les nègres *noveaux* ne paient, en arrivant dans la colonie, ce qu'on y appelle *le tribut.*"

following years.* Thus we find that negroes, by long residence in cool or temperate situations, become susceptible to the Yellow Fever, from the same physical causes as whites; though always in an inferior degree to the latter, under similar circumstances: and, to complete the analogy, they are, like whites, very liable to it, when, after having passed some years in cold countries, they are carried back to hot climates. This has been sufficiently proved by the results of the *three* great importations of blacks into Sierra Leone, which have been made within a few years; the first from this country, in 1787, and the others from Halifax, (Nova Scotia) in 1792, and in 1800. Upon each of these occasions, a considerable number of the blacks were seized with fevers, and many of them died; but the proportion both of sickness and of mortality, was much smaller in them than in the whites, who were sent out at the same time to superintend or assist the new settlers.†

In the preceding enquiries, I have attempted to ascertain and describe the uniform course of nature in regard to the formation and operation of miasmata, and in regard to the effects of heat on the human body; and, if the principles, to which I have been led, be not erroneous, it will, I think, be admitted, that the joint influence of marsh miasmata, and of an atmosphere unusually and sufficiently heated upon persons habituated to a cold or temperate climate, is, of itself, fully capable of causing an epidemic Yellow Fever, exactly resem-

* These negroes had many of them been born in Pennsylvania, and the others must have lived in that part of America, long enough to lose a great part of their constitutional peculiarities; for no importation of negroes had been permitted by the laws of that state during the preceding ten years.

† I have now before me a statement of the progress, extent, and decline of the sickness and mortality which attended each of these importations of negroes, compared with what befel the whites at Sierra Leone, in these respects, made from official documents, reports, and correspondence which I was permitted to inspect. The results accord with, and fully confirm, the principles and conclusions here advanced: but I abstain from printing this statement, as I had intended, because it may not be thought sufficiently interesting for the space it would occupy.

bling that which has committed such ravages in the West Indies, the United States of America, and the South of Europe; I mean a disorder which does not recur but after very irregular intervals, and with degrees of severity, varying at each recurrence, and which uniformly attacks with violence persons of a certain physical constitution, while it allows other persons to escape with a mild, and often without any disease. It will, moreover, be perceived from the same principles, that the influence of these causes, when it is sufficiently powerful to produce an epidemic Yellow Fever, can only produce one of the above description. With the assistance of those principles, it will be easy to understand by what simple means the atmosphere of a town, in certain climates, may become, and must occasionally become, so vitiated, at least in some parts, and so productive of dangerous fevers, as justly to claim the appellation of an epidemic constitution, and this, without requiring the smallest aid from contagion, or from those occult causes and supernatural agencies, to which writers have had recourse, when they undertook to explain the meaning of that term; without admitting, in short, the operation of a single agent, of whose actual existence there is not as complete evidence as we can possibly have of the existence of any agents in physical cases.* And, however few the causes may appear

* Sydenham appears not to have known, or even suspected, the influence of marsh effluvia as the cause of intermitting or remitting fevers, though Hippocrates, Galen, Varro, Columella, Palladius, Virtruvius, Diodorus Siculus, Dionysius Halicarnassensis, Strabo, and others, had observed, and very distinctly mentioned, the insalubrity of stagnant waters, swamps, &c. without, indeed, properly understanding the ways or means by which their morbid effects were produced. Sydenham, after telling us that he had, in vain, laboured to discover why seasons, apparently similar to each other, were accompanied or followed by very *dissimilar* effects, in regard to health and diseases, adds, "Ita enim se res habet; Variæ sunt nempe annorum constitutiones, quæ neque calori, neque frigori, non sicco humidove ortum suum debent, sed ab *ocultâ* potiùs et *inexplicabili* quâdam *alteratione in ipsis terræ visceribus* pendent, unde âer ejus modi effluviis contaminatur, quæ humana corpora huic aut illi morbo addicunt determinantque." (De Morb. Epid. c. ii. p. 41.) Thus he manifestly overlooked the materials, the situations, and the conditions of the atmosphere (in regard to heat and moisture) which contribute to the production of marsh miasma, and sought for *occult* and *inexplicable* changes in the very *bowels* of the earth, whence he supposed effluvia to issue, contaminating the

to be, by which I have endeavoured to shew, that the various gradations of severity, observed in fevers, which originate from miasmata, are produced, let it only be recollected that (as every new discovery demonstrates,) nature accomplishes all her wonders, not by employing a multitude of agents, but by merely varying the combinations of a few simple means.

That fevers, occasioned by marsh effluvia, often prevail epidemically, is a fact which has been so frequently observed and attested, that any proof of it would be superfluous.* It ought, however, to be remarked, that the epidemical progress of these fevers often very much resembles that of a dis-

atmosphere, and subjecting the body to various diseases. These notions he repeats in other places: and, when he attempts to assign a reason for the prevalence of intermittents epidemically in autumn, he seems to think, that so far as they resulted from a change of air, such change was *accidental.*

* "Intermittentes febres *sæpius epidemicæ* grassantur quam alii morbi. Van Swieten, Tom. ii. p. 264 sect 659.

"Certa Romanorum observatione constat, post ingentes Tyberis inundationes oriri febres *epidemicas* in urbe valde graves ac perniciosas." Baglivi Opera Omnia, p. 51.

Sir John Pringle (Diseases of the Army, p. 192) has justly supposed the *fifteen* plagues, mentioned by Livy, as having occurred at Rome before the year, (Urb. Cond.) 59 to have been so many destructive epidemics produced by exhalations from the adjoining marshes; and I may add from the low grounds, *at the bottom of its hills, especially those along the Tiber,* where marsh fevers appear to have greatly prevailed, from the earliest periods of its history. These seem to have been most commonly of the *semitertian* form; as Galen (de Temperam. lib, ii.) represents the *Hemitritæ* as being the common *epidemic* of Rome.

"Les maladies epidemiques ou populaires sont la source, presqu'exclusive, des mortalités." "Le rapprochement des observations sur les maladies populaires démontre leur parfaite identité. En effect, ces maladies sont les mêmes dans tous les pays, tous les climats, &c. elles sont dans l'abord, *de la classe des fievres remittentes & intermittentes*; elles éprouvent seulment quelques varietés qui ne sont pas plus considerables d'un pays a un autre, que d'un individu á l'autre dans le meme pays." Precis sur les maladies epidemiques qui sont les sources de la mortalité parmi les gens de guerre, les gens de Mer, &c. (á Paris, 1787) Par M. Retz, Medecin ordinaire du Roi, ci-devant medecin ordinaire des Hopitaux de la Marine, á Rochefort. Not to extend these references unnecessarily, I will only add, that Lancisi, in his second book De Nox. Palud. Effluv. has given distinct histories of five epidemics, from marsh miasmata, which, in his time, had greatly infested the Roman or ecclesiastical territory. He writes "Daturus autem hoc secundo libro, quinque historias *insignium epidemiarum*, quæ præ aliis nostrâ ætate propter palustres aquas, varia Ecclesiasticæ ditionis loca parvagatæ sunt," &c. p. 188, &c.

ease propagated from person to person by contagion, with only this exception, that it is frequently more rapid; a circumstance which has, however, been commonly overlooked by indiscriminating observers. Upon such occasions, those persons in each family who happen to be most exposed to marsh miasmata, and most susceptible of their effects, will be first attacked; those who are exposed and susceptible, in the next degree, will be the next attacked, and so on in succession.

This, which is the natural course of a disorder arising from that cause, has very commonly induced a belief, especially in times of general alarm, that those who first sickened, had communicated the fever to the next, and these to others successively; and, consequently, that the fever was contagious. And, in this way, even Stahl, with all his good sense, and discernment was so far misled, that, though justly convinced that regular sporadic intermittents were destitute of any contagious property, he strangely believed them to change their nature, and acquire that property merely by attacking great numbers of persons, in the same season and neighbourhood.*

It is not my intention here to notice all the supposed varieties of marsh fever, which have been minutely described by Torti and others, with little or no practical advantage. It will be sufficient for my purpose to observe, that the forms most prevalent in hot climates seem to be derived from the *tertian*, variously complicated, or compounded; and that of these, the form which was called Hemitritæus, or *semitertian*, by Celsus, (the Hemitriteon of the Greeks) and to which Lind, Sauvage, and many other modern writers, have applied the name of Tertiana Duplex, or double tertian, is generally the most malignant and destructive of all the marsh fevers in hot climates and seasons; and more especially when.

* "Quid notius quam febris tertiana? quid certiús quam quod illa legitima, longè absit a contagiosa communicatione? quid vero familiariús quam ut etiam epidemici, imo contagiosi grassari observetur?" Stahlius de Febribus, a Goez. p. 29.

by great *excitement** from the causes already mentioned, the paroxysms are so much prolonged and crowded upon each other, as to appear like a continual fever, or, at least, to leave no sensible remission during the first thirty-six, forty-eight, sixty, or seventy-two hours; as commonly happens in what is called *yellow fever*.† This seems to be the "*irregular* semitertian" of Dr. Fordyce, in which, according to his Statement, (4th Dissert. p. 61, &c.) "the hot fit is frequently prolonged, so as to leave no other mark of an intermittent to distinguish it from a continued fever, excepting the exacerbations not taking effect in the evening." He concludes, however, that it is not a continued fever, from "an agreement of all those who have had, or have seen, or have treated the disease, in the following observation." "It happens often, that a patient apparently becomes greatly relieved, and appears in a state as if he were recovering, when, all at once, a fresh attack takes place, and carries him off." This, he adds, "is the most *formidable* disease incident to mankind. It has *frequently been called the plague*."

Alexander seems, as Dr. Fordyce has observed, to have died of an irregular semitertian, caught by surveying the marshes adjoining the river *Euphrates*, to ascertain the means by which they might be most advantageously drained. The daily reports or bulletins, respecting the progress of his disorder, have been preserved and transmitted to us by Arrian.

* Dr. Fordyce, at p. 46 of his fourth Dissertation, mentions as a mischievous effect of strong arterial action, or general inflammation in intermitting fevers, its prolonging the hot fit, so as to render the intermissions imperfect, and converting an intermittent into a continual fever.

The late Captain Bernard Romans, who was a man of observation, as well as veracity, says, in his Natural History of East and West Florida, (p. 238) that the intermittent fever, in that part of America, "attacks people in the same form as the *continued* fever; the first fit frequently lasting *three days*, without intermission."

† Dr. Gillespie says of the Yellow Fever among the West India Leward islands, in 1794, 5, and 6, that it might "be called a remittent fever, or tertiana continua, as there were always remissions in cases terminating well,"—and, "as the epidemic insensibly changed into the form of a tertian fever."

In thus connecting what is called yellow fever with intermittents and remittents, as being only an aggravation of the latter, and, consequently, as being devoid of contagion, I feel myself supported by great authorities, and by facts, as decisive as they are indisputable. There are, however, persons of considerable eminence as medical men, who strongly contest the derivation of yellow fever from marsh miasmata, asserting that it has no relation to marsh fevers, but is exclusively produced and propagated by a *peculiar contagion.* This opinion seems to have been very extensively and inconsiderately adopted, especially in Europe; and as a demonstration of its fallacy, if it be, as I think, fallacious, must conduce to the best interests of mankind, I propose, in the next or fourth part of this Essay, to exhibit a summary statement of the principal facts regarding the history of yellow fever, in different parts of Europe and America, and regarding its *manifest* connexion with fevers notoriously originating from marsh effluvia; confidently believing that this statement, (supported by ample proofs,) with the conclusions fairly deducible from it, will completely remove all uncertainty or doubt from the mind of every impartial and judicious reader, who shall bestow proper consideration on the subject.

All this, however, will not accomplish my undertaking, because there are persons, particularly Dr. Chisholm, who admit that yellow fever, as it existed in the West Indies, previously to the year 1793, was derived from marsh miasmata, and destitute of contagion, but assert, that a "*nova pestis*, a peculiar, original, foreign pestilence, *recently generated*, and utterly unknown before, endued with a *new* and distinct character, possessing *new* powers of *devastation*, and capable of *propagating itself throughout the world*," was introduced by the ship Hankey, into Grenada, on the 19th of February, 1793. See Dr. Chisholm's Letter to Dr. Haygarth, p. 217, and 218.

From Grenada, Dr. Chisholm states this new pestilence to have been propagated over not only a great part of the West

Indies and North America, but also to Ireland, Cadiz, Malaga, Carthagena, and other places in Spain, and also to Gibraltar, and in some of these places, to have supplanted the yellow fever, properly so called. However strange and chimerical this recent generation of a new plague may appear, now that miracles are believed to have ceased, it has been, in some degree, admitted and believed by a considerable number of persons, among whom are several for whose judgment, in other respects, I feel great deference, and, as the purpose of this Essay will not be fully attained, unless it can be made evident that there is no reasonable foundation for believing that the supposed *nova pestis* was essentially or specifically different from the *common* yellow fever, of the West Indies, I shall undertake to perform this service, also, to what I think the cause of truth, in a separate Appendix, No. 7.

In addition to all this, I must observe, that some physicians, of great respectability, appear to have believed, that the yellow fever is either *commonly* or *occasionally* a sort of *hybrid* or mongrel disease, resulting from an application of the contagion of typhus fever, to persons who have been previously exposed to the impressions of marsh effluvia, or from the action of the latter, upon persons who have before imbibed the former. As either of these causes is undoubtedly capable of producing fever *alone*, in suitable circumstances, we may reasonably suppose, that an effect equally morbid would result from their joint operation, unless there be something in their natures, which disposes them rather to counteract than assist each other. But of this I believe nothing is known; and, therefore, I see no decisive objection to the production or existence of a fever, from the united action of contagion and of miasmata; though there is not, within my knowledge, any *fact* which either proves or renders it very probable, that any such hybrid production ever has taken place; and, in general, it is not philosophical, or proper, to assign *two* causes for an effect which may be produced by *one*. If, however, a mixed disease were producible by these causes, I should expect to find it at, or in the neighbourhood of Portsmouth, rather than in the West Indies; because, at the former, persons are often exposed simultaneously to the action of both of these causes, which can rarely if ever happen in the West Indies, where the heat soon extinguishes the contagion of typhus fever, and would, probably, hinder the *propagation* of any hybrid disease, like that under consideration, if it could be either produced in, or conveyed to, that part of America: whereas, such a disease, occurring at Portsmouth, in any season when the temperature is moderate, might, from its affinity with typhus, reproduce itself in persons who had been exposed to the exhalations of marshes, or in the absence of such persons, it might occasion a pure typhus fever only.

Sir John Pringle has supposed, (Diseases of the Army, p. 188,) that the Hungarian fever, described by Sennertus,* without his having any personal knowledge of it, and which is said to have spread widely in the year 1566, (as was believed by contagion) must have been "*a compound of our autumnal and hospital fever*," or the hybrid disease in question: and Dr. Lempriere, though he represents typhus fever as a rare disease in the West Indies, (see vol. 2d of Diseases of the Army in Jamaica, p. 25, 32, 33, &c.) yet at page 38 and 39, of the same volume, he treats of a fever at Jamaica, which he supposes to have been a typhus combined with yellow fever, or modified by the causes producing yellow fever: and, at page 81, he mentions a "variety of yellow fever," attacking sailors and soldiers almost exclusively, and with great mor-

* Medical writers have often described fevers as being contagious with so little consideration or foundation, that we may reasonably suspect the spreading of this fever, to have been merely an effect of marsh miasmata, imbibed by the imperial troops in the marshes of Hungary, and producing their morbid effects, *some months after*, when the army had separated and removed into other situations, as happened with the British troops who were lately at Walcheren. If this were the case instead of the mixed disease supposed by Sir John Pringle, the fever in question could not have specifically differed from the marsh fevers which so frequently prevail in Hungary, viz. the Morbus Hungaricus Lang. Lemb. l 1. cp. 4, the febris Hungarica seu castrensis of Juncker, the amphimerina Hungarica of Sauvage, &c.

tality, which he supposes to have been a combination of the tropical endemic and of typhus fever.

Dr. Chisholm, also, in some parts of his account of the supposed *New Pestilence* at Grenada, appears (as far as I am capable of discovering his meaning) to imagine that it was generated by a sort of conjunction of contagion, with marsh miasmata, as I shall have occasion to notice in my Appendix, No. 7. And, finally, Dr. Blane also appears to connect yellow fever with the contagion of typhus, at least *when it prevails extensively.* He says, (p. 609 of his Observations on the Diseases of Seamen.) "After laying together, and considering fully all the facts relating to this subject, it appears to me that the *yellow fever* cannot be produced, but in a season or climate, in which the heat of the atmosphere is pretty uniformly, for a length of time, above the 80th degree of Fahrenheit's thermometer; that, under the influence of this heat, Europeans newly arrived, and more especially in circumstances of intemperance, or fatigue in the sun, may be subject to it in many instances; but that it has usually *become general* only by the previous *influence of that infection which produces the jail, hospital, or ship fever,* or from the influence of putrid exhalations; and that, when so produced, it continues itself by infection. It would be too tedious to enumerate the multiplied proofs of this, which have occurred to me in my connexion with the public service." But, though Dr. Blane did not think it expedient, on this occasion, to favour his readers with a statement of the "multiplied proofs" in support of his opinion, which had occurred to himself, and which, by that circumstance, would have been highly important, he seems to have afterwards intended to make a full compensation for this omission, in his letter to Rufus King, Esq. (late minister plenipotentiary from the United States of America,) in which, to justify his belief of the contagious nature of the yellow fever, he refers to, and strongly *relies upon,* the events which followed the capture "of two French armed ships from Gaudaloupe," by the Thetis and Hussar frigates, in May, 1795, on the coast of America. This transaction, though unfortunately, he had no personal knowledge of it, Dr. Blane, *selects* and *holds up* to Mr. King, and the public, as affording "a conviction of the reality of infection as *irresistible*, as volumes of argument:" and he afterwards refers to it, in his letter to Baron Jacobi, the Prussian minister, as affording decisive evidence on the subject. I was, therefore, induced by my particular regard for the writer of that letter, and by the great estimation in which his judgment and experience are deservedly held, to enter upon a minute investigation of this transaction, which I should not have done had it been brought forward on less respectable authority. The results of that investigation will be found in my eighth and last appendix, and they will, I am persuaded, sufficiently prove that yellow fever was not the disease supposed to have manifested contagious properties on that occasion. Having thus, as I hope, encountered and removed all the difficulties which were opposed to the adoption of my own conclusions on this subject, I shall proceed to the *last* part of this Essay.

END OF PART THIRD.

# PART FOURTH.

As one *important purpose* to be attained by the view which I am about to take of the history of Yellow Fever, is that of establishing its *identity*, or *near affinity* and *connexion*, with the fevers which are indisputably and notoriously produced by marsh miasmata;—it seems expedient, first to ascertain the *characteristic peculiarities* of the latter, as they have been generally manifested in the temperate climates of Europe, in order that, being ascertained, they may afterwards serve as *points* or *features* of comparison and recognition, in regard to *those* which distinguish the Yellow Fever.

These *characteristic peculiarities* of marsh fevers appear to be, 1st, that of occurring in their simple and mild form of intermittents during the spring; 2d, that of being exasperated, and converted to *remittent*, and, apparently, to *continued* fevers, by excessive summer heat, and this generally with a great increase of malignity, (especially in low and moist situations) when this excessive heat is long continued and accompanied with *a total or very unusual deprivation of rain;* 3d, that of being reconverted and brought back to their mild intermitting form, at the approach or commencement of winter, and afterwards extinguisbed or suspended by a continued frost: 4th, that of most frequently and violently attacking strangers from colder climates, and more salubrious situations;—and 5th, that of never being communicated from person to person by a contagious property. Several facts and authorities, tending to prove these peculiarities of the fevers in question, have been incidentally mentioned in the preceding parts of this Essay. A multitude of others might be added, but the following will suffice.

Dr. Lind, in his Essay on the Diseases of Europeans in Hot Climates, after mentioning, that "in *particular spots* of the low damp island of Portsea, agues frequently prevail, and sometimes the flux, during the autumnal season," adds "in some years they are much more frequent and violent than in others." It is observable, that their attack proves always most severe to strangers, or those who have formerly lived on a drier soil, and on a "more elevated situation." He next mentions, the regular tertians with perfect intermissions, which prevailed at Portsmouth in May, June, and July, 1765, and then proceeds in these words, "In the month of August, the quicksilver, in Fahrenheit's thermometer, rose to 82° in the middle of the day. This *heat*, together with the want of refreshing rains, *spread* the fever, *increased its violence*, and, in many places, *changed its form*. At Portsmouth, and throughout almost the whole island of Portsea, an *alarming continual*, or *remitting* fever *raged*, which extended itself as far as Chichester. At the same time the town of *Gosport*, and the opposite side of the harbour, though distant only *one mile* from Portsmouth, *enjoyed an almost total exemption from sickness* of every kind; and in the neighbouring villages and farm houses on *that* side, only a mild regular tertian ague prevailed, which however distressed whole families. The violence of the fever, with its appearance in a *continued* remitting or intermitting form, marked, in some measure, *the nature of the soil*. In Portsmouth, its symptoms were *bad*, *worse* at Kingston, and still more dangerous and violent at a place called *half-way houses*, half a mile from Portsmouth, where scarcely one in a family escaped this fever, which there generally made its first attack with a *delirium*. In the large suburb of Portsmouth, called the common, it seemed to rage with more violence than in the town, some few parts excepted: but *even whole streets of this suburb*, *together with the houses in the dock-yard, escaped it.* P. 18, 19, 20.

This *exemption* of particular streets, &c. from the disease, is an important fact which often occurs in regard to marsh fevers, and will be easily understood from what has been mentioned between pages 224 and 229; and it well deserves to be remembered, not only as a distinguishing mark of these fevers, (produced by a cause arising immediately from the soil) but also as an *incontrovertible evidence* of their *total want of any contagious property;* for contagious fevers are not thus narrowly confined and limited in their progress.

To this testimony of Dr. Lind, I will join that of the late Dr. Robert Hamilton, who practised as a physician, with great reputation, for more than forty years, at Lynn, in Norfolk; and published "Observations on the Marsh Remittent Fever," which prevailed, with unusual violence and malignity, in that part of England, during several *uncommonly hot* summers, which followed a great inundation from the sea in 1779, so as very nearly to resemble "*its appearance in many places between the tropics.*" Of this fever, he says generally, "If a very wet winter and spring are succeeded by a *very hot* and *dry summer,* in which the *ditches* and *marshes are nearly dried up,* it is very *generally epidemical,* and spreads widely around us. It most commonly appears about the middle of August, and lasts till the ditches are filled with water, and the marshes *somewhat covered;* which, *with a frost,* usually puts a period to its raging in *that form, for that season;* for it now generally changes to the type of a genuine intermittent." He deemed this fever, as it prevailed after the inundation, in 1779, "to be the same distemper with the bilious remitting fever of the Netherlands, the tertiana duplex of Minorca, the remitting fever of Bengal, the *yellow fever* of the West Indies, and the bilious remittent of Senegal." Supposing it to "differ only in *malignity* and *fatality* from those of *hot countries,* in proportion to the difference of climate." See pages 27, 28, 32.

If we proceed to the continent of Europe, we shall find, that even in the *northern*, but low and damp island, upon which Copenhagen is situated, an excessively hot and dry summer, in 1652, was able to produce a violent epidemic tertian, of which Thomas Bartholine, (who was attacked, with all his family, by it) has given an account;* and which fever was the more remarkable, because, upon dissecting the bodies of those who died of it, he found the *stomach* and *duodenum* always mortified, or, at least, inflamed, as is almost invariably the case, in those who die of *Yellow Fever*.

Passing over what Forestus and others have related of the violent marsh fevers, (once called the plague,) which frequently infested the city of Delft, (almost surrounded by stagnant waters, and placed on low moist ground) we learn from Silvius de le Boe, that a very malignant remitting, and intermitting fever raged *within his own observation*, at *Leyden*, in 1669, in consequence of a very *hot summer* and autumn, with little or no rain, and an unusual stagnation of the air; by which the water of the canals and ditches became greatly diminished, and highly corrupted. He mentions two-thirds of the principal inhabitants of that city as having died of this epidemic. See Prax. Med. tract. X.

Sir John Pringle, treating of the autumnal fevers of the British army in Flanders, says, "this remitting fever attended every campaign, and was most frequent and fatal after the

* Thomæ Bartholini Historiarum ȧnatomicarum rariorum Cent. I & II. Historia LVI. Febris tertiana Epidemia.

"Æstate 1652 *præter morum* & cœli nostri consuetudinem *calidissima* & *siccissima*. Hafnice & vicinis locis grassabatur febris tertiana intermittens epidemia, quæ multas familias velut conspiratione quàdam invasit, prostravitque. Varium in hac observavimus typum; modo enim singulis recurrebat diebus, modò alternis, modò vaga, sæpissime post ἀπυρεξίαν redibat. Symptomata varia comitabantur, ingens capitis dolor, imprimis colli seu musculorum occipitis, lumborum & dorsi, calor urens, *vomitus biliosus*, sitis, inquietudines, nonnunquam deliria, & petechiæ remissionis tempore evanescentes, & cum paroxysmo revertentes," &c. "Multis in hac urbe diuturnu vomitus post febrem istam remansit, ut *ventriculum in febre hac imprimis affectum*, sicut in *malignis* solet non dubitarim," &c.

*hot summers* of the years 1743 and 1747;* but, in the campaigns of 1744 and 1745, the seasons being temperate, fewer were seized, and the cases were milder." See Observations on Diseases of the Army, p. 172. The same respectable author, treating, at p. 180, of the fevers among the troops stationed near the *inundations*, in Dutch Brabant, in 1748, observes that, "in the greatest *heat* of the weather, and rage of the distemper most of these fevers answered the description of the καύσος, or ardent fever of the ancients, which Hippocrates does not rank with the inflammatory diseases of the winter and spring, but with the *epidemics* of summer and autumn. (Aphor. lib. iii. Aphor. xxi.) Sir. J. Pringle adds, "but it was observable, that even in the *worst parts* of the country, *as soon as the weather cooled*, in the decline of autumn, the fevers began to assume a milder form; and, in the end of the season, differed little from the common intermittents of other places." In the next page, he makes the following observation: "At the height of the epidemic, it appeared that both intermittents and remittents, by *extending or doubling their paroxysms*, frequently *changed* into a *continued* and *dangerous form*, and that most of those whom we lost died in this way."

Dr. Francis Home, who also served at the same time, with the British army in Flanders and Holland, gives a similar account of these fevers. "The change of weather, (says he) from heat to cold, made always a sensible alteration in the symptoms. In *hot* weather they were *more severe*, and the disease inclined more to the *continued fever*." Medical Facts and Experiments, p. 46.†

* Werlhof de febribus, p. 3, mentions the excessive number of fevers of various types, which infested almost all Europe, and especially marshy places, from the *enormous heat* of two summers; "*ab enormi illo calore æstivo* annorum 1726 & 1727." Van Swieten also makes a similar observation: "Sic observatur post *fervidissimas* æstates prægressas autumnum hæmitræarum febrim *fera cissimum* esse," tom. ii. p. 455, and another, (tom. iii. p. 96) viz.: "Ubi post fervidissimas & siccissimas æstates autumnali tempore grassantur epidemicæ febres continuæ, remittentes, &c."

† All the epidemic marsh fevers described by Lancisi, appear to have begun about the summer solstice, and to have increased and become more exasperated by great

The latter of these authors treating, at p. 18, of the "Epidemic Remittent Fever" of the Camp at Worms, in 1743, says, "there is another symptom which attended this fever, and that is a *jaundiced colour* in their eyes and skin, and very often a complete jaundice." Sir John Pringle also describing (at p. 72.) the marsh fevers of Flanders, says, "some grow *yellow* as in the jaundice: this colour was observed to be more frequent in the first campaign than afterwards: it was an *unfavourable*, but *not* a *mortal* sign." Afterwards Dr. Brocklesby mentioned, (at p. 269 of his Observations) as occurring in the autumnal fever of 1758, among the soldiers, on the Isle of Wight, a "*suffusion of bile*, which had often tinged the skin of the *deepest yellow*, and sometimes blackish colour." We see, therefore, that even in temperate European climates, fevers from marsh miasmata sometimes have this symptom, in common with the bilious remittent, and Yellow Fevers between the tropics.

These testimonies, concerning the intermittents of Europe, and the changes of which they are susceptible, might, probably, be thought sufficient for the purpose intended to be answered by them; I cannot, however, omit to notice their appearance and effects, as they have occurred in Zealand, particularly during the late expedition to that province.

Dr. Wind, who translated into Dutch, Dr. Lind's Essay on Preserving the Health of Seamen, and who, with his father, had practised physic twenty-eight years in Walcheren, has added to that translation, as we are informed by Dr. Lind, the following account of these fevers, viz: "at Middleburgh a sickness generally reigns towards the latter end of August, or the beginning of September, *which is always most violent after hot summers.* Its makes its appearance after the rains which generally fall in the latter end of July: the

heat and dry weather until the autumnal equinox; after which they were found to decline: and, finally, cease upon the accession of cold winds and rains at the beginning of winter; and wherever fevers observe this course, they may safely be considered as resulting from marsh miasmata.

sooner it begins the longer it continues, being *checked only* by the *coldness* of the weather."

"Towards the end of August, and the beginning of September, it is a *continual burning* fever, attended with a *vomiting of bile,* which is called the *gall* sickness." He afterwards mentions that "strangers, who have been accustomed to breathe a dry *pure* air do not recover so quickly" as the natives; adding that this fever is "the same with the *double* tertian fevers between the tropics," that it is, "*not infectious,*" and seldom proves "mortal to the natives."—But that "the *Scotch* regiment, in the Dutch service has, at *Sluys,* been known to bury their whole number in three years." See Lind, on the diseases of Europeans in Hot Climates, p. 23, &c. This account of the morbid influence of marsh effluvia in Zealand, was but too well confirmed by the extensive sickness which so lately, and in a few weeks, disabled the British army there, at a time when no extraordinary heat or drought had occurred to aggravate the symptoms. In Dr. Blane's Letter to the Physician-General, dated Middleburgh, October 3d, 1809, and printed, with other official documents, presented to both Houses of Parliament, in February, 1810, marked *E.* p. 103, is the following passage, viz:

"It appears, from the *last general weekly return, that near two-thirds of the whole numeral strength of the army is incapable of duty.* The mortality, during *the last four weeks, has been about one thousand.* All the regiments are affected in nearly an equal degree; and it does not appear that their illness is connected with the nature of their duty, nor that it is owing to privation or neglect of any kind; for those are equally sickly who have enjoyed the utmost ease and comfort in cantonments, as those who have been engaged in the siege of Flushing." "Nor has it been owing to *any thing unfavourable in this year, in comparison of former ones;* on the contrary, the native inhabitants affirm that they are now less sickly than usual at the same season; and they account for it from the *unusual quantity of rain that has fallen the last two* *or three months;* and they consider it as fully established by observation, that the *most sickly seasons* are those in which there are *great drought and heat in the latter end of summer, and the early parts of autumn,* owing, probably, to the increased putrefaction and exhalation produced by these causes. I find, upon inquiry, that a like degree of sickness prevailed among the French troops, who occupied Flushing during the last seven years; and that, in former times, the Dutch troops from the Northern States of the United Provinces suffered equally. Other proofs, if necessary, could be adduced, to evince that the present unfortunate state of the army here, is solely imputable to *the contamination of the air, from a soil the most productive of deleterious exhalations of any perhaps in Europe, producing an endemic fever,* which has, at all times, been particularly *severe upon strangers* in the autumnal months. I find also, upon inquiry, that though this is by far the most sickly season, the residents of this, and the neighbouring islands, are far from enjoying at any season, the same degree of health as in the more salubrious parts of Europe;" and, in an *unpublished* letter on this subject from Dr. Blane to the Surgeon General, dated at Middleburgh, October the 4th, 1809, I find the following observation: "it is fortunate that the administration of medicine is simplified by the uniformity of the cases, *which almost all consist of the endemical intermitting and remitting fever.*"

This deplorable calamity has, however, enabled us to make some very useful additions to our stock of knowledge, respecting marsh fevers; and one of these is a *full and indisputable confirmation* of the fact, which the most judicious and best informed physicians already believed, that these fevers *do not possess any contagious power or quality whatever:* of this I have numerous and decisive proofs now lying before me: a part of them will, however, be sufficient.

But I ought previously to observe, that, as all the troops landed in Zealand were more or less exposed to the influence of marsh effluvia, it must have been difficult, if not impossible, to distinguish the effects of contagion, had it existed among them, from those of miasmata; and, therefore, I shall draw no conclusion on this subject from any thing which occurred to the twenty-six thousand eight hundred and forty-six patients, including relapses, who were admitted into the general and regimental hospitals of the British army *there*, previous to the 18th of November, 1809, or to those afterwards added to this number, before the final evacuation; but I shall confine my inferences solely to what followed the return of several regiments or battalions not required for the occupation of Walcheren, and the removal of twelve thousand eight hundred and sixty-three sick, (including a small number of wounded) from that island, and from South Beveland, between the 21st of August and 16th December, 1809, who were all landed, and placed in dry, wholesome situations, within the Kentish and Eastern districts, under the general superintendence of Mr. Keate, the Surgeon General. That gentleman, at different times, visited, and minutely inspected, the several hospitals in these districts; and, upon his return to London, on the 5th of October, 1809, immediately after he had performed this duty throughout the Eastern district, he obligingly put into my hands a statement of the results of his observations respecting the prevalent disease, from which I was permitted to make the following extract, viz.

"It is certain, that more or less of the poison which created this sickness, has been imbibed by all the troops previously to their leaving Walcheren, and that, in many instances, it has not produced its noxious effects until the men have reached this country, and have even marched to their respective quarters. This is obvious at Colchester, Weeley, and Woodbridge, where many have fallen down with this disorder, af- the slight fatigue of a short march."

"It does not appear that this disease is of a contagious nature, not one of the attendants, whether nurses, orderlies, or medical officers, having contracted it, as I am informed." Mr. Keate told me, relative to this point, that he had made very minute inquiries on the subject, of every medical officer with whom he had an opportunity of conversing, and particularly of Drs. Fellowes, Roberts, Wardell, and Tice, and of staff-surgeons, Ross and Emery, and that they were all unanimous in stating that no instance had fallen under their knowledge of the disorder having been communicated to any person about the sick.

Nor did subsequent information or observation alter the Surgeon General's opinion on this subject, as is fully proved by the following extract from his letter to Francis Moore, Esq. Deputy Secretary at War, dated 14th December, 1809.

"I must also remark, after much inquiry from the most respectable medical officers into the subject, that neither in the Kent nor in the Eastern district, had any instance of contagion been known to have occurred in the General Hospital;" (i. e. those appropriated for the Walcheren sick) "a fact which sufficiently demonstrates the fallacy of the assertion advanced against these establishments, and so readily credited by many persons, viz. that they are the chief cause and source of contagion in armies."

The observations made by the Physician-General, when he visited the sick, returned from Walcheren, at Harwich, Colchester, and Ipswich, in September, 1809, were similar to those of the Surgeon-General, in regard to the total absence of contagion, as appears by his three official letters to the Deputy Secretary at War, of the 11th, 13th, and 15th of that month, printed in the Minutes of Evidence on that subject, by order of the House of Commons; and that his subsequent information, from the army physicians, and other medical officers, was of the same import, may be inferred from his printed testimony to the House of Commons, on the 8th of March, 1810, when, being asked this question, "might not a fever, of a more fatal kind than that which subsisted among the troops at Walcheren, be generated on board the transports in which they came from that country, so that no inference is to be drawn as to the state of the sick, at the time they were put on board?" he answered, "if that had been the case, there would have been contagion in the different hospitals I saw at Harwich, Colchester, and Ipswich, and there was no contagion—*nor has there been any contagion.*"

To these testimonies I may doubtless be permitted to add the results of my own very minute and extensive inquiries on this, to me interesting subject, which have, without exception, most unequivocally manifested the total absence of contagion in or by any of the sick from Zealand. Such was the information given to me by Mr. Warren, Deputy Inspector of Hospitals, who, under the Surgeon General, had the superintendance of the sick from Zealand, in the Kentish district, as well as of Dr. Neale, then principal Medical Officer of the General Military Hospital, at Deal, and also of Doctors Faulkner, Faber, Turner, and Morgan, all Fellows of the College of Physicians, and physicians to the army, or employed as such temporarily, with the sick from Zealand, in that district; such also was my information from sir James Fellows and Dr. Roberts, army physicians, and from Drs. Harvey and Laffan, who were then employed as such in the Eastern district. These gentlemen, as well as those in the Kentish district, uniformly assured me, that no patient, having the Walcheren or Zealand fever, had, as they believed, given that disease to any other; and that, according to their knowledge and information, none of the attendants, or others employed in the several hospitals, and who had not been exposed to marsh miasmata in *Zealand*, were attacked with the fever in question.*

* Mr. Nixon, Surgeon to the first regiment of Foot Guards, favoured me with some valuable information respecting the extent and effects of the Zealand fever, upon the 3d battalion of that regiment, which, to the number of 872, landed at South Beveland, the 2d of August; of these 359 were attacked with this fever, between the 19th of that month and the 14th of September, (sixteen days:) and though, on the last of these days, they embarked on board the Leyden, to return to this country, and actually re-occupied the barracks at Chatham, on the 16th of September, only 117 of the battalion had ultimately escaped the fever, on the 8th of March following; and some of the 117, who had so escaped, were, for the *first* time, attacked by it,

In addition to those inquiries I made others, respecting the hospital ships and transports employed in removing the sick from Zealand to Harwich, Deal, and other ports in this kingdom, in order to ascertain whether any of their respective crews had been infected by this service; but I did not hear of a single instance in which this was even suspected to have happened. Deputy Inspector Warren, in a letter to me, dated the 9th of October, 1810, wrote as follows: "In reply to your question, relative to the state of health of the crews of the transports that were employed in conveying the sick from Walcheren to this country? I have *every reason* to believe they remained free from the disease, with which the troops were affected; as I do not find that more than two or three applications were made for admitting sick seamen into the Hospital at Deal, and such applications would have been made to a considerable extent, if they had at all partaken of the disease."

At my request, Dr. Faber made a similar application to Staff Surgeon Lidderdale, who had been employed at Flushing, in superintending the embarkation of the sick, removed thence to England; and, in consequence thereof, Mr. Lidderdale wrote to Dr. Faber, on the 22d of October, 1810, a letter, of which the following is an extract, viz. "The same transports were not *always*, but *frequently*, employed in removing the sick from Walcheren to this country; and I did not observe that their crews laboured under the same disease as the sick; and, from conversations with the navy, I learned, that only those men who were on shore, and exposed to the same causes as the troops, laboured under the Walcheren fever." He afterwards mentions his having had opportunities of observing the *Asia*, (a large Hospital ship, carrying about

as long afterwards as the middle of June, 1810, Mr. Nixon, however, assured me, that neither in the Hospital at Chatham, nor in that of Westminster, to which this battalian was afterwards removed, had there been any appearance of contagion. None of the Hospital servants who were not in Zealand having taken the fever, nor any of the soldiers' wives, or connexions.

sixty patients in cradles) and declares, that he "saw nothing like infection extended to the crews of that ship."

My inquiries were afterwards extended, in order to discover whether any effect, indicating the existence of contagion, had been produced by the bedding and clothes used by the sick at Walcheren, and removed to this country mostly without being washed, and in a very filthy condition, as I was informed by Mr. Moss, Purveyor of Hospitals there, and by his Deputy, Mr. Boning, verbally. To the former of these gentlemen I addressed certain written questions, in the month of August, 1810; but he, being then on the eve of his departure for Sicily, could not, as he stated in his letter to me of the 13th of that month, "answer them otherwise than in a general way," viz. "several thousand articles of wet and dirty bedding, cloathing, &c. were received from the regimental hospitals throughout the island, of which time did not permit either the drying or washing: I found it, therefore, necessary to require a board of survey, in order to decide upon the propriety of shipping them in their actual state; the board having decided that, in so short a voyage, no danger or damage could be expected,—the whole of the articles in question were shipped by the *Asia*, Ceres, and Eleanor." He adds—

"I have not heard that the least sickness whatever prevailed among the crews of either of the above ships; and when it is considered that *the stores were not landed for several weeks beyond the calculation* of the board of survey, it seems clear that the members were fully justified in their opinion."

The bedding and clothing in question having been landed, after considerable delay, was transferred to the store-keeper general, whose deputy, Mr. Barker, in answer to a note from the Surgeon-General, on this subject, wrote, (on the 25th of September, 1810) "that the *dresses** and bedding, received

* These dresses were *all of flannel*, and mostly worn by the sick *next to their skins*; they were, therefore, well suited to retain, as well as *imbibe*, contagion, if any had existed. And even if none had existed, it ought, according to the general opinion

from the several transports returned from Walcheren, were, in general, in *a very foul state*, and were immediately sent to the mills to be cleansed, *but he did not hear of any infection having arisen from them to any of the parties* through whose hands they passed.

Considering these facts as more than sufficient to prove that the marsh fever of Zealand does not possess any contagious quality, I shall now proceed to a rapid view of *the History of Yellow Fever in America and Spain.*

It has been asserted and believed, that the Yellow Fever, at least in its most violent form, did not attack the first settlers in America. Of the truth of this assertion it is now difficult to decide, because no one of the earlier historians has left us any account of their diseases sufficiently minute and discriminating. It is, however, manifest from various facts related on good authority, and several passages in the writings of Peter Martyr, that the earlier Spanish adventurers to the West Indies suffered greatly by marsh miasmata, particularly those who first attempted to establish themselves in Darien about the year 1512, at a place which the Spaniards called Sancta Maria Antiqua, where many of them died. Of this place Peter Martyr writes, in the 6th chapter of his third Decade, "that the air is more pestilential than in *Sardus* (Sardinia). The Spanish inhabitants are all *pale* and *yellow*, like unto them which have the *yellow jaundice*." And this he ascribes not to the latitude but to the local circumstances of the place, situated "on the banks of the river of *Dariena*, in a deep valley, and environed on every side with high hills; by reason whereof it receiveth the sun-beams at noon-tide,

on this subject, to have been *generated* by these dresses, and by the bedding of the sick, (which, as I was informed by Mr. Moss, amounted to more than 10,000 separate articles) considering the foul condition in which they were brought and kept together for I believe, six or eight weeks; and, as no such effect happened, we may here find another proof that contagion, properly so called, is not generated by accumulations of mere animal filth, even when derived from *living human* bodies, under disease, and especially that of marsh fevers.

directly perpendicular over their heads; and they are, therefore, sore vexed by the reflection of the beams," &c. "The place is also outrageous by the nature of the soil, by reason it is compassed about with muddy and stinking marshes, the infection whereof is not a little increased by the heat," &c. He adds, "now, therefore, they consult of removing their habitations: necessity caused them first to fasten their foot here, because they, which first arrived in those lands, were oppressed with such urgent hunger, that they had no respect to change the place, although they were thus vexed by the *contagion* of the soil, and heat of the sun; beside the corrupt water and infectious air, by reason of venomous vapours and exhalations arising from the same." See Robert Eden's Translation, *in black letter*, which I have here used, (but with modern spelling) not having the Latin original at hand. This extract will sufficiently prove what, indeed, could not reasonably be doubted, that marsh effluvia have, at all times, destroyed human life in America, as well as in other parts of the world: and if they did not produce an epidemic yellow fever, *in the highest degree*, among the first European adventurers thither, the reason, probably, was, that before considerable towns were built, the solar heat would not have arisen so high, as it now does in the cities where that disease has most often prevailed, nor would the soil, whilst nearly covered by trees, be accessible to the sun's rays, or capable of producing miasmata so *highly concocted*, (if I may use that expression) and virulent, as when deprived of its natural verdure and protecting shade, nor would they so readily find the means of intemperance and debauchery which large cities afford.

The earliest marsh, and probably *Yellow Fever*, of which we have any distinct account, as prevailing epidemically, and with great mortality, in the West Indies, was, I believe, that which occurred at Barbadoes, in the year 1647. From two letters, written by Mr. Richard Vines, then a planter and practitioner of physic in that island, (which were published by the late Governor Hutchinson, in his collection of Massachusett's Papers) and from Mr. Ligon's History of Barbadoes, it appears, that this fever began in or about the month of August, after a severe drought of six months continuance,* attended with *very hot weather*, and followed by a great scarcity of food. Mr. Vines' letters were addressed to Governor Winthorp, in New England, whence he had lately arrived; a circumstance which will account for his sentiments and language on this subject. He appears to have considered the fever as a punishment for the sins of the people of the island, proceeding from "the Lord's *heavy hand in wrath*;" and, being satisfied with *this cause*, he does not seem to have inquired for, or thought of any other, but says, in his second letter, dated April 29th, 1648, that, "the sickness was *an absolute plague*, very infectious† and destroying, insomuch that in our parish, there were buried twenty in a week, and many weeks together fifteen or sixteen. *It first seized on the ablest men* both for account and *ability of body*. Many who had begun and almost finished great sugar works, who dandled themselves in their hopes, were suddenly laid in the dust, and their estates left unto strangers. Our New England men here had their share, and so had all nations, especially Dutch-

* But little more than twenty years had then elapsed since the first settlement was made in that island; and so little of it was at that time cleared and cultivated, that *dry* weather, assisted by great heat, was best suited to the production of noxious miasmata; contrary to what has been the case at Barbadoes, since it attained its highest state of cultivation many years ago. About the time when this fever prevailed, there had been a great and sudden influx of inhabitants from England, in consequence of the civil commotions at home, and of other causes. Indeed, there never has been an extensively epidemic Yellow Fever known in the West Indies without the previous arrival of considerable numbers of persons from more temperate climates. Hence times of *sickness* have there commonly been times of *war*. During peace, a few passengers, arriving in single ships, and dispersing themselves in the cooler, and more wholesome parts of the country, are mostly enabled to escape the Yellow Fever; and the seasoned inhabitants are rarely *susceptible* of it.

† Believing the disease to be "an absolute *plague*," Mr. Vines, must naturally conclude that it was "very infectious," especially as many persons were attacked by it nearly at the same time; a circumstance which, in that age, was thought sufficient evidence of the contagious nature of a disease.

men, of whom died a great company, even the wisest of them."

Mr. Richard Ligon, whose History of Barbadoes was published in 1657, tells us that he arrived there early in September, 1647, when "the inhabitants of the islands, and shipping too, were so grievously visited with the *plague*, (or as killing a disease) that before a month was expired after our arrival, the living were hardly able to bury the dead." Though not a medical man like Mr. Vines, he appears to have bestowed some thought on the *physical* causes of this disease, and upon the question, "whether it were brought thither in shipping:" or occasioned "by the distempers (irregularities) of the people of the island, who, by the ill diet they keep, and drinking strong waters, bring diseases upon themselves." And though he observes that the truth on this subject "was not certainly known," he adds, that he has "reason to believe the *latter*; because, for one woman that died there were ten men; and the men were the greater *deboystes*" (i. e. most debauched). "In this *sad* time (says he) we arrived in this island; and it was a doubt whether *this disease* or *famine* threatened most, there being a general scarcity of victuals throughout the whole island." P. 21.

The reason which induced Mr. Ligon to think that the disease had not been imported, is certainly deserving of attention: for to have been imported it must have been contagious, and a *contagious* disease would not have spared the women in a manner so extraordinary, merely because they lived more temperately. There were, however, much better reasons for not considering it as an imported disease, which Ligon mentions at p. 25, without being sufficiently sensible of their operation at that time; I mean those arising from the situation and local circumstances of Bridgetown and its harbour, where the disease seems to have mostly prevailed. "Upon the most inward part of the Bay (says Ligon) stands the *town*, which is about the bigness of Hounslow, and is called the *Bridge*, for that a long bridge was made at first over a little nook of the sea, which was rather a *bog* than a sea. A town ill situate; for, if they had considered health as they did conveniency, they never would have set it *there,*" &c. &c. "But (adds he) the main oversight was to build their town upon so unwholesome a place. For the ground being somewhat *lower* than the *sea banks* are, the spring tides flow over, and *there* remain; making a great part of that flat, a kind of *bog* or *morass,* which vents out so loathsome a savour, as cannot but breed ill blood, and is, no doubt, the occasion of much sickness, to those that live there." And when it is recollected that this morass was at the *east* side of the town, and that the trade wind blowing over the morass upon the town, would directly convey its exhalations to the inhabitants, and that the long-continued hot and dry weather would necessarily render these exhalations *uncommonly noxious,* we surely need not look for any other morbific agent or influence.

A similar fever, and doubtless from similar causes, prevailed about the same time at St. Christophers, Guadalupe, &c. Mr. Webster has lately published the following extract respecting it, from a MS. of the New England Historian, Mr. Hubbard, viz.: "It extended through the plantations in America,* and in the West Indies. There died at Barbadoes and St. Kitts five or six thousand each; whether it was a *plague,* or pestilential fever, it prevailed in the islands, accompanied with great drought, which cut short *potatoes,* (doubtless the sweet potatoe, or convolvulus battatas) and fruit." See New-York Medical Repository, vol. vii. p. 322.

P. Du Tertre also mentions this disease, and calls it the

* Governor Winthrop, in a letter to Mr. Vines, dated at Boston, 24th August, 1647, had mentioned an epidemic sickness which then lately overran that part of America, and which appears to have been an *influenza,* and not a marsh, or Yellow Fever. Mr. Vines, after acknowledging the receipt of this letter, writes, (with sentiments like those he had entertained of the epidemic at Barbadoes) "I perceive by your letter, that the *Lord did shake his rod over New England:* it was his great mercy, only to put you in remembrance." By these expressions we may conclude, that the disease of New England was attended with little or no mortality.

plague, (*la peste,* jusqu' alors inconnue dans les isles, &c.) He says it began at St. Christopher, and in eighteen months carried off one-third of the inhabitants. That it was accompanied with a violent pain of the head, great debility of the limbs, and a constant vomiting; and that in three days it sent the patient to his grave. Perhaps this is nearly as good a description of the Yellow Fever as one, who was not a medical man, might then be expected to give of it. He adds, that this disease was brought to *Guadalupe* by a ship called "Le Bœnf," from *La Rochelle,* in France. He had previously stated that it was imported by some *ships* ("quelques navires") into the French islands, (St. Christopher's being then *half* French) without any mention of their names or the places whence they came, and, probably, he had no better reason for this loose statement that a belief that the *plague,* till then, as he says, unknown in that part of the world, must have been imported, and, of course, imported by ships, when the places to which it was supposed to have been introduced were islands. He did not know that the plague (even if it had then been at La Rochelle) could not exist, much less *spread epidemically,* within the tropics; I need not observe that this is also true of typhus fever; because the *latter* has not the smallest resemblance to a disease, attended with *constant vomiting,* and which generally proved fatal in *three* days. Therefore, Du Terre's supposition, that the disease was imported and contagious, deserves no attention.

Fortunately, those peculiarities of season, and of local circumstances, which are necessary to render marsh fevers both *epidemical and violent,* do not commonly recur but at considerable intervals: and I do not find that the Yellow Fever again became prevalent in the West Indies until about the year 1686, when it appeared at Martinico, and subsisted for several years. And, as a French ship of war, the *Oriflamme,* arrived about the same time at that island, with a number of French people, who had, some time before, settled themselves at Merguy and Bancok, in *Siam,* (whence they had been driven, when the French intrigues at that court were frustrated) the disease, as usual, was supposed to have been imported by that ship, and, therefore, was called *Mal de Siam*. There is not, however, so far as I can discover, any account of the supposed introduction of this disease by the Oriflamme, except that which is given by Father Labat, a Dominican or Jacobin Friar, who arrived at Martinico the 29th January, 1694, seven or eight years after the event in question; and his account (which must have depended on hearsay) is extremely loose and defective.* He tells us that the Oriflamme

* Father Labat, after mentioning that one of his order, le Pere Loyer, had been lately attacked with a disease of which, in the course of it, he was supposed to be dead five or six times, but which without proving mortal, *lasted thirty-two days*, adds, "On appelloit cette maladie le *Mal de Siam*, parcequ'il avoit été apportè à la Martinique par le Vaisseau du Roi, *l'Oriflamme*, qui, revenaut de *Siam*, avec les débris des Etablissemens qu'on avoit fait á Merguy & á Bancok, avoit touché *au Brésil, ou* il avoit gagné *cette maladie, qui y faisoit* de grands ravages depuis *Sept ou huit ans.* Ce vaisseau périt en retournant en France. Les Symptômes de cette maladie étoient autant differens, que l'étoient les tempéramens de ceux qui en étoient attaquez, ou les *causes* qui la pouvoient produire. Ordinairement elle commençoit par un grand mal de teste & de reins, qui etoit suivie tantot d'une grosse fievre, et tantot d'une fievre interne, qui ne se manifestoit point au dehors."

"Souvent il survenoit un *debordement* de *Sang, par tous les conduits du corps, meme par les pores;* quelques fois on rendoit des paquets de vers de differentes grandeurs & couleurs, par haut & par bas. Il paroissoit à quelques uns des bubons sous les aislelles & aux aisnes, les uns pleins de Sang caillé noir & puant, & les autres pleins de vers. Ce que cette maladie avoit de commode, c'est qu' elle emportoit les gens en fort peu de temps; six ou sept jours tout au plus terminoient l' affaire." He adds, that he had known but two persons who died of the disease, *after* it had continued more than fifteen days; that some persons, who felt nothing more than a little headache, fell down dead in the street, while walking for the air; and that in most of them, the flesh became as black and putrid in a quarter of an hour after death, as if they had been dead four or five days:—That the English, who were frequently made prisoners, carried the disease to their islands; and that it was communicated by the same means to the Spaniards and Dutch, and made great ravages when he left the islands, in 1705. He concludes by saying, that he was attacked *twice* with this disease; that he escaped the first time, after four days of fever, and *vomiting of blood;* ("aprés quatre jours de fievre & de vomissement de sang") but, that the second time he was in danger six or seven days. *Such* is his account, *faithfully* and *fully* extracted, from pages 72, 73, and 74, of the 1st vol. of the original Paris edition of his "Nouveau Voyage Aux Isles de l' Ameriques," in 6 vols. 12mo. printed in 1722. Dr. Chrisholm, at p. 105 and 106, of the 2d vol. of his essay, has given touched at Brasil, and caught the disease, which he represents as having been prevalent *there* for seven or eight years. There is, however, no reason, that I can discover, to believe that any *contagious* fever has, at any time, subsisted in Brasil, though that country is not exempt from violent marsh fevers; nor is it probable, considering the known jealousy of the Portuguese government, in regard to the admission of strangers at Brasil, that any would have been permitted to land from the Oriflamme, and communicate with the inhabitants, so as to become infected by a fever of that sort, if it had existed there. We are not even told which of the harbours in that extensive country the Oriflamme entered; she might have anchored in one which was surrounded with marshes, and I should conclude this to have been the case, if it were ascer-

an extract on this subject, taken avowedly from the Histoire Generale Des Voyages, which extract is *very incorrect* and defective, though he *asserts* that his readers will find it "*is literally taken from the original.*" An assertion which ought not to have been made, because it is not true, and because he manifestly had had no opportunity of comparing this extract with the original of Pere Labat. Unfortunately, there are but too many other instances of his want of *caution* in making assertions, even the most *positive;* though I am willing to believe, that an intention to mislead has not been among his motives. *Such* also is the *account* of a disease which, according to Dr. Chisholm, "differs materially from the endemic yellow remittent, and bears a striking *affinity* to the *true plague*, as well as to the malignant pestilential Fever of 1793;" and we, therefore, need not wonder that this gentleman, who is anxious to assimilate Yellow Fever with plague, should state this account to have been given by La Fabat, with his accustomed *accuracy* and *minuteness*," though I believe few other persons will be satisfied with it in these respects. To say nothing of its important omissions, who will believe the occurrence of *buboes*, sometimes *filled* with *stinking*, *black*, *coagulated blood*, and at other times *with worms?* or that there was any foundation for what he says of *packets* or *bundles* of worms, of different sizes and *colours*, discharged upwards and downwards? excepting this, that persons troubled with worms, often discharge them in consequence of fever, and some other diseases; though their doing so is not a symptom *peculiar* to any disease, and much less to the *Mal de Siam.* But in truth, Pere Labat does not seem to have intended fully to describe the symptoms of that disease, which he says were as *different* as the *temperaments* of the individuals attacked by it, *and as the causes by which it was produced;* a plain indication that he considered the disease as arising from *different causes*, and, consequently, *not* always from contagion. This *part*, however, of Labat's account, is one of those which are omitted in Dr Chisholm's extract.

tained, that a *fever* was prevalent among her passengers and crew before they reached Martinico, and not until their arrival in Brasil; but if, as seems not improbable, this only happened upon their arrival at Martinico, we can have no difficulty in finding abundant, and much more likely causes for the disease in the *carenage* of that island, which even Dr. Chisholm declares to be "the *most sickly hole* in the West Indies;" (vol. 2. p. 84.) or, indeed, at St. Pierre, the only other harbour of that island in which a ship of war, circumstanced as the Oriflamme was, can be supposed to have remained.* And, indeed, the greater part of Martinico, is so abundant in every thing favourable to the production of the *most deleterious miasmata*, that there is no island in the West Indies of the same size in which all the varieties of marsh fevers prevail oftener, or with greater mortality; nor can any thing be more chimerical or unreasonable than the having recourse either to *Brasil* or *Siam*,† for contagion, as the cause of any fever which ever was prevalent in the West Indies. Of this a multitude of proofs might be given, if the notoriety of the facts did not render them completely unnecessary.

* Dr. Chisholm, having mentioned that "the prevalence of the *yellow* remittent fever, at a certain period of the year, at Fort Royal, (Martinico) should not excite surprise, the cause being so abundant in its neighbourhood," adds, "that part of the city of *St. Pierre*, called the mouillage, being very *low* and *moist*, although *not marshy*, is also subject to the *destructive fever* during the same period, the *hotter months*."

† Kæmpfer, who touched at *Siam*, in his way to Japan, gives the following account of the river Meinan, the part most connected with the subject of marsh fever. "It overflows its branches like the Nile in Egypt, though, at contrary times, and by setting the country under water, renders it fruitful. This overflowing begins with the month of September." "In December, the waters begin to fall by degrees." "When the waters fall, and *return* to their former channel, (the river) they (the inhabitants) are apprehensive that a *great mortality will ensue*, among men and cattle; to avert which calamity, a solemn festival is kept throughout the whole country, in order to appease the destroying spirits, (miasmata) which remain after the water is run off" "The banks of this river are low, and, for the greater part, marshy, yet"—"they are pretty well inhabited: along them appear many villages, the houses of which are raised on piles." "From Bankok to the harbour, there is nothing but forests, deserts, and morasses." Kæmpfer's History of Japan, vol. 1. p. 44. In such a country and climate, marsh fevers may well be supposed to prevail, whenever the surface of the inundated ground is left *bare*, and exposed to the sun's rays.

Soon after this epidemic prevalence of Yellow Fever at Martinico, it seems to have occasioned great mortality at *Nevis*, (i. e. in 1689,) but no description of it, worthy of notice, has been preserved.

The same fever appeared again as an epidemic at Barbadoes, in 1695, and continued for several years after. Mr. Hughes mentions this on the authority of Dr. Gamble, who is stated by him to *remember* that this fever "was very fatal" in that year; and, as all who had any accurate knowledge of it in 1647, were probably dead or removed, "it was then called the *new* distemper," and afterwards "*Kendal's fever*;" also "the pestilential fever, and bilious fever." It is said to have been frequent and fatal in May,* June, July, and August, and *then mostly* "*among strangers*;" "though a great many of the inhabitants, in the year 1696, died of it; and a great many at different periods since." "The same symptoms did not always appear in all patients; nor alike in every year." See Natural History of Barbadoes, p. 37.

This statement is amply confirmed by Captain Thomas Phillips, in the account of his Voyage to Africa and Barbadoes, published in the 6th volume of Churchill's Collection. He was at Barbadoes with a large ship in 1694, and says, it was the fate of that island to be then "violently infected with

* Barbadoes was then so generally and highly cultivated, and the soil so much more apt to become *deficient*, rather than redundant in moisture, that the rains, which commonly begin there in the month of May, were then, *as at present*, better suited to the production of Miasmata, than the very dry weather which produced them in 1647. Some other West Indian islands have undergone a similar change, particularly Antigua. Dr. Chisholm states, that when the French, under Mons. D'Enambuc, were driven from St. Christophers by the Spaniards, in 1629, and "sought an asylum in Antigua, they found it so unhealthy, so marshy, and so incapable of cultivation, that they, with one accord, intreated their leader to conduct them to Montserrat," them "inhabited by the Caraïbes." Since that time, however, both the soil and atmosphere have frequently become so dry as to produce effects highly detrimental to the inhabitants and their cattle, &c. Some of these Dr. Chisholm mentions as having occurred in 1779, adding, "when these destructive dr tracts of weather are suddenly succeeded by a profusion of rain, which generally happens once in three or five years, a *very fatal epidemic remittent* is the consequence." See his *Essay*, &c. vol. 2d, p. 276 and 279.

the *plague*, so that, in the late war, it proved a perfect *grave* to most that came there, *all new comers* being generally seized with pestilence; of which very few recovered. Captain Thomas Sherman, of his Majesty's ship Tiger, in two years that he lay there, buried out of her, 600 *men*, as he told me, though his compliment was but 200; but still pressing *new* out of the merchant ships that came in to recruit his number in the room of those that died daily." "I lost (adds Captain Phillips) about eighteen of my men by it, and, in truth, I did not expect to escape myself, and was, therefore, so indifferent that there was not a friend, or acquaintance of mine, seized with the distemper, but I freely and frequently went to visit him, which possibly was the reason that I escaped, by having *accustomed* myself to the town, and most infectious air from the beginning, which I did by the advice of the ever-honoured and worthy Colonel Kendal, &c." "while those that kept in the country, in better air for fear of it, were commonly infected when they came on any business to town. Here died about twenty masters of ships during my stay here, of which number were Captains Gurney and Bowles, who commanded his Majesty's ships, *Bristol* and *Play-Prize*." P. 253.

It appears from this account that the disease prevailed chiefly in Bridgetown, and that persons coming to it from healthier parts of the country, and imbibing miasmata, produced by the local circumstances which Ligon had long before described, were attacked by the fever, as constantly happens on similar occasions, at Charleston, Philadelphia, &c.

Here it may be observed that, in every instance, wherein the causes of marsh fever have been so powerful as to produce a violent epidemic in the West Indies, and with that *exasperation* of symptoms which seems more incidental and *natural* to this *kind* of fever, than to any other disease, persons have been disposed to consider it as the plague, or a new distemper. Dr. Henry Warren fell into the same mistake afterwards, (as Dr. Chisholm appears to have done, more recently, at Grenada,) when the Yellow Fever again became prevalent at Barbadoes, between the year 1732 and 1738. The true plague, indeed, had not appeared in any part of Europe holding a communication with the West Indies, subsequently to the years 1720 and 1721, when it proved most fatal at *Marseilles* and in some other parts of *Provence*; and, therefore, Dr. Warren concluded, that the Yellow Fever, which he saw at Barbadoes, in 1732, and the following years, and which he denominated a "*malignant fever*," was a continuation of the *plague*, which he imagined to have been brought from Marseilles to Martinico, and thence to Barbadoes, in 1721, by the Lynn ship of war; although Dr. Towne, who lived and practised as a physician at Barbadoes, about that time, and, in 1724, wrote upon the Yellow Fever there, under the denomination of "Febris ardens biliosa," appears to have had no knowledge or suspicion of any such *importation*, or of any difference between the Yellow Fever of his time, and that which had previously occurred; nor of its being any other than an indigenous production of that island;* yet Dr. Warren charges Towne with having confounded "two most different maladies," viz. "the malignant and the ardent fever of Barbadoes;" and he represents the former as being a fever "*truly of the pestilential kind*," upon grounds and reasons which Dr. Hillary, and others, afterwards contested as being chimeri-

* Towne describes his ardent bilious fever as commonly terminating "in a favourable crisis, or the *death* of the patient, about the fourth day after the attack." (Treatise on the Diseases of the West Indies, &c. p. 20.) He supposes this fever to proceed from a redundance of bile, and that the yellow suffusion was produced by the efforts of nature, to *depurate* the blood, by throwing this redundant bile upon the surface. "The regular crisis, therefore, of this fever, (says he) generally discovers itself by a suffusion of the bile all over the surface of the body about the third day." He adds, that an appearance of it may often be discovered "in twelve hours after the attack, if *you carefully inspect the coats of the eyes*, and the *sooner* it appears the more encouraging is the prognostic," &c.

I have mentioned, at p. 34, my belief, that the *yellowness* might, with attention, be, in many instances, "*first* discovered on the *eyes*" Probably this would *always* be the case, if their *predominating redness*, in the early part of this fever, did not render the yellow tinge, in a great degree, imperceptible.

cal or fallacious. But, independently of their facts and arguments on this subject, what I have mentioned, at p. 222, 224, of the acknowledged and absolute impossibility of propagating the plague within the tropics, will sufficiently refute Dr. Warren's opinions and allegations.

Dr. Hillary was a well-educated physician, and practised, with unequalled credit, for many years, in Barbadoes; and, as the Yellow Fever does not appear to have prevailed *there epidemically*, during his time, he must have had the *best* opportunities for ascertaining whether it possessed any contagious property or not; especially as he, undoubtedly, saw cases of it, arising from *all* the several causes which have been already mentioned as capable of producing idiopathic Yellow Fever; some of which might be supposed, more likely than marsh miasmata, to occasion fever with a contagious quality. And he has delivered the result of all his observations in the following passage, viz.:

"I never could observe any *one* instance, where I could say, that one person was infected by, or received this fever from, another person who had it; neither have I ever seen two people sick in this fever in the same house, at, or near, the same time, unless they were brought into the same house when they had the fever upon them, before they came in. From whence (adds he) we may conclude, that it has nothing of a contagious or pestilential nature in it, and that it is a different fever in all respects, as it will more fully appear hereafter." See his volume on air, and Epidemical Diseases, in Barbadoes, 2d edit. p. 145, 6.

In confirmation of Dr. Hillary's testimony, I shall adduce that of Dr. James Clark, now, or late, of Dominica, delivered in his Treatise on the Yellow Fever, &c. after twenty-five years constant and extensive practice in the West Indies. At p. 22, he states facts respecting the appearance of this fever in Dominica, during the years 1793, 4, 5, and 6, in which years Dr. Chisholm asserts the fever at that island to have been, what he calls, the malignant pestilential fever, brought to Grenada by the Hankey, in 1793, and thence propagated to the other islands: these facts decidedly prove that this supposed malignant fever manifested no contagious quality in Dominica; but I shall reserve them to be employed in my appendix, No. 7, on that subject. The following paragraph, taken from p. 52, 3, of Dr. Clark's Treatise, appears to relate more immediately to the Yellow Fever, as it commonly occurred before the year 1793.

This fever has not prevailed much in these Windward Caribbee islands for many years past. At *Fort Royal,* in Martinique, where there is a great prevalence of mephitic effluvia, arising from the marshy ground at the back of the town, it generally broke out in the summer or autumnal season, on the *arrival of troops from France,* or of a number of *seamen who had never been in the West Indies before:* and the same thing happened at *Point à Petre,* in Grand Terre, *Guadaloupe,* almost annually, and from the same cause; but it *was never looked upon as an infectious disease,* nor did it ever spread among the *natives of the towns,* or among those who were *seasoned* to the *climates;* nor was it *ever carried from thence* to the other islands. In this island but few cases have occurred for these last twenty years, and these have chiefly been at Prince Rupert's Head, where from the stagnated water in a large *morass near the town and fort,* the marsh miasmata prevails in a *higher degree.* Since the *swampy* places, which were in the town of *Roseau,* have been *filled up,* this fever has been seldom observed; but, previous to the year 1792, we had generally violent thunder storms, heavy rains, or severe gales of wind, during the autumnal season:" and these Dr. Clark considers as obviating the prevalence of this fever.

I have the more readily availed myself of Dr. Clark's testimony, concerning the Yellow Fever at Martinico and Guadaloupe, because, *for more than twenty years,* he resided in an island *between* and *very near to both,* and because I believe that no French physician, practising in either of those islands, has written any thing on the subject worthy of notice.

In regard to *Grenada,* I have Dr. Chisholm's authority for asserting, that, from the year 1763, when that island "was ceded to Great Britain, and till the year 1793, *(thirty years) no contagious fever,* and no epidemic, of the character of the malignant pestilential fever, appeared" there. See Essay, &c. vol. 1, p. 295. Whether the fever of 1793 was such as Dr. Chisholm has described it, will, I hope, be fully ascertained in my seventh appendix.

And here I shall close my view of this subject, so far as relates to the Windward Caribbee islands, with an addition of only one document, declaratory of the opinions and experience, in those islands, and on this question, of the officers of the hospital-staff, in the army, commanded by Sir Ralph Abercrombie, during the year 1796, and a great part of 1797, when, unfortunately, they had but too many opportunities of seeing and treating the Yellow Fever. That my readers may be informed of the *origin* of this document, I must observe, that, in November, 1796, the medical staff-officers at Martinico, were assembled at the General Hospital of *La Charité,* when an order from the army medical board was read to them, requiring their opinions, concerning the disorders most prevalent in that army: and it being proposed that a committee should be appointed, to prepare a statement of our opinions, I suggested that a single general statement would, as I conceived, but imperfectly answer the purpose, and that it might be better that each individual should separately state his own opinions; and this suggestion being adopted, Mr. Young, Inspector-General of the Hospitals to that army, after some delay, was furnished with our separate opinions, and with those of the Hospital-staff officers in the other islands; and, in consequence thereof, he wrote a letter to the army medical board, dated St. Pierre, 23d July, 1797, of which the following is an extract, viz.:

By his Majesty's ship, Arethusa, I send, agreeable to my letter of the 25th ult. under cover to the Secretary to the board, the opinions of the medical officers of this staff, on the prevailing diseases among the troops in this country, by which the board will perceive that contagion, or infection has had little or *no* share in the mortality; and I must beg leave to add, that *it has never occurred in a single instance*, to my observation." My own individual opinion was in exact conformity with that which Mr. Young has here expressed. I had never discovered any appearance of contagion at St. Lucia, nor when placed at the head of the hospital department, at Barbadoes, in the summer of 1796,—nor afterwards, when officiating as physician to the forces at Martinico; nor again when placed at the head of the medical staff in Grenada, during a great part of 1797, and until my return to England.

St. Domingo next presents itself to our observation, in regard to the history of Yellow Fever: of its aptitude in many parts, to produce highly noxious miasmata, and the fevers resulting therefrom, in all their various forms, there has been but too much evidence even within a few years. Witness the many thousands of British soldiers, who perished there by these fevers, between the years 1793 and 1799, and the numerous French army, sent thither in 1802, under General Leclerc, and which in a short space was nearly *annihilated* by the same fevers; of which a very sufficient account has been given in the Medical History (Histoire Medicale) of that army by its chief physician, M. Gilbert.

The earliest mention which I recollect of the prevalence of Yellow Fever in St. Domingo, is contained in certain *manuscripts*, of M. Bourgeois, formerly Secretary to the Chambre of Agriculture, at Cape Français, published, after his death, by his nephew, in 2 vols. 8vo. under the title of "Voyages Interessans Dans Differentes Colonies;" from these it apyears, (p. 202) that, in the year 1731, a Spanish squadron arrived at Cape Français, commanded by Don Manuel Lopez Pintado, in the St. Louis, of eighty guns, who, besides several other ships of war, had, under his convoy, some very rich galleons. They were returning to Spain from Porto Bello, and, having suffered greatly by a storm, had put into the Cape to refit, where they remained *five* months, surrounded by sources of marsh miasmata; and having already, as we are informed by Don Antonio Ulloa, (in the 5th chapter of the first book of his Voyage to South America) been *grievously* attacked by the Yellow Fever, "*vomito prieto,*" before they left Carthagena, we need not wonder that, in this last situation, they continued to be afflicted by the same disease, and of a form probably more violent than had been previously noticed at the Cape: especially in regard to Petechiæ and Hemorrhages, from different parts of the body, proceeding, as I conclude, from a scorbutic disposition to which sailors, at that time, were almost invariably subject, especially after long voyages. Here they expended great sums of money, and thereby enriched many of the inhabitants, but, like the Oriflamme, at Martinico, they were accused of introducing, at Cape Français, a new pestilential fever: "On pretend," says the author of the *manuscript* in question, (p. 205) "qu'elle occasionna l'espèce de *mal pestilentiel* qui a long tems règné dans le Cap, & que l'on *traitait faussement du nom de Maladie de Siam,*" &c. At p. 432, M. Bourgeois, in a "Memoire," written by himself, "sur les maladies les plus communes à St. Domingue," reverting to this event, observes, that "Le nom de Maladie de Siam vint a l'esprit de quelqu'un, a cause d'une espèce de resemblance dans la malignité; aussitôt cela se repandit, & cette dénomination *impropre*, est demeurée aux fievres malignes, trés communes dans ce pays-ci. *Les plus mal fesantes,* s'attachent principalement *aux nouveaux arrivés,*" &c. He then proceeds to mention the most remarkable symptoms of these fevers, especially the violence of their first attack, with a strong determination to the head, inflamed appearance of the face, yellowness of the skin, and profuse hemorrhages from various parts of the body, and sometimes even through the pores of the skin. He adds, p. 434, that all the fevers of St. Domingo are of the same kind, and nearly related to the intermittents, double tertians, and continued fevers, with or without exacerbations, (" avec redoublemens, ou sans redoublemens") in Europe.

The name of Maladie de Siam having been thus applied to the most violent form of marsh fever in St. Domingo, it was *adopted* by M. Poupée-Desportes, a physician of great merit, who arrived there in the following year, and, during the fourteen succeeding years, kept an account (founded on accurate observations) of the weather and diseases, as they occurred first at the Cape, and afterwards at Fort Dauphin, by which it was acertained, that the prevalence or absence of Yellow Fever, at those places, *invariably* depended upon the changes of season or constitution, in regard to heat and moisture, especially during the summer and autumn. Thus at the *Cape*, in 1733 and 1734, after very copious rains, extremely hot and dry weather commenced, and lasted during the summer and autumn, inducing a violent Yellow Fever, (" Mal de Siam" which reigned exclusively for the space of four months, and carried off more than half of the new comers and sailors; while there was very little sickness in the more elevated country situations. Again in 1739, 1740, 1741, and 1743, after extremely hot and dry weather for a considerable time, the Yellow Fever again became prevalent, and fatal to a great part of those who were attacked by it, and who, as usual, were chiefly strangers. On the other hand, the temperature in 1735, 1736, 1737, 1738, and 1742, was mild, or, at least, moderate; and, in those years, this fever only occurred sporadically, and with diminished violence, so that most of the new comers, who were attacked by it, recovered. Mr. Poupée-Desportes also observed, that when this fever prevailed as an epidemic among strangers, and in its more violent forms, at the Cape, and at Fort Dauphin, it affected the *seasoned* inhabitants only as a mild bilious remittent, or, as he called it, a lymphatic fever. From this constant dependance of Yellow Fever upon the state of weather, this author infers that it ought to be regarded " comme une de ces maladies dont il faut chercher la cause dans la constitution de l'air;" and, consequently, not as produced by contagion. See Histoire des Maladies de St. Domingue, tom. 1. p. 191.

I ought here to mention, according to the information given by M. Valentin, formerly "Premier médicin des Armées de St. Domingue, &c. at p. 58 and 59 of his Traité de la fievre jaune d'Amerique, that, in the French West Indian colonies, and especially since the time of Poupée-Desportes, a distinction has been made between "la maladie de Siam, and la fievre jaune," which last was known under the denomination of "fievre ardente maligne, ou fievre bilieuse maligne;" and sometimes under that of "la Matelotte." That these diseases were "identiques," or, at most, only presented a *variety* in their effects. That the name *Mal de Siam* was given when the signs of dissolution of the blood were present in the highest degree, ("au Comble") when, besides a jaundice, the blood became extravasated under the skin, making its way through different natural outlets or passages, and transuding by the pores at some points of the cuticular surface. (par les pores de quelques points de la surface cuticulaire.") It seems probable, therefore, that this *distinction*, and this application of the name Mal de Siam, were derived from an unusual prevalence of Hemorrhages and Petechiæ, observed in the crews of the Oriflamme, at Martinico, and of the Spanish squadron, at St. Domingo, when labouring under the Yellow Fever; and that, in both cases, there was, as might well be expected in such long voyages, and at those times, a great *predisposition* to scurvy, or to a dissolved state of the blood, in the persons so affected; though Hemorrhages, &c. have occurred not unfrequently in other situations to persons under Yellow Fever, and sometimes when there was no appearance of a scorbutic disposition.

Unfortunately, this account of the weather and diseases at St. Domingo was not continued by any other physician after the death of M. Poupée-Desportes: though it appears from M. Valentin's Treatise, that the French physicians at St.

Domingo had generally thought the Yellow Fever to be not contagious, and that this was also his own opinion.

Of the Yellow, and other marsh fevers, as they affected the British army at St. Domingo, during the late war, a very good account has been given by Dr. Hector M'Lean, to which I must refer those of my readers who wish for more ample information on this subject; only observing that, he also delivers it as his decided opinion, that "what has been called Yellow Fever there, is not an infectious disease; that it is the common remittent endemic of that country, applied to the English or European constitution." This opinion is repeated almost in the same words, at p. 71, and again at p. 78, where he says, "there is no point on which I am more *decided* than the absence of contagion in the remittent of St. Domingo;" and this he declares to have been the opinion of Dr. Scott, Dr. Wright, and Dr. Gordon, (all physicians on the St. Domingo Staff,) and of every medical man with whom he conversed at that island.*

In regard to the fever which destroyed the army under General Leclerc, at St. Domingo, in 1802 and 1803, I must refer my readers to M. Gilbert's Histoire Medicale, only observing that he also declares it not to have been an imported disease, but to have originated in an atmosphere extremely heated, and filled with marsh effluvia ("elle a son origine dans un air trés-chaud, saturé d'émanations marécageuses")

* Dr. Jackson, who was also on the Hospital-staff at St. Domingo, has delivered similar opinions in his Outline of the History and Cure of Fever, &c.; in which he states the endemic fevers of the West Indies to be produced by exhalations from the surface of the earth, and that, "though they often destroy life, they beget no power of propagation in the patient,"—"that they may become epidemic, but not contagious." Of their varieties, he says, "the disease, in the more violent forms, is, or appears to be, *continued*, in some situations; in others it is remitting, and of regular type. In wet weather, and on swampy grounds, the endemic of the country (St. Domingo] is usually *remitting* in form; and, under this form, exhibits appearances of jaundiced yellowness, of black vomiting, purgings of black matter, hæmorrhage from different parts of the body, petechiæ, lividness," &c.

p. 93. He adds, in the next page, that it is *not contagious*;* and that this is the opinion of the generality of Practitioners. —But, that it is *epidemic* for almost all new comers; and a tribute which must commonly be paid by them within the first year after landing. And having asked, at p. 77, whether the *Yellow* Fever be a disease, from the bilious (or marsh) fevers, he answers, "il y a tout lieu de croire qu'elle n'est autre chose que le *maximum* des fièvres rémittentes bilieuses," i. e. there is every reason to believe that it is only the *highest* or most *violent form*, of bilious remitting fevers. At p. 80, M. Gilbert advises those who are under the necessity of living in the city of Cape François, to remove from the shores of the sea, and especially from the environs "de L'embouchure de la rivière du haut du Cap; lieux ou la brise de terre porte chaque jour les émanations marécageuses de cette *surface immense de lagons*, qui s'étendent de L'embarcadére de la Petite anse, au bourg du haut du Cap."

The first epidemic fever, in Jamaica, of which I have found any account, is that mentioned by Dr. Trapham, in a little volume, entitled, "State of Health of Jamaica," printed in 1679, about twenty-four years after the capture of the island by Cromwell's forces. In this volume the author, after representing Jamaica as not liable to any pestilential or epidemical disease, adds, p. 81,—"I know it hath been commonly received, that, about eight years since, when the vic-

* It must be observed, that M. Gilbert delivers this opinion that the Yellow Fever is not contagious with a sort of qualification; because he supposes that, when great numbers of patients, under this putrid gangrenous disease, as he calls it, are collected together, the emanations from their bodies may excite fever in persons who are constantly exposed to them, and also exposed to the *causes* which *originally produced the fever*;" ("a l'action des causes qui la font naitre") but these causes (miasmata) must be sufficient *alone*, and the *emanations* from the sick must, therefore, be superfluous. He had observed that the attendants on the sick in hospitals at the Cape, were frequently attacked with the fever; but, as this was the case of almost every one *out of the hospitals* also, and, as the hospitals were, according to his own statement, placed in the most unwholesome part of the town, (whence he says they ought to be removed) it would have been extraordinary indeed, if persons, by remaining in them, had escaped the disease.

torious fleet returned from the signal *Panama* expedition, that then they brought with them an high, if not *pestilential* fever, of which many died throughout the country. But, this being a foreign distemper, brought from abroad, the causes of which I could not so well judge of, I am not as yet forced from my opinion thereby, but conclude Jamaica more happy than to be annoyed therewith, directly and *originally*."

Dr. Trapham here alludes to the famous expedition under Henry Morgan, who, at the head of about 1200 Bucaneers, took Panama, in 1670, and returned to Jamaica with so much plunder that his own share amounted to 400,000 dollars. With this he became a planter, was made Lieutenant-Governor of the island, and knighted. Of the fever, with which these men were affected, at and after their return, I can find no distinct account; but, as in their march across that part of the continent, they must have been almost continually exposed to marsh effluvia, and, after their return, with so much wealth, would naturally have run into debauchery and intemperance of all sorts, there can be no difficulty in finding sufficient causes to produce among them even the most violent fevers in that climate. That marsh fevers have subsequently prevailed at Jamaica, to great and fatal extent is but too certain; though distinct and accurate accounts of them are wanting; at least, I have found none anterior to the Essay on Yellow Fever, by Dr. John Hume, who, for many years, had the direction of the Royal Marine Hospital, at Jamaica, and was afterwards commissioner for the sick and hurt of the Royal Navy. This gentleman computes that in 1741 and 1742, after the return of Admiral Vernon's fleet, from the unsuccessful attempt upon Carthagena, 11,800 sick were sent to the Royal Hospitals of Jamaica, and that, of this number, not less than 7000 were attacked with the *Yellow* or bilious Fever. "Of these (says he) I used to compute that 1500 died, that is something less than one in four; but, in this, I pretend not to be exact." See Dr. Hume's Essay, in the volume on West India Diseases, published by Dr. Donald Munro.

Dr. Williams, in his Essay on the Bilious *Yellow Fever* of Jamaica, (which Essay occasioned a duel between the author and Dr. Bennet, and the deaths of both) says, this disease, at the time of the expedition to Carthagena, was "so general and fatal, that people looked upon it as a plague, and shunned the sick as they would contagion." It does not appear, however, that he, or any well-informed medical man at Jamaica, then believed it to be contagious. On the contrary, Dr. Hume says, (p. 238) "we have *undoubted proofs* that the disorder is *neither a plague nor contagious*, as Dr. Warren has alleged." He observes, that it commonly made "its attack after hard drinking, violent exercise, dancing, and sleeping in the open air; that "strong muscular men are most liable to it, and suffer most;" that "*Creole* white men are rarely seized with it;" that he "never knew any *Creole* white women ill of it;" but has known it prove fatal to *European* white women, though "they are not so liable to it as the other sex." He adds, "I have never seen any *negro*, male or female, native or foreigner, attacked with the bilious fever," p. 237 and 238. In all these particulars the fever at Jamaica appears to have agreed with what has generally been observed of yellow and other marsh fevers. It appears also, that many of the cases which fell under the care of Dr. Hume, were extremely exasperated, and attended with vomitings "of a coffee colour," as well as "*black*," and with a *mortification of the stomach*, which, he says, *was always found after death*, "in all such subjects as I have either opened myself, or seen opened by others, after having had black vomiting." See p. 217.

Dr. Lind, in his work on Preserving the Health of Seamen, says, that the Lords of trade and plantations wishing to ascertain, for a particular purpose, whether the Yellow Fever of Jamaica was contagious or not, "a physician was consulted, who had long practised in that island, who gave it as his opinion, that from the *Yellow* Fever of that island *there was no infection.*" This (he adds) was not only the opinion of that gentleman in the court, but is the belief, as I am informed, of the best practitioners in that island, and also of Dr. John Eliot," (since Sir John,) "a skilful physician in London, of Mr. Nasmyth," (Surgeon to Admiral Holmes, of Jamaica) "and many others, who have had opportunities of being *well acquainted* with the diseases of Jamaica." See p. 292, 3d edit.

Of similar import is the testimony of the late Dr. John Hunter, as delivered in his excellent work on the Diseases of the Army in Jamaica, in which, at p. 83, he declares himself "able to say with *certainty*, that it, (the Yellow Fever,) is not infectious." He adds, "in the Military Hospitals, the sick, admitted with fevers, were above *three quarters* of the whole; and they were *often much crowded* together, yet there was no reason to believe that a man, with any other complaint, ever caught a fever in the Hospital." This testimony has, indeed, been repeatedly given, though not in the same words, by Dr. Hunter, in different parts of his work.

These facts and opinions might probably be thought sufficient in regard to the Yellow Fever at Jamaica, had it not been supposed by some persons that a malignant pestilential fever was introduced at that island, from Grenada, in 1793, and there mistaken for Yellow Fever: to ascertain the fallacy of this supposition, it may not be improper to adduce the testimony of Dr. Lempriere, now physician to his Majesty's forces, who was then employed in that island. This gentleman, at p. 22 and 23 of the 2d vol. of his Practical Observations on the Diseases of the Army in Jamaica, says of the Yellow Fever, that it became so prevalent, and proved so fatal in Jamaica, during the years 1793, 1794, and 1795, as to give rise to a very general opinion, that it was highly infectious, and that it had been imported from the windward islands by contagion."—"But, to those who understand the influence of contagion, it will appear, that this disease did not prevail as if it were of such kind; for it chiefly affected the newly-arrived European, as yet unnerved by the climate, whose high health alone rendered him subject to its ravages; while the delicate and weak persons, particularly liable to the influence of contagious diseases, were altogether exonerated from this fever." The same author, at p. 29 and 30, positively denies the existence of any contagious property in this fever; adding, p. 31, that it did not spread *generally over the island* as a contagious disease would have done, in the then existing circumstances; but "was confined to those situations only where remittent fevers are most prevalent and fatal, and to those subjects who had lately arrived from Europe with robust and plethoric constitutions." At p. 47, Dr. Lempriere, to account for the occurrence of this fever, with *uncommon* violence in 1793, mentions that the rains usual in the month of May were then *excessive*, and that they were followed by very *extraordinary heat* in June, July, and August; which naturally occasioned a more copious and more concentrated exhalation of miasmata, than in former years; and as a much greater number of persons arrived about that time from Europe, in consequence of the war, the violence of the fever, and the numbers attacked by it, were very much increased, as might well have been expected; though it attacked none who had long resided at Jamaica, and entirely *ceased*, as usual, about the month of January; until re-produced by similar causes, in the following summer and autumn.

In a paper concerning the Yellow Fever, which prevailed at Jamaica in 1793, 1794, and 1795, read by the late Dr. James Walker, to a medical society, which had been formed at Port Royal to investigate the nature of this fever, the author describes it as attended with a constant propensity to vomit, by which mucus and bile were first thrown up; and afterwards, generally about the third day, a matter resembling coffee grounds, and sometimes of the colour, consistence, and tenacity of tar. Hemorrhages were frequent, from the mouth, nose, and, sometimes, from the axilla, anus, and vagina. Near the close of the disease, in those cases which terminated fatally, a yellowness appeared, first in the eyes, and on the neck, gradually extending over the whole body, and acquiring a darker hue; very few had pætechial eruptions. In 1793 and 1794, the fever did not intermit, and often terminated fatally in two or three days. In 1795, it became somewhat milder, and more protracted. It was nearly confined to newly arrived Europeans, though some old residents in the interior (and probably high and cool) parts of the island, were reported to have been affected by it. The author believed the disease not communicable from one person to another; observing, that "in the public Hospital, where many people were necessarily in the same wards, with numbers in this fever, neither any of them, nor of the attendants upon them, were infected." See New York Medical Repository, vol. 1, p. 486, 7.

I shall dismiss this subject, so far as regards Jamaica, by referring my readers to a small, but very valuable, Treatise on the Yellow Fever of that island, by Dr. Grant, a physician (as I am informed) of the greatest eminence, and most extensive practice there. He considers this fever decidedly as not being contagious; and as being only an aggravated form of the remittent of hot climates, exclusively attacking those who have lately arrived from colder countries, and who bring with them an inflammatory diathesis. He observes, however, p. 27, that "in its mildest state, under its clear remitting form, it attacks both the long resident and native;" and that, "for several years past," (i. e. previous to 1801) "the native and European, of long residence, have experienced it, under a *greater degree* of aggravation of symptoms," than formerly.

Were I to extend this *view* to other West-Indian islands, and to the Spanish settlements on the continent of America, particularly at Carracca, La Guayra, Venezuela, Carthagena, Porto-bello, and Lavera-crux, it would present a repetition of nearly similar facts. Believing, however, that a statement of them would be thought superfluous, and even tiresome, I beg leave to direct the attention of my readers to the United States of America, where the occurrence of *frost* in winter presents the disease *in different circumstances,* and where the facts, regarding its origin, nature, and supposed contagious property, have been, within a few years, attentively observed, and also discussed with great ability, as well as nice discrimination.

Proceeding, then, from Jamaica northward, Charleston, in South Carolina, first offers itself to our observation. And here I gladly avail myself of a statement on this subject, made by Dr. David Ramsay, of that city, in his "Review of the Improvements, Progress, and State of Medicine, in the 18th century, already cited at p. 188. The statement in question is at p. 39, and in the following words:

"In the year 1699, a disease prevailed in Charleston, which swept off a great part of the inhabitants, and some whole families. This was then called the *plague,* though afterwards supposed to have been the Yellow Fever." *

In the year 1732, the Yellow Fever began to rage in May, and continued till September or October. In the height of the disorder, there were from eight to twelve whites buried in a day, besides people of colour. The ringing of bells was forbidden, and little or no business was done. † In the year 1739,

* This disease is mentioned at p. 142, of the first vol. of the History of South Carolina, (London, 1779) as having "carried off an incredible number of people;" among whom were the chief justice, the episcopal clergyman, the receiver-general, the provost marshal, "and almost half the members of the assembly." Indeed, the situation of Charleston, however convenient for trade and navigation, appears to have been, from the beginning, eminently productive of marsh fevers in summer and autumn. Governor Drayton, in his View of South Carolina, (p. 24) says, "at its first settlement, Charleston was said to be so unhealthy, in the autumnal months, that, from June to October, the public offices were shut up, and people retired to the country."

† Dr. John Moultrie, whose father was, during forty years, at the head of his profession in Charleston, and who, in 1749, published, at Edinburgh, an excellent inaugural Dissertation "de febre maligna biliosa Americæ, 4to. after mentioning therein, that this disease prevails most violently in proportion as the heat of the at-

the Yellow Fever raged nearly as violently as in the year 1732; and it was observed to *fall most severely on Europeans.* In 1745 and 1749, (rather 1748) it returned, but with less violence; however, many young people, *mostly Europeans, died of it.* It appeared again in a few cases, in 1753 and 1755, but did not spread. In all these visitations, it was generally supposed that the Yellow Fever was imported; and it was remarked that it *never spread in the country, though often carried there by infected persons, who died out of Charleston, after having caught the disease in it.*"

"For forty-two years after 1749, there was no epidemic attack of this disease, though there were occasionally, in different summers, a few sporadic cases of it. In the year 1792, a *new era* of the *Yellow Fever* commenced. It raged in this city in that year, and also in 1794, 1795, 1796, 1797, 1799, and 1800. In these last seven visitations of this disease, it extended from July to November, but was most rife in August and September; with a very few exceptions (chiefly children) it *exclusively* fell on *strangers* to the air of Charleston, and was, *in no instance, contagious.*"

mosphere is greatest, adds, "in *Caroli*-oppido exeunte mense Junio, anno, 1732, *cum nulla aura* per aliquot hebdomadas *æstum torrentem* refrigerasset, adeo sæviebat hæc febris, et tam acuta et lethalis erat, ut multis post diem 2m. vel 3m. mortifera esset." "Anno 1748, in eodem loco febris hæc iterum erupit, circa medium mensis Augusti, prima cujus septimana *nulla ibi unquam calidoir erat,* ut Mercurius in Fahrenhetii thermometro ad 97°, 97 1-2°, et 98°, in aere umbroso ascenderet, et calor hicce cum multis imbribus diu duravit," p. 8. He adds, at the top of the next page, that the atmosphere, sometime after, became cool, and the epidemic, from a *Yellow Fever, changed to an intermittent* "a cœli temperie in frigidiorem versa, mitescit et in intermittentem febrim mutabatur." He observes, in the same page, that though most people thought the fever contagious, he had seen many persons who maintained a close and daily intercourse with the sick, and did *not* get the disease, if they avoided violent exercise, and exposure to external injuries. He says, p. 6, that the North Americans pretend they derive the disease from the West Indies, but that the West Indians say it is not *indigenous* there. He thinks, however, that sufficient causes exist among both; with and among these *causes* he includes "*ingens æstus aeris,*" marsh effluvia, violent exercise, drinking to excess of ardent spirits, &c. He says, the epidemic of 1745 manifestly began from the *latter* cause, in a sailor, and not from any imported contagion.

This *uncontradicted* statement, publicly made by an eminent and respectable physician, in the hearing of hundreds, who, if it had been erroneous, and particularly if the Yellow Fever had manifested any contagious quality in South Carolina, must have been able, and disposed, to assert the truth, may well be considered as decisive evidence on the subject; especially as it was printed at the request of the Medical Society of that state. Dr. Ramsay had, indeed, previously stated this fact more circumstantially in his address to the same Society, on the 24th of December, 1799, when, speaking of the disease in question, as it had appeared there during the preceding summer and autumn, he says, " We have no reason to believe that the Yellow Fever was either imported among us, or communicated by contagion. It raged most in the north end of King Street, where the greatest number of persons from the country resided, and in those streets where sea-faring persons usually fixed themselves. No physician, or nurse, took the disease. Strangers, who left the city, and afterwards sickened and died in the country, were not the occasion of death, or even of disease, to those who attended them in their last illness." See New York Medical Repository, Vol. 4, p. 100.

Again, in the Charleston Medical Register, for 1802, Dr. Ramsay, alluding to the Yellow Fever, which had then recently terminated, declares that " no instance can be recollected in which there was any ground to suppose that the Yellow Fever was either imported, or had been contagious. No physician, nurse, or other person," having " intercourse with persons labouring under Yellow Fever, caught the disease. It was exclusively confined to strangers, and among them there was no evidence of its being communicated from one to another."

The like absence of a contagious quality continued to be manifested by this disease in succeeding years. Dr. Ramsay, in giving an account of the Yellow Fever, as it had appeared at Charleston in the summer and autumn of 1804, (in a letter addressed to Dr. Mitchell, and dated the 14th of December, 1804), writes as follows:—" A few cases of Yellow Fever occurred prior to the 10th of July; but, from that day till about the 20th of September, it might be said to be epidemic. From and after that time it gradually declined, and finally disappeared about the 1st of November." "The weather was uncommonly warm, while the epidemic raged, and the number and mortality of its subjects increased with the increase of *heat.* The disease was marked with the ordinary symptoms which have been so often described, and are so well known as to make a new statement unnecessary; but, in the following particulars, an unusual proportion of patients deviated from what had been the more common form of the disease in preceding years. *Neglected intermittents frequently terminated in Yellow Fever.* The *black vomit* was neither violent nor constant, even in fatal cases, *where the depleting system* was carried to a proper extent." "As usual, the disease was confined to persons who were strangers to the air of Charleston; but it attacked some who had resided among us one or two years, and, in a few cases, more." But in these there "has generally been a great proportion of *exciting causes,* such as intemperance, long exposure to the damps of night, or the scorching rays of the sun." This disease, in *no instance proved contagious.*" See New York Medical Repository, vol. 8, p. 365.

I shall conclude these statements, in regard to Charleston, by the following extract from Dr. Ramsay's Letter to Dr. Miller, dated the 18th of November, 1800, viz. "The disputes about the origin of the Yellow Fever, which have agitated the Northern States, have never existed in Charleston. There is but *one* opinion among the *physicans* and *inhabitants,* and that is, that the disease was *neither imported nor contagious.* This was the *unanimous sentiment* of the Medical Society, who, in pursuance of it, gave their opinion to the government last summer, that the rigid enforcement of the quarantine laws was by no means necessary, on account of the Yellow Fever." "My private opinion is, that the Yellow Fever is a *local* disease, originating in the air of Charleston." See New York Medical Repository, vol. 4, p. 218 and 219.

North Carolina having no large city, has been less frequently infested with the violent forms of marsh fever. But Dr. De Rosset, of Wilmington, in that state, has described, what he calls a pestilential fever, which prevailed there in the autumn of 1796, accompanied with yellowness of the eyes and skin; and "ultimately the *true black* vomit, as described by writers on the *Yellow Fever*." He describes Wilmington, as being much exposed to marsh effluvia, and the weather of that summer as having been unusually hot and dry subsequently to a very wet spring: Of this fever, he says, "I have no doubt, in my own mind, of its having originated among us, nay more, of its differing from our common bilious remittent but in degree; of its originating from the same causes, and being aggravated by the circumstances of the season." "I did not observe one instance of its being communicated by contagion; nor do I believe it was so." "A few cases every year, of our common *fall* fever, take on all the symptoms of a violent Yellow Fever." See New York Medical Repository, vol. 2, p. 143, 4.

In proceeding northward, our next object will be Norfolk, in Virginia, which, being a considerable port, and abounding in the sources of marsh miasmata, has, on several occasions, been severely attacked with Yellow Fever, as I have already noticed, at p. 191. Several of these attacks fell under the observation of M. Valentin, who landed in Virginia, with many other fugitives, from St. Domingo, in the summer of 1793, as is mentioned at p. 185; and this gentleman, after noticing the various attempts made at Philadelphia, to prove that the disease in question had been produced by importation, says, at p. 84. of his Treatise, "Nous avons vu la Maladie commencer à Norfolk, *sans qu'on ait pu en accuser aucun navire* recemment arrivé: les Medecins de ce lieu *n'ont même jamais eu cette opinion.*" In the next and following pages he observes, that the Yellow Fever never appears *there but* in those months when the air is extremely hot and sultry, with but little motion. That, in 1796, the summer was very wet, and the Yellow Fever only appeared sporadically; being little more than the common bilious remittent. But, in 1797, the drought was extreme at Norfolk, (and, consequently, the heat) during the whole of July, August, September, and October; and that the Yellow Fever then *raged furiously* as an epidemic, and with symptoms of unusual malignity, beginning about the end of August, and continuing until about the middle of November, when the weather, becoming cold for two or three days, the fever entirely disappeared, as it has invariably done in every part of the United States, soon after the occurrence of frost. Between pages 92 and 102, M. Valentin states a number of facts and reasons, proving the local origin of the disease at Norfolk, &c. and its having manifested no contagious property in circumstances where such property, had it existed, ought to have become evident. He describes the situation of *Water* Street, at Norfolk, and the composition of the new-made ground, as it is called, which serves as a foundation to the houses of that street, adding, that it is in that part of the town that he has constantly seen the greatest number of Yellow Fever patients, labouring under the disease in its *worst forms,* with hemorrhages, &c. See p. 101.

At p. 191 of this volume, I have inserted an *extract* from an account, given by Doctors Taylor and Hansford, of the Yellow Fever, as it prevailed at Norfolk, in the summer and autumn of the year 1800; and I will here subjoin extracts of another account, given of this disease at the same time and place, by Doctors Selden and Whitehead, two other physicians of Norfolk, viz. " Europeans and natives of the Northern States, who had not been accustomed to warm climates, were most exposed to the attack of the disease in its severest forms; those, from the same countries, who had resided here for some time, and strangers from this and the neighbouring states, were not exempt, but the disease (in them) put on a milder form; while those who were born in Norfolk, and were old residents of the place, *never enjoyed a greater portion of health,* in any former season, none of them died, or were even affected with the prevailing epidemic. This entire exemption of the *permanent* inhabitants of Norfolk, different from what was experienced in more northern parts of America, as Philadelphia, New-York, and Baltimore, may probably be accounted for, on the supposition that our situation and climate here, approach nearer to the circumstances of the West India islands, where strangers in general are the only persons attacked with Yellow Fever."

These gentlemen further observe, that, "for more than two months, subsequent to the 25th of June," (of that summer) "the inhabitants of Norfolk lived in an atmosphere constantly heated above the 85th degree of Fahrenheit's scale, and sometimes to the 94th and 95th degree, but very *frequently* above the 90th." That, "on the 5th of October, a deluge of rain fell, accompanied with a powerful sweeping wind from the North-East; the weather became suddenly very cool; the mercury fell to 48° on the morning of the 6th, and, on the 7th, it was as low as 42° of Fahrenheit. In a *few days after this, not a vestige* of Yellow Fever was to be seen in Norfolk." The same gentlemen add, "that part of the town, where the malignant fever chiefly prevailed, stands entirely on *made land*, reclaimed from the river by sinking pens of large logs, and filling them up chiefly with *green pine saplins*, which are slightly covered over with earth, or gravel. In some places large openings are left for the formation of docks; in others, wharves are formed next the channel of the river, while the interior parts are still covered with water, and, in many others, the lots remain in their original state." They also mention other sources of marsh effluvia, which acted upon, as they observe, "by the *powerful* rays of an almost vertical sun," must have been very sufficient to produce this disease, which, as they state, "for several weeks, after its commencement, *was quite local.*" See New-York Medical Repository, vol. 4. p. 129 and seq.

The same disease prevailed again at Norfolk in 1801, when the four physicians, before-mentioned, subscribed a declaration, dated the 12th of October, 1801, in these words, viz. "we do certify that the malignant Yellow Fever, which prevailed with violence for some time past, has now nearly ceased, and that the health of the town appears to be improving daily. *We know of no instance in which the disease has been communicated by contagion.*" Medical Repository, vol. 5, p. 225.

*Baltimore,* in Maryland, falls next under our observation. In this city, especially at *Fell's Point,* or East Baltimore, (which is greatly infested by marsh effluvia,) the Yellow Fever has several times prevailed, and with great mortality, since the year 1793. In a Treatise on this Disease, published in 1798, by Dr. Davidge, a physician of eminence in Baltimore, he states, that in the preceding year, 1797, "the bilious or remitting fever, in its ordinary form, prevailed in that town, and particularly at the *Point;*" and that this continued "until it was gradually *lost,* in the *severer degree of Yellow Fever,* as the season advanced in the *month of August:*" that, "from this time, until early in November, when it became entirely extinct, the Yellow Fever alone was observed; and it was obviously more severe, more early in its occurrence, and more general in its prevalence, in the direction of the winds which blew over certain marshes, stagnant waters, and depositions of filth." He, therefore, considers "intermittents, remittents, and Yellow Fever, as merely varieties of one disease," —asserting "that the Yellow Fever cannot be propagated by contagion, out of the sphere wherein it originated." See New-York Medical Repository, vol. 2. p. 83, 84.

Of the epidemic Yellow Fever which occurred at Baltimore, in the year 1800, the faculty of medicine of that city, in a report to the mayor, say, "after the most *scrutinizing* investigation, the faculty have found no proof, or even cause of suspicion, that the fever which lately so unhappily afflicted our city, was derived from foreign causes;" and, in support of this declaration, they give a particular account of thirteen cases, in which the disease first appeared, all of whom were in persons who had been exposed to marsh miasmata, but had not communicated with any vessel, " engaged in foreign commerce;" and " were attacked at such distances from each other as to preclude the probability of any one of them having derived it from the other." They proceed,—" the *gradual* manner in which this disease becomes epidemic, is an additional proof that it is not derived from foreign sources;" and after describing the milder cases which occur at the *beginning, before the causes acquire full force,* they add, " if this disease were imported, the prominent features would develope themselves at *first*, and these precursers, and more mild grades of the disease, could not affect thousands on shore, who never had any communication with vessels from the West Indies, or any diseased body." " The faculty believe the following to have been the principal sources of this late malignant fever," —First, " the *cove* which extends from the mouth of Jones's Falls to the interior of Fell's Point, the bottom of which was left bare, by the recess of the tide, for some weeks, immediately preceding the epidemic appearance of the fever. This was occasioned by the prevalence of north and east winds, which continued a great part of the summer." " Such is the situation of this pestilential cove, that all the filth conveyed into it by the west, north-west, and south winds, must remain to stagnate and putrefy under a summer's sun." " From the united testimony of the physicians at Fell's Point, the disease began on the *borders* of this *cove;* and its progress could be traced through the streets, in whatever direction the winds wafted its poisonous effluvia. Such was the pestilential condition of this sink of putrefaction, that the labourers, employed in filling up its northern shore, were compelled to relinquish their undertaking early in the summer."

" Second.—The docks, in general, but more especially the interstices between the wharves, where the water stagnated, and afforded a proper matrix for the generation of pestilential effluvia."

They afterwards mention several other causes, of more limited operation, such "as stagnant water retained in cellars," "ponds, and low grounds in the city, and its vicinity;" —and, finally, "the *made grounds,* of which the wharves, and the lower parts of some of the streets, are formed," and then conclude,—"from these sources we derive the first cases of the late fever, and, from these, fomented by the summer's sun, we believe it to have become afterwards epidemic. We are more strongly impelled to ascribe our late malignant fever to these causes, from having *ascertained* that it did *not exist* in the *higher parts* of the city, remote from exhalation, unless it had been carried there from the *Point,* or from the lower parts of the city."

The course I have taken leads us next to Wilmington, on the river of Delaware, situated on a spot which gradually rises to the height of 109 feet, as it recedes from the river of Delaware, between which, and the town, is an extensive marshy low flat, more than a mile wide. On two other sides, it is bounded by two large streams of water, called *Christiana,* and *Brandy-wine* creeks; and by the side of the first is an extensive marsh, which, in the spring of the year 1798, was drained for the first time, so that a surface of 100 acres of *mud* were in the following summer, exposed to the sun, which produced very offensive exhalations; and, in consequence of these, the Yellow Fever, with its most violent symptoms, became generally prevalent in September of that year, in the lower parts of the town, and occasioned the deaths of above 200 persons in that quarter; whilst the more elevated parts were almost wholly exempt from the disease. Dr. John Vaughan, who has given a minute account of this fever, in the 3d volume of the New-York Medical Repository, at p. 368 and seq. and who had previously believed the Yellow Fever to be a foreign and a contagious disease, was induced, by the facts which then fell under his observation, to adopt the contrary opinion. Here was abundant evidence to prove that the disease had arisen solely from the noxious exhalations to which the lower part of the town was particularly exposed;

and he could not discover a single instance in which the fever had been communicated to any person who had not been within the reach of these exhalations.

This fever again recurred at Wilmington, in September, 1802, subsequently to an interval of *very hot* weather, which began about the middle of August, and in which the temperature varied from 80° to 96° of Fahrenheit's thermometer, as is stated by Dr. Vaughan, in his "Concise History" of this fever, printed at Wilmington, in 1803: according to his statement, "the sources of noxious effluvia in the southern and *flat* part of the town were much increased by a regulation, but partially executed, for bringing the streets to an uniform descent from the summit of the hill. A number of cellars were filled with water; a new dock formed, and the gutters lowered in some places, and raised in others, forming numerous depositories of filth." "The fogs (says he) collected in the evenings, were suspended on the flats during the nights." "This *semi circuit of the fogs*, from Market Street southward and eastward, was *the seat of concentrated disease*."—"The poisonous matter exciting disease, was evidently a constituent part of the fogs. Many persons visited the infected district, in clear weather, and in the day-time, without injury; and several of the same persons contracted disease by a *single exposure* in the *night* time, after the fog had collected." "The *non-contagious* nature of the disease was repeatedly attested, by persons sickening after removal, from the lower to the higher parts of the town, and being nursed with every attention, and dying without communicating the malady to their attendants." He adds, "the *indigenous* nature of the disease was evidently characterised, by the *ultimate sameness of every form and grade of fever*. After the middle of September, the *subordinate* forms and grades of fever, not arrested within 48 or 72 hours, invariably *passed on to the malignant grade* of the disease." There was a final termination of its progress "by a *single frost*."

Philadelphia falls next under our observation. This city was originally intended to occupy the *flat* space of ground between the Delaware and Schuylkill Rivers, a few miles above their junction; it has, however, been found more convenient to extend it to a greater distance along the west bank of the Delaware, under which bank, upon a low space of ground, originally intended as a cartway to the wooden wharves which *abut* and encroach upon the bed of the river, an extensive street, called Water-Street, has been formed, extending from the northern line of the city, southward to the swampy ground given by the founder, William Penn, to be formed into a dock, which, not being done, and the exhalations from this swamp proving noxious to the inhabitants, it has been arched over, and covered with earth, so as to become the foundation of a street, called Dock Street, near which an offensive sewer empties itself into the Delaware.

The ground between the southern extremity of the city, or rather of the suburb, called Southwark, and the junction of the Delaware and Schuylkill, is generally very low, rich, and damp, I might say swampy in many places; and, apparently, is as well suited to produce marsh miasmata as any part of Zealand, and with *greater morbific* powers, because the summers at Philadelphia are much hotter. On the northern extremity also, in and about the suberb called Kensington, are low swampy grounds, of considerable extent; and a great part of the city itself stands upon a *loamy*, or brick earth, which is very impenetrable to water. Thus situated, it can hardly be necessary to observe, that Philadelphia has been frequently infested by marsh fevers, though it is not known that, previous to the year 1793, they assumed *epidemically* the violent form of Yellow Fever, except in the years 1699, 1741, 1747, and 1762.* The great mortality which the Yellow

* Dr. Rush, in the 4th vol. of his Medical Inquiries, &c. p. 69, refers to a clinical lecture, delivered December 3d, 1766, at the Pennsylvania Hospital, by the late Dr. Thomas Bond, and preserved by order of the managers, in the 3d vol. of their Minutes, in which he declares, "that he had seen the Yellow Fever *five* times

Fever produced in 1793 at Philadelphia, is well known. From the great numbers attacked by it, at that time, and the influence of pre-existent opinions on the subject of contagion, the physicians, as well as the inhabitants, seem to have immediately, without enquiry or consideration, concluded that it must be a contagious disease, and most of them inferred, as a natural consequence, that it had been imported from some part of the West Indies; though there was much disagreement in regard to the particular vessels, chargeable with this importation, as well as in regard to the places whence it had been brought;—some deriving the evil from Grenada, others from different ports of St. Domingo, &c. On that occasion Dr. Rush, dissatisfied with the contradictory evidence about the importation of the disease, though he admitted it to be contagious, and, consequently, capable of importation, thought he had discovered a more probable cause for its production in a cargo of damaged or putrefying coffee. Thus overlooking the influence of marsh miasmata, which ought, at least, to have been strongly suspected, considering that the fever *began* in *Water Street*, and afterwards appeared in houses adjoining other swampy grounds near Dock Street, Kensington, and Southwark; and considering, also, that it resembled a marsh fever in almost every circumstance, especially in the *season* at which it appeared; in the evident remissions with which it was attended; in the circumstance of its leaving unhurt all the French fugitives from the West Indies, and others who had long resided in hot climates; and, in its cessation immediately after frost.

At the commencement of this disease, M. *Devese*, late Surgeon-General of the troops, in the northern division of St. Domingo, who had long practised at Cape Français, having escaped from the ruins of that city, reached Philadelphia, and though the Physicians there had generally declared the fever

in Philadelphia. The second time it was indigenous, from *evident* causes, and was confined to *one square* of the city." Upon this Dr. Rush remarks, that "the locality of this fever designates its putrid origin," i. e. from marsh effluvia.

to be a contagious disease, he honestly and courageously maintained the contrary, and ascribed it, exclusively, to local causes, probably without making any converts to his opinion at that time; but the committee of superintendance, and the physicians, willingly availed themselves of his experience in the treatment of the disease, so far as to intrust him with the direction of the spacious Hospital at *Bush Hill*, which had been just established for patients under the Yellow Fever; and in which his practice was attended with great success, perhaps, in some degree, from its salubrious situation. In the following year, M. Devese published, at Philadelphia, a Dissertation on the Yellow Fever of the preceding year, (in that city) stating his reasons for believing the disease to have been neither imported nor contagious; which, though not so conclusive as we can now give, probably helped to induce some, at least, of the physicians there to inquire and think, with greater freedom, on the subject. For in 1796, Dr. Rush, when giving an account of this fever, as it occurred in 1794, (though much less extensively than in 1793) would not admit *contagion* to be one of its *characteristic marks;* deeming this quality to be rather *accidental*, and to depend on circumstances of season, country, &c.; and, on this occasion, he apparently, for the first time, ascribed the fever to exhalations from gutters, and stagnant ponds of water, in the neighbourhood of the city; having observed, that "where there was most exhalation, there were most persons affected by the fever." See his Medical Inquiries and Observations, vol. 4. p. 63.

In the month of November, 1797, Dr. Rush, Dr. Caldwell, and *eleven* other physicians, of Philadelphia, whose opinions, respecting the Yellow Fever, were now at variance with those of a majority of the College of Physicians there, being called upon by the governor of Pennsylvania to state, for the information of its legislature, the results of their researches and experience, respecting the "origin, progress, and nature," of the epidemic Yellow Fever, which had then just terminated, (on the occurrence of frost,) these gentlemen, in their answer, say, " we conceive the fever, which has lately prevailed in our city, commonly called the Yellow Fever, to be the bilious remitting fever of warm climates, excited to a higher degree of malignity by circumstances to be mentioned hereafter ;" and they allege the following, among other reasons, for this belief, viz :

1st. That both fevers have a similar origin.

2d. That the Yellow Fever occurs in " those months chiefly in which the bilious fever usually prevails, and is uniformly checked and destroyed by the same causes, viz. heavy rains and frosts."

3d. That " the symptoms of the bilious and Yellow Fevers are the same in their *nature*, and differ only in degree."

4. That the common bilious and Yellow Fevers often run into each other, &c.

5. But after having made these approaches to the truth, they state, as another reason, that " the common bilious and Yellow Fevers are *alike contagious, under certain circumstances* of the weather, and of predisposition in the body :"* Not suspecting it to be impossible that contagion should ever be either acquired or lost by the presence or absence of such circumstances. In the following year, the same physicians and others, being then incorporated as " the Academy of Medi-

* It deserves here to be remarked, that while Dr Rush and his associates thus erroneously represented the *common bilious*, as well as the *Yellow Fever*, to be *sometimes* contagious, the College of Physicians, in their answer to the governor of Pennsylvania, dated December 5, 1797, state, as their *principal* reason for believing the two diseases to be *essentially different*, " that a malignant remittent fever has never been, to our knowledge, contagious in this climate." They had, immediately before, admitted the occasional existence of " solitary cases of malignant remittent fevers, *the symptoms of which resemble so much the disease in question*, (Yellow Fever) *that they are very often supposed to be the same*." Hence we see, that to raise up a *baseless* distinction between these fevers, the College, after admitting one to be void of contagion, assume the other to *possess* that property, though without *proof*, and at a time when, except among the physicians in Philadelphia, there was not one in twenty of those who had seen the disease in other parts of the United States, that believed what was thus assumed.

cine of Philadelphia," were again called upon by the governor, in regard to the Yellow Fever of the preceding summer and autumn, (of which 3648 persons were computed to have died at Philadelphia, exclusive of many who fled and died in the country,) and they declared, in their letter, dated December 3d, 1798, "that the disease is not contagious in the West Indies, and *rarely, if ever, so in the United States,* in *hot* weather, at which time *only* it makes its *first* appearance in our country. So general (they add) is this opinion, that some physicians have unfortunately refused to admit the existence of the fever in its commencement in our city, *only* because it was not contagious." Here was a farther approximation to truth, made by these gentlemen; and it is probable that they would have *then* completely renounced all belief of the supposed contagion of Yellow Fever, had not their doing so been opposed by Dr. Rush, whose mind had been so strongly biassed in favor of that belief, that he could not relinquish the notion of an *occasional* or *accidental* occurrence of that quality until very many proofs, in opposition to it, and those of the most decisive nature, had been presented to his consideration. At length, however, he obtained the fullest conviction on the subject, and announced it by a long statement of "facts, intended to prove the Yellow Fever not to be contagious," &c. "In a letter to Dr. Edward Miller," published in the New York Medical Repository, vol. 6, p. 135 to 150. Towards the conclusion of this statement are the following passages, viz:

"You will perceive, from the facts and reasonings contained in this letter, that I have relinquished the opinion published in my account of the Yellow Yever, in the years 1793, 1794, and 1797, respecting its contagious nature. I was *misled* by Dr. Lining, and several West India writers," &c.——"I am aware of the *influence which such changes in medical opinions,* as I have acknowledged, have, upon *a physician's reputation;* but *small,* indeed, should I consider the *total sacrifice of mine,* could it avert the *evils* which are connected with a belief in the *importation* of pestilential diseases," &c. Not content with having done this, Dr. Rush, in the preface to a subsequent edition of his Medical Inquiries and Observations, made the following *declaration*, and caused it, moreover, to be inserted in the Medical and Physical Journal, No. 85, and in other periodical works, viz.

"In the 4th volume the reader will find a retraction of the author's former opinion of the Yellow Fever spreading by contagion. He begs *forgiveness* of the friends of science and humanity, if the publication of that opinion has had any influence in increasing the *misery* and *mortality* attendant upon that disease. Indeed, such is the *pain he feels, in recollecting that he ever entertained or propagated it*, that it will *long*, and, perhaps, *always deprive him of the pleasure* he might otherwise have derived, from a review of his attempts to fulfil the public duties of his situation."

And here I must observe, that if the *conviction* of any one man can reasonably influence the opinions of others, on this subject, the preceding declaration, by Dr. Rush, ought to produce that effect; made as it was, not precipitately or capriciously, but with slow and cautious deliberations; not from the impulse of former prejudice, but in direct opposition to it; not from a vain desire of appearing to be infallible, by vindicating opinions inconsiderately promulgated, but with a *conscientious* and humble, (I had almost said humiliating) purpose of condemning and renouncing such opinions, and of atoning for them, if necessary, even by the sacrifice of his reputation, at the shrine of truth. I flatter myself, however, that no loss of reputation has been incurred by this proceeding. To me Dr. Rush appears as being more *estimable* after this *honest* avowal of an error, than he would have been, had he never fallen into it: and I earnestly hope, that in similar circumstances, I should, most willingly, follow his example.

When Dr. Rush thus denied the supposed contagion of Yellow Fever, he had probably seen more of that disease than any other physician in any country; and if the many thousands of cases of it, which fell under his immediate observation, and the still greater number of which he was doubtless informed, not only did not afford any such evidence of contagion, as, with the aid of his prepossessions, could maintain him in his former belief on that subject, but, on the contrary, manifested so unequivocally a total absence of any contagious quality, as, in spite of these prepossessions to impel him to make the declaration before-mentioned, who that supposes Dr. Rush to have possessed common intellect and discernment, will believe the Yellow Fever to be contagious; unless he *knows* it to be so by unquestionable *facts* within his own observation, and of the *most decisive import*. Facts, such as I believe to have never existed, in regard to this disease.

Numerous statements, concerning the origin, progress, and nature of the Yellow Fever, at Philadelphia, in different years, are now before me; all agreeing in the most important points with those which I have recently introduced, concerning its appearance at Charleston, Norfolk, Baltimore, &c. and, therefore, lest I should exhaust the patience of my readers, I will only select a few passages, from a lecture, introductory to a course of clinical lectures, delivered at the Infirmary of the Public Alms House, in Philadelphia, by Charles Caldwell, M. D. respecting the Yellow Fever, as it occurred *there*, in 1803.

After various preliminary observations, together with an account of the very sultry, humid atmosphere, which prevailed at Philadelphia during the month of July, and excited many distressing apprehensions, (the thermometer commonly indicating a temperature between 80° and 90°) Dr. Caldwell mentions two cases of disease, which appeared on the 19th of that month, "in adjoining houses at the corner of Chesnut and *Water* Streets; and were but too well calculated to realize and confirm these melancholy anticipations." These cases were those of two "females, under the age of sixteen years; one of whom died on the 5th day of her illness, and the other recovered; but they both exhibited unequivocal symptoms of malignant fever.* It is worthy of remark, that, adjoining to the houses where these persons resided, were a yard and private alley, containing stagnant water and putrid substances, which, for a week or ten days previously, had emitted a smell highly offensive to the neighbourhood. The families where the sickness occurred, did not hesitate to attribute their misfortune to this insufferable stench." "From the 28th of July, till the 5th or 6th of August, four other cases of malignant fever appeared in the same neighbourhood, three of which terminated fatally." These persons had not "the slightest intercourse with each other, nor with any *common source*, except the atmosphere of the place where they resided. A knowledge of this induced most of our citizens to consider this disease as nothing else than a *high* grade of autumnal fever, or, what was afterwards very emphatically denominated the *Water Street fever*." Another "very decided case (of malignant fever) made its appearance in the person of Mrs. Cole, on the 23d, and terminated, in death, on the 27th of July." She "resided in *Water*, near South Street, upwards of a quarter of a mile from the former situation, and had not been out of her own neighbourhood for several weeks previously to her illness."

"There existed another point where malignant fever threatened at the same time to attack us. This was in *Water*, near *Race* Street, about a quarter of a mile in an opposite direction from Chesnut Street." "Mr. Jolly, of that neighbourhood, sickened on the 28th, and died on the 31st of July, with symptoms of high malignity. Between the 1st and 10th of August, six other persons, in the same neighbourhood, but who had no intercourse with each other, were attacked by the disease, one of whom died on the 4th day of his illness: the others all recovered." In none of these cases was the fever communicated to any other person.

* It will, doubtless, have been already observed, the terms *Yellow Fever,* and *Malignant Fever,* are used by the physicians of the United States as *synonymous.*

" As yet all parts of Philadelphia, except *Water Street*, which must be regarded as the *low ground* of the river Delaware, enjoyed an unusual exemption from disease."* " On the 12th of August we had a heavy fall of rain, which was succeeded by a remarkable change in the temperature of the atmosphere, the mercury sinking ten degrees," by a prevalence of northerly winds for ten days, during which " the city remained free from any further cases of malignant disease."

" On the 23d of August the wind shifted to the southward, and the atmosphere became humid, warm, and oppressive. This change was viewed by many as a precursor of further sickness." " On the morning of the 25th, a dreadful fire broke out in *Water*, near Market, *Street*," and " drew together a vast concourse of people. Of these some were engaged in violent exercise, while others were standing idle in the streets, on the tops of houses, or at windows, many of them only partially dressed." This occurrence " was well calculated to act as the *exciting* cause of disease; accordingly, in the course of two or three succeeding days, eight or nine persons, immediately adjacent to where the fire had raged, were attacked by malignant fever." They had all " been more or less exposed at the fire," without having " previously had any mutual intercourse." " The disease appeared suddenly, and nearly at once; in five or six families, the individuals of which had never exchanged a visit, or even a word, either business or ceremony. From this time, the number of sick continued daily to increase." " But this was not all. During the first week of September, the disease renewed its attack, *in all* those neighbourhoods where it had made its appearance in preceding parts of the season. In-

* Here Dr. Caldwell remarks, that, in the autumn of the summer in question, " the *low grounds* of most large rivers in the United States, were subject to *malignant* fever. Along the banks of the *Susquehannah*, the ravages of this disease were melancholy and unprecedented. In some instances it hurried whole families to the grave."

deed, there were now scattering cases of it in most parts of *Water* Street, between Race and Almod Streets; a distance of somewhat more than a mile. But the district, extending from Market to Walnut Street, and from the east side of Front Street to the river Delaware, constituted the principal theatre of its ravages. With such violence did it rage within these limits, that, on the 12th of the month, the Board of Health thought it right to advise the inhabitants to remove, and to interdict all unnecessary intercourse with the sickly neighbourhood."

"Throughout the remainder of the month of September, and till near the close of the first week in October, the disease continued, by feeble efforts, to advance slowly from the *low ground of the river*, towards the more elevated parts of the city. Except, however, in alleys, and other filthy places, inhabited by the poor, it did not make its way across *Second* Street, nor did it, save in a few places, advance even so far. It may be confidently asserted, that that portion of Philadelphia, which lies to the westward of Second Street, never enjoyed a higher exemption from disease, than during the late season. As the fever receded from the low ground, and malignant atmosphere of *Water Street*, it became more and more *mild*, and manageable, till its evanescent shades in Second Street were, in many instances, much lighter than the common remittent of the country."

"After the 10th of October, the disease was no longer spoken of as a thing dangerous or alarming; and, before the 20th, there was scarcely a case of it existing in the city."

"As it has never been alleged by any one, that the malignant fever of last season was introduced into Philadelphia from the West Indies, through the channels of commerce," &c. those who considered it as an evil necessarily of *extraneous birth*, looked to New York *alone*, as the immediate source of our misfortune."

"Though facts were daily occurring to convince them, that the disease could not, by any mode of communication, be transplanted from Water Street even to *Third* Street," (i. e. across two parallel streets only) "they still contended, that it had been conveyed from New York to this place, either by land, by water, or perhaps on the wings of the easterly wind." To shew the absurdity of this Dr. Caldwell observes, that this fever first "appeared in New York on the 17th, and in Philadelphia on 19th of July;" and that "contagion could not possibly be conveyed thence to Philadelphia, and there *communicated so as to produce its effects*, in the short space of *two* days;" and, moreover, that "it has been clearly proved, as far as a negative proposition is capable of proof, that the late fever of New York could not be propagated even in Newark, Brunswick, Amboy, nor any of the neighbouring towns or villages, though thousands of the citizens removed thither, *many of them actually labouring under the disease.* How then," says he, "could it be conveyed *ten times the distance* by one *person in health.*" Other cogent reasons are joined to these, which I omit as being superfluous. I have been induced to prefer this account of the Yellow Fever of 1803, in Philadelphia, (extracted from the New York Medical Repository, vol. 1, second Hexade, p. 143 and seq.) principally because the early interruption, which it received by a diminution of the temperature of the atmosphere, rendered its progress slower, and its extension more limited, than usual, and thereby afforded opportunities of distinctly observing, and circumstantially describing, the facts, regarding its commencement and propagation, which is often impossible, in more rapid, violent, and extensive epidemics.

Next after Philadelphia, the city of New York claims our attention.* Many sources of marsh miasmata appear to

* An accurate description of the local circumstances of this city is perfixed to Dr. Miller's report to the governor of the *state*, "on the malignant disease which prevailed in the city of New York in the autumn of 1805." It is in these words,—

"The city of New York lies in lat. 40° 42. 8. N. and long. 74° 9. 45. W.; at the confluence of the River Hudson, and of Long-Island Sound, or the East River; and on the southern and narrow extremity of Mahattan Island, which is about fifhave been created and annexed to this city, subsequently to the peace and independence of the United States, in 1783. It was, however, long since, and, probably, from its beginning, very frequently troubled with intermittent, and what were called bilious remittent fevers; probably sometimes aggravated, by great summer heat, into the violence and mortality of Yellow Fever: though, by the want, or negligence of medical writers, we have no accurate account of them. It may, I think, be presumed, that an epidemic of this kind occurred at New York, in the months of *August* and *September*, 1702; mention being made of one which in those months (when no other fever is likely to have prevailed epidemically) killed about seventy persons weekly, by George Keith, in the account of his travels from New Hampshire to Curituck: (printed in London, 1706.) He had then become a clergyman of the Church of England, after having been a Quaker; and appears to have preached on the 30th of September, at a *fast* appointed by the governor, on account of the recent great mortality which then amounted to 500 persons, a very

teen miles in length, and from one to two in breadth. The site of the city, as it originally stood, was very irregular, being broken into hills and declivities, and *indented with small rivulets or creeks, skirted with marsh.* Many of the hills are levelled; but *the marshy grounds, though covered with houses and pavement, are still low and moist.* The city is about twenty-seven miles from the ocean, and is washed on both sides with water of great depth, whose current is very rapid, whose tide ebbs and flows about six feet, and which is nearly as salt as that of the neighbouring sea. On both sides of the city *considerable encroachments* have been made on the water, by *artificial ground,* the whole extent of which may be computed at not less than 132 acres. Of this, *ninety* acres lie along the *East* River, and forty-two along the Hudson. *The portion of it on the East River* forms *that part* of the city where malignant fevers have *always first become epidemic,* and *chiefly prevailed.** The wharves and docks are constructed of *logs* and loose stones. All the fresh water used by the inhabitants, is procured from wells within the city, and is now become extremely impure. The population of New York may be estimated at about 76,000.

* Dr. Miller explains this in another place, by observing, that the "*made* ground on the *North*, or *Hudson's River*, is much less extensive, and the materials composing it much less foul and corrupt, than that on the *East* River;" and that "the miasmata come to *maturity* on the one side, two or three weeks *sooner* than on the other."

considerable proportion of the inhabitants at that period! Overlooking the intermediate space of time until 1791, we find, that a considerable number of cases of Yellow Fever occurred in the autumn of that year, in a part of *Water Street*, near *Peck Slip*, then noted for the filthy state of the neighbouring docks. Others occurred to a greater extent in 1794; but were confined to persons who either lived, or were commonly employed near the slips, wharves, and other obvious sources of marsh miasmata; and this was the case in the three following years. In 1798, the disease prevailed more violently and extensively as an epidemic, and was computed by Mr. Hardie, in his plain and circumstantial account of it, (printed in 8vo. at New York, 1799,) to have occasioned the deaths of 2086 persons in that city. The committee, appointed by the Medical Society of the State of New York, to inquire into the symptoms, origin, &c. of this disease, appear, by their printed report, to have been convinced of its domestic origin, from local causes: and they declare, as the result of their experience, that "it is not a contagious or *catching* disease," in the popular and common acceptation of the phrase; that it is not communicable from person to person," &c. See New York Medical Repository, vol. 3, p. 293.

During the summer and autumn of 1800, in the neighbourhood of *Water Street*, and of the different slips, and other acknowledged sources of miasmata, one hundred or more cases of *Yellow Fever* occurred *simultaneously and intermixed*, in the *same situations*, with intermitting and bilious remitting fevers: of the former, more than fifty terminated fatally, in a few days; many of them "exhibiting the symptoms of yellow skin, *black vomiting*, and stools, Hemorrhages, &c." On this occasion it was not pretended that any importation of the disease had been made, nor that it had been propagated by contagion, "beyond the limits of that portion of the atmosphere of the town, allowed by every body to have been contaminated by the exhalations of putrefaction; and within such limits it is well known that an adequate cause is constantly in operation, independently of contagion." See New-York Medical Repository, vol. 4, p. 207-8. About twice as many cases of Yellow Fever occurred in the following year at New-York in the same months, and in nearly the same situations, with similar results.

In the summer and autumn of 1803, this disease recurred more extensively, and with greater violence, in consequence of the excessive hot weather which began early in July, and, excepting five or six days in that month, (say Doctors Mitchell and Miller,) was more intensely and uniformly hot, than we ever remember before to have experienced in this climate, for the space of some time." "The first public alarm took place from some deaths about the Coffee-House *slip*, and in that neighbourhood, where, from the number and malignity of the cases, the atmosphere must have been charged with miasmata of great virulence." "The streets lying near the *margins* of the two rivers, and those inhabited by the poor," &c. "suffered the principal ravages of the disease." A large portion of the sick "consisted of instances in which *one* individual *only* was attacked in the midst of a family, the members of which assiduously attended the patient, without contracting the disease. Many *aged, and very young* persons, whose condition imposed confinement in their houses, without the occurrence of any preceding case in their families, were attacked with the disease in its most virulent form. Multitudes, also, took the disease, who had not previously approached any sick person, any suspected vessel, or any families alleged to be imbued with contagion. One person was attacked in the Debtor's Prison, who, for three months before, had not been beyond its walls, and no other person in the prison was previously, or subsequently, affected with the disease; many who fled from the city, were attacked with the disease and died, not only in all the surrounding country, but at Newark, Elizabeth town, Brunswick, &c. without communicating infection, in a single instance, to physicians, nurses, or any other attendants. But, they add, one of the most decisive proofs of the non-contagiousness of Yellow Fever, is derived from the absence of all contagious influence from our Yellow Fever Hospitals." "These asylums are generally erected within two or three miles of the cities to which they belong, but entirely beyond the reach of an atmosphere contaminated by the local miasmata of the city. Neither in New-York nor in Philadelphia, is there a *single* example of a person employed in these Yellow Fever Hospitals, being attacked by that disease, unless he had previously passed sometime within the limits of the sickly city." See New-York Medical Repository, vol. 7, p. 183-4.

In a letter from Dr. Miller, the resident physician for the city of New-York, to his excellency Governor Clinton, dated January 6, 1804, the preceding facts are more minutely stated, with the others, proving, as far as the subject was susceptible of negative proof, that the fever in question did not arise from any foreign or imported contagion. Of the mortality produced by this fever, from its commencement, about the 20th of July, till its cessation, at the end of October, Dr. Miller says, "the number of deaths in this city amounted to 503; those at the Hospital at Bellevue, to 103, and those at the Marine Hospital, in Staten island, to 68; making a total of 674: to which should be added an indefinite number, who fled from the city, and died of this disease in the neighbouring country and villages." See printed "documents, relating to the Board of Health." (New-York, 1806, 8vo.) p. 35.

The summer of 1804 was but very moderately warm along the coast of America, northward of Carolina, and, as might be expected, the Yellow Fever did not recur at New-York, &c. in that year. But the summer of 1805 was, according to Dr. Miller's *official* statement,* "remarkable for the *duration*, as well as the *intensity*, of heat, along the whole of our coast (United States); and the consequence was, not only that

* In his *masterly* report, made as resident-physician for the city of New-York, to the governor of the state, dated January 12, 1806, and printed with "documents relating to the Board of Health," 8vo.

nearly all the Atlantic cities were visited with pestilence, but that, in several of them, it made its appearance in forty-eight hours, or nearly of the *same time*, an occurrence which cannot be explained on the contingency of contagion." As usual, sporadic cases of the disease first occurred: but, at the beginning of September, these, according to Dr. Miller's official report, "had become so numerous as to ascertain the existence of the epidemic;" which, throughout September and October, continued to prevail with more or less severity, according to the fluctuating states of the weather; but, towards the close of the latter month, the coldness of the season had evidently checked its progress, and, at the beginning of November, the city was nearly restored to its usual health.

From returns, authenticated by the city inspector, (Mr. Pintard) it appears that the number of inhabitants then amounted to 75,770, and that of these 26,996 retired from the city: and, as may be presumed, from the most *unwholesome* parts of it; by which the number of persons attacked by the disease was doubtless much diminished: as it only amounted to 645, and that of the deaths to 302: Of these twenty-eight were in the Marine Hospital, and fifty-two in that of Bellevue: and about forty others are supposed to have sickened and died in the country, after their flight.

Dr. Miller, in his report to the Governor, states that, "during the early period of the epidemic, nearly all the cases took place" (as formerly) "on the *eastern side of the city*, in Front, Water, and Pearl Streets, and principally below Burling Slip:" But "afterwards became more diffused:" and, "about the 20th of September, began to prevail near the north river:"* Where, from circumstances already mentioned, he

* Party zeal and prejudice often render men incapable of deriving any evidence or information from facts, but such as suit their own views and purposes. Dr. Chisholm, at page 205 of his *letter* to Dr Haygarth, introduces *one* to himself, from Dr. Hosack, dated New-York, July 9th, 1808, in which there is an abundant display of zeal, I do not say for maintaining the cause of truth, but for asserting the supposed contagion and importation of Yellow Fever: He also communicates his intention to *write* and *print*

supposes that "the miasmata come to maturity" later than on the east side. He adds, "on the whole, the *low grounds*, on the margins of the two rivers, certainly produced a chief part of the cases." P. 46.

a letter, charging Dr. Miller with "*want of candour*," in his official report to the governor; which charge he founds upon the following statement, viz.

"As a member of the Board of Health, he (Dr. Miller) must have known that the disease was confined for many weeks to a *small portion* of the *eastern side* of the city, and that, not a case occurred in any other part of the town, that was not referable to *that*, as its source. Such was the statement of the Board of Health, to our citizens, and, in consequence of which, they forbade intercourse with the infected portion of our city, and ordered an abandonment of that part of the town, &c"

He adds, "a few weeks after, the infection extended a few streets further. The Board of Health accurately defined its limits, and again declared, that still not a case occurred but could be traced to this *spot* of the city, as its source. Dr. Miller carefully enumerates the cases occurring, and the numerous parts of the city in which the sick reside, but as carefully *suppresses* the *observation* of the Board, of which, too, he was a member, and must have known, that the persons so taken sick, had, prior to their attack, been exposed to the infection by frequenting the infected spot." How strangely Dr. Hosack *here* mistakes the obvious import or evidence of an important fact? One of the strongest proofs of the *local* origin of Yellow Fever, results from the circumstance of its *beginning*, and remaining, almost exclusively, in particular spots or situations. Of this, Dr. Miller was very sensible; and, if he omitted to state "the observation of the Board," respecting it, he could only have done so because it appeared *superfluous*, after he had so distinctly mentioned the facts to which it related. And is it, then, possible, that Drs. Hosack and Chisholm can have been so inept as to believe that these facts could operate in supporting their opinion of the importation and contagious nature of this disease? Do they conceive, that if it were contagious, it would have been so many weeks confined to *one spot*, and that, when it afterwards "extended a few streets further," those only would have been attacked by it, who had *visited* that identical spot? This is exactly what would happen in regard to a disease not contagious, but arising from miasmata; because, the soil in which they are produced being *immovable*, and its exhalations incapable of causing disease, at any considerable distance from their *source*, persons to be acted upon by them must necessarily approach that source. But contagion having no such immoveable origin will not be thus confined; persons infected by it, and sickening in different places, naturally infect others, who soon spread the disease widely, so that the spot where it first appeared often becomes less dangerous than most others, and its atmosphere does not continue to produce disease, when the sick have been removed, and the houses shut up, as happens in cases of yellow fever. With the same *fatuity*, Dr. Chisholm, in his letter to Dr. Haygarth, has published one from Dr. A. Fothergill, late of Bath, who, writing to the American consul, at Bristol, of the Yellow Fever at Philadelphia, in 1805, and mentioning that it had, in November, received a *check*, apparently from the *cooler season setting* in;" adds, "It remained for some weeks a *local disease* in the southern suburbs," (adjacent to the

Dr. Miller *adds*, at p. 53, "It appears, from the records of this epidemic, that there were *thirty-one* streets of the city, most of which continued to be crowded with inhabitants, in which *only a single case* occurred in each; and, in the mass of six hundred cases, reported to the Board of Health, there were only *thirty-five* houses in which more than a *single case* was found." "The great mass of persons attacked with the disease, consisted of such as never had approached the sick, or any other assignable source of contagion; and, on the contrary, as will presently appear, great numbers were exposed to close intercourse with the sick, without injury."

Further, in regard to this epidemic, the health officer, Dr. Rodgers, made a long, minute, and satisfactory report to the Board of Health, (dated December 19th, 1805) of every vessel, and of every circumstance, connected with the *possibility* of an *importation* of the disease, which is concluded in these words. "I have now clearly shewn, as far as *negative* proof can go, that whatever might have been the cause of the late epidemic, it did not arise from any neglect of duty at the *quarantine* ground, *nor did it come through that channel.*"* Pre-

*marshy low grounds*, at the confluence of the Delaware and Schuylkill) "but, at length was communicated to several of the principal streets in the city, as far as Eighth street, westward," (persons who had been exposed to miasmata in other places, happening to fall sick in these) "but it chiefly *infested Water* street, *Front* street," (running parallel with, and next to *Water* street) "*and the margin of the Delaware.*" He adds, "many of the professors and medical practitioners here, *deny* that the disease is contagious, and in this *notion* the body of merchants bear them out." Dr. Fothergill, however, adheres to the *old notion* of contagion and importation, and no wonder that it should have adherents, in men who fancy they can see evidence of a contagious quality, in facts which decidedly prove the contrary, to all who are able to reason with impartiality.

In the letter first mentioned, Dr. Hosack invited Dr. Chisholm to "visit the United States," adding, "you would find materials for giving a *final blow* to your enemies; three or four months residence *here* would be sufficient." Was this a sincere effusion of intolerant zeal, or did Dr. Hosack *intend* that his *invitation* should reach Dr. Chisholm on the *first* of April?

* One motive for this report appears to have been, that "*attempts* had, as is stated by Dr. Rodgers, (p. 19) been made by some, to prejudice the public mind against him, because he," (who had had the best opportunities of observing and judging of the facts)

vious to this conclusion, the health officer had given an account, *in detail,* of all the early cases of Yellow Fever, which came under his notice, in order that the board might judge whether the disease went from the quarantine ground to New York, or came from the city," to the Marine Hospital. Of these cases a great proportion occurred in "a particular neighbourhood in *Water Street;* which neighbourhood did not exceed two hundred yards from one extremity to the other." "The circumstance (says he) of so many sickening in one house, and within so narrow a compass, in *Water Street,* and the disease having appeared in a former year *first* in that very *neighbourhood,* and in *every year* of pestilence, *always first* shewing itself in situations precisely similar, *establish the position of a domestic origin.* Nor can it be pretended, (he adds)

"*believed in other causes of disease than importation.*" These "*attempts*" seem to have been connected with a newspaper attack, (which I have not seen) made by Dr. Hosack, of New-York, on Dr. Rodgers, in the preceding month, when the latter had shown some unwillingness to allow the former, accompanied by two other persons, to *inspect the Marine Hospital,* without some pledge on their parts, to secure himself against a repetition of the ill-treatment or misrepresentation which he complained of having lately suffered. Dr. Chisholm, in his letter to Dr. Haygarth, (pages 72 and 73) avails himself of this transaction to charge Dr. Rodgers with having "made a most singular and unexpected attempt to exact from these gentlemen a preliminary stipulation of *concealment;*" and appears to attribute it to guilty apprehensions in Dr. Rodgers; asking, "why *have* recourse to arts so unbecoming; were there not sinister objects to be obtained?" Knowing nothing of this transaction but the little which Dr. Chisholm has stated, I am but ill qualified to answer his question. I can, however, easily conceive, that, as Dr. Hosack had been long known to be a very zealous advocate for the supposed contagion and importation of yellow fever, Dr. Rodgers might well suspect, what seems to be true, that the former had come to inspect the Marine Hospital, and quarantine ground, in the *hope* of discovering something to enable the believers in importation to obviate the triumph of their opponents, or, at least, make it a matter of doubt, whether the disease of that season had not arisen from imported contagion; and, as this would, of necessity, imply negligence on the part of Dr. Rodgers, and as he could not well expect candour and fairness from one so much prejudiced, and acting from the motives just mentioned, it seems to me that Dr. Rodgers might, very honestly, have wished to secure himself from misrepresentation, and that it would have better become Dr. Chisholm had he been less eager in attributing *bad* motives to Dr. Rodgers, *especially* as it appears, that after Dr. Hosack and his friends had *seen* all they wished to see, they could find no subject of complaint, except Dr. Rodgers's backwardness in gratifying their curiosity.

that the communication between quarantine ground could produce it *here*, because this was exactly the place where, of all others, the communication was least: nay, there could be none at all, for there were no stores," &c. "nor had any of these thirty-five patients been at the quarantines, at any time of the summer previous to their admission, nor had any of them any connexion with infected ships or diseased persons."

In order to bring this *view* to a conclusion, I have only to notice, in a few words, the occurrence of Yellow Fever at *New London*, in Connecticut. At Providence, in Rhode Island, and at Boston, in Massachusett's Bay. At the former of these towns, this disease appeared as an epidemic I believe for the first, and only time, in the months of August, September, and October, 1798, in which the summer was intensely *hot and dry, beyond all example;* the thermometer having been, for several days, in succession, at from 95° to 97°. The disease first appeared about the 26th of August, and terminated in October. "The whole number of persons, whose complaints clearly indicated the pestilence, or, as it is called, the Yellow Fever, did not exceed 246;" and, "of the above number, 231 cases were clearly traced to the *spot* where the sickness commenced; that is, the patients were conversant, or had been in that part of the city, a few days, before they were seized. The part in which the septic gas appears to have been so highly concentrated, extended sixty rods north and south," (*along* the harbour) "and about twenty rods *west, being bounded easterly on the harbour.*" This circumstance clearly manifests the connexion of the disease with miasmata, proceeding from the grounds, wharves, &c. immediately *adjoining the water;* though the respectable clergyman, by whom (with the approbation of several other gentlemen members of the committee of health, &c.) the account on which I principally rely, was drawn up, appears to ascribe the disease chiefly to exhalations from a quantity of salted fish then beginning to putrefy, within the space where the disease began. He adds, "we have not even a shadow of ground to suppose the disorder was not of domestic origin." Dr. Coit, a physician of New London, in a letter to Dr. Mitchell, states, that the persons first attacked, (and to whom he was called) were an innkeeper, with his wife, son, and daughter, living in a street *next* and close to the *Water*, (Bank Street) and that within a very few days he was called to eleven other patients in the same street, and all within eight rods of the innkeeper's House. Eighty-one persons died in all. There was no appearance or suspicion of contagion from the sick. See New York Medical Repository, vol. 2. p. 304-5, and 372 to 378 ; also vol. 3, p. 229.

At Providence, the Yellow Fever became prevalent in 1797 ; also in 1800, and again in 1805. "It has uniformly made its appearance, and committed its principal ravages, in the south part of *Water Street*, or the lanes and alleys immediately adjacent ; and those solitary cases, which have occurred elsewhere, could, with very few exceptions, be traced to this devoted spot." "The portion of Water Street which has been thus *repeatedly the seat of Yellow Fever, is less than a hundred rods in length.* It has, in this place, a south-east direction. The houses on the water side are built, as near as possible, to the natural bank of the river. The wharves, of course, which are extended in rear of them, westward to the channel, are artificially raised, partly filled with earth, and partly constructed with logs, covered with oyster-shells and earth, leaving vacuities beneath, &c." "The south-end of the street is bounded by a small *cove,* or *inlet* from the river, which receives, through a swale, or ravine, (before noticed) the *wash of an extensive range of meadows ;*" which, fermenting in hot weather, afford unwholesome exhalations. To these and other local circumstances, described by *Dr. Wheaton,* (in his "Brief Account of the Yellow Fever, which has appeared, at different times, in Providence,") he adds, that the spot just mentioned is "confined by hills eastward and westward ;" and that, by "presenting a south-western slope to the water, it gives to the mid-day, and afternoon sun, an almost vertical power."

Dr. Wheaton informs us, that the summer of 1797 had been remarkable for a long continued drought; but, about the 1st of August, there ensued "abundant rains," which "were succeeded by *an intensely hot sun*; the thermometer, in an airy situation ranging from 86° to 91°." "The Yellow Fever made its first decided appearance on the 13th of August," and "continued to rage to the 30th of September, in which time it attacked 102 persons, of whom forty-five died. Its early disappearance was ascribed to the almost universal desertion of that part of the town, and to a "very heavy rain and tornado," which occurred on the 8th of October.

"In 1800, the first case of Yellow Fever occurred on the 15th of August, and the disease continued to prevail until the 5th of October." "There were, this year, eighty-three reported cases of Yellow Fever, of which *fifty proved fatal*." The sickly district was more "*universally deserted* than in the preceding year; a circumstance which may account for its early disappearance, as the other parts of the towns were uncommonly healthy."

"In 1805, the disease made its appearance as early as the 25th of July, after a very unusual duration of *hot* and *dry weather*. As the town council directed an *immediate* and *complete* evacuation of this part of the town, it soon subsided, and, on the 10th of August, had disappeared. The people, however, being impatient to return to their habitations, several *new cases* occurred in September." "Here, as elsewhere," (adds Dr. Wheaton) the Yellow Fever "has not been propagated by the sick in situations otherwise healthy; or, in other words, *has not been found contagious*. Of a great number removed to the Hospital, in an airy situation, southwest of the town, there has been no instance of the disease being taken by the attendant physicians or nurses. See New-York Medical Repository, vol. 10, p. 329 to 337.

At *Boston*, during the prevalence of very warm weather, in the month of August, 1796, and "at the south-east part of the town, near a considerable extent of flats, which were daily exposed, for some hours, to the action of the sun, a fever began, and spread thence to the neighbourhood of some of the docks, proving fatal to about thirty persons. Dr. *Warren*, an eminent physician there, in a Letter to the American Academy of Arts and Sciences, then declared that he had, almost every autumn, seen at Boston a considerable number of similar cases, "not excepting the *black vomit*, nor the yellow skin;" and that it was, "what is properly termed, a *bilious remittent fever*."

"In the year 1798 this fever recurred in a very aggravated form, during the prevalence of extremely hot weather, when the thermometer was often above 90°, and sometimes at 96°, of Fahrenheit's scale. It attacked only those who lived, or passed sometime in the vicinity of a *mill-pond, drained of its water*, so as to leave the mud, and other impurities, exposed to the sun's rays; and in the vicinity of several large docks, into which large quantities of refused vegetable and animal matters were conveyed from the market.

According to Dr. Brown, "*not one of twenty*, and upwards, who *first took* the disease, *recovered*;" and, in all, about three hundred died of it. "The fever prevailed with much malignity, till about the middle of October, when it was completely checked by an *inundating storm*, from the north-east, of three days' continuance:" He adds, from the latter part of July, to the middle of September, "the weather was, perhaps, never known so *uniformly and excessively hot and debilitating*." It is asserted also, "that the fever did not seem to be contagious:" that there was "no instance of its being communicated to the nurses or attendants of the sick, *in places where the disease was not originally contracted*." See New-York Medical Repository, vol. 2, p. 360 to 363.

This fever appeared again at Boston, in August, 1802, and prevailed until the month of October, (in the same parts of the town as before,) "and with *greater malignity* than in any former year, equalling the *worst* species of genuine plague; yet the range of the disease was quite limited." "It commenced in debility, which increased with the progress of the disease, till it terminated in death, more commonly on the third day, seldom so late as the fifth: indeed, the patient might be said to be dying from the moment of seizure. The venous congestion was very apparent from the bloody suffusion of the eye; from the cadaverous appearance of the countenance, and from the livid tinge of the whole surface of the body and limbs; the cuticular vessels, and those of the adipose membrane, being loaded with putrid blood." The disease was "*wholly confined* to houses promiscuously situated at *the heads of wharfs, in the south part of the town;* and it was remarkable, that if a patient, under the disease, was carried out of the range of the morbid atmosphere, into a healthy part of the town, and attended by persons *there* resident, the disease was not communicated in a single instance: but not so if he remained on the spot where he took the disease." New-York Medical Repository, vol. vi. pages 338 and 9.

I do not find that the disease was *even suspected* to have arisen from any importation of contagion.

Those of my readers who, by a love of truth, may have been induced to follow me attentively, in the *view* which I have now taken of the Yellow Fever, in different parts of America, and whose minds are unbiassed, will, I am confident, clearly recognize in that disease *all the peculiar features,* and *characteristic marks* by which *marsh* fevers are distinguished in all parts of the world. And they will naturally conclude that, though it be the most aggravated and violent of the fevers arising from miasmata, this aggravation and violence are produced only by a greater concentration or virulence in the latter, joined to a greater intensity of atmospherical heat, acting on persons, but little accustomed to bear it, whilst they retained the excitability of cold or temperate climates, together with an habitual disposition to generate that portion of animal heat which such climates require. They will have seen that the yellow, like other marsh fevers, is always exasperated by great heat, and extinguished or greatly mitigated by cold; that, between the tropics, it prevails *simultaneously* with the milder forms of marsh fevers, violently attacking *strangers* from cold climates, whilst the natives or long residents are at most only subject to intermittents or mild remittents: they will have also seen, that in temperate situations, this disease in the early part of the summer, before the atmosphere has become intensely hot, is commonly preceded by, or *rather shews itself in*, the forms of intermitting or remitting fever; and that when being exasperated by excess of heat, it has assumed, and for some time prevailed under, the appearance of an epidemic Yellow Fever, the accession of cool weather speedily reduces it again to its milder forms; and that a freezing temperature soon puts an end to its appearance, even in those forms, as it commonly does to other fevers occasioned by exhalations from marshes, *and to no others:* And they will also have seen, that the common bilious remittent, of hot climates, which is universally admitted to be the effect of miasmata, differs from the Yellow Fever, only by being a little less violent: that, at the utmost, their symptoms vary only *in degree;* and that, in truth, even this difference is often so imperceptible, that the College of Physicians of Philadelphia, when anxious to assign a distinction between the *yellow*, and the *bilious remittent* fevers, thought it necessary to allege *one* which is not only *invisible*, but without *existence* (i. e. contagion.*)

* It has commonly happened in places liable to yellow fever, when the cause of that disease (miasmata) exists, without being sufficiently powerful and abundant to create an *epidemic*, that a few cases of yellow fever occur, intermixed with what are called bilious remittents and intermittents, *all from the same cause*, and sometimes even in the same families. In other seasons, when the miasmata are sufficiently abundant and powerful to produce an epidemic yellow fever, the first cases of it are often accompanied by, and scattered among, remittents and intermittents; and if, as happened at New York, in 1805, some of the physicians should believe that yellow fever can only be produced by an imported contagion, and no vessel should have arrived from the West Indies, so as

In fact, there is no difference between these fevers, excepting the greater violence, and consequently greater danger, attending the former, than the latter; for the Yellow colour appears in both, and, supposing the fatal *black vomit*, with profuse hemorrhages and petechia, to occur only in what is called *Yellow* Fever, (though they are sometimes seen in fevers known and admitted to arise solely from marsh effluvia) they cannot be included among its essential or distinguishing symptoms, unless *death* be also considered as *essential* to the disease.* Nor can any exasperation of symptoms, which has been preceded by a great increase of heat, give any reason to suspect that a fever, whose symptoms are thus exasperated, did not originate from miasmata; because such an eaxsperation is invariably produced by that *cause,* in marsh fevers; and by it they are susceptible of the most dangerous and malignant appearances. Of this Sir John Pringle was fully convinced, when, at p. 324, of his work on the Diseases of the Army, he made this observation, viz.

"I shall observe, *upon the whole,* that the autumnal remitting and intermitting fevers, of *low* and *wet* countries, *when at the worst,* may be considered as another species of *pestilence,* since they have been seen with *all the virulent symptoms* peculiar to that class of diseases."

As a farther proof of the identity of Yellow and marsh fevers, I shall remark that, besides their *simultaneous concur-*

to be chargeable with such importation, these physicians have commonly thought it proper to declare, that these cases were *not the yellow*, but the common indigenous, bilious fever; and thus they have, as is asserted, sometimes hindered the inhabitants from leaving a noxious situation until it was too late.

* Dr. Lind, at p. 118, of his Essay on the Diseases of Hot Climates, (5th edit.) treating of the yellow fever, says, "having considered this disease with attention, I am *now* of opinion, that the remarkable dissolution of the blood, *the violent hemorrhages, the black vomit*, and the other symptoms which characterize the yellow fever, are only *accidental* appearances, in the common fever of the West Indies. They are to be esteemed merely as *adventitious* in the same manner as *purple* spots, and *bloody urine*, are in the small-pox, or as an hiccup in the dysentery; like these they only appear when the disease is accompanied with an high degree of malignity, and, therefore, always indicate great danger."

*rence*, and *mutual interchanges*, as before-mentioned, they are not unfrequently *converted* one into the other, *in the very same individual.* Of this there are many instances and proofs, some of which having been already noticed, I need only adduce the following extract, from Dr. Rush's Letter to Dr. Miller, dated October 8th, 1802, concerning the Yellow Fever which then prevailed at Philadelphia, and with more than common mortality, at least, in proportion to the numbers of sick. "Never (says Dr. Rush) has the *unity* of our autumnal fever been more clearly demonstrated, than in our present epidemic. Its *four* principal grades, viz. the intermittent, the mild remittent, the inflammatory bilious fever, and the malignant Yellow Fever, *have all run into each other* in many instances. A *tertian* has ended in death, with *a black vomiting;* and a fever, with the *face* and *eyes suffused with blood,* has ended in a *quotidian,* which has yielded to a few doses of bark. The Fever, in Baltimore, I have been informed, has put on the same multiform appearances and changes." See New York Medical Repository, vol. vi. p. 249. Greater proofs of near affinity than these can hardly be desired.*

* The convertibility of yellow and marsh fevers into each other, was attested, almost half a century since, by the late Dr. Huck Saunders, who had become well acquainted with them, as an army physician in the West Indies and North America. "It sometimes (says he) depends upon the manner in which a patient is treated in the beginning, whether he shall have a yellow, or only a remitting or intermitting fever." See Pringle's Diseases of the Army, p. 108.

Dr. Rush, and his associates, who were incorporated as the Academy of Medicine, of Philadelphia, in their letter to the Governor, December 1st, 1797, say, "by *depleting* remedies, the most malignant yellow fever may be changed into a common bilious fever ; and, by *tonic* remedies, *improperly applied,* the common bilious may be made to assume the symptoms of the most malignant yellow fever." Dr. Drysdale, writing of the yellow fever at Baltimore, in 1794, observes, that this fever, in its favourable issue, would sometimes terminate in a *tedious* quotidian, or tertian *ague.*" See Cox's Medical Museum, vol. i. p. 41.

Dr Gillespie, also, in his "Observations, &c." already quoted, after mentioning the epidemic fever, which had proved fatal to at least one half of the crews of the Spanish squadron, captured by Admiral Harvey, at Trinidada, in February, 1797, adds, that from "the accounts which our medical gentlemen collected at Trinidada," this epi-

Finally, the Yellow and marsh Fevers resemble each other by *attacking* the *same organs* or parts of the body, especially the stomach and smaller intestines, which were found to be inflamed in remittent fever, *even at Copenhagen:* and they also lay the foundations of *similar* chronical affections.

With so many proofs of identity in their cause, and of the nearest affinity in their symptoms, and reciprocal conversions into each other, as well as in their effects on the human body, and their changes by heat and cold, &c. it would be highly unreasonable not to consider them as being only *varieties of one disease.* And I think with Dr. Rush, that we might as well "distinguish the rain which falls *in gentle showers,* in Great Britain, from that which is *poured in torrents from the clouds in the West Indies,* by different names and qualities, as impose *specific* names and *characters* upon the different *states* of bilious (or marsh) fever." See Medical Inquiries, &c. vol. 4, p. 45.

Among the points in which the *yellow,* resembles other marsh fevers, (and which, therefore, co-operate in proving their identity) I might have included that of its possessing no contagions property: for certainly this fact has been attested and demonstrated in so many places, and by so many unquestionable authorities, that no unprejudiced reader, who shall have bestowed proper attention upon the proofs and occurrences which have been stated in the preceding pages, can entertain the smallest doubt respecting it. Wishing, however, not merely to convince the unprejudiced, but to reclaim and undeceive those of an opposite description, so as to obviate all future disagreement on this important question, I shall here introduce a few additional facts, testimonies, and arguments, respecting it.

demic appears to have been "an *ardent Yellow Fever, terminating* in *remittent* and *intermittent* fevers, in a manner *similar to what* happened *here,* (i. e at Martinico) in 1796," as mentioned by him, at pages 130, 1, and 2; and also at page 164, 5. I could easily fill many pages with similar facts and authorities.

One fact, which decidedly proves the Yellow Fever to be destitute of any contagious power, is that of its never having been communicated to others by any one of the many thousands who, in the West Indies, as well as at Charleston, Norfolk, Baltimore, Philadelphia, New York, &c. were removed beyond the *reach of marsh miasmata,* whilst labouring under the disease, or after having imbibed its poison; though, in many of these, the disease appeared in its worst forms, and proved mortal. That this has been the case in all these places, will have been seen in the view which was lately taken of this subject. Of the importance, as well as of the certainty of this fact, the late Dr. John Hunter was justly convinced. "What may be considered (says he) as an *experimentum crucis,* to prove the non-existence of contagion is, when the sick leave their usual residence, and go to other places which are healthy, without spreading the disease. *This* (he adds) *constantly* happens in the *remittent* fevers of the Wast Indies," (among which he includes the Yellow Fever.) See Diseases of the Army in Jamaica, p. 322.

Dr. Miller, in his report to the governor of New York, has made a similar statement, in these words: "Many who had contracted the disease in New York, died of it at Boston, Albany, and other cities at a distance; many, likewise, at Greenwich, Brooklyn, and other villages, in the neighbourhood. In no instance did these victims of the epidemic communicate contagion."

To invalidate this statement, Dr. Chisholm, at p. 177 and 178, of his Letter to Dr. Haygarth, refers to *another,* said to have been made by Dr. Wistar, respecting a case of Yellow Fever, at Germantown, in the year 1798, when "one person, who had been in Philadelphia" four days, and in a sickly neighbourhood, returned home, on the 7th of August, and, on the 9th, was attacked with Yellow Fever, which terminated fatally in four days." "Ten cases," says Dr. Chisholm, "are specified of the disease excited by contagion emanating from the body of this person." To specify such cases is not difficult for one who acts under the influence of party zeal; but this is not proving their existence. We are told, indeed, that in most of the cases the disease appeared to have been contracted at the house of a Mrs. Johnson; that "one person received the infection from sorting the clothes of her deceased daughter, and another from the bed on which his mistress died," and *these pretended circumstances*, the only ones of which any mention is made, are to serve as proofs, sufficient to overturn the evidence of, probably, *fifty thousand cases*, in which this disease has *manifested that it possessed no contagious quality*. Persons who believed that nothing could produce the Yellow Fever except contagion, might conclude that it had been produced in ten persons, from one who had been at Philadelphia; for they would naturally neglect to inquire whether these persons had not also been there, or to some other source of marsh miasmata, which is a million of times more probable than that nature should depart from her constant uniformity, and render the same disease contagious in one instance, and not contagious in 50,000 others. Relying on this undeniable and fundamental truth, I should think it a waste of my own, and of my readers' time, were I to employ it in a further investigation of the cases in question, given, as they are, without *any proof*, or even any circumstance, but those just mentioned, and which are *suited only to vulgar apprehensions*. If such cases had really occurred, and with such evidence as to render them credible, Dr. Rush, who must have heard of them, and who, at that time, believed that the disease might sometimes be contagious, would, probably, have been so far confirmed in that belief, as to have abstained from the public retraction, which he afterwards made of it; and he certainly would not, in the very letter containing that retraction, have declared, as he did, that the Yellow Fever "*has uniformly perished* in the high and healthy village of *Germantown*, when carried from Philadelphia." See Medical Repository, vol. 6, p. 165.

The impossibility of spreading the disease in situations remote from marsh miasmata has been attested, not merely by persons who believed it not to be contagious, but by one of the strongest assertors of the contrary opinion, by the very person who appears to have first misled Dr. Rush, and others on this subject, (I mean Dr. Lining, of Charleston) more than half a century ago. His words are these, "although the infection was spread with celerity through the town, yet, if any from the country received it in town, and sickened on their return home, *the infection spread no farther, not even so much as to one in the same house.*" *See* Dr. Lining's Letter in the Edinburgh Physical Essays, vol. 2, p. 373. This admission, of a truth, of the most decisive import, from Dr. Lining, ought to have opened the eyes of Dr. Haygarth, or, at least, to have moderated that *overbearing* confidence which he has repeatedly manifested in his belief of the contagious nature of the Yellow Fever. That he was sensible of the weight of this admission, and of the evidence afforded by it, I am forced to conclude, from the circumstance of his having very unfairly excluded, and, to the utmost of his power, *suppressed it,* when he thought proper to fill the two first pages of the appendix to his Letter to Dr. Percival, with other parts of the same Letter, (from Dr. Lining) in order to persuade his readers that Yellow Fever was contagious.

What Dr. Lining thus admitted, and Dr. Haygarth suppressed, was found true even in regard to the fever which Dr. Chisholm supposes to have been imported to Grenada, by the Hankey. For persons who took that disease from the atmosphere at St. George's, and sickened in the country, *did not,* as I am well informed, communicate the disease to any person there, and of this Dr. Chisholm cannot, I think, have been ignorant. This was also the fact at *Dominica,* where, according to Dr. Chisholm, this malignant pestilential fever was transplanted, and prevailed in 1793, 4, and 5. Dr. James Clark, (a writer of unquestionable veracity) states, at p. 64, of his Treatise, that—"when patients, labouring under *this fever,* were removed to high situations, for the sake of breathing a cooler and purer air, and who, notwithstanding, *fell victims to it*, the people about them *were never infected, nor did the disease ever prevail afterwards in such places*."—But I will not abuse the patience of my readers by adducing further evidence on this point.

Another, and, if possible, a stronger proof, of the non-existence of contagion in Yellow Fever, is derived from the Hospitals. If a disease be supposed to possess any contagious power, however small, there is no way in which that power can be so readily concentrated, and rendered manifest, as by collecting great numbers of persons, ill of that disease, within the wards of an Hospital. And it is in those which have been *exclusively* appropriated for cases of Yellow Fever, at New-York, Philadelphia, &c. that we ought to find the most striking and irresistible evidence of its contagious quality, if it does really possess any. But the evidence which they have afforded is of a very different import. And here I will recur to Dr. Miller's (before-mentioned) report to the governor of New-York, against which Dr. Hosack, with all his eagerness to object, could find no objection but the omission of one unimportant observation, favourable to Dr. Miller, and unfavourable to his opponents. Dr. Miller's words are,—" no communication of the disease was ever observed in Yellow Fever Hospitals, situated at a small distance from the cities to which they belong. *No exception to this has ever occurred in any of the numerous seasons of this pestilence at our Hospital at Bellevue*, the *Marine Hospital at Staten Island, that of Philadelphia*, or any other in the United States; provided the malignant air of the city had been avoided.* The force of this

* Dr. Chisholm attempts, at page 174 of his letter to Dr. Haygarth, to pervert the plain meaning of Dr. Miller's proviso, that "the malignant air of the city had been avoided." "Then (says Dr. Chisholm) this malignant air could be diffused to distant and scattered points, and yet could not extend itself to Hospitals at a *small* distance from its origin." But the Hospitals were *not at a small distance from* its origin, nor had Dr. Miller said or admitted that this malignant air could diffuse itself to distant and scattered points;—for, though persons occasionally sickened at such points, the

fact seems never to have been duly *considered* or *appreciated*. The numerous retinue of medical attendants, nurses, washer-women, servants, &c. which belong to an Hospital, must be known to every body. How greatly they are all exposed to contagion, if it could be supposed to exist in this case, is equally known. The most malignant degrees of the disease are constantly found in these institutions. The exposure of the physicians, and their attendants, is well understood. The duty of the nurses leads to an incessant, and unreserved, intercourse with the sick," &c. &c. yet, not only all these *have invariably escaped the disease*, but likewise *all* the persons occupied in the *removal of the sick*, from the city to the Hospital, who, in this service, went, without reserve, into the most pestilential quarters of the town, entered the most filthy apartments, &c."*

The facts, here stated by Dr. Miller, are of such notoriety that no farther proof of them can be necessary; especially as my readers will have seen the like testimony given by others in the *view* lately taken of this disease; I find, indeed, that in taking it I overlooked a Letter from Dr. Ramsay, published in the Medical Repository, vol. 4, at p. 220, and dated

miasmata might have been exhaled from particular unwholesome spots, very near to them, and when this was not the case, the persons in question, without waiting for the miasmata to reach them; might have gone to the places where they were produced, as was undoubtedly the fact. Dr. Chisholm tells us, that *he* has inquired, at p. 274 and 288, of vol. i, of his Essay, "how far the influence of marsh miasms extend," and this, by reference to the pages in question, appears in his opinion to be *two miles*; and he thence concludes, that if these miasms existed in Philadelphia or New-York, they "would certainly affect the adjoining Hospitals." How little he knew, and how much he was mistaken, on this subject, must have been manifested by the facts already stated, at p. 162, 3, and at p. 164, 5, and 6, of this volume.

* Dr. Miller, in a note, explains the escape of these persons, by stating, "that they all resided, during the season, at the *alms-house*, in an elevated and healthy part of the city; and, consequently, were only, for a short period, *at any one time*, immersed in the noxious atmosphere." But their communication with the sick lasted for a longer time, and was such as must have propagated the disease, had it been contagious. Dr. Miller adds, in another note, that several persons died of the yellow fever in the *alms-house*, in 1798, and that, "although the house then contained *about eight hundred persons, no communication of contagion took place*."

Charleston, November, 18, 1800, in which he writes, "that of forty-one cases of Yellow Fever, which" (in the preceding months) "took place in the Marine Hospital, forty were brought there from the shipping in the harbour, with the disorder on them, and, in many cases, far advanced. Only one case originated in the Hospital, and that was of an old *intemperate* man. While so many patients were dying of the Yellow Fever in the Hospital, there was always a considerable number of other patients under the same roof, who all, with this one exception," (originating probably in drunkenness) "escaped it. This is the more remarkable, as the greatest number of them, as well as several of the nurses, were strangers, and affords additional evidence, that the disease was not contagious." He mentions, that the Hospital is half a mile out of Charleston.*

Of the non-existence of contagion in Yellow Fever, Dr. George Fordyce seems to have been convinced, near the close of his life, though in opposition to all that he had formerly believed and taught on the subject; for, in his fourth Dissertation on Fever, the last which was published previous to his death, he states, as the result of all his inquiries, and of the best information which he had been able to obtain by conversing with, and *cross*-examining, individuals, who had had opportunities of observing the *semitertian fever* of hot climates, (for, so he called the Yellow Fever) in those towns where it

* My readers scarcely need be reminded of the facts which I have mentioned at p. 182—3, on the authority of Dr. John Hume, Dr. John Hunter, Dr. Walker, &c. of the absence of contagion in the different hospitals at Jamaica, notwithstanding the many thousands of patients with yellow fever, admitted into them at different times. Nor of the opinions of the medical officers of the army under sir Ralph Abercrombie, on this subject. I can safely aver, that several thousand cases of this disease fell under my own observation in the West Indies, and that I did not find the least appearance of a contagious quality in any of them. The fevers of the East Indies, though so extremely violent as sometimes to produce death in a few hours, are equally destitute of contagion. Dr. Wade, on the diseases of seamen and soldiers in Bengal, asserts, at p. 3, that, during the course of a long and assiduous practice there, he "had not observed, to the perfect conviction of his own judgment, either in or out of the hospitals, a single instance of contagion" among fevers.

prevailed, "that in Hospitals where patients were received ill of that fever, the physicians, surgeons, and other attendants, were not oftener seized with it, than the other inhabitants of those towns."

The advocates for contagion in America, (who, though, pertinacious, are now very few in number,) when embarrassed by the well-known and admitted fact of the disease's never spreading in the country, pretend that the *air is there too pure;* which is doubtless, true, if by *purity* they mean that it is not sufficiently charged with marsh miasmata to produce fever; and this is also true in those hospitals, in which the disease cannot be propagated. And if this be their meaning, it only amounts to this, that their supposed contagion does not act, unless there be also present another very sufficient, and in fact, the only *cause* of the yellow fever. But, if they mean any thing else, or mean what is commonly supposed to constitute atmospherical purity, this certainly does not exist in crowded hospitals; and their explanation is a mere subterfuge, to which persons will often have recourse, rather than candidly retract an erroneous opinion.

Besides the universal exemption of the physicians at New-York, from yellow fever, during the epidemic of 1805, (in consequence of their having then learned, in great degree, to avoid the spots infested by marsh miasmata) Dr. Miller, in his report, mentions the dissection of persons who had died of yellow fever, which, if the disease had been contagious, must have proved a source of danger. "Many of the physicians of this city," says he, "were frequently engaged in this mode of investigating the disease, and minutely examined bodies in a very advanced state of putridity; and yet *they all* continued in perfect health." But, besides the dissections here mentioned by Dr. Miller, it appears that a great number have been made without harm in other places, and particularly at Philadelphia: together with many experiments upon the black matter vomited in the last stage of yellow fever, calculated to ascertain whether it possessed any contagious property.

Among these may be noticed some very remarkable ones, lately exhibited by Dr. Ffirth, at Philadelphia, in the presence of several medical gentlemen of good characters. After some experiments of less importance, he inoculated himself in the left fore-arm with the black matter which had been *just before* vomited by a *moribund* Yellow Fever patient; a slight inflammation ensued, which subsided in three days, and the wound readily healed; he then confined, by a black sticking plaster, some of the same matter, immediately after its ejection, over a cut in his right arm, *for two days*, and then found that it had occasioned no inflammation, the wound readily healing, without any formation of pus.

These experiments he repeated above twenty times, in various parts of his body, with similar black matters ejected by Yellow Fever patients, in Philadelphia, during the epidemics of 1802 and 1803. He also put it into his eye, without experiencing any more inconvenience than cold water produces; he also inoculated himself with the saliva and serum of patients under Yellow Fever, and with as little effect. He exposed himself to the exhalations of the same black matters, (which had been recently vomited) *heated in an iron vessel*, and experienced no unpleasant consequence, or sensation. He swallowed the inspissated matter, which remained after this evaporation, made into pills, without finding his stomach incommoded thereby; and he, finally, drank *two ounces* of the recently-vomited black matter *undiluted*, and found it harmless, after having previously taken without any bad effect, considerable quantities of similar matter, diluted with water. These nauseous draughts, and hazardous inoculations, will, doubtless, be thought sufficient to prove, that neither the blood nor the saliva of patients, under Yellow Fever, nor yet the black matter, the vomiting of which is justly deemed the most fatal symptom of the disease, possess any contagious property. (See Dr. Ffirth's Dissertation on Malignant Fever; Also *New-York* Medical Repository, 2d Hexade, vol. 2. p. 70.)

I have already noticed the constant extinction of Yellow Fever by frost, as one of those points in which it exactly resembles the *marsh*, and only the marsh, fevers. This instructive and highly important fact is, however, capable of a more extensive application, for it not only proves the origin and nature of all these fevers, but it also proves decidedly that the Yellow Fever has no power of *propagating* itself by contagion; and, consequently, that it never *proceeds from it.* In considering this fever as produced by marsh miasmata, we readily understand why (like other marsh fevers) it should cease when the atmosphere no longer retains sufficient heat for their generation and exhalation; but this diminution of temperature could not extinguish a fever subsisting by contagion. Frost has no access to the apartments, and still less to the bodies, of persons under Yellow Fever. Upon its occurrence, fires, with additional bed clothes, secure them from its approach, and from even the smallest interruption to those morbid secretions, or actions, by which contagion is supposed to be generated: and, therefore, if contagion were the cause of Yellow Fever, new cases of it ought to occur, during winter, especially as we are not acquainted with any febrile contagion, which is liable to be rendered inactive, merely by such a diminution of temperature as is sufficient to stop the progress of Yellow Fever. It certainly would have no such effect upon typhus fever, small pox, measles, or, indeed, any contagious disease, within my recollection. Dr. Chisholm, however, from the resolution which he seems to have made of resisting or evading the evidence of all facts repugnant to his promulgated opinions, endeavours to account for this extinction of Yellow Fever, (at p. 177) by pretending that "it is admitted that Yellow Fever is the product of infection, in combination with a high temperature"—adding, "If it is so, and of this there can be no reasonable doubt, cold weather *must* have extinguished it." By infection, I presume Dr. Chisholm means contagion, and if so, I must deny the existence of any such *admission* as he pretends, except by himself, and those who have adopted his peculiar opinions, in regard to the fever alleged by him to have been derived from the Hankey; for he has repeatedly and strongly denied the existence of contagion in the Yellow Fever which prevailed in the West Indies before the year 1793. But perhaps Dr. Chisholm, as the *inventor* of that fever, or, at least, of its supposed origin and contagious power, may think himself intitled to *endow* it with any qualities which may suit his own purpose, and, consequently, intitled to combine febrile contagion, even with that high temperature which has always proved destructive of it; but, even with this incongruity, which I am not disposod to adopt, we may reasonably expect a little uniformity and consistency in his account of the properties and effects of this extraordinary combination; and require that he should not represent it as *necessarily extinguished by cold* weather in *North America* after having asserted, as will hereafter be seen, that it prevailed extensively among the British troops in Ireland, in the winter of 1795, 6.

As the more violent forms of yellow or marsh fever have chiefly occurred in the sea-port towns of North America, as well as of the West Indies, and from the same causes, those persons who, in regard to the former, have chosen to represent the Yellow Fever as the product of an *imported* contagion, have in several, *though not in all cases*, been able to discover some vessel, recently arrived from the West Indies, about the time when the local causes of Yellow Fever, from excessive heat, &c. were become powerful at Philadelphia or New-York, and this vessel being placed in immediate contact with these causes, and some of her crew being, consequently, soon attacked with fever, the sufferers, or the innocent vessel, to which they belonged, have been charged with the guilt of importing a contagious disease, and this under circumstances where it has sometimes been difficult to demonstrate the truth, in opposition to confident statements, which, in such cases, are commonly made, and which, with a few of those *omissions* or *misrepresentations*, naturally resulting from party zeal, often give an appearance of probability to the charge. Indeed, statements of this nature have been adopted and published by the College Physicians, at Philadelphia; the only corporation, I believe, of any kind in the United States, which entertains similar opinions of this subject.

But, as the existence of a contagious quality in the Yellow Fever has not only never been proved or made probable, either in the West Indies or North America, but on the contrary, this disease has, in many thousands of instances, manifested, in the clearest and most decided manner, that it possessed no such quality; and, as without *it*, no importation of the disease, from the West Indies, is possible, and propagation of it in the United States, such statements can have had no foundation in truth, nor the smallest claim to serious notice. If the disease were contagious, the fact must have been indisputably demonstrated more than ten thousand times, considering the multitudes who have been victims to it, and there would long since have been no more doubt on the subject, than there now is of the contagious nature of small pox and measles. The uniformity of nature, and the necessary connexion between cause and effect, will not allow us to believe in the *fortuitous* occurrence of a few rare instances of contagion from Yellow Fever, in opposition to the immense mass of facts by which that disease has been proved destitute of any such quality, and the probabilities will always be a million of times greater that these supposed rare instances, have originated in ignorance, error, prejudice, or falsehood, than that effects so *monstrous* should ever have really occurred.* *With this conviction*, I cannot resolve to misemploy my time by undertaking a particular examination of the instances in which the Philadelphia College suppose the Yellow Fever to have been imported, especially as Dr. Caldwell seems to have sufficiently noticed and *refuted them all*, in his several publications; and particularly in his

* While it is notorious that the yellow fever cannot be propagated a single mile from Philadelphia or New-York, it is completely absurd to suppose, that it can have been transported by a contagious quality one or two thousand miles across the ocean.

Essay on the pestilential or Yellow Fever, as it prevailed in Philadelphia in the year 1805, subjoined to his translation of Alibert's Treatise on Malignant intermittents.

Believing that I have done enough, and *more than enough*, to prove that the Yellow Fever has no contagious power, and that it arises exclusively from local causes, such as have been described, I shall conclude my Essay in regard to the United States of America, by adducing the hitherto uncontradicted testimony of Mr. Jefferson (their late president) delivered by an official message to both houses of congress; which affords a complete general confirmation, by the most unobjectionable, as well as *highest*, authority of the facts stated in the preceding pages. The message was communicated on the third of December, 1805, and the part, to which I refer, was in the following words, viz:

"In taking a view of the state of our country, we in the first place, notice the late affliction of two of our cities under the fatal fever, which in latter times has occasionally visited our shores. Providence, in his goodness, gave it an early termination on this occasion, and lessened the number of victims which have usually fallen before it. In the course of the several visitations by this disease, it has appeared that it is strictly local, incident to cities, and on the tide waters only, incommunicable in the country, either by persons under the disease, or by goods carried from diseased places; that its access is with the autumn, and it disappears with the early frosts. These restrictions, within narrow limits of time and space, give security even to our maritime cities, during three-fourths of the year, and in the country always."

Having thus established the origin and non-contagious nature of the Yellow Fever, I might here finish this inquiry did it not seem expedient, for many reasons, to extend it to the violent and very destructive fevers which, within a few years, have prevailed epidemically in the south of Europe, and more especially in Spain, and at Gibraltar, in order, by comparing them with the Yellow Fever, to ascertain their identity, which, however manifest to my conceptions, has been denied by many, and even by some who have had opportunities of becoming acquainted with the *former*, at least.

And here I regret extremely, that, having determined to confine this publication to a single volume, I am under the necessity of laying aside a great mass of facts, which I had collected, with no inconsiderable trouble, respecting the prevalence and different forms or gradations of marsh fevers, not only in Africa, and the East Indies, but in various parts of France, Italy, Sardinia, Sicily, Malta, Greece, &c. and which, had I room, would greatly illustrate the subject, and confirm what I have already written, as well as what I am about to write of the Yellow Fever at Cadiz, Gibraltar, &c. I must, however, introduce what I have to say in regard to the latter, with a few observations respecting *Rochefort*, Bourdeaux, and Lisbon.

The former of these cities is nearly surrounded either by salt, or fresh-water marshes, so that, in summer and autumn, strangers can seldom reside there a fortnight, without an attack of marsh fever. On the South-east side, in particular, are extensive fresh-water marshes, which, when the wind blows over them upon the city, in very hot dry weather, produce violent and dangerous fevers, and render them epidemical at Rochefort. This was the case to a remarkable degree in 1694, when, according to Sir John Pringle, (Diseases of the army, p. 323) " a fever broke out in Rochefort, in France, which, on account of the *uncommon symptoms* and *great mortality*, was at first believed to be the plague. But M. Chirac, who was sent by the Court to inquire into its nature, found the cause to arise from some marshes that had been made by an inundation of the sea: and observed, that the *corrupted streams*, which smelled like gunpowder, *were carried to the town by the wind* that *had long blown from that quarter*. About *two-thirds* of those who were taken ill *died*. This fever raged in *June*, *July*, and *August*, and then ended upon a *great fall of rain*, which purified the air, and *refresh-*

*ed the stagnating water.*" Sir John Pringle refers to Les Œuvres Posthumes de M. Chirac, for an account of this fever. In the *Eloge* of M. de Chirac, printed in the Hist. & Mem. de L'Acad. R. des Sciences," for the year 1732, at p. 121, this fever was designated as a "Maladie Epidemique, qu'on appelle *de Siam*," (an Epidemic called *Siam* fever) doubtless from its resemblance of the Yellow Fever, to which that name had then been given at Martinico. Mr. Chirac is stated to have dissected the bodies of nearly 500 persons who died of it, and he mentions his having generally found the *stomach greatly inflamed, and, in some parts, mortified.*

Violent fevers, which were considered as pestilential, formerly prevailed very often in the summer and autumn, at Bordeaux, and frequently compelled the parliament to remove to other places. The Cardinal de *Sourdis*, having formed a just opinion of the cause of these fevers, undertook to drain a very offensive marsh, which then existed on the west side of the city.—Accordingly, two great canals were dug by his orders, and at his expence, to convey the stagnant water into the river, and a fine *causeway* being erected over the infecting spot, and planted with rows of elm trees, the *plague*, so called, ceased to appear. "L'on eleva dans le lieu où etoit un Cloaque infect, une belle Chaussée, que l'on borda d'Ormeaux. La *Peste* n'a point reparue depuis cette epoque." See Memoires de la Société Royale de Medecine, tom. viii. p. 272 and seq. where, also, a very interesting report to the Society will be found, of the different parts of France in which marshes abound, and of the fevers, intermitting, remitting, and sometimes apparently continued, which prevail in their neighbourhood, with greater or less malignity during summer and autumn. It seems, however, that though Bordeaux has been, in a considerable degree, relieved from these fevers, since the work, executed at the expence of Cardinal *Sourdis*, yet the very hot, as well as dry, summer and autumn of 1804, (which occasioned the Yellow Fever in many parts of Spain, Gibraltar, &c.) assisted by some new attempts at draining, begun at an improper season at Bórdeaux, produced in this city a malignant epidemic fever, which Dr. Galway, a physician of eminence there, states to have had very short remissions, and to have often terminated fatally, before the fourth paroxysm. The daily mortality from it is said to have exceeded fifty for some time. It prevailed chiefly in the quarter called the "*Departement*," where the *ground is lowest*, and the streets narrow: and this quarter was deserted by all persons in easy circumstances. See New-York Medical Repository, vol. 4, of 2d Hexade, p. 263, 4.

Lisbon, since the earthquake, in 1755, has been, in a great degree, exempted from marsh fevers; which, however, was not previously the case, especially in the *lower* part of the town *between the bottoms of the hills* and *the river;* a spot where the streets were narrow, dirty, and, as I believe, *unpaved.* In the eloge of Mr. Sanchez, formerly first physician to the Empress of Russia, (Anne Ivanowna,) inserted in the 4th volume of the "Hist. De la Societé Roy. de Medeçine de Paris, it is stated, (in a note, at p. 215) that he had witnessed the ravages made at Lisbon in 1723, by a very mortal epidemic; and that Dr. Bertrand, a physician, who had distinguished himself during the *plague* at Marseilles, in 1720, being consulted by the King of Portugal, declared the latter to have been a different disease fnom the epidemic, at Lisbon, in which *black vomitings* were the most alarming symptom: (" des *vomissemens noirs* etoient le Symptome le plus effrayant.") Mr. Sanchez is also stated to have observed, that this epidemic rarely attacked women, and that the negroes of both sexes completely escaped it. These facts were obvious indications of the Yellow Fever; but I found others more decisive, In the library of the British Museum, M. S. 4376. Plut. II. J. is a letter, dated Lisbon, October 30th, 1723, signed William Cayley, (apparently the British Consul there) and addressed "to Dr. Kennedy, Physician to the British Factory at Lisbon," in which, after mentioning "the sickness," which had prevailed for some time, without any abatement; "still continuing to take off numbers of its inhabitants, some of whom die suddenly, and in such a manner as has given cause to suspect the disease to be attended with *pestilential* symptoms," he calls on Dr. Kennedy for his "opinion" respecting it, "and the observations he had been able to make in the course of his practice;" to the end that he, Mr. Cayley, might, according to his "*duty*, transmit the same to his Majesty's Principal Secretary of State, in order that, if deemed *contagious*, due precautions may be taken to prevent the same from being introduced into his Majesty's Dominions," &c.

To the preceding letter is subjoined an answer, signed Gilbert Kennedy, and dated, the 31st of the same month, of which the following is an extract, viz.:

"The *heat* last summer began late, but continued *very violent*, and much *longer than usual*, so that the grapes, which were more plentiful this year than many years past, were *burnt up*, when pretty ripe. All the summer was sickly, but, about the middle of August, there appeared the fever which now reigns, accompanied with a pain of the head and loins, a great sickness at the heart and stomach, with reachings to vomit, very contagious in the *lower* parts of the city, going generally through a family, and very few families escaping it, especially in the close, narrow streets. The *high* parts are *much freer* than the *low* parts, and *the villages and country houses about town are entirely free from this distemper, notwithstanding the great communication.* The recovery is generally accompanied with the yellow jaundice. The only *mortal* symptom in this epidemic is the *vomiting black choler*, but this symptom is so rare, that I have met with it but twice in above a hundred I have seen with this distemper."*

* It seems probable, from several circumstances, that the worst cases of this disease occurred among the poorer inhabitants of Lisbon, and that Dr Kennedy's practice being nearly confined to the British Factory, who were in better circumstances, he saw fewer cases of *black vomitings* than the Portuguese physicians. I ought

"By his Portuguese Majesty's order, there have been three bodies of persons dead of this distemper opened in the public Hospital, in all which the blood appeared dissolved like the thin lees of wine; and in one of them, who had the black vomit, there appeared, besides, *black spots* upon the skin, which were only superficial; the gall bladder and intestines, with a small quantity of *black* gall in them, and the stomach, with a *large* quantity of the same."

"The reasons why great numbers have died of this distemper are; firstly, the many poor who live here most miserably; and before they can procure any assistance, are carried off by the force of the disease, which is extremely violent for the two or three first days, having rarely any crisis that is usual in other fevers;—Secondly, the Portuguese method of treating all fevers by bleeding, *morning and evening, while they last;* which has generally been *pernicious* in this epidemic. From all its appearances, hitherto, it seems very clear, that it is *contagious only among those whose bodies are predisposed by living in a close, noisome air,* and abound with bile, or labour under some error in the non-naturals. *Strangers,* who drink wine, (i. e. are intemperate) are more universally attacked with it than the Portuguese: among the latter no man of fashion has had it, nor has it yet *entered any convent,* except the Irish, *although they all assist the sick.*"

With so many facts, decisively proving that the disease was not communicated by the sick to the well, and that it only attacked persons who had been exposed to the influence of marsh effluvia, we might well wonder that Dr. Kennedy had not discovered the truth, did we not recollect the general dispo-

further to observe, that Dr. Kennedy mentions, in his letter, that the weather had "changed several times to rain and cold," without any abatement in the distemper. It may, however, be presumed, that these changes were not very considerable, and they obviously were not lasting; and, therefore, not likely to produce any sensible benefit. Moderate showers of rain, occurring with some intervals, have been found, in America, rather to promote than diminish the progress of yellow fever; probably, by favouring the extrication of miasmata; and at Lisbon, in the month of *October*, no *great* diminution of temperature is likely to have been thus produced.

sition which then prevailed to believe in the supposed contagion of fevers, without discrimination or consideration. To those who have formed correct ideas on this subject, it must *now* appear absurd, to represent a fever as being "very contagious in the *lower* parts of the city," very little so in the "*high*:" and as having *no existence* in "the villages and country houses about the town, notwithstanding their *great communication*."

I will now proceed to *Cadiz*. This eminent maritime city has frequently been afflicted by violent and destructive epidemics.

Dr. Felix Pascalis, who lately travelled over a great part of Spain to procure information, and ascertain facts, on this subject, mentions on the authority of Dr. M. Gonzales, (author of a "Dissertation Medica sobre la Calentura maligna contagiosa," or Yellow Fever, which prevailed at Cadiz, in the year 1800,) that, "during the seventeenth century," this city "had been *four* times visited by *horrid pestilences*, of which there are monumental indications, and religious festivals, instituted to commemorate their duration, cessation, &c. There are, however, no such discriminating accounts of these epidemics, as will enable us to decide, whether they arose from marsh miasmata, like those of the last and present centuries at the same place, or whether they were the plague, properly so called; but, considering how frequently and loosely the latter appellation was given to very different diseases, when attended with great mortality, the form is highly probable. Dr. Pascalis, however, asserts, that the *Yellow Fever* prevailed at Cadiz in 1730, and that the physicians sent from Seville, and who critically examined the disease, "did not omit to notice the (yellow) *colour* of the patients, and the *vomito prieto*:" and for evidence of this he refers to "Cadiz Illus trada, lib. 6." He asserts, also, that this disease prevailed there at other times in the last century, particularly in "1736, 1744, 1746, and 1764." As authorities for the three first of these epidemics, he refers to the writings of Dr. N.

Rexano, Dr. Gregorio Condemina, and El Vicario Ecclesiastico de la Isla de Leon."

Of the epidemic, in 1764, an account has been written, in Latin, by Dr. Salvarez, (or Salvareza) then a principal physician at Cadiz, by which it appears to have exactly resembled the Yellow Fever of America. He, indeed, calls it *Vomito Prieto*, because black vomitings, as well as yellowness of the skin, frequently occurred. Dr. Lind, also, in his Essay on the Diseases of Hot Climates, at p. 122, after mentioning, that Fevers, similar to Yellow Fever, have appeared "in some of the southern parts of Europe, during a season when the air was *intensively hot* and unwholesome," adds, "this happened at Cadiz, in Spain, in the months of September and October, 1764, when *excessive heat* and *want of rain* for some months, gave rise to violent epidemic, bilious disorders, resembling those of the West Indies, of which an hundred persons often died in a day." He afterwards gives a particular account of the symptoms, and then informs us, that "the dead bodies having been examined by order of the court of Madrid, the stomach, mesentery, and intestines, were found covered with gangrenous spots, the orifice of the stomach appeared to have been greatly affected, the spots upon it being ulcerated." He also informs us, that the dread of this disorder forced many people of fashion to retire into the country, where they remained in perfect safety from it." Reverting, again to this epidemic, at p. 161, he says "it did not extend its influence to any ship which lay at a distance from the city; as I am informed by Dr. Maguire, an eminent physician of that place. His Majesty's ship, the Tweed, was then at anchor in Cadiz bay; an officer, and several of her men, who had been on shore, were seized with this fever; but all those who were sent on board the ship recovered, no bad symptom appearing in their fever, whilst a disease, similar to black vomit, and the Yellow Fever, and equally mortal, depopulated that large city." From these accounts, it appears that the disease was not propagated in the country, and that seamen, who had imbibed miasmata on shore, and became sick, did not infect others when sent on ship board: and, consequently, that the disease was not contagious.—Nor does it appear that any suspicion then existed of its having been imported from any other place.

In the summer of 1800, this fever again appeared as a most formidable epidemic, not only at Cadiz, but in almost all the sea port towns of Andalusia, particularly Seville, St. Lugar, or Lucar, Puerto-Santa Maria, Puerto-Real, Rota, and L'Isla; occasioning the deaths, as is computed, of nearly eighty thousand persons. It also prevailed, at the same time, on the coast of Barbary, particularly at Tangiers and Tetuan. On this melancholy occasion, the government of Spain (which, some years before had dictated certain prescriptions, written by the king's physician, Dr. Masdevall, to be exclusively employed by the Spanish physicians in fevers similar to the present, and certain opinions to be also subscribed by them on the subject,) thought proper to order a long and circumstatial publication to be made as a supplement to the Madrid Gazette of the 28th of October, 1800, under the title of a "Description of the Epidemic Disease which began at Cadiz,—its origin and propagation—the different symptoms and effects of it, and the method of cure, &c. Published by order of government, for the instruction of the public, and particularly for the notice and *regulation of the practitioners of physic*, &c." (See Medical Repository, vol. 5, p. 103, &c.)

In this publication it is stated, that the preceding winter had been long and wet, the rainy season having been prolonged till the month of May; after which the summer became *intensely hot*, and the thermometer, from the middle of July, rose to 85° of Fahrenheit's scale;* that an easterly

* Soon after the epidemic in question had ceased, a pamphlet, written by Don Rodriquez Armesto, an officer of the Spanish navy, and entitled "Reflections on the Epidemic which prevailed in Cadiz, and the neighbouring towns, in the autumn of 1800," ("Reflexiones sobre la Epidemica padecida en Cadiz," &c.) was published,

wind succeeding, lasted forty days, and being (as is usual there) very hot, subjected the inhabitants to *extreme heat,* with sweatings so profuse that they could obtain no relief but by bathing. That, about the 8th of August, *bilious fevers* began to appear; and, from the 10th to the 15th of that month, many people in the quarter of Santa Maria were attacked with what proved to be the epidemic in question.

Among the more general symptoms of the disease, those are mentioned which occur in other marsh fevers—Those of the more violent cases are stated to have been Subsultus tendinum, Delirium or alienation of mind, Hiccough, convulsions, *hemorrhage at the nose,* bloody *vomitings, bloody and black stools, yellowness of* skin, *petechia,* and, finally, *black vomitings, similar* to those which, at certain seasons of the year, are *endemic at Vera Cruz, Honduras,* &c. ("La hemorrhagia de narices, la vomicion sanguinolenta por la boca, la melena, deyecciones de Sangre, la ictericia, las petechias, y ultimamente el *vomito atrabiliario,* à que han querido llamar *vomito prieto, semejante al que es endemico en ciertas est aciones del ano, en Vera Cruz, Honduras,* &c.) These symptoms,

after having been licensed in the usual manner. The author was, however, soon arrested on a charge of having diffused false, dangerous, and seditious opinions, and compelled to subscribe a formal retraction of them, so far as they were at variance with the creed which the court thought proper to adopt respecting the supposed importation and contagious nature of the fever in question. More than fifteen hundred copies of this pamphlet were publickly burnt; a few, however, escaped destruction, and Dr. Pascalis, going afterwards to Spain, found means to procure two of them, and has given an abstract of their contents in the New-York Medical Repository, vol. v. Hexade 2d, p. 131, &c. *By* this abstract, it appears that the author endeavoured to prove that the epidemic had been produced by atmospherical and *local* causes; and that for this purpose he adduced accurate meteorological observations, demonstrating, among other things, the extreme heat which had then prevailed." "These," says Dr. Pascalis, "were *official,* as he was, by royal authority, keeper of the royal observatory of the Isle of Leon:" and by these it appears, "that the thermometer marked 95 degrees," (instead of 85°) a degree of heat "equal to that of Senegal." I presume, however, that we are not to consider 95 degrees as the constant temperature of that season, but only as that which it sometimes attained in the *shade,* for in the *sun* he states it to have often been at 112 degrees, even in very *damp* places.

and especially those of which the original description is repeated in *Spanish*, can leave no room to doubt concerning the nature of the disease.

It is added that the most fatal symptoms were coldness of the extremities, feebleness of the eyelids, vomiting of matter like coffee-grounds, hiccough, convulsions, and coma. The account of the methods of treating the disease, and of the reasonings of the Spanish physicians, respecting its causes, are not sufficiently interesting for the space which they would occupy, were I to give even an abstract of them. It is stated, however, as a fact, that persons, *who had lately arrived from the West Indies*, owing to their being accustomed to the like seasons, *did not suffer an attack of this epidemic*, and that even those who had been *long resident in that part of Spain, were, in a great degree, exempted from it;* and, on the other hand, that *persons from Canada*, and other (northern) countries, *were extremely liable to it*, which are circumstances clearly manifesting its resemblance to Yellow Fever, and its want of contagion. The physicians are also stated, not to have observed pestilential buboes, carbuncles, or anthraces, in any of their patients: and, we are told that, though several were afflicted with phlegmonous tumours, ending in gangrene, others with erysipelatous vesications of like tendency, and some with *parotids*,* yet they were not of that kind which appertains to the *true plague*, and which had been described by Chicoineau, at Marseilles, and Samoilowitz, at Moscow.

In looking for adequate causes of this epidemic, we shall readily find them in the situations, and local circumstances of Cadiz,† and the other towns of Andalusia, in which it

* Parotids have been repeatedly mentioned by Lancisi, and others, as occurring in the marsh fevers of Italy, and they are sometimes observed in the yellow fever of the West Indies and North America.

† The town of Cadiz is upon a point of land, which, by advancing into the sea, forms within itself a spacious harbour. The external part of the city, or that which is nearest the ocean, is chiefly built on a rock, and is a little elevated. But the part

prevailed. About the end of July, the epidemic made its first appearance in the Barrio of Santa Maria, inhabited chiefly by New Castilians, who were generally poor and laborious, and it soon extended itself to the low and damp

which is eastward, and adjacent to the harbour, is placed on very low, damp ground, contiguous to marshes. Indeed, almost the whole country round the Bay is flat, low, and swampy, and the sides of the harbour are, moreover, covered by salt pans. An intensely hot easterly wind blew constantly in 1800, for six weeks, over the harbour, and over the other sources of miasmata just mentioned, conveying the latter directly upon the adjoining quarter of Santa Maria, at the south-eastern side of the town, where the streets are narrow and filthy, and where the epidemic appeared *first*, and continued *longest*. Thence it extended itself *westward*, exactly in the direction of the wind.

Puerta Real, Puerta Santa Maria, Rota, and the town of Isla, (which last is surrounded by salt pans) adjoin either the harbour or the bay of Cadiz, and they all partook of the epidemic, as might be expected, from their *low* situations, and other circumstances.

Not far from Rota is the river Guadalquiver, on whose left or northern bank is placed the city of Seville, round which the country, to a considerable distance, is *so low*, that, as Mr. Townsend has observed, (Travels through Spain, vol. 2, p. 353) "it is frequently overflowed, and, upon some occasions, the water has been eight feet high, even in their habitations. He adds, p. 356, that "the soil is rich, and being, at the same time very deep, its fertility is exhaustible," p. 357. That, "in consequence of vapours and miasmata, occasioned by stagnant water, and by frequent floods, the inhabitants of Seville are subject to tertians, and putrid fevers," (meaning the more violent marsh fevers.) What Mr. Townsend has mentioned of the soil round Seville is true, also of the whole country along the Guadalquiver, between that city and S. Lucar, a space of twenty leagues; and, therefore, we need not wonder that the towns and villages contiguous to this river should, in all ages, have been noted for the prevalence of autumnal marsh fevers, called by the Spaniards, *Tavardillos*, or *Tabardillos*, and resembling what, in the West Indies, are commonly named *bilious remittents*. These Tabardillos, indeed, were, in some extraordinary seasons, so much aggravated in this part of Spain as to be deemed the plague. (See Ray's Travels, p. 416.) Don Rodr. Armesto informs us, "that Seville is proverbially offered as an instance of *annual* (autumnal) *plagues*, where it never was thought necessary to establish rules of quarantine on any description of vessels." (Medical Repository, 2d Hexade, vol. 5, p. 131, and seq.) In an ancient work, by Doctor Juan Ximenes Savariego, printed at Anteguera, in 1602, and entitled Tratado de Peste, I find the authour mentioning, at p. xvii. as the cause of the fever which he calls *Tavardillo*, *pools* of stagnant and corrupted water, like that of lagoons and *inundations* of rivers, such as those of the Guadalquiver, at Seville, these late years. ("Estangues de Aqua estanqua, y corrompida, como de lagunas, y inundaciones de Rios, como las a avido de Guadalquibir en Sevilla estos anos passados.") He adds, that persons ill of this fever do not generally give streets of Sopranis and Boqueta, (near the sea gate) and thence to the quarters of Ave Maria, and St. Antonia; and having, by this time, attracted the notice of government, a meeting of physicians was convened, who, after consultation, unanimously agreed in reporting the disease to be a simple synochal fever, and *not contagious*. Indeed, the epidemic was so mild (as often happens at the early appearance of marsh fevers *without* the tropics) that even at a *third* meeting of the physicians, several of them declared they had not lost a single patient by the reigning disease, and many asserted, that of two or three hundred cases, not more than one or two had terminated unfavourable. At this meeting, however, a student of the College of Medicine, at Cadiz, Friar Juan de Acosta, belonging to the Convent of St. Juan de Dios, who had seen most of the sick brought into the Hospital of this convent, (adjoining the Barrio of Santa Maria,) declared the fever to be *very acute, and of a very bad sort*, but concurred with the others in thinking it not contagious.*

Some days after this, says Dr. Arejula, "we, physicians in Cadiz, began to observe these fevers more seriously and attentively. It was natural that, having called them *gastrico bilious* fevers, void of contagion, we should also believe the cause of them to be *general*, existing in the town; we recollected, according to the text of the great Hippocrates, and the observations of succeeding physicians, that much rain in winter and spring, followed by great heat in summer, like that which had been experienced at Cadiz, was the cause of fevers,

it to their attendants. When we consider how unusually the rains were prolonged throughout this part of Spain, in the spring of 1800, and the inundations which must have been thereby produced, together with the *intensely* hot and *dry* weather, which succeeded and lasted for a long time, we certainly need not be at any loss respecting the cause of this epidemic.

* See "Breve Descripcion de la Fiebre Amarilla padecida en las Andalusias," &c. or a Brief Description of the *Yellow* Fever which prevailed in the Andalusias, &c. from 1800 to 1804 inclusive, by Don Juan Manuel de Arejula, printed in 1806, at the *Royal Press* in Madrid, 8vo. pages 154. 156. 318.

epidemics, and the plague. We thought, moreover, that we had found another powerful cause in the kennels (Canerias) occupying the middle of our streets, and receiving all the foul water, and excrementitious matters, which we considered as sources of carbonic, hydrogenous, ammoniacal, sulphuretted-hydrogenous, and other unwholesome gazes." It seems, however, that, notwithstanding these opinions, the physicians of Cadiz, (as those of other places have often done) soon lost sight of the effects of miasmata, or rather ascribed these effects to a supposed contagion, extending from the bodies of the sick to the well. They observed, says Arejula, " that the person nearest the sick was commonly the first attacked with the disorder, and that, if it got into a house, all had it in a few days; that it proceeded from one to the next house, and thus extended the length of a street;" and this course, which, however, was neither constant nor uniform, they considered as a decisive proof of contagion; though, supposing what must have been true, that where the disease made this progress, the houses and persons were within the reach of miasmata, the facts in question might be as well explained *without*, as by the operation of a contagious influence.

At p. 248, Arejula endeavours farther to account for the spreading of the disease, by stating that the New Castillians, among whom the fever began, and who, being greatly attached to the *second* person of the trinity, and members of a fraternity, bearing his name, and believing that their devout and fervent supplications to him, would stop the epidemic, determined upon making a *solemn procession, with his image*, and obtained permission for that purpose, from the magistrates, though not until they had had recourse to menaces, which intimidated the government. The procession accordingly took place, about the 5th of August, an immense concourse of the people joining therein, and it lasted seven hours; during which time, these unfortunate people were exposed to the rays of a burning sun, and, under *great fatigue*, to all the mental agitations which religion or fear could produce; and this chiefly in those *parts of the town where the marsh miasmata were most powerful.* That a procession, in such circumstances, should produce a great extension of the disease can hardly be doubted; though not in the way which Arejula, and others, have supposed; for, as the *well,* and *not the sick,* joined in the procession, personal contagion was not likely to be present and active among them. Similar processions were continued almost daily, and, undoubtedly, with very mischievous effects, until Don Tomas de Morla, as Captain General of Andalusia, assumed the government, and put an entire stop to them; but the disease had then nearly reached its full extent.*

At *Seville* the fever first appeared in the *low,* unwholesome suburb of Triana, consisting chiefly of narrow *unpaved* streets, adjoining the Guadalquiver, but on the side opposite to the city. It next appeared in another unwholesome suburb; that of Los *Humeros,* also adjoining the river, and only separated by it from Triana; and thence by the middle of September, it had extended nearly over the whole city, occasioning, before it terminated, the deaths of fourteen thousand, six hundred, and eighty-five persons therein.

When the physicians at Cadiz had mistaken the effects of miasmata for those of personal contagion, (a mistake which, in that crowded city, and with its numerous processions, &c. it might have been difficult to avoid) the next step was to ascertain its origin. I do not find that, on any former appearance of Yellow Fever, in that city, its introduction, from any part of America, had ever been suspected, or that any precaution had ever been employed for its exclusion: though, if it had been a contagious disease, such precautions would have

* Don R. Armesto says the evil was augmented by making public fires in the Streets of Cadiz to purify the air, and thus producing an artificial heat, which precluded the salutary effect that might have sometimes resulted from a slight refreshing breeze: that it was augmented also by the dread and panic of reported contagion, and by numerous acts of religion, e. g. by the funeral processions which succeeded every death; by the "holy images, relics, and sacramental objects which were incessantly offered to the eyes of a dejected people," and by "*thundering* preachers solemnly warning every one of his approaching dissolution."

been highly expedient, considering how often the galleons, and other ships from Porto Bello, Havanna, and other parts of the West Indies, had been attacked by the disease, in returning thence to Cadiz; as happened to the squadron, &c. under Don Lopez Pintado, mentioned at p. 337.

But in the summer of 1800, the government, as well as the inhabitants, of Cadiz, appear to have adopted the belief of an importation of the supposed contagion from America, and a ship, called the Dolphin, belonging to Baltimore, was generally and decidedly selected and accused as having been the *vehicle* of this mischief: and reports were fabricated, by which three persons were stated to have died of Yellow Fever on board the Dolphin, during her passage, and what had been supposed to be the first cases of the fever at Cadiz, were declared to have occurred in different individuals, who had all *directly* communicated with the Dolphin, or some of her crew; and other sailors belonging to the same ship were said to have found their way up the Guadalquiver, through St. Lugar, (in which town however, the disease did not appear until the middle of September) and, by lodging in the suburb of Triana, at Seville, to have produced the Yellow Fever there, *some days before its appearance at Cadiz*. These stories, in point of detail and seeming accuracy, were such as Dr. Haygarth, by his letter of the 23d of May, 1799, solicited Professor Waterhouse to procure for him respecting the importation of Yellow Fever into Philadelphia, &c. and they were circulated generally, and with great confidence,* so that Don Pablio Valiente, Intendant of Cuba, who had chartered the Dolphin, to bring himself and his family to Spain, was, notwithstanding

* These stories were adopted, and most of them published, in substance, by Professor J. N. Berthe, of Montpellier, who was sent by the French government, with two other physicians (M. M. Pierre Lafabric, and Victor Broussonet,) into Spain, to ascertain facts and collect information, respecting the epidemic of Andalusia, in 1800, of which he has given a *Precis* Historique, in a large volume, 8vo. printed at Paris, in 1802. Some account of the stories, relating to the ship Dolphin, may be found at and between p. 52 and 59.

his rank and connexions, arrested upon a *criminal* charge, tried before the Royal *Audienza,* at Seville, and, after a full investigation, and *eleven months imprisonment,* fully and honourably acquitted of having introduced the Yellow Fever at Cadiz; and he was, probably, as a compensation for the injustice he had suffered, afterwards promoted by the government. In the course of this prosecution, it was juridically proved, that the Yellow Fever had not appeared at the Havana, whence the Dolphin sailed in May, 1800, until some time after her departure; and though she touched at Charleston on the 2d of June, and sailed thence on the 10th of that month, it was, (in consequence of an application from the Spanish government,) certified unanimously, at an extraordinary meeting of of the Medical Society of South Carolina, on the 5th of April, 1801, (twenty-two respectable physicians being present) that "to the best of their knowledge, no Yellow Fever had existed in that town, or in the Port of Charleston, prior to the 20th of June, in the year 1800." They also declared, on the ground of specified facts, their conviction that the disease in question *had never been propagated by contagion.* It was also proved, and particularly by the testimony of Don José Caro, a Spanish physician, who had returned as a passenger on board the Dolphin, and was examined, on oath, by the judges at Seville, that the diseases, of which the *three* sailors* had died on board of that ship, were *not of the nature of Yellow Fever,*† but different diseases, of which an account was given. It was, moreover proved, that no symptoms of the Yellow Fever had appeared in any person on board the Dolphin, and, consequently, that the disease could not have been introduced by that ship. Dr. Arejula has, therefore, deemed it proper to reject the stories concerning the Dolphin, and to confess that it was impossible to ascertain whence the

* Professor Berthe, at p. 340 of his volume, has multiplied these deaths to three times three, or nine.

† See Dr. Pascalis's Account of this Prosecution, &c. in the New York Medical Repository, vol. 9 p. 386, 7, and 8.

epidemic was derived. He, however, represents it as having been spread by contagion from Cadiz, to the other places where it prevailed, and, as having been exactly similar to the Yellow Fever of America, (see p. 153) in which his opinion agrees with that of Professor Berthe, Dr. Gonzales, and other Spanish physicians, by most of whom it is now called, "*la Fiebre Amarilla,*" or the Yellow Fever.

After the treatment which Don R. Armesto and his publication underwent, it can hardly be expected that any Spanish author would openly profess to disbelieve the contagious nature of this epidemic, or that I should be able to adduce Spanish authorities to support my own opinion on that point,* and I shall, doubtless be thought to have done enough, if, availing myself of the facts asserted or admitted (for other purposes) by those who represent the disease as being contagious, I demonstrate the *contrary* from these very facts.

Among the facts in question, one which has been much insisted upon by the contagionists, and particularly by Professor Berthe, is what may be called the *Geographical Progress* of the disease, which, though readily explained, by supposing it to proceed from miasmata generated in particular situations, and wafted in one direction by a long-prevailing wind, is utterly inexplicable upon the supposition of its resulting from personal contagion, because, in great cities, men do not always communicate, in the slightest degree, with their next neighbours, and they never communicate exclusively with these, but very often with persons at considerable distances,

* Don Armesto is not the only person who has been punished in Spain for expressing his sentiments on the subject of Yellow Fever. Doctor, now Sir James Fellows, who went from Gibraltar to Malaga and Cardiz, in order to procure information on this subject, after mentioning, in a letter to me, (dated Algesiras Bay, February 27, 1806) the obstacles which he had encountered, says, the greatest "was the mystery and secrecy with which all the information I obtained was enveloped."--I found the Spanish physicians very willing to afford me information, but I could not get them to tell me *all they knew;* for, in *this country, Doctors,* who give their opinion *too freely, about the nature of a disease,* are *banished,* as was the case of one at Malaga, and another at Carthagena."

and in various directions, by whom the effects of personal contagion would soon be felt, and spread on all sides. "It was distinctly observed, says Professor Berthe, at p. 74, that the malady affected to seize, with scarcely any interruption, all the houses which were situated on the *same side* of a street, and that it rarely passed over to the *other* side, where the streets were *wide*, and well aired. In some parts of the town, (he adds) the distemper has been seen to stop, as it were, for a time, as soon as it had reached to houses standing in a public square, and even to *retrograde*, with respect to the direction in which it had previously advanced, by appearing in the *adjoining* houses, rather than in those which were separated by the breadth of the square." These Mr. Berthe has strangely conceived to be clear indications of the contagious nature of the disease;—as if the *next* neighbours, on the *same side of a street or square*, had been the only persons in all Cadiz who visited, or approached, each other. And here I must remark, that, though the professor represents his supposed contagion, as so feeble and inert that it could not make its way from one side of a street or square to the other, he has most inconsistently described it in several parts of his work, and particularly in his Letter to the French Ambassador, at Madrid, as possessing an *incalculable activity*. And it is by *this*, and the supposed *rapidity* of its extension, that he endeavours to account for the nearly simultaneous appearance of the disease at places so distant as Cadiz, Seville,* and other towns and villages along the river Guadalquiver; an effect which could have been produced only by miasmata becoming abundant in those low situations, and acquiring maturity *nearly at the same time*.

Another proof of the non-contagious nature of this epidemic is derived from the fact, (admitted by Professor Berthe, and all the contagionists) of its not having spread in the towns or

* Dr. Pascalis asserts, that the epidemic first broke out "on the 23d of July, in the suburb of Triana, in Seville, a little before it was noticed at Cadiz." See Medical Repository, vol. 9, p. 389.

villages, which are at a small distance from the *low grounds* of the Guadalquiver, particularly the elevated village of *Alcala* de los Panaderos, which is distant only three or four miles from Seville, and takes its name from the occupation of its inhabitants, who are Bakers, and make all the bread consumed in Seville. "There was, consequently," says Mr. Berthe, (see p. 157, 8.) "a daily communication between *Alcala* and Seville, through a considerable number of individuals, and this communication was never interrupted, not even during the time when the distemper was committing the greatest havoc in the town;" and when, out of a population of 80,000 persons in Seville, above 76,000 were attacked by the Yellow Fever.* He adds, that, according to the report of the physician at Alcala, twenty-four persons had had the disease in that village, who *all brought it thither*; ("l'ont apporté du dehors") that eighteen of these had died; and *yet*, that in no instance had the fever been communicated *there*, from one individual to another.† Professor Berthe also men-

* When Sir James Fellows returned to England, in 1806, I mentioned to him, what Professor Berthe had stated respecting *Alcala*, and he confirmed the statement, as a fact, of which he had been informed, on good authority, in Spain; adding, that the like had happened, in 1804, at two villages near *Malaga*, principally inhabited by Bakers, who supplied that city with bread; the persons who brought and delivered the bread at Malaga, sometimes remained there all the following night, and, in that case, were afterwards very commonly attacked with the Yellow Fever, at their own houses; but *the fever was never propagated by them to any other person*. He made a visit to one of these villages, (Turriano) situated upon the *declivity* of a hill, westward of the Agual Medina, about five miles from Malaga—a situation which, being like Alcala, removed from all probable sources of marsh effluvia, may account for the non-appearance of contagion in those who sickened there, much better than a supposition mentioned by Sir James, of its having resulted from the burning, in the Bakers' ovens, of certain aromatic herbs, collected in the mountains. Had they burnt all the spices of the Molucca islands, I am persuaded they would have proved as useless, for any such purpose, as the fires made in the streets of Cadiz were found to be, in 1800.

† M. Berthe endeavours to account for the non-communication of the disease at Alcala, by supposing that the fires of the Bakers' ovens had produced a greater ventilation in that village, though, in another place, he acknowledges that the fires made in Cadiz, to produce a similar effect, were not of the least benefit; and, in towns where the true plague has become epidemic, the Bakers, instead of being

tions the small town of Scipiona, as one in which the fever did not appear, though but a few miles from *San Lucar*, where a sixth part of the inhabitants died of it. Scipiona had, however, the advantage of being higher and at a greater distance from the low grounds adjoining the river. M. Berthe also mentions the large elevated town of Medina Sidonia, between Cadiz and Gibraltar, at the distance of eight miles from the salt pans of La Isla de Leon, as another to which the epidemic did not extend.*

Another fact, stated by M. Berthe (at p. 69) on the authority of a principal magistrate at Cadiz, (Le Procurador Mayor Don Miguel de Irribaren) is, that, on the *day after* the great procession, which I have mentioned at p. 309, the number of sick was increased by between five and six thousand *new attacks:* and these are supposed to have been the effect of *contagion received during* the procession. But it is utterly impossible that any contagion yet known, should have operated so

exempt, have been found the greatest sufferers. This is mentioned, in regard to the plague at Toulon, in 1721. See Traité de la Peste, 4to. p. 49, 50. In my copy of this work, there is a marginal note to this part of it, in the hand-writing of *Dr. Russel*, in which he states, that "Bakers were remarkably subject to the plague at Aleppo, not from any peculiarity in the contagion, but from circumstances favourable to infection."

* It ought to be mentioned, that in the following year, 1801, when the Yellow Fever is understood not to have appeared in any *sea-port town* of Andalusia, it prevailed, to a considerable extent, in a *particular quarter* of the *inland town* of Medina Sidonia, and as may be presumed, from the agency of marsh miasmata, rendered active by causes which I am not able to explain, not being sufficiently acquainted with the local circumstances of the place, and the state of its atmosphere, at that time. There was, however, no suspicion of any *new* importation, of contagion, and even if the epidemic of the preceding year at Cadiz, &c. had resulted from contagion, and that contagion had been capable of subsisting in a dormant state, over the winter, and becoming active in the following summer, the effects of its activity would, doubtless, have been manifested in the places where there had been *most of it*, and where its ravages had been greatest in the year 1800, and not in a town where it had not existed. On that occasion, however, guards were employed to obstruct all communication with the sickly part of Medina Sidonia, and as the miasmata could only reach to a certain extent, the fever did not prevail in those quarters which were too remote, and the guards naturally had the credit of having kept the epidemic within certain limits.

suddenly, though it has been ascertained that marsh miasmata, in particular situations, do sometimes produce fever within even less than twenty-four hours; and that these *new* cases, as well as the epidemic, *generally* resulted from the latter cause is abundantly manifest from its similitude in every respect to what have been heretofore noted and ascertained to be the *peculiar characteristics* of marsh and Yellow Fever; especially the following:

1st. Its having been preceded by that state of weather which notoriously renders marsh effluvia most abundant and noxious. That this was the case, to a remarkable degree, is admitted by *all:* and Professor Berthe was so sensible of it, that, at p. 366, he does not scruple to admit, that if no contagion had been introduced at Cadiz, in 1800, the causes of disease *there,* and in other parts of Andalusia, were such that a violent bilious epidemic, or marsh fever, must have been produced by them, similar to that which was, at the same time, produced by these causes at *Cette,* and other places along the Mediterranean coast. He is, indeed, not willing to consider the fever at the latter places as *exactly similar* to the epidemic of Cadiz, &c. because he wishes to have it believed, that contagion had *co-operated* with the other causes in producing this epidemic, and he represents the fever at *Cette,* as not having been contagious. But, after attentively considering his own descriptions *of both,* it is evident that, at the utmost, they could only have differed in (a small) *degree,* and not in their nature.*

2d. By the greater prevalence and mortality of the disease, in situations *nearest* to the sources of marsh miasmata.†

* Don Rodr. Armesto considers the Mediterranean fever as resembling that of Cadiz, and says, the same epidemical constitution of the atmosphere extended along the Mediterranean, as far as Leghorn and Genoa, adding that, in the latter, "where 150 persons died every day of *Yellow* Fever, no American vessel could be accused of importing it, as Genoa was, long before, closely besieged by land, and blockaded by sea."

†Dr. Berthe, for himself and his colleagues, makes a *general* admission of this *fact* at p 161. "The epidemic, (says he) was singularly rapid in its progress, and *always most destructive, in low and humid situations.*" "I might, (he adds) cite on this subject the ravages committed by the distemper in several villages built on

This was strongly manifested in the Barrio of Santa Maria, which is stated, even by Mr. Berthe, to have been "*le premier Foyer*," of the disease, which, (he adds) in that spot, produced "*une mortalité effroyable*," (see p. 162.) The malignity was, indeed, such, that the proportion of deaths among those who were attacked, exceeded, by *ten times*, that of some other situations. Here the disease not only began first, but lasted, after it had ceased in all other parts of Cadiz. A similar difference, in respect of situation, was observed at *Seville*, where, according to M. Berthe, (p. 103) only one in eighteen of the sick died in the wider and more elevated streets, while, in those which were damp and low, as in Triana, and Los Humeros, the mortality amounted to one-third, and even to one-half; and this difference was observed, not only in regard to *streets*, but to *single houses*, in some of which, from their situation, the disease was much more *fatal* than in others. *Such wide differences would not have accompanied a disease produced by contagion.*

3d. By the influence of extreme *hot* weather, in exasperating this epidemic, and of *cold* weather, in mitigating, and finally producing a *cessation of it*. These effects were generally observed; and Professor Berthe has mentioned them distinctly, and in very strong terms, particularly at p. 154 and p. 324.

By the marked *severity* with which it universally attacked *all strangers* from colder climates, particularly those from

the *banks of* the *Guadalquiver*, and compare them with the *very different results* which it had in other villages at a small distance, *but farther from the river*, or standing on *rising ground*, at a greater or less elevation. We have procured the most accurate accounts in this respect, which it is useless to mention in detail, as they all resemble each other." This is in exact conformity with the experience of former times. Dr. Lecaan, who, in the reign of Queen Anne, was physician to a British army sent to Spain, in his "*advice*" to that army, p. 5, says, "It is generally observed, that all over Spain, *dwelling houses* or *towns* built near any *river side* are always unhealthy, and much worse near a *marshy ground*, where fevers or agues are very common, and more frequently *mortal* or *difficult to cure than in any other part of the world*."

England, Germany, and Prussia, as specified by M. Berthe, at p. 175, 323, and those from Canada, as mentioned in the publication made by the Spanish government. And, on the other hand, by its invariably sparing negroes, *Creoles*, and persons who, after residing for some years in situations between the tropics, had recently come from them, as is mentioned by Professor Berthe, at pages 166, 7, 8, and 9; also p. 323.

With all these *prominent features*, it is impossible not to recognise in this epidemic, a *marsh* or *Yellow Fever*, and consequently, a fever *destitute of contagion*.

A similar fever less violent, and much more limited in its attacks, occurred again at Cadiz and Seville, in the months of August and September, 1801, and, for some weeks, excited considerable alarm; but the weather proving to be neither so hot nor dry as in the preceding year, the fever did not increase, and was finally deemed a Tabardillo,* or bilious remittent, such as occurs to a greater or less extent, almost every year, in the southern parts of Spain, as well as on the coast of Barbary.

Though the Yellow Fever had, in the year 1800, prevailed at Malaga, and some other Spanish towns on the Mediterranean coast, it was with much less violence and mortality than in Andalusia. It recurred, however, at the first of these towns, with great malignity, in 1803, so as to occasion the deaths of 12,000 persons: and, in the following year, it prevailed there again with almost unexampled violence and fatality; it being computed that more than twenty-six thousand

* If I am not mistaken, the Tabardillo (though often used in a more general sense *strictly* means the fever, which Burlet, (in his Dissertation sur les Maladies des Espagnols, Ann. 1714,) has named Tritæophia Syncopalis; having paroxysms which return every day, but correspond *alternately* with each other, as in the double tertian. Riversius calls it Tertiana maligna pestilens. It is said not unfrequently to prove mortal at the second or third paroxysm. Dr. Pascalis says that, during the epidemic of 1800, the Spaniards, not being aware of the absolute *unity* of the disease, considered the milder cases of it as Taberdillos. See Medical Repository, vol. 9, p. 379.

of the inhabitants of Malaga, died in that summer and autumn. of this fever.

In 1804, as well as in the preceding year, this epidemic appeared first, and prevailed most generally and destructively in a *low Suburb*, called the *Barrio de Perchel*,* and in other contiguous *low* parts of the town, liable to great humidity by inundations, and percolations of water from the river *Guadalmedina*. And, as the summer of this year, in the south of Spain, France, and Italy, resembled, and even surpassed that of 1800, by its *excessive* heat, and great want of rain, so the Yellow Fever prevailed, not only in Malaga, but Cadiz, Gibraltar, Carthagena, Alicant, and even as far eastwards as Leghorn, and in all nearly at the same time.

It seems, however, to have appeared a few days earlier at Malaga, than at any of the other places, probably because, from *local* circumstances, the heat of this city (and especially with the *Terral*, or land wind) is often greater than in any other part of Spain. (See Carter's journey from Gibraltar to Malaga, vol. ii. p. 406.†)

* *Malaga* is situated at the *foot of a mountain*, and in a *very low valley*, through which a stream passes, called the Guadel-Medina. This is properly a torrent, which is sometimes nearly dry, but sometimes is so full as to overflow its low banks, and inundate several parts of the town on one side, and the whole of the suburb called the *Barrio d' Perchel*, situated on the other side. I have been informed by Sir James Fellows, who visited Malaga in 1805, that the *sites* of the houses in this suburb, and in many parts of the town, are from two to three feet *below the bed of the river*; and that, when the stream is full, the water enters into *all the lower apartments copiously*.

† He tells us at the next page, that in 1637, "20,000 inhabitants of this city died of the *plague*, which visited them again twelve years after and carried away the greater part of the citizens" Probably this plague was an epidemic Yellow Fever, like that of 1804. The only objection to this supposition is, that Mr. Carter mentions the plague of 1637, as appearing in the month of *May*, which according to the old style then used, would have extended to the 10th of June. But an extraordinary concurrence of circumstances has, in that part of Spain, sometimes produced *intensely hot* weather, even at an earlier period. The like happened at Charleston, South Carolina, in the year 1732, when a most violent and destructive Yellow Fever appeared there in the month of May, though that disease commonly does not begin to prevail epidemically in that city till August.

Early in August, the deaths had become very numerous at Malaga, and had produced great alarm: but a diminution in the heat of the atmosphere having lessened their number; the physicians, on the 14th of that month, subscribed a certificate, declaring that "no epidemic then existed in Malaga, and that the disease which had appeared, was only a sort of intermittent fever of a malignant character, similar to that which prevailed in other parts of Spain; and that its malignity was already so much abated that only five out of twenty then died, though, at its commencement, it had proved fatal to fifteen out of twenty!" (a strong proof, indeed, of malignity!) It happened, however, that almost immediately after this certificate, the weather again became *intensely hot*, and the deaths increased so rapidly, that, on the 21st, they amounted to 148 in that single day. The fever was then deemed not only contagious, but pestilential; and effectual measures were unfortunately taken to cut off all communication between the city and the country, by which the miserable citizens were compelled to remain exposed to the morbid exhalations, which caused the disease, and ultimately to *perish by it*, (as the greater part of them did) and, in the mean time, they were deprived of the necessary supplies of food.

As the disease had made its first appearance this season at *Malaga*, and as no body ventured to doubt of its contagious nature, all the other Spanish towns (fifteen in number, besides villages) where it soon began to rage, were presumed to receive the contagion from the former city;* and (which is perhaps, still more extraordinary) *Arejula*, who admits (p. 153)

* Dr. F. Pascalis mentions (Medical Repository, vol. 9 p. 391) that the physician who, at the beginning of the epidemic, in Malaga, in 1804, first announced its true character, "became so disgraced as to be compelled to exile himself. In three days he arrived in Cadiz, where his servant soon died with Yellow Fever" This happened exactly at the time when other sufficient causes had rendered the miasmata active at Cadiz, and, the Yellow Fever soon becoming prevalent, as in 1800, it was ascribed to this servant, and, of course, to contagion, not miasmata, received by him at Malaga.

that this was the true Yellow Fever of America, pretends that the contagion of it was introduced at Malaga, by *two brigs* which entered that port, not from America, but from Marseilles, on their way to St. Domingo; so that the deaths of 120,000 persons by this disease, during that year, in Spain only, are thus derived from a *French* port, where quarantine regulations are executed with the great exactness, and where no such disease existed. But, after having so often proved that this fever is void of contagion, and incapable of importation, I shall not be expected seriously to examine this charge against Marseilles, nor the allegations respecting other places said to have become infected by sommunicating with Malaga; all of them, so far as I have been able to ascertain their local circumstances, having had within themselves such sources of marsh miasmata, as in such an extraordinary season might, with the experience of former years, have been expected to prove highly morbific.* Instead, therefore, of exhausting the patience of my readers, by describing sources of marsh miasmata in Spanish towns, where their existence has been proved by the frequent recurrence of marsh fevers, I will proceed directly to a place where these sources are less obvious, I mean

GIBRALTAR.

And here it is to me a matter of regret, that in describing the situation, and *local peculiarities* of this interesting spot, I I am, in regard to many circumstances, compelled to rely on my own observations, made at times, when, not foreseeing

* Carthagena, besides other sources of miasmata, has within about a quarter of a mile of its bastion, a very extensive swamp, called the *Almojar*. Mr. Townsend, at p. 137, of the 3d volume of his Travels in Spain, says of the diseases of Carthagena, that "the most endemical are intermittent and putrid Fevers. These arise from the *proximity of the extensive swamp*, already mentioned, containing many hundred acres, which might easily be drained, so as to produce the most luxuriant crops."— He adds, "in the year 1785, during the three *autumnal* months, they lost 2500 persons, and, in the succeeding year, 2500 more; yet the Almojar remains undrained." To these deaths may be added 14,940 others, produced in 1804 only, by the same marsh fever, aggravated into the form of Yellow Fever.

their connexion with this subject, my examinations and inquiries were not so minute and particular as they would otherwise have been.

The town of Gibraltar is built upon a narrow strip of nearly *flat* ground, extending along the foot and western side of a stupendous rock, (in some places between 2000 and 3000 feet high) which, at its summit, is very sharp, and runs as a longitudinal ridge, nearly a mile and a half from north to south.

This rock, on its eastern side, forms an abrupt and almost perpendicular descent, from top to bottom; but, on the western side, towards the town, its declivity is but moderately steep, presenting, according to my best recollection, an inclination of about thirty-five degrees. So that all the rain discharged from the clouds, attracted and arrested by the summit of the rock, and falling upon a declivous surface of, I believe, at least 400 acres, excepting what escapes by evaporation, or is absorbed by the scattered palmettos, grasses, and plants which find means to grow on the rock, must descend to the town *in addition* to that which falls directly upon its *own surface*. But a part of this rain water is intercepted at the bottom of the declivity by a large ancient stone Aquæduct, five feet deep, and covered by an arch; having on that side, which is nearest to the acclivity of the rock, a considerable number of apertures, called weep holes, through which the descending water, after percolating through the sand, enters the Aquæduct, for the use of the garrison. This Aquæduct begins about half way between the south portgate and the new barracks: and is continued close along the side of the road, into the middle of the town. It, however, now receives less water than formerly, because General O'Hara, to form a parade for exercising the troops, caused a ridge of sand which ran parallel with the Aquæduct, and through which water was percolated into it, to be levelled and covered by a mixture of slate, hard clay, &c. which have formed an impenetrable surface, so that a considerable portion of the falling water, which used to find its way into the Aquæduct, now *runs over it*, into the town. How far this change may have contributed to increase the quantity, or virulence of the miasmata, in the summer of 1804, I am unable to decide.

The whole Peninsula of Gibraltar consists of a rock, covered more or less, in particular spots, with earth. Consequently, the town itself is placed on the rock which extends even to the water's edge; and this being nearly impenetrable by water, must, in that respect, produce the effect of a substratum of hard clay, and hinder the escape of moisture, excepting only those excesses of it, which the soil cannot absorp or retain, and which must, consequently, descend to the ocean, whose level is, I believe, thirty or forty feet below that of the streets, running generally with a small descent towards it. The town, therefore, though elevated in regard to the sea, is *very low* when compared with what may be called the mountain behind it: and, as the great quantities of vegetable and other matters constantly brought into the town, to supply the various wants of the garrison, and the other numerous inhabitants, must even from their refused or useless parts, afford matters sufficient (with water) to produce abundant miasmata, especially when assisted by the fragments of vegetables growing upon, and frequently washed down, from the mountain,* we might, I think, very reasonably expect in that climate, and in such a situation, that marsh fevers would sometimes become prevalent.† It is, however, I believe, true that agues do not often occur in the town of Gibraltar; probably, because in ordinary seasons, the quantities of rain water which either fall

* Sir John Pringle, in his work on Hospital and Jail Fevers, (p. 46) says, "I have observed, in a *fixed* camp, that the rottenness of straw concurred to affect the health of the men, as was visible by a *general convalescence* upon changing the ground."

* I have already referred to Dr. M'Lean's testimony that fatal miasmata arise where there are no very certain appearances of marshy soil, and to the instances of this which he adduces from the fevers at Cape Nicholas Mole and St Mark's. Many others might be added to these, if necessary. Even Dr. Chisholm, at p. 122, of his second volume, has mentioned a production of Yellow Fever, in 1798, at Fort Edward, in Martinico, by percolations of water, through the sides of a bank, lodging under "the floor of the barrack."

directly upon the town, or find their way into it, from the declivous surface of the rock or mountain, are so great, as to wash into the sea, the decomposing organized matters contained in the soil, before they have had time to form *noxious* miasmata; and it seems to be only in seasons when there is but little rain for six or eight weeks, that such miasmata can, in Gibraltar, acquire maturity and force sufficient to manifest themselves extensively.

But, besides the materials necessary or conducive to the formation of miasmata, there is, at Gibraltar, sometimes a cooperation of causes suited to render them extremely powerful and virulent, especially when, as in 1804, an intensely hot levant or easterly wind prevails during a great part of the summer, with no rain excepting a few slight showers, just sufficient for the extrication or evaporation of these miasms in a state the least diluted, or most concentrated with such a wind intercepted by the perpendicular acclivities of the mountain on its east side, the atmosphere of the town of Gibraltar would remain nearly *stagnant*,* and the exhalations from the earth, instead of being blown upon the ocean, would be left to accumulate in the narrow streets, and produce the most violent form of marsh fever. To these causes, we may add the great augmentation of temperature, occasioned by the rays of an unclouded summer's sun, reflected upon the town, during the hottest part of the day, from the acclivities of the rock.—Even the ordinary temperature of the atmosphere in this town appears to be *very great.* Dr. Lind (on Preserving the Health of Seamen) says, " the heat *at land,* in Gibraltar, exceeded that in the ship upon the water, by eight or ten degrees and also that of *Oran* in *Africa,* by six degrees ;" and he adds, " that the common heat, during the summer, in the garrison

* Dr. (Lind, on Preserving Health in Hot Climates, p. 117) writing of Port Maho, which is almost surrounded by high mountains, in the Bay of Mexico, says, " the stagnated air thence becomes so unwholesome, that men, after being there a few days, are suddenly seized with violent vomitings, head-aches, delirium, &c. and, in two or three days more, the dissolved mass of blood issues from every pore."

of Gibraltar, is from 79° to 87 degrees. See p. 168. We need not, therefore, wonder, that though agues do not often occur at Gibraltar, the more violent forms of marsh fever frequently prevail there, during the summer and autumn, as is well known to be the case.

The late Dr. Donald Monro, in his Observations on the means of preserving the Health of Soldiers, vol. 1, p. 23, says, "at this place (Gibraltar) June, July, August, and September, are constantly hot, and the two last sultry; and in these months the garrison, and inhabitants, are subject to *bilious* and *putrid** disorders; but *new comers* seldom escape, and have them in a *violent degree.*

This statement by Dr. Monro, having been shewn by me to Sir James Fellows, on the 3d of June, 1806, was by him confirmed, on the ground of his own experience, at Gibraltar, in the preceding year, and also on that of documents which he had collected there respecting the state of health of the garrison in former years.

Mr. Pym, Garrison Surgeon at Gibraltar, and long a resident there, mentioned to me on the 27th of November, 1808, that, during the hot months, persons died there every year of fevers, which, by his description, resembled the bilious remittent or Yellow Fevers of the West Indies; the patients often becoming yellow before death; and, it appears from the 3d volume of Dr. Trotter's Medicina Nautica, (p. 420 and seq.) that a similar fever prevailed, to a considerable extent, at Gibraltar, in the autumn of 1799.†

* The words "bilious and *putrid*," as here used, were apparently intended to signify fevers resembling the Yellow Fever of the West Indies, though, perhaps, less exasperated.

† In the year 1766, orders from his Majesty being sent to the governors and commanding officers at Gibraltar, Grenada, Antigua. Jamaica, Senegal, and North America, "to transmit a report of the most eligible season for landing troops in each of their respective districts, so as to avoid, as much as possible, the inconveniences of the climate," answers were returned to this requisition by the several governors, under their signatures, after proper consultations and inquiries: and a copy of these answers having been transmitted, by the Adjutant-General, on the 29th of May, 1809,

With such evidence of the morbid effects of marsh miasmata at Gibraltar, there can be no doubt that, however produced, they often exist in that place, during the summer and autumn; and when, by the unprecedented heat, and drought of these seasons in the year 1804, the marsh fevers of Cadiz, Malaga, and other towns, but little removed from Gibraltar, had been converted into the most violent epidemical Yellow Fever, can it be surprising that this should also have happened at the latter of these places?

Many stories, contradicting and refuting each other, were confidently propagated respecting the supposed introduction, at Gibraltar, of the contagion to which this was by many attributed.

Among these stories, one derived it from Cadiz, by means of a Spaniard, named Sancho. Another from Malaga, by a different Spaniard, named *Santos*. Others designated individuals, with different names, from these places, as having done the mischief. That some persons arrived, and sickened at Gibraltar, after having imbibed the noxious exhalations of Cadiz, and Malaga, may be true, and, also, that this happened about the time when miasmata, similar to those which had occasioned the epidemic in those cities, were beginning to operate at Gibraltar, may also be true; but this is all that can be said with truth.

The accounts are almost as contradictory, in regard to the particular *time* and *spot* at which the first case or cases of this fever appeared in the town of Gibraltar. This contradiction, which could not have happened, if the disease had originated from an imported contagion, would naturally occur in regard to an epidemic arising from miasmata, which beginning to act, often at several places, and always in dif-

to the army Medical Board, for the *re-consideration*, I copied the following extract, relating to Gibraltar, (which has also been noticed by Dr. Monro) viz. "Gibraltar, from the middle of November to the latter end of March, the *best* time for *landing*; June, July, August, and September, the *worst*; the garrison being *very subject* in these months to *bilious and putrid fevers*."

ferent persons, *about the same time,* and producing in them diseases of various degrees of violence, it must have been difficult, (as it has been found to be in New York, Philadelphia, &c.) to draw a line of separation, between the common bilious remittent, and that which, by being a little more severe, were mistaken for a *new* fever; especially as these marsh epidemics commonly begin with the milder forms, and increase by almost insensible gradations. Hence some accounts represent the epidemic in question as having first appeared at Gibraltar about the tenth of August, and others, as having begun about the 8th or 10th of September, and this in different individuals, as well as places.

It appears, however, that the fever began to attract particular notice in several houses near the governor's parade, a little before the middle of September, and soon appeared in so many others, that it was found utterly impossible, as might be expected, in an epidemic from such a cause, to trace any sort of connexion, in regard to its progress; and though most of my information has been obtained from gentlemen who had believed in the existence of contagion, yet that information warrants me in asserting, that no one fact has been substantiated, to prove that there was a single instance, in which the disease had been communicated from one individual to another: Indeed, it must have been difficult, in a place so *confined* and *crowded* as Gibraltar was, to have distinguished between the effects of miasmata and those of contagion, otherwise than by the greater rapidity with which fevers, from the first of these causes, generally appear to spread within certain limits, as then happened at Gibraltar. But I do not find that any experiment was devised, or pains taken, for the purpose of ascertaining the truth, had it been practicable, on this point. Dr. Nooth, an army physician, of great experience, as well as learning, who was then at the head of the medical department in Gibraltar, had, during his long services in different parts of America, become well acquainted with marsh fevers, in their several forms, and he was soon convinced that an exact similitude existed between the most violent of these, and the disease then prevalent at Gibraltar, and, consequently, that the latter was void of contagion; and, though persons less acquainted with these fevers, and, therefore, less qualified to decide respecting that of Gibraltar, very generally, concluded the latter to be contagious; and, probably, for no better reason than the fallacious one of its spreading epidemically, Dr. Nooth, as I am informed, did not see any cause to change his opinion on this subject.

In regard to the symptoms of this fever, they were, in every respect, similar to those of the epidemics then and lately prevailing at Cadiz, Malaga, &c. and, therefore, all my observations on the latter will be applicable to the former. It began at the time when marsh fevers are commonly most prevalent, and was preceded by that intensely hot and dry weather, which renders them most violent, and which would have destroyed a typhus or contagious fever: and it was extinguished like other marsh fevers, by the rains and cool weather of December, which would not have extinguished any contagious fever yet known: like the yellow and other marsh fevers, it attacked, with the greatest violence and mortality, persons from cold climates; and either did not affect, or affected but slightly, persons who had resided for a long time between the tropics, and had but lately quitted that residence. Thus it appears, by the official returns, that the 10th regiment, lately arrived from the East Indies, by the way of Egypt, though 748 in number, besides commissioned officers, lost only twenty-eight men during the epidemic; and of these, the greater part were unseasoned *recruits* sent to Gibraltar from England: whilst, on the other hand, the regiment of De Roll, consisting principally of Germans, who had not previously served in any hot climate, lost 187 out of 605 men: almost a third of their number. The mortality was greatest on the 2d of October, when nearly 150 died. After the 1st of November, it diminished greatly.

Strangers arriving at Gibraltar, while the fever prevailed, were most commonly attacked with it on the 2d, 3d, or 4th day, after their arrival; a space much too short for the operation of typhus contagion. Mr. Pym, Surgeon to the garrison, and now Deputy Inspector of Hospitals, who had read and believed Dr. Chisholm's account of the alleged introduction of a *new* and highly contagious fever at Grenada, by the ship Hankey, imagined the epidemic at Gibraltar to resemble that fever; and he endeavoured, in a conversation with me, to explain how it might be distinguished from the ordinary bilious fever of Gibraltar; and this was by an aggravation of symptoms similar to that which Dr. Chisholm has mentioned as having distinguished the supposed Hankey fever from the ordinary Yellow Fever of the West Indies. Expecting, as I do, to demonstrate, in my seventh appendix, that *no fever*, of any kind, was derived from the Hankey, and that no such contagious fever as is supposed, ever existed, it cannot be necessary that I should here discuss that subject.

In the year 1810, a state of weather, similar to that of 1804, in regard to heat and drought, subsequently to a very wet spring, occurred at Gibraltar, Cadiz, Carthagena, &c. and re-produced the Yellow Fever at all these places; but, as this state of weather began later, and lasted a shorter time, than in 1804, the disease was not, in *many cases*, so violent, nor did it prevail so extensively, or so long, as in that year. The worst cases at Gibraltar appeared in a few transports, upon their arrival from Carthagena, where their crews had been exposed to more virulent miasmata than those at Gibraltar.*

* Most of these cases fell under the care of Mr. M'Arthur, Assistant-Surgeon of the th Veteran Battalion, who, in giving an account of them, dated October 29, 1810, writes as follows: "After carefully observing the symptoms and progress of the fever that lately raged here, and comparing it with the accounts of the malignant Yellow Fever that prevails in the West Indies, I am clearly of opinion, that it is *exactly the same disease*. In its symptoms and character, it almost accurately corresponded with the description of that disease given by Mosely. N B. Dr. Moseley's description was written long before the Hankey was built, and is generally well suited to the *severer* forms of the Yellow Fever in America.

In the town of Gibraltar, some persons died of the fever in three or four days, but many of the cases were milder, and so much like the ordinary bilious remittent, that some doubted whether it was similar to the fever on board the transports. Others imagined that there were two sorts of fevers in the town, one contagious, and introduced by some means or channels unknown, from the transports; and the other, an indigenous, marsh, or bilious fever. Nearly the same variation, in the degrees of violence, occurred in the fever at Cadiz, and would naturally occur to marsh fevers, and to no others in such a season. The following is translated from a declaration, dated Cadiz, November 2, 1810, and signed by Sir James Fellows, and nine Spanish physicians, viz.:

"We, the undersigned physicians, having deliberated on the questions proposed to us by the Supreme Junta, relative to the nature and symptoms of the disease now prevalent, have agreed that it is the same in kind as that which raged here in 1800 and 1804, but that it is less frequently malignant and contagious; we having observed, in many of the sick, disorders of a different character, which cause the reigning fever to be less intense and infectious."

How a contagious fever, in one set of patients, should become less contagious and intense, because others had different disorders at the same time, I do not understand—probably these other disorders were remittent fevers, or *Taberdillos*. These Spanish physicians, however, seem to have become less confident than formerly, in the supposed contagion of their epidemic; and I am well informed, that but a day or two before the date of this declaration, Sir James Fellows had decidedly expressed his belief, that it *was not contagious*, though he formerly entertained a different opinion of the epidemics in Spain and at Gibraltar.*

* Whether Sir James Fellows would ever have entertained such an opinion of these epidemics, had he been left, by Dr. Haygarth and others, to the unbiassed conclusions of his own reason, may, I think, be questioned. And I think it right to mention, as an instance of Dr. Haygarth's confidence in his own judgment, respect

Probably my readers will agree with me, that my *view* on this subject may *now* be closed with propriety. I might, indeed, have taken into it hundreds of facts, in addition to those which have been recently noticed, and of *similar import;* but I think too highly of their understandings to suppose that such additions can be necessary.

There are but few things more obscure or fallacious than the subject of febrile contagion. The matters, (whatever they may be) which produce contagious fever, being like marsh miasmata, imperceptible by our senses, and like them applied to the body through the medium of the atmosphere, we can never *directly* trace either of these morbific agents *backward to its source,* so as, by that means, to ascertain whether it had emanated from a *person* labouring under fever, or from the *earth.* Fortunately, however, the fevers, occasioned by these several causes, are distinguishable from each other by certain characteristic marks and peculiarities, with which, by long experience, and numerous observations, we have been made acquainted, and thereby enabled, with cer-

ing a disease which he had never seen, (and of which his notions were most erroneous) and of his solicitude that others should *adopt* and *act exclusively* upon his opinion, that, in a letter to a member of the late Army Medical Board, and dated December 16, 1804, he wrote as follows, viz.: "Can you think it proper to leave upon the medical staff, *a single person* who *doubts* whether this pestilence, (the fever at Gibraltar) and all the calamities which it has occasioned, were *solely produced by contagion?* Every doubt of this kind will hinder the vigorous measures which will be required for the thorough cleansing all the houses, boxes, furniture, and clothes, from every particle of the infectious poison. Every hint suggested to discourage the necessity and utility of such purification, will destroy the *salutary confidence* which, in truth, ought *universally* to prevail." I hope, and, indeed, believe, that Dr. Nooth's retirement from active service was with him a matter of choice, and not an effect of this letter; but I must think that Dr. Haygarth ought at least, to have had some *personal* knowledge of the disease in question, and of the facts connected with its origin, before he attempted, on that ground, to deprive the public of the services of gentlemen who *did possess that knowledge,* together with *as much intellect* as himself, and who were therefore, *better qualified and entitled* to form opinions on the subject. I certainly do not impute to Dr. Haygarth any other than benevolent intentions in this exercise of his zeal; for I believe such intentions have often actuated the most violent zealots, whilst persecuting others, *even to death,* to compel their assent to articles of faith.

tainty, to refer each of these fevers to its separate cause. We are, moreover, often enabled, in some degree, to ascertain, by a proper attention to facts, which of these different causes, or rather which of their sources, has been sufficiently approached by an individual or individuals, for the production of disease; and thus to discover the father, by the features of the child, and by the *exclusive* opportunities of access which the father has enjoyed.

My readers will recollect, that all these means have been employed by me in regard to the *yellow*, and its *kindred marsh* fevers, especially in the view which has been just closed, by which it has been clearly seen, that they are all the offspring of one parent; and a solid foundation has been thus acquired for presuming, that the yellow, like other marsh, fevers is void of any contagious quality.* It has also been seen that, in many thousands of instances, the Yellow Fever has *clearly manifested and proved itself to be completely destitute of contagion.*

* Those who have imagined that marsh fevers *might become* contagious, have, probably, never reflected upon the *monstrous effects* which, in that case, would necessarily have ensued. If the small-pox, measles, &c. had not been rendered contagious, these diseases would have *existed only* in the persons *first attacked by them*. and the purposes, which they were intended to fulfil among mankind, must have failed. But the cause of marsh fevers exists to such an extent, and so permanently, that, probably, the loss of human life occasioned by it, without any aid from contagion, exceeds that of all other diseases incident to mankind: and if fevers produced by marsh miasmata could acquire a contagious power, and thereby produce other fevers, in addition to those which their original cause will, doubtless, continue to produce abundantly, and *without end*, such enormous additions, to the widely-extended and powerful mischiefs of marsh miasmata, would long since, in the ordinary course of things, have exterminated the human race. For the coldest habitable regions would not, in that case, have afforded an asylum to mankind, as they now do, from the evils of marsh miasmata. Because that *monstrous* production of fever, from the *contagion* of *marsh fever*, would be enabled to reproduce itself as a *contagious fever*, even in Lapland. And of what description can we suppose such a fever would be? Does any one conceive that a fever, resulting *immediately* from the action of marsh miasmata, would resemble one produced by a different cause, i. e. the *contagious* quality which had been *acquired by a marsh fever?* This, and many other absurdities, might have been avoided, by attending to one *great fundamental truth*; viz. that *no disease is ever contagious, unless* it has *originated from contagion.* And that *contagious diseases can only be produced by their respective contagions.*

And, as truth is *invariable*, and the same disease, in suitable circumstances, is *always* or *never* contagious, we may safely rely on this immense mass of evidence, and *conclude*, that the few instances of a contrary *appearance*, (and there are but very few which have not already been shewn to be founded in error or misrepresentation) were observed and reported imperfectly, or under the influence of prejudice, or deception.* This *conclusion* will, I hope, produce important benefits to mankind, notwithstanding the *outrageous criminations* by which Dr. Chisholm has endeavoured to obstruct all free inquiry, and frighten or overwhelm his opponents on this subject. (Letter to Haygarth, p. 159.) Great as the evils may be, of mistaking a contagious for a non-contagious disease, those produced by the opposite error, are, at least, of equal magnitude: though, under the influence of terror, the former have been seen with microscopic eyes, and the latter, in a great degree, overlooked.

Without insisting upon those expensive delays and embarrassments, by which useless quarantine laws shackle the commerce, and naval operations of a great maritime country, I

* I have already given instances of very erroneous inferences in favour of the supposed contagion of Yellow Fever, from facts which, rightly understood, were capable of proving it to be *non*-contagious. The following is an instance of such an inference, from a fact which at the utmost, is but equivocal, viz. Dr. Dancer, of Jamaica, in a note to his "Observations on the Contagiousness and Importation of Yellow Fever, in the New York Medical Repository, vol. 7, p. 253," says, "agreeably to the information I have received, from several country practitioners, the crews of ships stationed at the *out-ports are generally healthy until one or more persons fall sick*." *A truism* which seems applicable to ships at the *in-ports* also. He adds, "but, as soon as a single instance of Yellow Fever occurs, the disease spreads from one person to another, till it goes through the whole ship, and afterwards from ship to ship." But this, which Dr. Dancer mistakes for a proof of contagion, is only, what might be expected from miasmata, among ships lying within their reach. From various circumstances, their morbid effects appear sooner in some persons and situations than in others. Some will, therefore sicken earlier and others later; and Dr. Dancer's mistake consists in supposing that the person who first was attacked, gave the fever to the second, &c. while, in fact, they all drived it from one common source. The like mistake has been made by many others; and even by Dr. Lind, at p. 184, 5, of his work on Preserving Health in Hot Climates.

may ask, if it be an harmless error which misleads mankind respecting the cause of an epidemic, and not only induces them to employ vexatious precautions to guard against imaginary dangers, but also to neglect the only proper means of future preservation, by meliorating the condition of the places they inhabit? Dr. John Hunter has well observed that, "by supposing a fever produced by marsh effluvia to be the effect of contagion, an army may be left to perish, in an unwholesome situation, which might have been saved by removing to one where such effluvia did not exist." (Diseases of the Army, p. 320.*) And it is notorious that, during the late epidemics in the south of Spain, an unfortunate exercise of civil authority, occasioned the loss of many thousands of lives, by compelling the inhabitants of the towns,

* It is not always in the power of a general to choose or change his situation, especially when his army is weaker than that of the enemy. But when this is the case, there is good reason to believe that the morbid effects of marsh miasmata may, in a very considerable degree, be obviated, by administering the Peruvian bark, with apsicum, or ginger, allspice, and bitters, copiously to those who are in apparent health. In September, 1809, when an application was made to me by the Surgeon-General, with the sanction of his Majesty's ministers, to join Dr. Blane in the mission then proposed for Walcheren, I fully intended, if possible, to procure a fair trial of these *prophylactic means*, upon the British troops there. And when I was hindered from joining in that mission, by causes which it would be improper to mention here, I communicated my ideas and wishes on this subject to the Secretary of State for the War Department; in which, however, a change was then taking place; and from that, or some other cause, my communication remained without effect. It would have been my desire that each soldier should take night and morning a drachm of the bark in powder, with half as much of powdered ginger, or allspice, or an equivalent portion of capsicum, in a moderate glass of rum, brandy, or gin, or half a pint of porter, for the space of a fortnight; and this I would have repeated, after the *interval* of a week, again and again, till the beginning of December, when the noxious influence of marsh effluvia would have ceased to operate, until the month of May, and *then* I would have recommenced a similar course, but with smaller doses of the bark, &c. which would, I think, have been sufficient, if begun so early in the year. By such means I certainly should have expected to save many lives, and obviate the necessity of abandoning that island. And this expectation would have been founded upon several facts published by Dr. Lind, Dr. Robertson, M. Alphonse Le Roi, &c. and upon others not published, which have either occurred within my own knowledge, or been communicated to me, upon unquestionable authority.

where it prevailed, to remain in their morbific atmosphere, lest, by quitting it, they should infect others.

These, however, are but part of the evils resulting from a dread of contagion, where it has no existence. Few in this country have heard or can conceive how often, and to what an extent, the strongest and best ties which unite and benefit mankind, have been cruelly broken, within the last twenty years, in some parts of America and Spain, by persons acting under the terror of imaginary dangers; and driven by it, to abandon their homes, their occupations, and even their nearest relatives and dearest friends, in the hour of sickness; and, by this desertion of the duties of humanity, this denial of that assistance, and those consolations which might have been afforded, without the smallest danger, to render these visitations of disease incalculably more afflicting and fatal than they otherwise would have been. Don R. Amesto, in the work heretofore mentioned, asserts that the barbarous and anti-social belief of the importation and contagion of Yellow Fever, has, from its baneful influence in Spain, caused many unfortunate victims to be abandoned, and left to starve in their beds. That others have been *shot at the very doors of houses* in which they endeavoured to find an *asylum;* and that many others were carried alive to their graves." Let the zealots, who have contributed to this monstrous inhumanity, reflect upon it, and if their intentions have, as I hope, been good, let them, at least, maturely examine and re-consider the *foundations* of their belief, before they again endeavour to carry it into *action.* It neither requires, nor indicates even a mediocrity of understanding, or learning, to adopt the common notion of contagion and foreign derivation, in regard to epidemics; nor does the facility, with which these notions are propagated, afford the smallest presumption in their favour. By much the greater part of mankind do not possess either sufficient industry or knowledge for the due examination of a subject so intricate, and complicated, nor have they so much of the power and habit of close, and accurate, reasoning, as is necessary to decide respecting it. Every one, however, can believe, and the belief of contagion affords a ready solution of all difficulties, without the trouble of inquiry or even of thought; and we need not, therefore, wonder that contagion, like witchcraft, should have been universally believed, (though as little understood) and often with as little foundation.

END OF PART IV. AND OF THE ESSAY ON YELLOW FEVER.

# PART FIFTH.

## CHAP. I.

### OBSERVATIONS ON TYPHUS, OR CONTAGIOUS FEVER.

Though the Greek noun Τυφος (stupor,) was applied by Hippocrates to several diseases, all of them very unlike the fever in question, (which probably was unknown in Greece,) I shall not object to the name, as distinguishing those fevers which accord with Dr. Cullen's definition of this *genus.** But I think there is great reason to object to the vague and loose application of it, which is now become frequent, to designate generally all low or slow fevers, arising from great fatigue, cold, and damp habitations, unwholesome, or insufficient food, anxiety, grief, fear, and other depressing passions, and debilitating causes, which have *no connexion with contagion, nor any power of producing a contagious disease.* I believe in the existence of a fever, *sui generis*, strictly contagious, (unconnected with any of the exanthematous diseases,) and, therefore, according to my view of the subject, derived exclusively from its own *specific cause*, or contagion. In this, which I consider as the *only* contagious fever, there are, I think, some varieties; but without any differences sufficient to form more than one species.—The facts and

* Cullen. Gen. Morb. p. 71. "Typhus morbus *contagiosis*; calor parum auctus; pulsus parvus, debilis, plerumque frequens; urinam parum mutata; sensorii functiones plurimum turbatæ; vires multum imminutæ."

reasons which have led me to this belief, will have been seen in the second part of this volume, at p. 89, and seq. and they are sufficient, in my judgment, to outweigh all the great authorites to which they are opposed, and render it absolutely incredible that any inanimate matters, even those excreted by living animals, should by any natural, or artificial decomposition and recomposition, ever require a power strictly contagious, or in other words, be enabled, like living animals and vegetables, to assimilate other matters to their own nature, and thus multiply and perpetuate their existence. Some writers of considerable reputation, sensible, perhaps, of this difficulty, have made a distinction between contagion and infection, and have ascribed the production of typhus fever, and some other diseases, to the latter. One of these writers, Dr. Adams, (in the quarto edition of his Observations on Morbid Poisons, at p. 6,) after adopting this distinction, says of "infectious diseases," that they "do not require for their production matter similar to their effects, but *may at any time be generated, by crowding together the sick or wounded, of any description.*"* He then mentions

* Scores of justly distinguished authors have made similar assertions, without any decisive fact or evidence in support of them, so far as I can discover; probably relying on the authority of those who had written antecedently. Had this assertion been true, the sick and wounded at Walcheren ought to have produced at least as many cases of typhus fever, as had previously occurred in the same year, among the British troops at Corrunna, by *actual contagion.* The sick, at least, if not the wounded, at the former place, having been more numerous, and in many cases more crowded. But, contrary to general expectation, not a single instance of typhus fever appears to have been thereby occasioned. That such a production of typhus fever might have been expected, according to the common opinions, will appear from a letter written to the Deputy Secretary at war, by the Physician General, Sir Lucas Pepys, (after he had visited the hospitals in the Eastern District,) dated, "Army Medical Board Office, September 14, 1809." in which he says, "the disease afflicting the troops returned from Holland, is the bilious remittent, and intermittent fever *liable* to degenerate into typhus and contagious fever; but it has not, so far as I have witnessed, *that character at present*, nor is there any dysentery." See Military Papers, letter E. p. 52.

Other eminent physicians have not only believed this liability of marsh fever to degenerate into contagious fever, but imagined that such degeneration had actually "hospital, prison, or ship fever, camp dysentery, and some peculiarly malignant ulcers," as being infectious diseases; adding, that "though these diseases when formed, *may produce their like effects in others*, yet we can always trace their

begun to take place. Thus Dr. Blane, in his letter to the Physician General, dated Middleburg, October 3, 1809, says "the fever known by the name of typhus, with which armies in ordinary circumstances are chiefly affected, has been rare,"——"as yet, among the troops here I am sorry to say, however, that both diseases *begin* to shew themselves, particularly at Flushing, where the accommodation is far inferior to that at Middleburg." Military Papers, E. p. 103. And in another letter, dated the 6th, he explains the causes which render the situation of the sick at Flushing "much worse;" adding, "they are also over crowded and dirty." E. p. 98. Afterwards, in a joint letter from Dr. Blane, Dr. Lempriere, &c. to the Secretary at War, dated October the 10th, 1809, we find this observation, viz. "The malady is not contagious in itself, but liable to assume that new form of fever, wherever ventilation is defective, the patients crowded, or where other local causes of impurity prevail.—This has been strikingly proved in some instances, particularly at Flushing, where we found the accommodations too confined and crowded." E. p. 107. That these gentleman in this instance mistook a few cases of low, or slow nervous fever, for a contagious or typhus fever, (as has been done thousands of times,) was manifested afterwards, by the facts stated at p. 215—19 of this volume. And certainly if the generation of contagious fever in the way, and by the means in question, had been possible, the supposed instances of it would not have been confined to Flushing; other places having been very sufficiently *crowded*, as is proved by a letter from the general in chief, Sir Eyre Coote, to Lord Castlereagh, written on the 17th September, 1809, immediately after a personal inspection of the different regimental, as well as general hospitals. "Middleburg, (says he,) from the size of its building, affords the best accommodations, but even in that town the sick are so *crowded*, as to lay (lie) two in one bed, in several places, and have no circulation of air."—"At *Veer*, a large church contains about 400 patients, the other places are *miserably small*, and *excessively crowded* At Armuyden the accommodation for the *numerous sick is wretched*."

I have admitted at the begining of this note, that the number of wounded men at Walcheren, was greatly disproportioned to that of the sick. But there is sufficient evidence to prove, notwithstanding all that has been written and believed to the contrary, that patients of the former description do not generate contagious fever any more than the latter. Dr. Blane, who was physician to the fleet under Lord Rodney, mentions that the battle of the 12th of April, produced an addition of 810 men to the list of *wounded*. But though the whole fleet was detained at sea until about the end of that month, and the last division of it did not reach Port Royal, in Jamaica, until the 25th of May, and though the discharges from so many *severe* wounds, must in that climate have become highly fœtid, "yet" says Dr. Blane, (p. 76) "there was less sickness and less death from disease in this month origin in *causes different from their effects.*" That Dr. Adams, who is accustomed and qualified to reason, should have believed any thing so unphilosophical, and *incongruous*, would have been incomprehensible to me, if so many others had not discarded common sense on the subject of contagion. To represent a disease which is *notoriously contagious*, and propagated by contagion, as capable of being also produced by other, and those very different means, is to multiply causes unnecessarily, and, therefore, unjustifiably; and it moreover destroys the natural, and just influence of causes upon their effects, by making the same disease result from very dissimilar causes. In this way, the *infectious* diseases of Dr. Adams are supposed to acquire all that is wonderful in *contagion*, I mean the power of reproducing, and perpetuating themselves, without deriving any thing from that original product of divine wisdom and power, to which I am forced to *refer* the beginning of all strictly contagious diseases; and while thus enabled to multiply themselves, ad infinitum, similar diseases with the same reproductive powers, are supposed to *originate* from time to time, in thousands of other persons, without any legitimate or suitable cause, or, in the language of Dr. Adams, by the agency of matter, *dissimilar to its effects*: and these monstrous products, of an equivocal incomprehensible generation, are to be considered as similar in their natures and effects, to those resulting from specific contagions. Were it possible for typhus thus fre-

(April), than in any of the former 23 months, in which I kept records of the fleet, and less than in any subsequent month, till the fleet got to the coast of America." And that an accumulation of wounded men is no more productive of fevers of any kind in *cold* weather, than in *hot*, as just mentioned, I need only copy what Dr. Lind has stated at p. 213 of his work on the Health of Seamen, concerning the *Magnanime*, ship of the line, viz. "She was seventeen weeks at sea, and for a *whole month* of that time, during very bad and stormy weather, *had on board the men wounded* in the *general engagement* on the 20th of November: notwithstanding this long continuance at sea, and the violent storms she encountered, yet of 700 men, 5 persons only were reported to us to be sick, besides the wounded, and these chiefly in chronic disorders."

quently, and easily, to originate without contagion, and at the same time acquire and multiply itself, by a contagious quality, who could ever hope to escape that disease?

If Dr. Adams had supposed typhus fever to be not contagious, his opinion that it might be produced by a great accumulation of sick and wounded, in close or ill ventilated places, would not to me appear, *a priori*, improbable; as the atmosphere may become unwholesome, either by not containing a sufficient portion of the vital part of it, or by the addition of noxious vapours dispersed therein; and it might be very naturally expected that fever would occur among the disorders resulting from such a *deficiency* of oxygenous gaz on one hand, or such noxious *additions* on the other; though we are not entitled to believe that this really does happen, without some evidence of the fact, and of this (as has been observed at p. 89 and seq.) there is none within my knowledge, excepting that which relates to fevers produced by exhalations from the earth, and excepting that connected with specific contagions.

Though contagious fever has probably existed for many ages in this, and some other northern countries, its history is involved in great obscurity, because it was not, until very lately, observed and distinguished with any tolerable accuracy.* Even Sydenham did not consider this fever as pro-

* One of the most early and unequivocal accounts of typhus which I have met with, is in the first volume of the "Acta Medicorum Beroliniensium," of which I have the 2nd edition, printed at Berlin, in 1719. The first article, the title of which is "Anni præter lapsi, 1716, Status epidemicus speciatim historia febrium petechialium, tunc temporis grassantium," containing an account of a contagious petechial fever prevalent "in Pomerania citeriore," p. 10 "Ultra fluvium Pene, Grypswaldæ, Stralsundæ atque in insula Rugia, (Rugen) magnus incolarum numerus, hâc malignâ febre afflictus fuit. Regnavit dirum hoc contagium per totam fere hyemem anni præterlapsi 1715, ultimumque suum non ante effudit impetum quam æquinoctium vernale, anni 1716 superaverat, *solstitiumque æstivale plenius attigerat.*" This account, or, as it is called "Methodus, &theses practicæ, secundum quas febres accutæ petechiales, anno, 1715, post solstitium hyemale & dein porro in nosocomiis castrensibus & alibi tractatæ fuerunt," was written by the *Archiater* regius; and to these, "nonnihil de suis quoque addidit, Doctor Schwartzius." It is stated at p. 18, that both these physicians died of this fever; the treatment

ceeding from contagion, but as depending on a particular state, or constitution of the atmosphere. Huxham and Pringle, as I have formerly mentioned, were the first who gave us distinct and just notions of contagious fever, though Dr. Ebenezer Gilchrist, of Dumfries, had previously written two papers on it, under the name of "*Nervous Fever*," one in the fourth volume of the Edinburgh Medical Essays and Observations, printed in 1737; and the other in the fifth volume of that work, printed in 1744. And though he was silent concerning its *contagious* nature, his descriptions, separated from the theoretical reasonings adapted to his time, are generally correct. He states "this fever to be very different in its nature and changes, from other fevers," and to have "something *peculiar in it which neither the ancients nor moderns perhaps had described, if at all thought of.*" Vol. 5, p. 507. He adds, at the next page, "as our fever seems to be peculiar to this age, it is not a little surprising that much more had not been said upon it. Some scattered hints are to be found in late authors, both just and ingenious, but not sufficient to make out a system of the disease." He did not, however, mean to represent this as a *new* disease, because in the preceding volume, at p. 348, he had stated it to have "been these *many years fatal in Britain.*" He appears to have treated the fever judiciously, and to have formed just opinions of the effects of opium, bark, and wine, in certain circumstances.

Typhus is properly the disease of cold climates, and in this, as in almost every other particular, it is in *direct opposition* to the *yellow fever*. The late Dr. John Hunter, in a paper on the gaol, or hospital fever, (Medical Transactions, vol. 3, p. 348,) says, "I have never seen the fever earlier than the month

which they chose for themselves, is also mentioned; and, it is not surprising, that bleeding and emetics, repeated at advanced stages of the disease, should have brought on hiccuping and petechiæ, and have occasioned death.

The description of the symptoms, and of the effects of some of the remedies employed, which will be chiefly found between pages 10 and 25, are judicious, and afford unquestionable proof of the existence of this fever in the army, and lower classes of the people, in certain parts of Pomerania.

of November, and I believe it seldom appears so soon. It becomes frequent about Christmas, and increases during the months of January and February. If March and April are warm, it grows less frequent; but if they are cold, it continues nearly as common as in the preceding months, which was the case in the two last winters, both of which were unusually cold. When the weather begins to grow warm, it gradually disappears." P. 350. He adds, (p. 366,) "I would,) observe, that for upwards of two years, that I remained in Jamaica, I never saw one instance of the hospital fever, though the military hospitals were often as much crowded as they are in Europe." "The heat proves a prevention of the disease, as much as cold forwards its production."*

The influence of heat in mitigating, and finally extinguishing contagious fever, was very fully manifested in regard to the troops which sailed from Cork, under the command of

* Numerous facts might be mentioned in confirmation of this general assertion. Mr. Howard has repeatedly noticed the greater prevalence of jail fever, during winter, than in summer; and Dr. Trotter, in the first volume of his Medicina Nautica, 197, observes that "as cold weather, and a winter season, favour the action of typhus infection, we know that warm weather, and a summer season, assist in its extinction." And of this he gives several decisive instances and proofs. Dr. Blane had previously made a similar observation, at p. 233 of his work on the Diseases of Seamen. Dr. Lind also has mentioned facts, in which heat produced similar effects, though he appears not to have understood the cause; at p. 319 of his volume on Preserving the Health of Seamen, he observes, that "this infection (of typhus fever), after every method used to destroy it has proved ineffectual, will often *of itself*, gradually abate, and at length entirely vanish." This he adds, "I often observed in our prisons, during the last war, where, after committing great ravages among the French prisoners, the infection often stopped of a sudden, and they were sometimes so entirely free from it, that in the month of *September*, 1762, when I was employed by the government, to muster the prisoners of war in the castles of Porchester and Winchester, which in the preceding year had suffered much by the jail distemper, I did not find *one person* labouring under that distemper, among 7000 prisoners, many of whom had been confined for several years." (See Lind on the Health of Seamen, p. 320, 321.) Here there is good reason from the month (*September*) in which this *entire cessation* of the disease was found to have taken place, to conclude that it had been produced by the heat of the preceding summer months, though Dr. Lind assigns no cause for it. which seems indeed to have been an extraordinary oversight and omission, especially as at p. 233 of the same volume, he had inferred from several facts, "that a cold damp air increases the power and vigour of contagion;" meaning that of typhus fever.

major-general White, for St. Domingo, in February, 1796. Two hospital ships, in which I had embarked, and sailed from England with the army, under sir Ralph Abercrombie, having by storms been rendered unable to continue the voyage, and the last of them having landed me on the south-west coast of Ire, I embarked on board a very large hospital-ship, the Bridgewater, (formerly an Indiaman,) destined to receive the sick of general White's division, among which a severe typhus fever had prevailed to a great extent, and with great mortality, previous to our sailing from Cork, where most of the sick were left at our departure; but many of the soldiers, apparently well, being exposed to the contagion which existed in many of the transports, or having imbibed it previously, whilst detained at Cork, fell sick on the passage, and were from time to time removed into the Bridgewater, which soon became full of patients, under typhus fever, which was communicated to several of the orderly men, and nurses, to some of whom it proved fatal. It became evident, however, that as we reached, and proceeded in the warmer latitudes, the cases of fever gradually diminished in number, and became much milder; though, from the shortness of our passage, and the cool season in which it was made, the *full* effect of heat in extinguishing contagious fever, could not have been produced, and, therefore, it was not surprising that a few patients with the same fever, in a milder form, and apparently divested of its contagious power, were sent on shore to the hospitals, immediately after our arrival at Barbadoes. These had probably imbibed the contagion before our arrival within the tropics, and its effects, though moderated, were not wholly prevented by a change of temperature. One of the last persons attacked, was my own servant, who had indeed been sufficiently exposed to the contagion on board the Bridgewater. But in his case, as well as in all the others which occurred between the tropics, the fever was slight, and not being communicated in any instance, at least after our arrival at Barbadoes, it *there terminated.*

In voyages to the East Indies, ships remain for a much longer space of time between the tropics, and being also exposed to an *higher temperature*, the power of heat in destroying typhus fever, is in them more decisively manifested; an entire cessation of the disease, (however prevalent) commonly taking place before they can reach the Cape of Good Hope. It has indeed never been known, as I am informed, that a single case of this fever had occurred on either side of the Indian peninsula.

But without going from home, or farther back than the year 1809, we may find strong evidence of the power even of the moderate summer heat of our climate, in extinguishing typhus fever. It is well known that the contagion of this disease, had been imbibed by many of the soldiers who returned from Corunna, in the beginning of that year; who, being attacked either on board of the transports, or soon after landing in this country, the disease was communicated to nearly 10,000 persons belonging to the army, (including those in the artillery and ordnance departments,) and this within little more than two months. I had a short time before obtained leave to *retire* on half-pay; but finding that there was no army physician in the western district, I offered my services there, and obtained by doing so, an opportunity of observing the influence *even of our vernal warmth*, in mitigating and checking the fever, which ceased I believe completely, at least in regard to new cases, before the end of May, and even sooner in the western and warmer districts.

Whether heat interrupts or suspends the influence of typhus contagion, by dissipating the corpuscles, of which it consists, or by rendering the body less susceptible of their impressions, or both, I will not venture to decide; but certainly, those who by birth and residence, have been long habituated to intertropical climates, are, when they remove into the cold, particularly susceptible of the action of typhus contagion, if exposed to it; and this has been found to be the case of Negroes, to a remarkable degree, particularly in the New England states, and in Nova Scotia, where the people of that race, who were exported at different times to Sierra Leone, had been very extensively attacked with contagious fever; and indeed many of them were ill, and some died of it, in their passage to Africa; but in all cases it was very soon extinguished after their arrival at Sierra Leone, if not previously. Dr. Trotter has observed, at p. 205, of the first volume of his Medicina Nautica, that the natives of Africa "are very liable" to the fever in question; adding, that "in ships they are commonly the *first* sufferers." So that in this respect also, the yellow and typhus fevers are directly the reverse of each other; as they are moreover in the following particulars, viz. Typhus is aggravated by that degree of cold which extinguishes yellow fever. It never prevails epidemically. It commences much less violently than yellow fever, and is protracted to a greater length. It manifests no disposition to remit, unless the patient has imbibed marsh miasms, whilst even in the violent forms of yellow fever, there is generally about the 3d or 4th days, a very sensible, and often a very delusive cessation or abatement of the febrile commotion, and of all the inflammatory symptoms. In both, however, it is of high importance towards the cure, that the patients should be removed to, or kept in a pure wholesome atmosphere.

In regard to their effects on the human body, Typhus is generally accompanied with less mortality, and the derangement which it occasions in the system, is much less permanent and mischievous, than that which accompanies or results from even the remittent fever of Europe.—Witness the events produced by the former disease in the British Army, subsequently to the return of the troops from Corunna, in 1809, and those which attended, or followed the expedition to Zealand, in the same year. In regard to the former, it appears that the deaths did not exceed one in ten of the sick, though the accommodations were in many situations but ill suited to that disease; and a considerable number of the medical persons employed by Mr. Knight, were but ill qualified to direct the treatment of the sick; the whole were, moreover, restricted by his parsimonious regimental hospital system, from directing those allowances, and indulgencies, in regard to nourishment, wine, porter, &c. which are highly important to patients under Typhus fever. On the Zealand expedition, however, and without these advantages, the deaths were but a small fraction less than one in eight, the recoveries were much more tedious, relapses, perhaps, one hundred times more frequent, and very often followed by permanent obstructions, or morbid alterations of the Viscera, ending in Dropsy, or other chronical affections; which rarely occurred as the consequence of Typhus, in the troops from Spain; who were in general fit for duty in six or eight weeks after becoming convalescent, which was never the case with those from Walcheren.

Whilst I was employed with the troops from Spain labouring under Typhus, I thought it very desirable that so good an opportunity of ascertaining the time which the contagion thereof may remain latent after its application to the human body, should not be lost, as so many other opportunities had been; and I therefore obtained, chiefly through the kindness of Mr. Grant, deputy inspector of Hospitals in the Western district, returns of the orderlies and nurses who had attended the sick in question, and had been afterwards attacked with the same fever; and also an account of the time when the attendance of each began, and of the interval which succeeded previous to the attack. I found, however, that it was necessary in these returns to make a distinction between the orderlies and nurses, who had returned in the transports from Corunna, and, consequently, had, at least in some instances, been exposed to the contagion, previous to their attendance on the sick here, (as was proved by the different results in these cases, from those of the persons who had not left England.) And, accordingly, this distinction was made in most of the returns. In all of them, however, there was an omission of the persons who had only been temporarily employed; though their number exceeded that of the regular orderlies and nurses; but in regard to their cases it was difficult to ascertain dates.

Having selected such of these returns as appeared to be most correct and suitable, I found that they produced the following results;—viz. of thirty-five orderlies and nurses who had returned from Spain, and, therefore, might have been previously exposed to contagion, it appeared that one was attacked on the first day after beginning to attend the sick; one on the 2d, one on the 6th, two on the 7th, one on the 8th, one on the 9th, two on the 11th, one on the 14th, one on the 15th, one on the 16th, three on the 17th, one on the 18th, two on the 20th, one on the 21st, one on the 22d, two on the 23d, two on the 24th, two on the 25th, one on the 26th, three on the 27th, one on the 28th, one on the 36th, one on the 38th, one on the 40th, and one on the 44th days. Of ninety-nine orderlies and nurses who *had not been out of the kingdom, nor*, as far as was known, *exposed to febrile contagion*, it appears that one was attacked on the 13th day, one on the 14th, two on the 15th, one on the 16th, four on the 18th, two on the 19th, three on the 20th, six on the 21st, four on the 22d, four on the 23d, two on the 24th, six on the 25th, four on the 26th, four on the 27th, eight on the 28th, five on the 29th, three on the 30th, three on the 31st, two on the 33d, three on the 36th, four on the 37th, one on the 38th, four on the 39th, one on the 40th, two on the 42d, three on the 44th, one on the 45th, five on the 47th, one on the 48th, three on the 52d, two on the 54th, one on the 58th, one on the 60th, and one the 68th days. It results, therefore, from this statement, that among the ninety-nine orderlies and nurses, who had probably *not* been exposed to the contagion before their attendance on the sick commenced, the *earliest* attack was on the 13th day, and the *latest on the* 68th; but these returns were made up about the 20th of April, and it appears that some who had escaped till that time, were afterwards attacked;* and, therefore, though there may be reason to conclude that febrile contagion does not remain inactive so long after being received into the body, as marsh miasmata, I see none for believing that an interval of five or six months, may not sometimes elapse before the actual production of fever by it; especially if the summer should intervene previous to an attack; in which case the occurrence of fever would, I think, almost always be postponed until the following winter, and often completely obviated:† and I cannot help strongly suspecting, that such a postponing of the disease happened to some of the troops from Corunna, in 1809. It will be recollected that *sickness* prevailed to a very uncommon extent in the army at home, during the early part of the preceding year; and though it did not consist exclusively of contagious fever, that disease made a considerable part of it, until it became extinct at the approach of summer. It will also be recollected, that many of the regiments in whom this sickness occurred, were, after its cessation, employed under Sir John Moore, and Sir David Baird, in Spain, where Typhus fever cannot exist in the Summer, and where, I believe, it never appears even in Winter, unless by an extraordinary introduction of it. Such an introduction took place in that year by the Spanish army under the Marquis de Romana, which had been removed from Holstein and Denmark, (where Typhus is a

* Many circumstances or causes may accelerate the actual production of Typhus fever in persons who have imbibed a sufficient portion of the contagion; particularly the effects of Cold, Drunkenness, excessive Venery, deficient nourishment, and indeed of every thing occasioning debility, especially, in slender, feeble constitutions, in which the disease will also commonly prove most severe: and hence, *contrary to what happens with Yellow Fever*, those who are in the decline of life suffer more from it than the young, and females more than males. On the other hand, a robust young man, who after exposure to febrile contagion, prudently avoids all debilitating accidents, or excesses, may, by the strength of his constitution, and of its conservatory energies, not only resist for a long time, but finally overcome, such a portion of infection as in most cases would have soon occasioned disease.

† In case heat would produce, in regard to Typhus contagion, an effect analogous to that of cold, upon marsh miasmata, when the morbid action of the latter is suspended, until the following Spring or summer.

frequent disease) back to their own country in British transports; and though this fever did not appear in that army, after their arrival in Spain, until the Autumn and Winter, it certainly began then to prevail therein to a considerable extent. It will be remembered that in the latter part of that campaign, the British army, twice crossed that of Romana, and on both occasions Mr. Warren, Deputy Inspector of Hospitals, informed me that he had observed a considerable number of the Spanish Soldiers to be labouring under Typhus fever. He added that the disease afterwards appeared among the French troops, as might well have been expected from their having occupied the barracks, quarters, and hospitals, which, in a long line of march, had just before been used, as well by Romana's as by the British army under Sir John Moore. This fact Mr. Warren stated, on what he thought good information, and particularly that of a British medical officer, who had remained with the British Hospital left at Lugo.

Whether at either of these crossings the British army received any febrile contagion from the Spanish, or whether they found any of it on board the transports in which they returned from Corunna, some of which had, as I understand been employed in removing the Spanish army from Denmark, is not, I believe, well ascertained; but I think it highly probable that in many cases, the cold, rain, and excessive fatigues to which the British soldiers were exposed, during a considerable part of Sir John Moore's retreat, and after their embarkation, might have brought into action the latent infection of the preceding spring, the morbid influence of which had been suspended by the summer; and I believe that typhus fever has, on some former occasions, suddenly made its appearance from similar causes.* Circumstances to be explained in

* The learned and Reverend Stephen Hales, D. D. in his treatise on Ventilators, (8° 1758) states at p. 106, that the convicts from Newgate often carry the Gaol distemper with them, "to Virginia, *before it breaks out.*" As Dr. Hales, had some time before taken great pains to introduce the practice of frequent Ventilation in

another place, compel me to desist from any farther observations on this subject, and to conclude the present chapter, by subjoining extracts from two letters, written to me by Mr. Grant, dated Plymouth Dock, the 24th and 27th of April, 1809, viz.

"Before the arrival of the troops from Corunna, this garrison was extremely healthy. Typhus did not exist in it, nor were there scarcely any sick in the hospitals, confined to bed; but the effects of contagion very speedily developed themselves amongst the orderlies, and others, employed in fatigue duties, connected with the hospitals. They seem, however, *as speedily to have disappeared*, as we have *scarcely a dozen febrile diseases now in the garrison*, and these are orderlies who have been taken *ill since collecting the inclosed returns*. The disease also became latterly very *slight* in its attack. In answer to your question, respecting the yellowness of skin, I have not seen many instances of it in this garrison; the proportion not exceeding 2 to 100: scarcely an hospital that has had two or three cases. One of the medical officers who died, Hospital Mate Williams, was of this description."

Extract trom the second letter, viz.—

"It may be also aiding your enquiries to remark, that in the *naval* hospital here, where some of the sick from Spain

the ships employed in transporting criminals from Newgate to America, and had collected good information on the subject, it is to be regretted that he did not mention this fact more *circumstantially*, and especially that he did not inform us, whether this complete suspension, for probably two or three months, of the action of Typhus contagion, after it was received into the body, happened in cold or hot weather. That it was not always suspended so long, is evident by a fact which the same respectable author had mentioned in the preceding page, of the breaking out of this distemper, "in Mr. Reid's convict transport ship the Laura, notwithstanding the ship was frequently refreshed by Ventilation." The Convicts (says he) were put on board, the latter end of April, in seeming good health, and continued so until they anchored in *Stromness* Bay, in the *Orcades*, when between the 11th and fifteenth of May, a great part of the people fell sick of the Gaol distemper, in the compass of two days." He adds in the next page, that the contagion was in that case supposed to have been brought on board the Laura by the convicts from Newgate. But we have no means of ascertaining how long they had imbibed it before they were removed from that prison.

were accommodated, 24 nurses, and seven labourers, wer taken ill of fever, in attendance upon them; of which number, four nurses, and three labourers have died. Previous to this occasion, I am informed by the medical officers of that establishment, that it was a rare occurrence for the servants of the hospital to be taken ill with fever, in attendance upon the sick.

"At Pendennis Castle, Falmouth, where some sick from Spain also debarked, the progress of contagion was more rapid, extensive, and fatal, (in proportion to its field of action) than in any of the other hospitals in this district. All the medical officers and servants stationed there, (the North Hants Militia,) were speedily taken ill, and one-fifth of the regiment, viz. 103, of which number, 11 died."

## CHAP. II.

### OBSERVATIONS ON DYSENTERY.

By the Greek name of this disease, (Δυσεντερια) signifying a pain, or griping of the bowels, Hippocrates intended to designate both ulcerations and hemorrhages from the intestines, and every kind of flux, with, or without blood, from them. After his time, however, this name seems, in its application, to have been restricted to an ulceration, or supposed ulceration, of this part of the alimentary canal, with gripings and tenesmus, producing or attended by mucous, or bloody stools. Now, however, an intermixture of blood with the stools, though of frequent occurrence, is not deemed a characteristic of dysentery, nor is ulceration of the intestines; but when the disease has been of long continuance, they are often found after death, to have been ulcerated, and even sphacelated. A spasmodic constriction of the colon, retaining the natural, but hardened, fæces commonly attends this disease; and Dr. Cullen superadds, as a part of character "*contagious pyrexia*," though this addition seems objectionable, in regard to contagion, which I am convinced is not generally, if ever, connected with dysentery. Sydenham, when treating of the dysentery of 1669, says, "After having attentively considered the various symptoms of this disease, I *discovered* it to be a *fever, sui generis*, turned *inwards* upon the intestines." Dr. Balfour, in his account of this disease, as it occurs in Bengal, has called it an "*intestinal* remitting fever;" and Dr. Rush, who supposes it may be connected with jail fever, as well as with the fevers from marsh effluvia, omits the word *remitting*, and calls it "the *intestinal* state of fever." (Medical inquiries, &c. vol. iv. p. 167.) He moreover contends, that after a fever has been thus thrown upon the intestines, so as to occasion dysentery, it may, by a *retroversion*, be translated to the skin, and there produce rash, prickly heat, and eruptions of various kinds.*

Dr. Akenside, in his Commentarium de Dysenteria, instead of Sydenham's belief that this disease was a "*febris introversa*," seems to consider it as an *introverted rheumatism*, or, as Dr. Rush would call it, the *intestinal state of rheumatism;* and with this notion, the former supposes that like ordinary rheumatism, dysentery may be either accompanied with fever, or divested of it. He supposes, also, that it always begins with the smaller intestines, and gradually descends to the rectum, and that rheumatism and dysentery are frequently converted into each other.

Sir John Pringle considers "all the *epidemic* dysenteries," as being "of the *same nature;*" (see p. 223,) and in support of his own opinion on this subject, he appeals to the experience of the late Dr. Huck Saunders; "not only in Germany, but in Minorca, America, and the West Indies;" in all of which, notwithstanding the differences of climate, this disease appeared "with the same symptoms, (though with more or less violence, according to the *heat*,) and yielded to the same

* If such a translation of dysentery to the surface of the body can be effected by *art*, it should, I think, always be attempted, as speedily as possible, because the attempt will be more likely to succeed while the disorder is recent, and because the danger of a disease in the *skin*, is much less than of one in the intestines. In a case of severe diarrhea, with which I was partially acquainted, and which was suddenly stopped by opium, Peruvian bark, and sudorifics, near a dozen *large biles* were produced on different parts of the body, within three or four days; and when these had suppurated, others supervened in succession, for several months, but gradually diminished in size, though not in number, until near the time of their total disappearance; since which, the patient, though more than sixty years of age, has for six years enjoyed better health, than in any former part of his life. I barely mention the fact, leaving others to judge, whether this improvement in the patient's health, resulted in any degree from the biles, which to him were a new, as well as a troublesome occurrence.

medicines." That this is true of *epidemic* dysentery, I can readily believe; being convinced that this disease never prevails *epidemically*, unless it proceeds, from marsh miasmata, whose morbid influence is then, from particular circumstances or causes, directed and exerted upon the intestines, rather than upon the heart and arteries.

This connexion between dysentery and marsh fevers, has been suspected and believed, by several respectable authors; but I do not recollect that the *identity* of their causes has been any where so *decisively* manifested, as it was at the town of Sheffield, in the state of Massachusetts, during the summer and autumn of 1796, according to a very circumstantial and apparently accurate statement, made by William Buel, the principal physician of that town, who had previously given an account (published by Mr. Webster) of the febrile disorders which prevaled there, during the three preceding years. The statement regarding the year 1796, may be found in vol. 1, of the New-York Medical Repository, p. 439—459, and the following are extracts from it, viz.:

"The part of the town in which the sickness prevailed, is almost a perfect level. The river Housatonak, whose width is generally between 30 and 40 yards, runs through it in a serpentine direction, and with a very gentle current."—"On each side of this river, there is a considerable extent of luxurious meadow-ground, whose surface is generally overflowed, when the snow melts in the spring, and sometimes by freshets, at other times in the year. This meadow-ground is all much interspersed with coves or pools, which are left after the subsiding of the flood, full of stagnant water;"—and this "is in the course of the summer evaporated from some to dryness, and from others nearly so."—"Beside the meadow adjoining the river Housatonak, there are several other streams, which run through large tracks of flat, and very marshy land. On one of these streams, towards the north part of the town, there is a mill-pond, which appears to have been the common centre of the sickness in 1796, and the preceding sickly years."

"This pond overflows a large track of land, which was formerly covered with a luxuriant growth of timber, and other vegetable productions, which are all now dead, and in a state of dissolution, in consequence of the action of air and water upon them:"—"whenever a dry season occurs, the water recedes from almost the whole of the land *last flowed*,* and leaves the whole mass of dead animal and vegetable substances, lying on its surface, exposed to the action of a scorching sun."

"The fætor which arises from this drowned land, when made bare by dry and hot weather, is extremely disagreeable, and offensive to all who approach its borders."—"The stench is smelled by the inhabitants at times even to the distance of half a mile: an exposure to the effects of this noxious effluvium contiguous to its source, not unfrequently in the year 1796, produced immediate nausea and vomiting." The writer, after observing that the spring of this year had been uncommonly wet, adds, "we had such an excess of rain even through the month of June, that all our streams, ponds, coves, and marshes, were kept full, and even our dryest land was highly surcharged with water." But from the beginning of July forward, we began to suffer from *the other extreme;* we very seldom had rain, and uniformly the weather was *intensely* hot, particularly in the month of August:"—"the drought was so great, that vegetation was much injured; grazing grounds, particularly, were parched almost to dryness."

After these explanations, the writer gives a particular acaccount of a considerable number of cases of dysentery, which occurred before the 20th of July, within what he afterwards describes as "the sickly circle;" adding, that within a few days other persons, within the same limits, were attacked with bilious or marsh fever. "From this time, (says he) instances of this fever frequently occurred, so that it was ap-

* By the "*land last flowed,*" the writer means a large extent of ground, over which the mill-pond had recently been extended, by raising the *mill-dam* seven feet above its former height.

parent both disorders were endemic."—"In a short time, both prevailed to a degree truly calamitous and alarming."—"Let (says the writer) an imaginary circular line be described, from a point on the south-eastern side of the above-mentioned mill-pond, whose radii shall be one and one-half mile in length; this circle will embrace about 100 families, and about 600 inhabitants; it would comprehend the whole territory in which the sickness prevailed, with so much exactness, that there would be considerably short of 10 families without its limits in which there was sickness, and there certainly were not 10 within which were exempt." Of about 450 persons in the eastern half of this imaginary circle, "at least 250 were affected with sickness; of the 150 who dwelt nearest the pond, there were not 10 who escaped."*

"The *dysentery frequently came on while the patient was affected with bilious fever.* In this case the type of the fever soon became obliterated, and the accompanying febrile symptoms were similar to those in original dysentery. The change of the fever into dysentery, did not, however, secure the patient from the tendency to relapse, so peculiar to that disorder. But the convalescence of those who had simple dysentery only, was generally short, and the recovery perfect.

"*Sometimes the fever came on upon the dysentery.* The type of the fever was not in this case easily ascertained, until an abatement of the dysentery took place, when, *as the dysentric symptoms subsided, the fever would appear in its proproper form.* The *two disorders appeared to be complicated*; that is, they both seemed to *exist at the same time, rather than to act in alternation.* The fact is certain, that in cases of accession of dysentery upon the fever, the latter disorder always showed itself in its true form, after the symptoms of the other had subsided.

* It is not to be supposed that marsh miasmata, arising from the mill-pond, exclusively produced disease at the distance of a mile and an half; other sources of them were interspersed throughout the whole circle here imagined.

"In the sickness which makes the subject of this communication, there is every reason to ascribe *identity of cause to the two disorders.* They were *circumscribed* in a very striking manner, by *precisely the same limits. They both began and ceased to prevail at the same time.* Neither disorder occurred, (except in a few instances of both disorders about the pond, at the south part of the town,) at any considerable distance from the limits, but in persons who had previously resided within them. There were instances of both disorders affecting persons in different parts of the country, who *had* resided within these limits. A stay of only *one night* in the central part of the sickly territory, in some instances produced these disorders.

"The facts which I have stated, prove sufficiently, that neither of these disorders was propagated by specific contagion, at least beyond certain boundaries, otherwise they must have extended, for there was no interruption of communication. I have remarked before, that I was myself convinced that neither disease was propagated by specific contagion, *even within these boundaries.* In all cases which came under my observation of *sickness without the limits, and acquired by a residence within them,* there was no instance of either complaint being communicated from the person affected." Here it should be observed, that the town in which these disorders were produced, is situated near the *northern* boundary of Massachusetts, where the summer heat is commonly much more moderate than in Pennsylvania, and other states, in which the yellow fever has often prevailed.

This connexion of dysentery with marsh fevers has been also noticed, in different parts of the United States, and in many other parts of the globe.* Dr. John Vaughan mentions the former disease, as prevailing over certain low districts,

* Dr. Cleghorn (Diseases of Minorca, p. 134,) says, "Sometimes a tertian is changed into a dysentery, or a dysentery becomes a tertian; and when one of these diseases is suppressed, the other often ensues." He adds, that it is not uncommon for "the fits of tertians to be regularly accommpanied by gripes and stools."

adjoining the Delaware, and evidently resulting from marsh effluvia.—See N. York Med. Rep. vol. iii. p. 223. Dr. De Rosset, also, in giving an account of the Bilious Yellow Fever, of 1796, at Wilmington, in North Carolina, (which I noticed at p. 248-9 of this volume) mentions it to have been *preceded* in July and August, after excessive heat, by the dysentery, which "soon became general, proving fatal in many instances." He adds, "towards the close of August, when the first cases of bilious fever occurred to me, the dysentery began to decline; and scarcely one new case of it occurred after the fever became more prevalent. It may be here remarked, that every person who had laboured under the dysentery, without an exception within my knowledge, *escaped the fever*."

It is remarkable that here, as well as at Sheffield, the morbid influence of marsh miasmata first manifested itself in the form of dysentery. This, I believe, does not always happen. Dr. James Clark, in his Treatise on the Yellow Fever at Dominica, says, (page 103) that "the dysentery generally prevails at the *same time* that the remittent and intermittent fevers do, in the West Indies, and probably from the same cause." Dr. Trotter observes, that on the coast of Africa and the West Indies, dysentery "is *joined* with intermittent and remittent fevers." (Med. Nautica, vol. i. p. 378.

Of the East Indies I have no personal knowledge; but it is notorious that marsh fevers and dysentery are there commonly produced by the same cause, and at nearly the same time:* that both often occur in the same person, and they are said to be not unfrequently complicated with chronic inflammation of the liver, to which the greater heat of that climate seems to dispose the inhabitants in a remarkable degree.

* Dr. Ffirth says, that of the crew of the ship in which he went to *Batavia*, 76 in number, all, except eight, had either marsh fever or dysentery; that "the fever appeared to alternate with dysentery; when he weather was *bad*, the *latter* prevailed; when *good*, the *former*."

But, without leaving Great Britain, we may find evidence of the influence of marsh miasmata in producing dysentery, from its appearing in those places, and at those seasons, in which they were known to be morbidly active. Even London, though now almost exempted from their effects by a change of circumstances, was formerly very much infested by them; and Sydenham remarks, that the dysentery never *prevailed* until the latter part of summer, and that it disappeared at the approach of winter, resembling marsh fevers in these respects. He has, indeed, omitted to notice those peculiarities of the season which render marsh effluvia most powerful, because he ascribed diseases not so much to the *sensible* as to the *occult* qualities of the air, which he called its *constitution.* Dr. Willis, however, has (as Sir John Pringle observes) supplied this omission, in regard to the dysentery which prevailed in London in the autumn of 1670, by mentioning that it began after an *exceeding hot* and *dry* summer; "post æstatem *impense calidam & siccam.*" (See Pharma. Ration. sect. iii. chap. 3.) Sir George Baker, also, (de Dysenteria Londin. an. 1762) mentions that this disease, in the latter year, appeared as an epidemic, about the end of July, after *very hot* and *dry* weather; and that it raged until November. Sir John Pringle also observes, (p. 251) that in this year 1762 "the summer *heats* and *drought* were of a longer continuance" than he ever observed in this country, and that in the autumn more cases of dysentery "occurred, than in all the sixteen years that" he "had resided here."—And Dr. Huxham, without appearing to suspect the influence of marsh effluvia, has remarked the prevalence of this disease as a consequence of hot summers. "Post fervidam æstatem, constanter fere sequunter choleræ, dysenteriæ, alvi fluxus." (De Aere & morbis epidemicis, t. ii. p. 176.)

But that I may not unnecessarily extend these quotations I shall content myself with referring to Sir John Pringle's "Observations on the Camp Dysentery," among which are the following, viz.

"I have never known the dysentery epidemic, unless in *summer* or in *autumn*, when the primæ viæ are most liable to be disordered." (Diseases of the Army, p. 224.) He might have added, and when *marsh miasmata are most powerful.* Again, at p. 226, "Frequently the beginning of a flux will have all the appearance of an autumnal fever; for the patient will be feverish, with disorder in his stomach and bowels, for two or three days before the purging comes on; but after that, the fever sensibly gives way." And again, at p. 253, "Hitherto we have seen how similar the causes are, of the remitting and intermitting fevers, and of the bloody-flux. Nay, the affinity extends even to the occasional or exciting causes; such as when, in the end of summer, or in autumn, the men are exposed to *night damps* and fogs*, especially after a hot day, or lie upon wet ground, or in wet clothes, *part of them will be seized with that kind of fever, and part with this flux;* and perhaps some of them will have a *disorder compounded of both.* Add to this, that those fevers begin to be frequent in camp whilst the dysentery still subsists; that the *first symptoms are often similar,* such as the rigors, and disorder of the stomach; that the *remitting and intermitting fevers of a bad kind* have sometimes *ended in a bloody-flux;* that *such countries as are most subject to those autumnal remitting fevers, are likewise most liable to the dysentery;* and that the *analogy continues even to the method of cure,* in so far as the principal part of it consists in clearing the primæ viæ. Upon the whole, the *nature of the two distempers appears so much alike,* that, at first sight, Sydenham seems to have expressed himself justly, when he called this flux "the fever of the season turned upon the bowels." But upon a nearer view we shall find this no-

*It is remarkable that, notwithstanding all that Lancisi and others had written of the influence of marsh effluvia in producing fevers, Sir John seems to overlook their effects, and ascribe them to *cold* and *moisture.* Hence, though he considers dysentery, and remitting or intermitting fevers, as having similar causes, he makes no mention of marsh miasmata as occasioning either, even where they prevail most extensively, as in Flanders, &c. It is, indeed, true that he supposes the moisture to be rendered more hurtful by the effects of putrefaction in marshes, tion more ingenious than solid, since the circumstance of its being *contagious*, shews that the dysentery is *essentially different* from those fevers."

Here we find that this justly-distinguished physician, after stating facts and reasons the most forcible, for considering marsh fevers and dysentery as produced by the same cause, gratuitously assumes the latter disease to be contagious, and, on that assumption, in opposition to these facts and reasons, infers that the latter disease "is essentially different from those fevers." We shall, however, soon find reason to think that contagion is not a quality belonging to dysentery, unless it be in cases which are occasioned by, or complicated with, typhus fever, if, indeed, such cases ever exist; and we may therefore conclude, that marsh miasmata, acting in a particular direction, are a frequent cause of dysentery; indeed, there is good ground for believing that it never becomes an epidemic, without their co-operation.

The causes which determine the morbid influence of marsh effluvia towards the intestines, so as to excite the disease in question, rather than intermitting or remitting fevers, do not seem to be yet well understood. Dr. Blane thinks, when persons are pre-disposed to that morbid action which may terminate either in fever or dysentery, that the latter disease "is more likely to arise from an irregularity in eating or drinking:—a fever from being exposed to the weather," &c. There can, however, be no doubt, but the latter of these causes, (supposing it to include the application of cold and wet to the skin,) is often productive of dysentery, either alone, or in conjunction with miasmata and other causes. Indeed, there are but few persons who have not some time been made sensible of the sudden effect of such applications, in producing diarrhœa at least; though I am far from thinking that improper food, by irritating and disordering the bowels, does not also co-operate in exciting dysentery; and under the head of improper food, I would include sharp, acid fruits, when eaten to excess, such as pine apples, which Dr. Moseley mentions as having caused the disease. I think, however, that in this, as well as other cases of dysentery, which were chiefly in his contemplation, marsh effluvia must have been the principal cause; for at p. 214 he notices the stools, as being "more frequent, and all the symptoms more aggravated, at those hours when the *current fevers* are in their *exacerbations,* and the reverse when those fevers are in their remission; besides the alternate *succession* of one disease to another, which (says he) I have frequently observed:" and this, to my apprehension, clearly indicates the influence of marsh miasmata, though he adds that it cannot "be doubted but this fever of the intestines, like most others, is caused by obstructed perspiration."*

But besides the production of dysentery by the operation of wet and cold, conjointly with miasmata or other causes, this disease has, in many instances, been apparently occasioned by their operation alone.

Of this Sir John Pringle gives a remarkable instance, at p. 19 of his Observations on the diseases of the Army, viz.—"On the 26th (of June, 1743,) in the evening, the tents were struck, the army marched all night, and next morning fought at *Dettingen.* On the night following, the soldiers lay on the field of battle without tents, exposed to a heavy rain. Next day we moved to Hanau, and encamped on good ground in an open field; but it was then wet,† and for the first night or two, the men wanted straw. By these accidents, a sudden change was made in the health of the army; for the summer

* An obstruction of perspiration, to with (in conformity with the principles of the Humoral Pathology) the dysentery is here ascribed, probably is not a cause of it, as occasioning a retention of matters which ought to have been excreted (and for which nature has provided other outlets when wanted,) but as being generally accompanied with a morbid distribution of the blood, and an improper determination of the living power inwardly to the intestines, followed by increased, or inflammatory actions in their vessels.

† Dr. F. Home (at p. 26 of his Medical Facts and Experiments,) says there were "*two rainy nights,* after the battle of Dettingen," which "produced the bloody flux."

had begun early, and the weather had been constantly warm, &c."—" Now the pores were suddenly stopped. the body was chilled, and the humours tending to resolution from the preceding heats, were *turned upon the bowels*, and produced a dysentery, which continued a considerable part of the campaign. In eight days after the battle, about 500 men were seized with that distemper, and in a few weeks *nearly half the men* were either ill, or had recovered of it."

Dr. Trotter mentions a lamentable dysentery, which was produced on board the Berwick, ship of the line, in October, 1780, in consequence of the hurricane on the 5th of that month, by which the clothes and bedding of the seamen, and indeed every part of the ship " were soaked in water," and many of the men " slept for nights together on the wet decks, overcome with fatigue, and debilitated from the want of food." In seven weeks thirty of the best men died of this disease, in some cases complicated with scurvy, and " near 300 of the ship's company were ill," when she arrived at Spithead. (Med. Nautica, vol. i. p. 378, &c.) Dr. Mosely says, " it has often happened that hundreds of men in a camp have been seized with the dysentery, almost at the same time, after one shower of rain, or from lying one night in the wet and cold." (See his Treatise on Tropical Diseases, 3rd edition, p. 268.) I suspect, however, that in such cases, the disease is not exactly like that which principally results from marsh effluvia; that it has a greater similitude to diarrhœa, and if accompanied with fever, that this is nearly related to that of *catarrh*.

Another supposed cause of dysentery has been alleged by so many respectable authors, that it would be improper in me to reject it, though I have never seen any decisive or convincing evidence of its operation in this way: what I mean is typhus fever, or its contagion. Dr. Blane, at p. 394 of his work on the Diseases of Seamen, says, " when this (typhus) fever prevailed on board of any ship that arrived from a northern climate, it was soon after succeeded by, or converted into, a dysentery; for the ships that arrived either from England or North America, with the greatest stock of *feverish infection*, were the most subject to fluxes, after being two or three months in the West Indies." Dr. Trotter asserts that typhus fever was combined with dysentery in the transports which conveyed the army under Lord Moira to Ostend, in the year 1794; (see p. 378) and Sir John Pringle says, (p. 227) "The most fatal sort of fever, which so often attends the dysentery of the army, though not essential to it, is the hospital or jail distemper."—"This fever (he adds) combined with the bloody flux, was generally mortal." But supposing, as I am willing to do, that Sir John Pringle has committed no mistake concerning the true nature of the fever in question, it may, notwithstanding, become a matter of doubt, whether the dysentery in these cases, was the consequence of a typhus fever inverted or thrown upon the intestines, or whether the patients had been exposed both to marsh effluvia and febrile contagion at different times, and that each having produced its effect separately, the fever and flux were thus accidentally combined? In either of these cases, however, we may understand, and perhaps believe what the same author asserts at p. 103, i. e. that "the putrid effluvia of the dysenteric fæces, are not only apt to propagate flux, but likewise to breed the jail or hospital fever, with or without bloody stools:" for the excretions of patients under the action of contagion, may reasonably be expected to become contagious; though I cannot believe that dysentery ever possesses that quality, when it is not derived from, or connected with that cause.

G. Fabricius Hildanus, in his Treatise de Dysenteria, cautions persons in health not to approach the places where dysenterical excrements are deposited, lest they should be infected; adding, that the exhalations of such excrements affect the bowels of persons in health, by some *occult* quality. Afterwards, Sennertus mentioning the dysentery, which occurred in 1624, after *great heat and drought*, says that one person was infected by another, and that whole families died of it. But there seems to be good reason for believing, that the disease here mentioned was occasioned by marsh effluvia, and that their effects were, as they have been on so many occasions, mistaken for those of contagion; and this was probably the case with Sir John Pringle in Flanders, whenever the disease prevailed as an epidemic, which was always at a time of the year when febrile contagion must have been nearly inactive, and when marsh effluvia were most powerful. Sydenham no where intimates that his epidemic dysentery was contagious, and Willis distinctly asserts that it was not. Even Sir John Pringle admits (p. 235) "that this disorder is *not so catching as most others of the contagious kind;*" but adds that he "always found it in some degree infectious, especially in military hospitals," whenever it was "*epidemic;*" and this, according to his own explanation in other places, was always in the summer and autumn, when marsh effluvia were most abundant and active, and when febrile contagion must have been least so, and therefore when it was most easy to confound their effects. It is moreover absolutely incredible, that marsh effluvia should produce contagion, when they disorder the bowels, and not produce it when they occasion intermittent and remittent fevers. Mr. Boag doubts whether dysentery is ever contagious in the East Indies;* and all the medical gentlemen from that climate with whom I have conversed, have entertained similar doubts, or rather believed it not to be so. Dr. Mosely says, "as to contagion from infection in dysentery, I must confess I never saw an instance of it; neither do I believe there is any such thing." (p. 267.) M. Bruant, physician to the French army in Egypt, says the dysentery was not contagious in the great hospital (the house of Ibrahim-Bey,) at Cairo, where he officiated with three other physicians, though it had long been *crowded with sick.*" See Hist. Medicale de L'armeé d'Orient, 2de partie. And in

* See Medical Tracts, &c. vol. iv. p. 13. He thinks the climate of that country unfavourable to the production and propagation of contagious diseases,—observing that even the small pox gradually disappears as the summer advances.

regard to my own experience, I have no hesitation in declaring, that with thousands of soldiers in that disease, under my care at different times, and often much crowded in hospitals, barracks, and transports, I never have been able to discover any sufficient reason to believe that the disease was communicated by any of them to any nurse, orderly man, or other person.*

I shall conclude this chapter by a very few general observations on the treatment of dysentery.

As in this disease there is manifestly a morbid determination of febrile or inflammatory action upon the intestines, I think, and have always found it beneficial, speedily to counteract this disposition, and produce an opposite determination; so far at least as to create a salutary distribution of the blood, and of the living power, throughout the body, and especially upon its surface, by suitable diaphoretics, combined with opium in small doses; by the application of flannels, immediately to the skin, and more especially round the abdomen; and in urgent cases by the warm bath, (continued for the space of an hour, if the patient can bear it so long,) warm fomentations, and especially blisters upon the belly, taking care at the same time to promote sufficient evacuations by stool, to relieve the intestines as much as possible from all irritation and uneasiness, which they might suffer by a retention of hardened fæces, or scybala, and other matters. For this last purpose, the neutral purging salts, with manna, are proper, or a mixture of the oleum ricini, with the juice of a ripe orange, and a little mucilage of gum-arabic, which will agree better with most stomachs, and prove equally efficacious; emollient purgative clysters may also be employed. Should the disease be attended with considerable fever, care

* I have now before me a statement which I made on the 5th of May, 1806, of certain facts communicated to me on that day, by Dr. Macdonald, who was a staff surgeon with the army in the Netherlands, under the Duke of York, in 1793 and 4, decisively proving, that a dysentery which prevailed to a great extent, and in the worst form among the French prisoners, accumulated at *Ghent* to the number of nearly 4,000, did not manifest the slightest contagious property.

must be taken not to increase it by the too frequent use of diaphoretics and opium. When the disease, by long protraction, has occasioned ulcerations of the intestines, and more especially when it is complicated with an affection of the liver, calomel should be preferred as a purgative, and it should also be employed with opium, so as to excite a soreness of the mouth.

The food in dysentery ought to be light, and easy of digestion; indeed, the stomach will commonly bear no other. The amilaceous matter of the Maranta arundinacea, or Indian arrow-root, boiled with milk, barley, and chicken-water, salop, tapioca, &c. are generally the most acceptable, as well as salutary. But if the patient should have any particular craving, it may almost alwys be safely indulged.

The best means of obviating this disease, especially in armies, deserve consideration; and, among these means, there is, I believe, none which would prove more generally efficacious, than constantly wearing flannels round the belly, and next to the skin: the allotment of muscular fibres to that part of the body is very sparing, and so is its power of resisting cold. In Germany, Italy, Spain, and Portugal, people of the lower order, very generally wear sashes of woollen stuffs round their waists; and I have observed a similar practice among the Turks and Arabs, which, however it began, has probably been continued from a conviction of its beneficial effects in preventing disease. Dr. Grainger, at p. 36 of his Essay on the more Common West India Diseases, (2nd edition) makes the following just observation. "One should imagine it would be hardly necessary to advise to cover the bellies of the diseased (under "fluxes") *with warm blankets;* and yet for want of this simple precaution, I have known many negroes lost."

## CHAP. III.

### OBSERVATIONS ON THE PLAGUE.

Upon my return from Egypt, in 1802, I employed some time in reading, and making extracts from, such scarce books, and manuscripts, relating to the plague, and sweating sickness, as I could find in the British Museum, the libraries of Universities, (particularly that of Oxford) and other collections, partly for my own satisfaction, and partly with an expectation of publishing something on these diseases, they having previously engaged my attention in some degree. This expectation, however, has not been fulfilled; because, though my researches were not unproductive of curious matters, I have doubted whether they would prove so generally interesting, or so practically useful, as to render a publication of them desirable; and in regard to what I had either seen or thought of the plague, I hoped that Drs. Buchan and Price, army Physicians, who underwent the disease in Egypt, would render any contribution from me of no importance, by giving to the public the results of their own more extended experience on that subject. But as this hope is now almost extinguished, and as opinions which I think erroneous have been extensively propagated by high authorities, some of which confound the plague with Typhus, and others with yellow fever, I cannot allow the present volume to go into the world, without adding some facts and conclusions, tending, as I hope, to stop the progress of error; and founded, not only on a considerable share of reading, and some personal observation; but on valuable communications, with which I have been favoured, by medical gentlemen, who were employed in the Pest-houses of Egypt, and some of them for a longer time than myself.

In regard to the history of plague, I shall here introduce but a very small part of what I had collected and written on the subject. In the Hebrew, Arabic, Greek, Latin, and all other Ancient languages, with which we are acquainted, words are found signifying generally, (like the English word *Plague*) an extensive and destroying malady, when applied to diseases; and Galen, who was for many centuries the oracle of medicine, has sanctioned this application of the term; for he expressly says* that epidemic and plague, are not names of any particular disease; but that the former designates a disorder attacking many persons in a district; and that when it proves *mortal to great numbers*, it then becomes a plague: and with these vague significations the words in question were long used. But in modern times, writers who aimed at more accurate discriminations, have appropriated the word plague, and its correspondent terms in other languages, to signify exclusively a peculiar and very fatal disease distinguished by symptoms, to be hereafter mentioned, among which *Glandular* swellings in the groin, axillæ, and neck, are the most constant and remarkable.

I am convinced, by facts the most indisputable, that the disease just mentioned, and to which alone I shall apply the name of plague, is not only distinct from all others, (or sui

* Οὐ γὰρ δὴ νοϛήματός γέ τινος ὄνομά ἐϛιν ἐπίδημον ἢ λοιμῶδες, ἀλλ' ὅτι περ ἂν πολλοῖς ἐν ἑνὶ γίνηται χωρίῳ, τοῦτο ἐπίδημον ὀνομάζεται προσελθόντος δ' αὐτῷ τοῦ πολλοὺς ἀναιρεῖν, λοιμὸς γίνεται. Vid. Foesius Œconom. Hippoc. p 638.

I am informed by Mr. Brown, the African traveller, that Koubeh, and Webe, or Vebe, are terms now used by the Egyptians to designate the plague. The former signifies *a great mortality from disease*, and the latter a *grave*; and conjoined these words imply a disease which sends many people to their graves.

generis) but that it is, morever, *specifically contagious*, and, consequently, incapable of being any where produced, except from its own contagion; and I think it highly probable that this disease has subsisted almost from the commencement of human existence, and been continued from generation to generation, by its peculiar contagious quality; though, from the want of proper discrimination, which is found in all the ancient medical writers, and which might well be expected in regard to those diseases, which were ascribed to *supernatural causes*, there is, I believe, no clear, definite, and certain description of the plague, anterior to that which *Procopius** gave

* Procopius, a Greek Byzantine historian, was secretary to Belisarius, and attended him in the wars of Persia, Africa, and Italy, after which he became prefect or governor of Constantinople. It is in the second book of his History of the *Persian War*, (Chap. 22,) that he gives an account of this plague. I have made the following translation of those passages in which he describes the disease, viz. "Persons were seized in this manner: they suddenly became feverish, some whilst asleep, and others, in their ordinary occupations. The body did not change colour, nor grow hot, nor inflamed, even with the fever, which was so moderate from the beginning, and until evening, that neither the patient, nor his physician, by feeling the pulse, apprehended any danger; for no one could have suspected death from it: A *tumor* rose, in some on the first, and in others on the second day, but sometimes later, not only in the groin but also within the axillæ, (armpits) and sometimes behind, or under the ears, or in some other part as might happen. These symptoms have hitherto appeared very constantly, in all who were attacked by the disease; other symptoms were less constant. I cannot say whether from any differences in the bodies of the sick, or from the will of him who sent the disease." "In some there was a profound coma, in others a violent delirium." "Some died very speedily, others lived several days. The body was spotted with black pustules of the size of a lentil, in some who did not live a single day; but died immediately: others were carried off by a spontaneous vomiting of blood This I can declare truly, that the most able Physicians often prognosticated the deaths of numbers who shortly after recovered surprisingly; and, on the other hand, they have pronounced the recovery of many who were doomed to speedy death: so that in this distemper there was no reasoning which could assist human judgment; no one being able, in most cases, to foresee the event. Bathing was beneficial to some, but to others it did harm. Many died who were left without attendance, while others in the like circumstances unexpectedly recovered. Again, all methods of treatment succeeded with some, though, in short, no certain means were discovered, either to cure or prevent the disease." To pregnant women the disorder was certain death, for those who miscarried died, and those who were delivered perished with their offspring." "It generally happened that those whose buboes grew large and suppurated, recovered from the disease, which seemed to spend its violence upon these tumors;—

of the great plague that began about the year 542, and lasted more than half a century, destroying a great part of mankind, as far as the world was then known. But there is, I think, great probability, that this disease was known to the

whilst in those whose buboes remained without suppuration, it had an unfavourable termination." "This disease lasted for four months at Byzantium, (Constantinople) though it raged most during three only. In the beginning few died beyond the usual number; but the evil soon increased to such an extent, that the deaths amounted to 5,000 *daily*, and at length to 10,000 and upwards. At first, care was taken to bury the dead in the vaults of their families, but afterwards they were thrown into the sepulchres of others, sometimes from ignorance, but often by violence. In the end an universal confusion prevailed; servants being left without masters, and masters, once very opulent, deprived of servants by death or sickness. Many houses became desolate, and bodies sometimes remained in them unburied, because none of their inhabitants had survived."

*Agathias*, another Greek historian, who lived about the same time, and wrote an history of Justinian's reign, beginning where that of Procopius ends, mentions another violent eruption of the plague at Byzantium, where, he says it had never entirely ceased, from its commencement in the 5th year of that Emperor. He says, it destroyed myriads, and with symptoms like those which at first distinguished it. See the Greek edition, with the Latin version, and notes by Bonaventure Vulcanius, printed at Leyden, 1594, (4°) Lib. V. p. 148.

A farther account of this plague was given by *Evagrius* Scholasticus, an ecclesiastical Greek historian, who lived in the same century. He calls it, the *inguinal* plague, and says it began two years after the taking of *Antioch*, by the Persians (under Cosroes, A. D 540,) and that whilst he was writing it had invaded that city for the fourth time, having then lasted 52 years, and almost depopulated the world. The following is a translation of his description of the disease; viz. "In some persons, seizing the head, it rendered the eyes sanguineous, and the face tumid; then falling on the (glands of the) throat, it put an end to life, in all who were thus seized.*— Some were afflicted with discharges from the bowels; in others an abscess formed in the groin, and, being followed by a raging fever, the patient died on the second or third day with his body and mind apparently sound. "Some were seized with delirium and expired. Carbuncles arising on the body, extinguished life in many. Others recovered once, and afterwards died of the same disease. The modes of contracting the disease were various, and baffled all calculation. Some perished by once

* In modern times, patients under plague have been speedily suffocated by such an affection of the sub-maxillary glands.

Orræus, at p. 96 of his Descriptio Pestis, after mentioning parotid swellings adds, "At quotquot submaxillaribus obnoxios videre mihi contigit ob enormem intumescentiam partium laryngi proximarum, et inde *productam suffocationem* omnes e vita migrarunt. Horrendum sane aspectum, a tumore vastissimo deformes oribus hiantibus et lingua exserta anxio anhelantes, præbuerunt miseri."

Jews at an early period of their history, and that it was the disease mentioned in 1st of Samuel, chap. v. verses 6 and 9; and ch. vi. v. 19, under the name of "Emrods" (or Hemorrhoids) according to the English translation; which seems entirely gratuitous; the Hebrew word being (*Apholin* or *Apholim*) and its root (*Apol.*) signifying an elevation, eminence, or hill, it properly denotes a tumor, or swelling, in some secret part; and I believe, there is no sufficient authority for referring these tumors to the *anus*, rather than to the groin, where they would constitute *buboes*, and indicate the plague; a disease which is much more likely than the *piles* to have spread extensively and destructively among the Philistines at *Ashdod*, and to have been communicated by them, when they restored the ark to the Israelites, at Beth-Shemesh, and have there caused the deaths of 50,070 men;" who were thus afflicted, as is said, "because they had looked into the ark of the Lord." These swellings would have been as much in the "secret parts" of men wearing clothes, when placed in the groin, as if placed in the other situation. It may indeed be alleged as an objection to this construction, that, among the dreadful curses denounced in the 28th Chapter of Deuteronomy, v. 6, the Hebrew word which denotes *Pestilence* (Deber) is different from that which in the 27th verse, has been translated Emrods. But there is no force in this objection, unless it can be ascertained, that the word Deber then signified the true plague, and if that be ascertained, it will prove this disease to have been known to the Jews at an earlier period, or soon after they left Egypt.

From several passages in the writings of Hippocrates, it seems probable, at least, that the true plague had fallen under his observation. He mentions, at the 55th verse of the

entering into infected houses." "Some by only touching the sick." "Many who remained with the sick and freely handled them as well as the dead bodies, wholly escaped the malady." Hist. Eccles. lib. iv. c. 29.

4th book of his Aphorisms,* that fevers accompanied with *buboes are all dangerous*, except ephemeral† fevers; and this passage is repeated in the 7th section of the 2d book of Epidemics, with this addition,‡ "and buboes occurring in fevers are more dangerous, especially if in fevers which are acute, they subside soon after their appearance:" and these observations may be considered, as correctly applicable to the plague. It is also probable, that the disease which is described as‖ prevailing among the *fullers* in the 5th book of his Epidemics; 7th Section (page 1155 of Fœsius's edit. E.) and also in the 7th book of the same, 7th Sect. (p. 1229. H.) was the plague: and this probability is heightened by this consideration, that, if the plague existed in the country, no class of men would be so liable to be infected by it as fullers, who were then very much employed in cleansing the woollen clothes almost universally worn in those days.

* Hippocrates Aphor. l. iv. 55. Οἱ ἐπὶ βουβῶσι πυρετοί πάντες κακοὶ πλὴν τῶν ἐφημέρων.

† By the exception here made in regard to ephemeral, he appears to have meant those short and mild fevers, of one paroxysm, which so frequently attend the suppurative process in buboes, caused by absorption. These buboes are well described by Galen, in the 15th book of his Methodus Medendi; of which description the following is a translation. "Thus also, from an ulcer coming on a finger, either of the foot or of the hand, the glands in the groin and the axilla, swell and inflame, upon first receiving the blood returned from the extremity of the limb: and about the neck, and the ears, glands have often swelled from ulcerations either upon the head, or the neck, or some of the neighboring parts. They call glands thus swollen buboes."

‡ Οἱ ἐπι βουβῶσι πυρετοὶ κακὸν πλην τῶν ἐφημέρων, καὶ οἱ ἐπὶ πυρετοῖσι βουβῶνες κακίονες ἐν τοῖσιν ὀξέσιν ἐξάρχης παρακμήσαντες. Hipp. Epid. Lib. 2. Sect. 7. Fœs. p. 1025.

‖ "Among the fullers tubercles arose in the groin, which were hard, and not painful; others also of the same size about the pubes, and in the neck. There was fever, at first attended with coughing. On the third or fourth day the bowels were affected with diarrhœ; heats supervened, with dry tongue, and thirst. The alvine discharges were very bad; *they died.*" In this translation I have taken the liberty of altering the passage, τρίτῃ μηνὶ τετάρτῳ. which is obviously defective, into τρίτῃ μὲν ἢ τετάρτῃ, which I conceive to be the only proper reading.

These are the parts which favour most the supposition that Hippocrates was acquainted with the plague. Galen, indeed, mentions (in lib. de Therm. ad Pison.) that Hippocrates put an end to the plague at Athens, by lighting fires; but, after the definition, before quoted, of the plague by this author, we can have no certainty that the disorder then prevalent in Athens was the true plague.

It does not appear, however, that Hippocrates knew either the peculiar nature of the plague, or its contagious property. I have not indeed met with any passage in his writings which clearly shows, that he was aware even of the existence of such a quality in disease as contagion.*

In regard to the description given by *Thucydides* of the plague at Athens, in the Peloponnesian war, I must observe, that whatever its merits may be in other respects, it is too vague and inaccurate for any medical purpose; for it does not unequivocally designate any known disease. The parts of it

* Galen was manifestly convinced of the contagious quality of the plague when he used these expressions, συνδιατρίβειν τοῖς λοιμώττουσιν ἐπισφαλές, ἀπολαῦσαι γὰρ κίνδυνος. ὥσπερ ψώρας τινὸς ἤ ὀφθαλμίας Lib. 1. c. 2. de Different. Febrium. The last observation respecting the contagion of Opthalmia is become remarkable, since this quality has been ascertained to exist, in that species of it which has been imported from Egypt. Galen morever acknowledges that he left Rome and returned to his own country (Pergamus) to avoid the contagion of plague. *Aristotle* also believed the plague to be contagious; for he asks.

Prob. Sect. 1. prob. 7. Διά τίποτε ὁ λοιμὸς μόνη τῶν νόσων μάλιστα τοὺς πλησιάζοντας τοῖς θεραπευομένοις προσαναπίμπλησιν. η ὅτι μόνη τῶν νόσων κοινὴ ἐστὶν ἅπασι ὥστε διὰ τοῦτο πᾶσιν ἐπιφέρει τὸν λοιμὸν. ὅσοι φαύλως ἔχοντες προυπάρχωσι, καὶ γὰρ διὰ τὸ ὑπέκκαυμα τῆς νόσου τῆς παρὰ τῶν θεραπευομένων γινομένης ταχέως ὑπὸ τοῦ πράγματος ἁλίσκονται Also,

Probl. Sect. VII. 1. Διά τὶ ἀπὸ μὲν νόσων ἐνίων νοσοῦσιν ὁι πλησιάζοντες, ἀπὸ δὲ ὑγιείας οὐδεις ὑγιάζεται; which is a *plain mention* of contagion, though not applied to any particular disease.

Aretæus, in the last chapter of his second Book of Therapeutics, observes, that elephantiasis is as contagious as plague.

"Δέος δε ξυμβιοῦν τε, καὶ ξυνδιαιτᾶσθαι, οὐ μεων ἤ λοιμῷ, ἀναπνοης γὰρ ἐς μετάδοισιν ῥηίδίη βαφη'."

most applicable to the plague are the alleged contagious property of the disorder, and the mention of small pustules and sores, as marking the bodies of the sick. Φλυκταίναισι μικραῖς καὶ ἕλκεσιν ἐξηνθηκός. There is, however, no intimation of any glandular tumours, which must have occurred in the true plague; and all things considered, the disease seems most likely to have been a marsh fever, combined with scurvy, modified and aggravated by an accumulation of eight times the usual number of inhabitants, afflicted by famine, despair, and all the calamities of war. It is remarkable, that the disease began, and prevailed most, at those seasons when marsh miasmata are always most powerful; and though said to have been contagious, it was not communicated even to the neighbouring towns of Peloponnesus and Bœotia: a plain indication of its having been produced, and propagated by local causes.

Among the definitions of plague given by nosologists, that of Dr. Cullen* seems to be one of the least objectionable. But even this is faulty, by including typhus fever, which probably *never occurs* in this disease, and is as distinct from it, as from small pox or measles. It may indeed be possible that a person who has been exposed to the contagion, both of plague and typhus, should be attacked by both diseases together; but in this case, the infection of each would doubtless remain distinct, and only be able to propagate its peculiar disease; because one is communicable by *immediate contact* only, and the other, so far as we can judge, exclusively through the *medium of the atmosphere.*

The definition of Pestis, by Sauvage, accords with that of Cullen, and is liable to a similar objection. Linnæus, substitutes a most acute *Synocha* Typhus fever; a substitution which will often be at variance with fact, as in most cases no such fever is present. Vogel errs in a greater degree, by arranging plague among simple *continued fevers*, stating it to be epi-

* *Pestis.* "Typhus maxime contagiosa, cum summa debilitate."—"Incerto morbi die eruptio Bubonum vel Anthracum."

demic and most acute; and superadding a great number of symptoms, neither essential nor constant to the disease. Sagar's definition is less exceptionable than Vogel's, but it is at least objectionable, as including typhus fever.

*Orræus*, who was sent with superior medical authority, by the late empress, Catharine of Russia, to advise and assist during the plagues at Jassia (or Yassy) and Moscow, has refused to admit fever of any kind among the characteristic distinctions of plague; and assigns his reasons for doing so, at pages 71, 2, and 3, of his *Descriptio Pestis*, (4°, printed at Petersburgh, 1784,) where, though he acknowledges that febrile symptoms occur in most cases of plague, he maintains, that fever does not essentially belong to the disease, nor constantly attend it. And that it would be just as reasonable to give the name of fever to that acceleration of pulse, and other effects of morbid excitement, occasioned by acrid poisons, fumes of charcoal, &c. as to the febrile symptoms which often attend the plague. And certainly fever, properly so called, does not constitute the disease, nor is it in all cases perceptible, *even as a symptom;** but the latter part of this observa-

* In very mild cases of plague, buboes arise and suppurate or disperse, and the disease terminates without any manifest febrile symptom. I saw some such cases, in the pest-houses at Aboukir, in 1801, and a greater number have been observed by others in different situations. The like happens sometimes in the mildest cases of inoculated small-pox; on the other hand, I believe (though such cases did not fall under my observation) that the morbid impressions from the contagion of plague are sometimes so powerful as to extinguish life, before any such reaction of the system can take place, as would produce an appearance of fever. In general however, and excepting these extreme cases, there is so much of *reaction*, or of effort, by what has been called the vis medicatrix naturæ, that febrile symptoms, more or less violent, as well as variously modified, do (as in small pox) occur to persons attacked by the plague, during a part of its continuance. See Waleschmidt de sig. Pest. Holsat. 1712. Haller (Disp. ad morb. Hist. et Curat. v. 5. p. 555.) "Pestis, proprii loquendo febris vocari nequit; est enim sæpissime sine febre, et sub varia larva sua ludit dramata."

Diemerbroek observes (de peste, p. 12) "Pestis *sine febre* paucis incipiebant et finitur; pluribus sine febre quidem incipiebat, sed quæ tamen non diu post insequebatur," &c He adds, in a note, "Pestem esse quid diversum a febre, et febrem ejus esse symptoma, durante hac pestilente constitutione multoties observavimus, ac tion would apply to every other symptom of the disease, no one of which invariably and manifestly occurs to every patient; so that if, in defining the plague, we were to reject all the symptoms which are not constant and inseparable, we should have none left to denote the disease; fever, therefore, though of no particular form or species, may perhaps be admitted as part of the definition, and this with a swelling of some of the lymphatic glands, or with exanthema, including carbuncles, may serve generally to designate the plague, if accompanied by that particular contagion which is its *cause and essence, and without which there can be no plague.*

The limits prescribed for this publication, will not allow me to describe the several forms, modifications, and degrees of fever which accompany the plague in different seasons, situations, and individuals. They are in fact as numerous and various as the human constitution is capable of exhibiting. In the young, robust, and plethoric, we find synocha, or ardent fever, with the usual inflammatory appearances, and in feeble or debilitated constitutions we have the appearance of low nervous fever. The intermediate degrees and combinations are, however, much more frequent than either of these extremes; indeed, the cases of ardent fever commonly bear a very small proportion to the others, perhaps because the action of this contagion is found generally to depress or diminish the living power. In all these cases the patient's constitution and circumstances have a great, if not an exclusive influence upon the febrile and other symptoms; with this exception, however, that in persons who have previously been sufficiently exposed to marsh effluvia, the fever to which this contagion acts as an exciting cause, generally

proinde nonnulli pestem malè per febrem definiunt, cum febris non sit de ipsius essentia, ut infra probabitur cap. 12. annot. 2"

Dr. Sotira, at p. 3. of his Memoir sur la Peste, observée en Egypte, in describing the varieties of that disease, says, "Finalement, il y en avait quelques-uns attaques d'un léger mal de tête, avec un dégoût passager, *sans fievre*, qui souffraient un picotement sous l'aine, a l'endroit ou le bubon pestilentiel se manifeste. Peu a peu le bubon se prononcait, *pour disparaitre*, ou pour *suppurer* long-tems apres," &c.

takes on the form either of a quotidian intermittent, or of a double tertian."*

I have assumed this disease to be essentially and specifically contagious, and I shall presently mention sufficient proofs and grounds for the assumption. But I think it expedient first to offer some observations, concerning the channels through which its contagion is received by the human body; because the production of buboes, in my opinion, depends entirely on this fact, that the contagion after being applied by contact to the skin, is exclusively received *through it*, and conveyed by the lymphatics into the blood vessels,† as in the case of inocu-

* Dr. Price informed me that in all or nearly all the Sepoys or other East Indians attacked by the plague, who had fallen under his care in Egypt, the accompanying fever was of the intermitting form; that the paroxysm began with cold and shivering; and it was during the *cold stage* that most of these blacks died. They had probably all been exposed to marsh miasmata, in the neighbourhood of Rosetta; and to this circumstance I should ascribe the occurrence of this form of fever among them.

† The facts which prove the necessity of actual contact with some infected person or thing to communicate the plague, are so numerous, and many of them so notorious that it must be unnecessary for me to enter upon a detail of them, after what Dr. Russel and others have published, and after the experience of the British army in Egypt, which invariably demonstrated this necessity, by shewing that all those who avoided contact invariably escaped the disease, whilst those who did otherwise in suitable conditions, were very generally infected. Nor was there, so far as I have been able to discover, any instance, in the French Egyptian army, of a communication of the disease without contact, though the Physicians to that army, who have written on the subject, do not, I believe *positively* assert the impossibility of such communication. But M. Desgenettes, the chief Physician to that army, at p. 248, when writing upon this contagion, says, "on a vu un *simple fosse, fait en avant d'un camp, en arreter les ravages*; et c'est sur des observations de cé genre, que est fondé *l'isolement avantageux* des Francs, dont la pratique a ete suffisamment, detaillée par divers voyageurs." And Dr. Pugnet, one of the physicians of the same army, (whose experience in this disease was very extensive) at p. 130 of his Memoires, sur les Fievers Pestilentielles, &c. du Levant, not only supposes the necessity of contact, but adds that even this will not suffice without an *aptitude* in the *receiver* of the contagion. He indeed afterwards intimates, that from the crowded and confused state of the army, he had not been able to *ascertain* "si le contact de la personne malade, ou de ce qu'il a touché *est indispensable*, pour donner la maladie," &c. It is apparent, however, that he did not know of any other way in which the disease had been communicated; and the physicians employed at Moscow, during the plague, which destroyed nearly 60,000 inhabitants of that city, in

lation for the small pox; and hence we may account for the morbid state of the lymphatic system, which has been observed, in the few cases of dissection, where proper attention was paid to its condition. The effects of morbid poisons, and other noxious matters when absorbed, upon the glands connected with the absorbents, have been sufficiently manifested; and it is from this cause that the axillary glands of one or both arms, when the small pox has been introduced by inoculation, become swelled about the time or a little before the commencement of the eruptive fever; and (as is well known) it is also by an absorption of venereal poison, through a particular organ, that the inguinal glands, to which that poison is directly conveyed by the absorbents, become affected; and it is because the contagion of plague is not commonly applied and communicated through the same organ, that pestilential buboes near the groin are not often, if ever, formed in the very same glands as the venereal, but in the femoral and

1771, appear, by abundant experience, to have left no room for doubt on the subject.—Dr. de Mertens, in the English translation of his account of that calamity, says, "the contagion was communicated *solely by contact* of the sick, or infected goods." "It was not propagated by the atmosphere." He adds, "when we visited any of the sick we went so near them that frequently there was not more than a foot distance between them and us; and though we used no other precaution than that of not touching their bodies, clothes, or beds, we escaped infection." M. Samoïlowitz, who was surgeon to the great military hospital, where the plague in question first appeared, and who besides the most extensive experience in Moscow, had been greatly employed for that disease, in Poland, Moldavia, and Wallachia, asserts, in the preface to his "Memoire sur la Peste,"&c. that "il est certain que la peste ne se develloppe, et ne se propage que par le *contact* ainsi que je le *demontre* dans mon memorie;" which indeed he afterwards does, by numerous facts. But after all these observations, I would not be understood as maintaining that the air expired from the lungs of a patient under the plague, and loaded with humidity, may not contain some contagious matter, capable, if *immediately received into the mouth and lungs* of another peason (by a very near approach of their faces to each other) of being absorbed, and taken up by the lymphatics spread over these internal surfaces, or perhaps by the lungs, so as to produce the disease: This would, I think, be nearly equivalent to contact, and attended with no more difficulty than there is in an absorption by the skin. Orræus says, p. 151, "*communissima* affectionis via per *contactum* observata."—He thinks, however, that the disease may be taken by the breath; but if this were true, the other would not be the *most common way*, because people frequently can and do avoid contact but cannot avoid breathing with the sick.

other glands which are connected with the lymphatics coming from the lower extremities. Orræus, indeed, mentions a fact, which, without his appearing to be sensible of it, demonstrates the production of pestilential buboes by absorption. He says, p. 154, " In quibus escharæ carbunculorum, post superatam pestem acutam, diutius neglectæ restitarunt, partes adjacentes valde intumuerunt, et in non nullis *bubones* de novo suscitabuntur." These secondary, or new buboes, could only be caused by an absorption from the protracted and neglected carbuncles.*

While the lymphatic system and its uses were but very little known, and pestilential buboes were considered as an effect of the vis medicatrix naturæ, and as being intended to facilitate a critical separation and discharge of the pestilential virus from the blood, we need not wonder that a morbid absorption was not suspected to have been their cause. But it seems extraordinary that in recent times, and with modern discoveries, not only Samoïlowitz should suppose buboes to be formed by the contagion of the plague thrown *outwardly* from the blood, (See his Memoire, p. 112,) but that a similar opinion should have seemingly been entertained by M. Desgenettes, the chief physician of the French army in Egypt. I conclude at least that this must have been his opinion, because he states buboes to have been produced by an *inverted action*

* Platerus had also observed the production of buboes, by the influence of carbuncles, though he probably did not suspect the way by which that influence was exerted. He says (Praxeos Medicæ, t. ii. p. 79) " Sed et fit ut bubones in peste correptis, non semper, a venenata illa vi" (veneni pestiferi) " in corporis emunctoria excussâ, verum ob carbonis vicini ardorem doloremque influxum hunc in adenes commoventis, uti in aliis quoque inflamationibus accidit proveniant." He afterwards mentions the formation of *carbuncles*, particularly in pestilential fevers; adding, " a quo *anthrace* ab initio lineam rubram ad bubonem, qui plerumque illum comitari solet" " porrigi sæpe observavimus." The red lines here mentioned are *now* known to proceed from an inflammation of the absorbents ; and they were observed, even by Galen; see the note to page 556. He adds, concerning these buboes, " cernitur autem aliquando ipsa quo que *vena* per totum membrum *rubra* et calens, et distenta." &c. But though secondary buboes may be produced by an absorption from carbuncles, the latter can never produce them on patients in whom carbuncles do not occur, or only occur *subsequently* to the buboes, as is often the case.

*of the absorbent system*,* contrary to every thing analogous with which I am acquainted. And in the very next paragraph, after mentioning carbuncles as being *eminently contagious*, he says (that in opposition to buboes) they are produced by *direct absorption*:—" par *absorption* directe, c'est a dire dans l'ordre ordinaire, et par la voie la plus courte, et *le plus simple contact*." To me, however, it seems most probable, that if either buboes or carbuncles result from any thing thrown outwardly by arterial action, or by any effort of nature, it must be the latter, rather than the former, which are so produced. I have insisted the more on this subject, because the truth concerning it seems to be of some importance in regard to the prognosis, as well as treatment of the disease.

Dr. Price informed me that in all the bodies of persons who had died of plague, which he dissected in Egypt, the *glandular system* was morbidly affected;† and Dr. Sotira, who was physician to the French army there, observes, that according to his information, those who died of that disease, and had been examined by the French medical officers, besides a morbid state of the brain and spinal marrow, were found to have "tout le systeme des glandes lymphatiques engorgé." See Memoire sur la Peste Observée en Egypt, &c." p. 8.

With these, and other proofs of morbid absorption by the lymphatics, it is not surprising that buboes should be the most frequent of all the symptoms which occur in this multiform disease. On a general computation, I think it would appear that glandular swellings have been observed in nearly three-fourths of those who were supposed to have had plague,

* " Les bubons pestilentiels sont des engorgements des glandes lymphatiques, qui s'operent évidement par un *mouvement inverse* du systeme absorbant." Hist. Medicale de L'armée d'Orient, p. 109.

† Dr. Price informed me, also, that in all the bodies which he had dissected, the liver was *greatly enlarged*: but these, excepting one, had all been born in the East Indies; and on my asking, whether he did not think it more likely that an affection of that viscus should have existed previous to the attact of plague, than that such enlargements should have been so suddenly produced by that disease, he answered in the affirmative.

and many are erroneously supposed to have had it, when it prevails extensively and destructively, and as no sufficient examination takes place in many of the more violent cases, where buboes often do not appear till the approach of death; and there are others, where the rudiments, or *germs*, are discoverable only after death, and by such applications of the fingers as are both dangerous and unpleasant, it may be inferred, that but very few if any persons have undergone this disease, who either had not glandular swellings, or in whom they would not have occurred, if life had not been extinguished, before there was sufficient time and reaction of the system for their production. I do not, however, think it impossible that so much of the contagion of the plague as will suffice to produce the disease, should find its way into the system by the absorbents, without producing a swelling of the glands, though facts prove that this does not commonly happen. Whether in any of those mild cases, where buboes have appeared *without fever*, and which have been supposed to be most liable to *re-infection*, the contagion had affected the glands, *without finding its way into the blood vessels*, I am unable to determine. I am also unable to explain why the fermoral or inguinal glands, should he much oftener affected than those of the axilla; a fact which has been generally observed, and which seems to make it probable, that the contagion of the plague has been more frequently taken up by the absorbents of the lower extremities, than by those of the hands and arms. This might well be the case with persons, who, like the inferior inhabitants of warm countries, seldom wear shoes and stockings, but there is some difficulty in understanding how it could happen to others, unless stockings by absorbing and retaining the contagion, favour, rather than obstruct, its approach to the skin.

When the disease is likely to prove mild, its commencement is commonly first indicated by hardness of the glands, and in many cases this occurs with, or soon after, the first febrile or other morbid symptom: often, however, and especially in cases of great debility, no glandular affection is discoverable for several days, nor even until the near approach of death. So much has been written by various authors (and particularly Orræus, at p. 95 and 6,) in regard to buboes, their appearances, situations,* numbers, sizes, &c. that as I am not giving a treatise of plague, I may be allowed to pass over these topics.

For similar reasons, I shall offer very few observations respecting the *anthrax* or *carbuncle,* of which Orræus seems to have given the best account.† Their occurrence is, I believe,

* " Dès qu'un bubon parait soit aux aines ou aillieurs il se place tousjours de côté, au-dessus ou au-desous de la glande et jamais sur la glande même comme les bubons veneriens. Ceux des aines prominent ordinairement deux doigts au-dessous des glandes inguinales." Memoires ur la Peste, par M. D. Samoïlowitz, M. D. &c. p 138.

† Orræus p. 96. " Carbunculi nihil aliud sunt quam *siderationes* partiales *cutis,* et *ei* proximæ *cellulosæ,*" (membranæ) " anigredine crustæ mortuæ sic nuncupati."

He makes a distinction of carbuncles into the *moist* and *dry,* which I do not recollect to have been made by any other writer, though it appears a very proper and necessary one. The former is that which seems to agree best with what authors have described as the pestilential carbuncle. Orræus describes it thus :—" Febre pestilentiali *jam* oborta, vel interdum *simul cum eâ,* pars *qualiscunque* corporis, nunc majoris, nunc minoris ambitus *ardere, dolere, rubescere, et tumescere incipit :* (in aliis non nisi *macula rubra,* vix *supra superficiem* cutis prominens, conspicitur,) non diu post, in *medio* tumoris una vel plures, haud procul a se invicem distantes *pustulæ,* quasi capitula acuum," (pins' heads) " *majuscula,* altitudinem lineæ," (1-12 of an inch) " raro superantes, pallidiusculæ & sanie turbida repletæ exsurgunt, quæ post breve intervallum crepant ; cutis vero subjecta livescens & mox ingrescens sphacelum jam factum indigitat. Nigrities hæc paulatim in omnes dimensiones ulterius serpit, cum peripheria semper inflammata. *Sæpe ex* carbunculo in variam directionem præsertim ad tractum majorum vasorum & tendinum, *vibices* sat insignes protenduntur." Samoïlowitz (p. 142) says, that the only parts in which the carbuncles do not happen are " les parties recouvertes de *poils,* ainsi que celles ou se manifestent les *bubons.*" He is probably *wrong* with respect to the latter for Orræus (p. 98) says, that sometimes the moist carbuncle " bubonibus implantatur ;" Samoïlowitz is incorrect, too, as to the *progress* of the carbuncle ; for after saying, (p 143) that " les pestiféres eprouvent deja une douleur très vive à *l'endroit* ou ils doivent se placer :"—He mentions, that " il faut *ausitot* visiter *l'endroit* qu'il indique. On y trouvera *d'abord* un tres petit bubon, ou pustule rempli d'une serosite jaunatre, sans *oucun* signe *d'inflammation.*" Now this is what I believe never takes place, for the excessive pain felt at the part is only the effect of very considerable inflammation existing in it, which usually arises to such a degree of violence, as at last to destroy the vitality of the part : this progress, too, through the various degrees of inflammation and mortification is in the *moist* bubo by no means so rapid as is totally unconnected with that of buboes,—I mean that buboes have no influence on their production. When they appear very early, they assume a dark brown or black colour, and remain forty-eight hours or more, without being circumscribed by an inflamed margin; they generally indicate the greatest danger.

*Exanthemata* are of several species:—one is a vescicular eruption, sometimes of the size of a pea, or larger, appearing without any determinate situation, of a yellowish or livid colour, and with an inflamed margin; they were formerly known to the people of England by the name of *blains:* when three or four of them arise near to each other, they often become confluent, and, by uniting, produce what Orræus calls a dry carbuncle, to which from the first they have great affinity; those which are of a livid colour, flabby, and confluent, may be considered as a very unfavourable symptom.

Another exanthematous eruption attending the plague, may be considered as petechial; it renders the skin spotted, and assumes different colours, sometimes reddish, but it more frequently approaches to blue, purple, or brown. The dark coloured spots were in this country called, and deemed to be, *tokens* or signs of death, and found to be such in Egypt.

commonly supposed; for the inflammation of the carbuncle may proceed to a certain height, and *then stop before any mortification has begun,* and this after the inflammation has existed for a day or two. Thus Orræus (p. 112) "Quam primum *febris,*" (accompanying the plague,) "funditus sublata fuerit, rudimentum carbunculi inflammatum *dissipatur;* interdum (uti *in me ipso* accidit) humor purulentus quasi sub *vesiculâ* grandiori *derepente obortâ* colligitur, & evacuatione per incisionem facta, *fundus cutis ruberrimus,* & minime *sideratus* per suppurationem leviorem sanationem facile *admittit.*"

The dry carbuncle (says Orræus p. 97) "e contra sine ullis inflammationis indiciis *e maculis*" (petechiis) "latioribus *confluentibus enascitur,* quæ *sæpe ante febrem* aderant: hâc vero" (febre) "jam *accensâ* cutis nigerrima facta arescit, corrugatur, et . . . . vicina depascitur;" *rubor* "*marginis*" "fere nullus est." He adds, "Periculossisimus est & vix multi eo affecti ex naufragio vitæ emergunt, dum exhumido" . . . . "maxima ægrotorum pars convalescit, nisi in partibus nobilioribus . . . locatus fuerit," &c. or unless it shall grow to a vast size, and produce suffocation, or exhaust the patient's strength. This dry carbuncle is not very painful.

These petechial spots do not change their form or character like the vesicular eruptions. Orræus mentions, at p. 113, a case in which these spots made their first appearance in great numbers, immediately *after* death.

Of the contagious nature of the plague, I should hardly have thought it necessary to adduce any proofs, after all that has been *experienced*, and written of its dreadful effects, had it not lately become fashionable to entertain doubts, at least, on the subject, without any other foundation or reason, so far as I can discover, but that of the *escapes* of persons who sometimes are seemingly exposed to this contagion, sufficiently for the production of disease. I have certainly not been inattentive to facts of this nature, nor unwilling to allow them their full force; and the opinions and modes of reasoning, which I have entertained in regard to yellow fever, have led me to endeavour, as far as possible, to ascertain how far the multitudes of opposite facts could be explained, by supposing the operation of any local or atmospherical cause, distinct from personal contagion, and particularly that of marsh miasmata, to which plague has recently been ascribed by writers whose opinions are justly of great weight: I have however, found insuperable difficulties in the way of every supposition which does not admit the influence of a specific contagion.

When I took charge of the pest houses at Aboukir, in 1801, Dr. Buchan, my predecessor, and every other medical officer employed in that dangerous service, had already caught the disease; and of these officers, *twelve in number*, seven had died, besides a considerable number of nurses, and other attendants on the sick; though if there be any spot on earth exempt from the operation of marsh miasmata, it would, I think, have been that upon which these *pest houses* were placed, together with the surrounding dry, barren sands, within which, those who took the plague in this manner, had in effect been confined. The cause which had thus created a specific disease in *every* medical officer exposed to its action, must have been *peculiar* and *powerful*, and there was not the smallest reason to suspect the presence of any morbid influence, except that of pestilential contagion, nor could marsh effluvia, had they been present, have occasioned such a disease,* nor, indeed, could any thing else within our knowledge, other than its own specific contagion.

The medical officers of the French army had previously experienced the effects of this contagion to a much greater extent. Dr. Sotira, one of its physicians, after expressing his astonishment that there should be men "assez bizarres pour ne pas croire a la contagion de la peste," among other proofs of its possessing that property, mentions the loss which was sustained from this disease by that army in the seventh year of what was called the French Republic, "d'environ quatre vignts officiers de sante" of about *eighty* medical offi-

* There are many *irresistible proofs*, that the cause of plague is perfectly distinct and unconnected with that of yellow, and other marsh fevers. Were it the same, we should certainly find the former disease most prevalent between the tropics, instead of being, as it notoriously is, totally excluded from so great a part of the globe; and we certainly should not find its progress suspended in Egypt during the hottest months, when marsh miasmata are most active and powerful; nor should we find the nat ves of Africa and of the East Indies, who are least susceptible of morbid impressions from the latter, and in whom marsh fevers, when they do occur, are mildest, not only taking the plague frequently, but dying of it in far greater proportion than any other race of men; as was found to be the case by the British East Indian army in Egypt, and as Desgenettes, Pugnet, Sotira, and the other French physicians, declare to have happened to the negroes who fell under their observation Dr. Sotira, indeed, asserts, that all of them who had the disease died of it very soon. The circumstances which influence the production of marsh miasmata appear to have no share in causing the plague; its ravages being as great in the *high, arid* and *barren* parts of *Syria*, as among the canals, and upon the rich soil, of *Lower Egypt;* and indeed, it prevails least in those parts of Lower Egypt which are most productive of marsh effluvia; and particularly the *Delta*. I havesaid nothing of the very important and *essential* differences which must always subsist between the plague and yellow fever, notwithstanding all the ingenuity and labor which have been employed to give them an apparent similitude. Nor have I noticed the certainty with which the Franks secure themselves from plague by *shutting up*, provided the known precautions are not neglected or transgressed, as sometimes happens. Would such precautions exclude marsh miasmata, or would a ditch ward off their morbid influence, and as Desgenettes asserts, have secured an army from the plague?

cers; a loss which, he says, was the more deplorable, because it could not be repaired. He adds, that in the *two following years*, it was thought expedient to employ Turkish barbers, to dress buboes, carbuncles, and blisters, as well as to bleed and apply frictions of oil, under the inspection of French physicians and surgeons, and that by these means only twelve medical officers died in *twice* the former space of time.* As the deaths of the first year afforded a strong proof of contagion in the disease, their great subsequent diminution manifested the probability of escaping it, by abstaining from the *actual contact* of infected persons and things.

When the plague re-appeared in the British Indian army, during the autumn of 1801, and the succeeding winter, more precautions were used by the medical officers employed in the pest houses, to guard against contagion, and a greater proportion of them escaped: but still a majority of these gentlemen took the disease, and to more than half of them it proved fatal. I could fill volumes with valid and well-attested proofs of the contagious nature of plague. But I must refer those who may entertain doubts on this subject to the facts published by the French physicians who were in Egypt, and by Orræus, Samoïlowitz,* and others, who saw the plague in Russia, Moldavia, Wallachia, Poland, &c.

* See Memoire sur la Peste observée en Egypte, &c. par Gaetan Sotira, Docteur en Mèdecine, Medecin de l'Armé d' Orient, &c. p. 5. He also mentions, that more than half of the Turks, who were thus employed to asist the French surgeons, took the plague, which in several instances proved mortal; though among a considerable number of other Turks employed at Rosetta by the French, to bury the dead, only one caught the disease. This is one of the many facts which indicate that there is much greater danger in handling the bodies of infected persons whilst *alive*, than after death.

† Dr Samoïlowitz, who for many years officiated as an army surgeon in places where he had numerous opportunities of seeing persons under the plague, and who when that disease was so destructive at Moscow, in 1771, was most extensively employed there, has filled nearly one hundred pages, in the early part of his volume "*Sur la Peste*," with proofs of its contagious influence; and, among these, he mentions, that having successively volunteered his services as chief surgeon, in three of the principal hospitals at Moscow *all* the assistant surgeons who were employed under

Though nearly two thousand deaths, by plague, occurred to the French army whilst in Egypt, it was thought expedient, for a time, to deny the existence of the disease; and both the general, Buonaparte, and the chief physician, Desgenettes, exposed themselves to some dangers, in order to allay the general apprehensions of the soldiers on this subject;† and among other expedients, the latter, after dipping the point of a lancet in the pus of a bubo, on one of the convalescents, slightly pricked his groin and his arm, near the axilla, taking care, however, to wash himself immediately with soap and water, which, as he says, were brought him for that purpose; a small inflammation was produced in the spots which had been thus pricked, which lasted three weeks, but produced no worse consequence. Whether the disease of the convalescent, from whom the pus was taken, had passed beyond the stage in which it is contagious, as is probable, or whether the pus was applied in too small quantity, or washed off too soon, I will not decide. Desgenettes, indeed, acknowledges,( p. 89) that this ex-

him, *fifteen in number, took the disease*, and of these *all died*, excepting *three*; whilst the physicians who walked among the sick, but carefully avoided all contact with them or their clothes, &c. generally escaped.

Samoïlowitz was himself *three* times attacked by the disease, a circumstance which he ascribes to the dispersion of his buboes without suppuration, the first and second times.—See p. 39, 40, &c. also p. 35.

Dr. Pugnet, among other instances of pestilential contagion, says, "Huit Francais a *Caipha*, se sont successivement communiqué le germe de cette maladie, en se tran mettant une pelisse; cinq sur six, a *Gaza*, en se disputant un habit de drap, la depouille d'un de leurs compatriotes; quatre a *Jaffa* en mettant a leur usage des mouchoirs de *Col* qu' un Pharmacien de troisieme classe, *mort*, avait apporté d'Italie. Ces quatres heritiers, furent *en meme temps, atteints de bubons a l' entour du Col et perient du troisieme au sixiéme jour.*" See p. 229, 230. These four instances of persons becoming infected by tying round their necks handkerchiefs which had imbibed the contagion, *and all getting buboes round the neck*, are strong proofs of the production of glandular swellings by absorption through the lymphatics, leading to the glands which thus become affected, as I have lately mentioned.

† Desgenettes, as an explanation of the motive by which he was actuated in regard to the plague on this occasion, and also in refusing ever to give that name to the disease, says, "Je crus devoir dans cette circonstance traiter l'armée entiere comme un malade, qu'il est presque toujours inutile and souvent fort dangereux, d' éclairer sur sa maladie, quand elle est très critique."

periment proves nothing against the transmission of contagion, which, says he, *has been demonstrated by a thousand examples.* "Elle n' infirme point la transmission de la contagion, *demontrée par mille exemples;* ille fait voir seulement que les conditions necessaires pour qu' elle ait lieu, ne sont pas bien determinées." Whether Dr. White, who entered the pest house of the Indian army, at El Hammed, early in January, 1802, was misled by this experiment, I know not; but, from a persuasion that the plague was not contagious, he immediately rubbed some pus, taken from a pestilential bubo, upon the inside of his left thigh, and the next morning inoculated himself in the wrist, with matter running from another bubo. Four days, however, had scarcely elapsed from his entering the pest house, before he was seized with shiverings, followed by febrile heat, &c. which he flattered himself would prove to be an intermittent. But he died of the plague before the end of the third day; and thus, unfortunately, added another to the proofs—alas! too many—of the contagious nature of this terrible disease.

We probably do not know so much of the facts and circumstances which either favour or retard the transmission of pestilential contagion from an infected person (or thing) to those who are uninfected, as would enable us, in all cases, to assign the true cause, why persons often escape harmless, whose exposure to contagion has seemingly been such as ought to have subjected them to the disease; much seems to depend on the unfitness of the atmosphere to become a vehicle of this contagion, and on the necessity of an actual application of it to the human body, and of a subsequent absorption through the skin, all which must render its introduction into the system more difficult and precarious. Volatile contagions, particularly those of small pox and measles, will necessarily be taken into the lungs of one who breathes the air in which they are diffused; and the lungs, being peculiarly fitted to imbibe a vital part from the inspired air, they, in doing this, may probably imbibe contagion also; and therefore we might naturally expect, what seems to happen, that persons who have never been attacked by these diseases should seldom escape, when sufficiently exposed to their contagion: whilst, on the other hand, we find that those morbid poisons which being fixed, can only be received by contact, through the skin, very often fail in producing their effect; this is particularly true of the virus of rabid animals, that of syphilis, &c. which are not always of the same force, nor are the absorbents equally disposed to receive them in all men, nor at all times even in the same man. Dr. Pugnet, though he is justly convinced that nothing will produce the plague but its peculiar contagion, thinks the susceptibility of the human body for it is greatly increased by a *moist* and *moderately warm* atmosphere—that children, females, and persons of delicate, feeble constitutions, are most apt to become infected; and that those who are naturally robust and vigorous seldom take the disease, unless weakened by excessive fatigue, or by excessive indulgence with women, or intoxicating drinks. See p. 205. Dr. Sotira entertains nearly the same opinion. Desgenettes remarks, p. 248, that the plague seemed more particularly to attack those who were exposed to sudden transitions from a hot to a cold atmosphere, and vice versa: such as bakers, cooks, and blacksmiths; and that men addicted to excesses with women, and spirituous liquors, very seldom recovered from the disease.

It has been supposed by Orræus, Pugnet, and others, with some probability, that abundant transpiration through the skin, may hinder the absorption of pestilential contagion, and even wash it outward from the pores;* and on this supposition, the former has strongly recommended the taking of exercise sufficient to produce a copious discharge of sweat, after a real or supposed exposure to the contagion;—and it seems to

* If the contagion of plague be thus washed outward upon the skin, might it not descend to the legs or thighs, and after the sweating has ceased, be there taken up by the absorbents, and in this way, render inguinal, femoral, or crural buboes more frequent than those in the upper parts of the body?

have been on this supposition, that Desgenettes, after his visits to the pest houses, always mounted his horse, and rode until he found himself in a free prespiration. See p. 90.

Another probable cause of unexpected escapes from pestilential contagion may be the short time which persons under the disease continue in an infectious state. Our knowledge on this subject is very deficient. It has been ascertained that variolous patients do not infect others, at soonest, until their pustules begin to maturate, and they are probably most infectious when these are in a state of desquammation; whilst persons who have the measles, to my knowledge, have communicated the disease before any eruption was discoverable. It has not, however, been sufficiently ascertained when patients under plague first acquire the power of infecting others, nor to what stage of the disease they retain this power. I was confidentially informed, when at Aboukir, of an instance in which no infection resulted from a most intimate connexion with a female, a single hour before she was attacked by the plague. Dr. Sotira thinks the disease is *most*, if not exclusively, communicable during the existence of fever; and Pugnet thinks the disease ceases to be cóntagious so soon as the fever terminates.* Dr. Desgenettes, in his Resumé, p. 248, says the body whilst warm, and especially in the febrile state, seemed to give out contagion most easily. Orræus, however, at p. 151, represents the disease as being infectious *only* when at its acmé:—"Contagium ab iis solum, qui in *acmé* pestis constituti sunt propagari videtur." And by the account which Sonini has given of his own case, (See Essais Philosophiques, &c. p. 177, and seq.) it seems probable that when the disease has so far advanced, as that the buboes suppurate, the body ceases to give out contagion.

* If the contagion of plague depends exclusively upon the febrile action which most frequently accompanies it, those cases of the disease in which there was no fever, (such as that of a cook at the pest houses of Aboukir,) may be supposed to have been incapable of giving the disease to others.

But besides all these impediments to the communication of disease by persons ill of the plague, (and which will account for many of the supposed extraordinary escapes,) there are others arising from the influence of atmospherical heat and cold, which, in their extremes, either render the contagion dormant, or suspend that susceptibility or affinity of the human body, without which it cannot produce disease in ordinary circumstances. Pestilential contagion probably exists at all times in Lower Egypt, Syria, and many of the great cities of the Levant, and it is frequent on board Turkish and Greek vessels. It appears to have been first introduced into the British hospitals at Aboukir, by the carpenter of the Dictator, of 64 guns, who was sent in a boat to visit a Greek vessel at sea, and thus caught the disease.* This was about the beginning of May, and the disease was readily propagated, and prevailed with its usual mortality, during the whole of that and the following month, after which it was communicated with greater difficulty, and when communicated, the disease was much milder, though one case fell under my observation, towards the end of July, which proved fatal. The disease was, however, this year protracted in Egypt several weeks beyond the time when it usually disappears, which is commonly supposed to be about the 24th of June, the Nativity of St. John the Baptist, and its cessation at that time, is by superstitious christians ascribed to his benignant interference. On this occasion the effect of heat in lessening the susceptibilities of individuals, or their aptitudes for taking the disease. was most evident in those who had lately arrived from cold climates, and who were comparatively most affected by the summer's heat. This was my case, and my escape from the disease is doubtless attributable to my being in that condition, for I employed no unusual precaution, nor ever avoided feeling the pulse of a patient having the plague, when my doing so could be of any benefit.

* I afterwards discovered the plague on board a Greek ship employed by the British government in the Bay of Aboukir, and reported the fact to Lord Keith.

There were, however, persons in Egypt who had been long accustomed to greater degrees of heat, and who were therefore not rendered insusceptible of the disease, and some few of these caught it, after it had become extinct in the British army, and when a person recently landed from England would not receive it, though he slept in an infected bed; and it was from this cause, that in the autumn of the same year, the disease began at Rosetta nearly two months before the usual time, i. e. on the 13th of September, when I first discovered it in two natives of the East Indies, attached to the Indian army; and it was propagated with some rapidity for six or eight weeks, among persons who were either born in, or had just come from, a climate *much hotter* than Egypt, whilst the British troops directly from England did not receive, and probably could not have been made to take the disease. These facts are in perfect concord with what I have mentioned of the influence of heat and cold upon the human body, at p. 115 and seq. It has indeed been alleged, as a reason why the plague first appeared in, and was afterwards confined to, Rosetta, in the autumn of 1801, that it was the only open port to which vessels from Turkey and Greece resorted, and that by some of these the disease probably had been imported, because it did not, as is pretended, occur during the preceding season at Rosetta, or at least that if any case of it did occur there, it was *concealed*. This is, however, certainly erroneous; for to my knowledge, several persons at Rosetta had been attacked by the plague previously to the arrival of the Indian army, and had, without any concealment, been sent to the pest house near the town.

It is by this effect of heat, that the plague seldom appears in Upper Egypt, and never farther south than the *Cataracts*, (as I was assured by Mr. Brown, the African traveller,) and that it ceases earlier at Cairo than at Rosetta. Indeed, it was this effect which had enabled the Indian army to escape the plague until it reached Rosetta.

The cold in Egypt is never sufficient to stop the progress of the plague, and it is therefore commonly most prevalent there some weeks before and after the vernal equinox: but in Russia Poland, and even in Great Britain, the winter has commonly produced an almost complete cessation of it. This happened to the great plague at Moscow in 1771, though the manner and extent in which the houses are there warmed, and the cold air excluded, counteracted the effects of severe frost, so far, that some cases of the disease occurred during the whole winter. De Mertens tells us, however, that after the month of October, there was a great diminution in the number of attacks, and of their mortality; and this is more accurately proved, from the statement given by Orræus at p. 48, by which it appears that the deaths in September were 21,404, in October 17,561, in November 5,235, in December 805, and in January 330. Samoïlowitz also informs us, that though the hospitals then contained many persons who had been newly entered for the service of the sick, as barbers, nurses, &c. scarcely any of them had the disease after the month of November, and never but in its mild forms. Desgenettes has also observed of the plague in Egypt, at p. 248, that "les vents du nord, lex extremes du froid & du chaud, la font cesser presque entierement."

These facts will enable us, in a great degree, to understand why, notwithstanding the contagious nature of the plague, an exposure to its contagion is frequently harmless; and it is fortunate for mankind that divine providence has made its communication to depend upon the co-operation of so many favourable circumstances, and particularly that of a suitable temperature; that of its application by actual contact probably continued for some time; and that of certain aptitudes and susceptibilities in the human subject; for without such requisites, or such obstacles to the propagation of this disease, the earth might have long since become desolate.

The contagion of plague, like the poison of rabid animals, varies considerably in regard to the interval between its application to the human body, and the manifest production of disease: three, four, or five days, seem most commonly to intervene. Samo lowitz states the interval between infection and sickness, as extending from two to fifteen days inclusively; but in one or two instances which occurred at Aboukir, I was inclined to believe that the disease had been produced within 24, or at most, 36 hours after the contagion had been applied to the body.*

In regard to the means of obviating the disease, by those who cannot avoid touching infected persons, or garments, &c. I have not much to propose. Pugnet says, that in the plague at Damietta, he used no other precaution than that of immediately washing his hands, after they had been applied to an infected person, or thing, and taking care that his own clothes should not touch those of the sick, or any thing likely to impart contagion: in other respects he breathed the atmosphere of the pest houses freely, both with an empty and a full stomach. Desgenettes says (p. 90) that he lived as well as his situation would permit, and used spirituous liquors in *small* quantities at a time; that on leaving the pest houses, he carefully washed his hands with vinegar and water, or soap and water, and *galloped* home to excite a moisture on his skin; that he then changed his linen and clothes entirely, and washed his body all over with luke-warm water and vinegar. In addition to these precautions, it might, perhaps, be well to cover the hands with gloves of oiled silk, or oiled fine linen, or with a thin coat of bees-wax, softened by oil, during the time in which they are likely to come into contact with the infected matters. Mr. Baldwin has asserted, that dealers in oil generally escaped the plague; but Orræus asserts, (p. 59) that those whose occupations were much connected with ani-

* Diemerbrooeck, p. 52, col. 1, quotes a passage from Franciscus Valleriola, in which that author says that he has frequently seen persons falling down with the plague *a few hours* after having been exposed to the contagion of it:—"cum aliquis integre sanus accessu ad peste correptum (hominem) peste quoque inficitur, atque *paucis post horis* concidit, quod fieri sæpe videmus."

mal fats, such as candle and soap makers, curriers, &c. were the most liable to be infected.

When the pestilential contagion has been received into the system, it seems in a peculiar degree to exert its morbid influence upon the *brain* and *nerves*, producing (the slighter cases excepted,) shiverings, tremors of the limbs, and affections of the head, such as stupor, vertigo, coma, or delirium, with sudden and excessive prostration of strength, and depression of mind; and it is by this mode of action, that it renders the bodies of those who die of plague, remarkably soft, flaccid, and variously discoloured, with a permanent flexibility of the limbs, as in those who are killed by electricity, or by any cause which destroys, or exhausts the excitability or living power. The *prognosis*, therefore, is always unfavourable, in proportion as the symptoms denote a greater degree of morbid affection in the brain and nervous system.

It is not my intention to enter upon a particular account of the various symptoms of plague, for which, indeed, my own observations have been too limited; but I cannot avoid noticing that peculiar appearance of the eye, which Dr. Russel has called the *muddy dull eye*, mixed with something (not very intelligible) of lustre; an appearance which has also been noticed by Orræus, (p. 109) and others, as being peculiar to this disease. Dr. Price informed me, that by minute examinations he had satisfied himself, that this appearance of the eye was occasioned by the different colours of the fluids contained in, and distending the vessels of its external coat, which fluids were sometimes bloody, at others yellowish, bluish, or dark coloured, and caused the vessels to appear as variously shaded streaks, or lines, which sometimes were circular, at other times diverging like radii from a centre, and in some cases by running together, they produced irregular spots, the ultimate effects of all which, he thought, aptly enough expressed by the term of a muddy eye.

Dr. Price also mentioned a peculiar appearance of the tongue, which sometimes occurs in this disease, and which has been called the streaked, or *fiery* tongue, as produced by *alternate streaks,* or *patches* of *white and red.*

In regard to the proportions of death from plague, it varies greatly in different seasons and temperatures; but I am afraid that when the disease prevails extensively, and with its usual violence, more than one-half of those attacked by it, have commonly died, under the most judicious modes of treatment, and with the best accommodations. De Mertens says, (p. 45) that until the disease was mitigated by frost, at Moscow, in 1771, scarcely *four* patients in a hundred recovered: but this must have been a most uncommon degree of mortality. Desgenettes says that the French Egyptian army lost 700 men of this disease, during their expedition into Syria, in the year *seven;* and that in the year *eight,* about one-third were cured; and he expresses great satisfaction in recollecting, that of 700 men under this disease, in the citadel of Kairo, in the year nine, *more* than *one-third* had escaped.*

The deaths from this disease generally occur between the 2nd and 5th days; those who survive the 7th day, are supposed to be in the way of recovery. Orræus describes an acute inflammatory form of plague which produced apoplexy or suffocation, and terminated fatally in 24 hours. (In this bleeding might probably have been useful.) Savaresi, one of the French physicians in Egypt, says the disease sometimes occurred there with a fever, which he calls a *synochus,* and killed the patient in 24 or 36 hours, before any buboe, carbuncle, or eruption had manifested itself. Some cases oc-

* Desgenettes Hist. Medicale, &c. p. 250, says L'an ix, ou nous avons eu dans la citadelle du Kaire jusqu' à 700 pestiférés, nou avons en *le douce satisfaction* d'en voir guérir au dessus du tiers, & dans quelque circonstances pres de la moitié : les *jeunes* NEGRES & les Syrians au service de la republique ont particu-lierement souffert de la peste.

Small as this success may be thought, it is great, compared with the results of the treatment, when sweating was practiced in the fullest extent. Hieron. Mercurialis de Pestil, p. 11, says, "Qui versati sunt in curandis ægris hoc tempore, (plague of 1576,) facilè cognoverunt, *ex centum ægris etiam decem* et *plures* fuisse *servatos.*"

curred in the British and Indian armies, in which the powers of life seemed to be suddenly overcome by the disease, and in which death took place within a few hours, without any apparent effort or reaction of the system. I believe, however, that when persons are said to have suddenly dropped down dead from an attack of the plague, that the disease had commonly subsisted some hours at least, though not avowed, or perhaps known; and in those cases where persons supposed to be convalescent suddenly expire, it is probably from some over exertion, too great for the exhausted state of their excitability by the previous disease.

Two cases of *re-infection*, or second attacks of plague, fell under my observation in Egypt;—one occurred in Mr. Webster, then an Assistant Surgeon, and the other in a soldier of the 27th regiment, each of whom had a buboe; they were, however, but slightly indisposed, the weather having become hot. Dr. Buchan had a second attack, but with only a small carbuncle, as he informed me; Dr. Price also had a second attack without either buboe or carbuncle, but, according to his account, with a *violent* affection of the head and nervous system. In general, I think, second attacks are milder than the first, though Dr. Price informed me of his having seen a lad, who under such an attack, died on the second day. Pugnet says, p. 140, that reinfections, when they occurred, were oftenest in persons who had been mildly treated by the first attack; and that several of these had the disease very violently the second time, immediately after using the beds or blankets of persons who had died of it.

Having, as I believe, already at pages 61 and 387-8 sufficiently shewn the impropriety of attempting to assimilate the plague with yellow fever, it seems expedient that I should do the like in regard to the endeavours which have been made to confound the former disease, at least as it has appeared in this country, with typhus fever.

Sir John Pringle, at p. 319 of his work on the Diseases of the Army, says, "I shall not enter upon the distinction to be made between a *pestilential* fever and the *true plague* ;* the ancients are not clear upon this head, and those of the moderns who contend for a real difference, have not been able so to ascertain it, as to end the dispute.† I shall, therefore, only remark, that though the jail and hospital fever may differ in *specie* from the plague, yet it must be accounted of the same *genus*, as it proceeds from *a similar cause*, and is attended with the *like symptoms*." ! ! ! Where this distinguished writer could imagine that he had observed any likeness in the symptoms of these diseases, or any ground for considering them as the effect of a *similar cause*, I am unable to conceive. The late Dr. George Fordyce, however, believing that Sir John Pringle had not done enough, in considering the plague and typhus fever as diseases of one genus, has strongly intimated that the former disease never existed in this country, and that the latter was always mistaken for it. In his Dissertation on Simple Fever, this author makes the following observation respecting the plague, viz :—

"This infection has sometimes been brought into Europe, as was the case at Marseilles; but *that disease* called the plague, which ravaged *this country*, on considering the histo-

* The connexion, real or supposed, between a particular state, or constitution of the atmosphere, and the extraordinary prevalence of plague, has induced persons, in different ages, to consider fevers which either preceded, or followed such an event, as partaking of the nature of the plague ; and hence, fevers which had neither the characteristic symptoms, nor the *contagion* of plague, have been denominated *pestilential fevers*. Sydenham, on the ground of this connexion, has not only described a pestilential fever of 1665 and 1666, but also a *variolus fever*, (of 1667, 8, &c.) as he called it, because in his opinion, it "depended upon that epidemic constitution of the air, which (as he says) *at the same time produced the small pox* ;" though this fever was not attended with any eruption, nor with any of the symptoms connected with an eruption; and though it did not possess that peculiar contagion, which is essential to small pox. In regard to the production of any disease, specifically contagious by any "constitution of the air," it can only have been imagined ; and, therefore, those appellations of *pestilential*, and *variolous*, were highly improper and fallacious.

† A little common sense, according to my conceptions, would easily "end the dispute." A disease is the plague, or it is not the plague.—If it be the plague, it should receive that name ;—and if not the plague, it should not be called *pestilential*, by those who would attach correct and precise meanings to words.

ries of the disease, seems to have been a fever produced by infections of the first class which have been enumerated. ("Infectious matter produced in the body of a man afflicted with fever, or produced by a number of men living for a certain time in a small space." p. 121.) For the inhabitants of this country, (he adds) it is undoubtedly of great moment to decide this point, but it would make too great a digression. The author may perhaps lay the evidence before the public in an appendix." Unfortunately, however, the author is dead, and no publication of this evidence has been made, or is, as I understand, ever likely to be made; I must, therefore, conclude, that the judicious editor of the *posthumous* part of Dr. Fordyce's work, either did not find the evidence in question, or did not think it worthy of publication; for otherwise, considering the importance of the subject, we may presume that it would not have been suppressed. But another physician, respectable by his own talents, character, and rank in our profession, as well as by those which his father possessed when alive, has adopted, and endeavoured to support this opinion, that the destructive plague which formerly committed such ravages at various times in London, and for the last time in 1665, was no other than our ordinary typhus or contagious fever; an opinion for which I am unable to discover the smallest foundation. Those who believe the physicians of the 17th century to have been so egregiously mistaken, must necessarily suppose they were unacquainted with the true plague, or that this disease has so much similitude with typhus fever, as to make it difficult to distinguish one from the other: that the first of these suppositions is at variance with the truth, must be evident to all who will refer to the descriptions of the plague, given by medical writers in those times, and more especially to the instructions prepared by the College of Physicians, and given to the *Searchers* at the beginning of the plague of 1665, in London, which point out *most clearly* and *distinctly* those symptoms and appearances which characterize the true Egyptian, or Levant plague;* and which, without the grossest inattention, would have rendered it impossible even for the most ignorant, to have been mistaken in regard to the *disease generally*, though they might have been liable to err in a particular case, where, from the causes heretofore mentioned, the appearance of glandular swellings, exanthemata, &c. was either obstructed or retarded. And in regard to the supposition of a similitude in the two diseases, I am not a little surprised that it should have been entertained by any one who had ever read even a tolerable description of the two diseases; and I should be *astonished* if it were countenanced by one who had actually *seen them*.

That the plague, as it formerly prevailed in London, was not a typhus fever, must be evident from the notorious fact of its having always been most extensive and fatal in the summer months, particularly August and September, when

* The searchers appointed by authority in 1665, were required, by the College of Physicians, "to take notice, whether there be any swellings, risings, or blotch, under the ear, about the neck, on either side, or under the arm-pits of either side, or the groins; and of its hardness, and whether broken, or unbroken?"

2ndly, "Whether there be any *blains*, which may rise in any part of the body, in the form of a blister, much bigger than the small pox, of a straw colour, or livid colour, which latter is the worser; either of them hath a reddish circuit, something swollen, about it which circuit remains after the blister is broken, encompassing the sore?

3dly, "Whether there be any carbuncle, which is something like the blain, but more fiery and corrosive, easily eating deep into the flesh, and sometimes having a black *crust* upon it, but *always* compassed about with a very fiery *red* or *livid* flat, and hard tumor, about a finger's breadth more or less; this, and the blain, may appear in any part of the body?

4thly, "Whether there be any *tokens*, which are spots arising upon the skin, chiefly about the breast and back, but sometimes, also, in other parts; their colour is something various, sometimes more reddish, sometimes inclining a little towards a faint blue, and sometimes brownish, mixt with blue; the red ones have often a purple circle about them; the brownish,—a reddish?

5thly, "Whether the neck and the limbs are rigid or stiff, or more flexible and limber, than in other dead bodies?"

there is a cessation of typhus fever: and the fact of its having been rendered nearly, if not completely, *extinct*, by the cold of winter, when typhus is commonly most active and prevalent; for though intertropical heats exterminate or exclude the plague, and the summer heat of Egypt suspends its progress, the warmest weather in our country, is not too warm for the greatest ravages of the plague.

That other diseases prevailed in London during the summer and autumn of 1665, and were confounded with the plague, I am disposed to conclude, because it was then commonly believed that this disease, when raging so extensively and destructively, had the power of converting all other diseases to its own nature; and with this notion, fevers which had none of the distinguishing marks of the plague obtained that denomination; but there are numerous facts and reasons which warrant a belief that these were *marsh*, and not typhus fevers. It will not be expedient that I should here adduce proofs (now well known) of the frequent and extensive prevalence of intermitting and remitting fevers, in and about London, before the sources of miasmata were removed, or rendered unproductive, as they have been, in a great degree, for near a century. Morton affirms, that remittents were very destructive from 1658 to 1664; and that sufficient causes for their recurrence existed in 1665, may be presumed from the long continued dry and hot weather which took place in the summer of that year, though neither Sydenham nor Hodges have distinctly mentioned it.* The fever accompanying the plague

* At page 13 of Loimologia, &c. Hodges observes, "the whole summer was refreshed with *moderate* breezes, sufficient to prevent the air's stagnation and corruption," &c and "the heat was likewise too mild to encourage such corruption and fermentation as helps to taint the animal fluids," &c. It is probable, however, that, by these loose expressions, the author only meant, that the air did not stagnate, and that the heat was not so excessive as to produce the corruption, &c. which are here mentioned; for, at page 20, he thinks it proper to advertise his readers, "that this year was *most luxuriant* in most fruits,

of that year was very commonly a remittent. Hodges, whose authority on this point is better than any other within my knowledge, mentions, at p. 49 of his *Loimologia*, that in this pestilence "persons frequently died without any preceding symptoms of horror, thirst, or *concomitant fever*;" and of this he gives two instances, in which the disease undoubtedly was the true plague, adding, that although sometimes "no appearance could be discerned, even of a *lurking fever*, yet, for the most part some fever did shew itself." (p. 50.) And, at p. 51, he observes, that "the fever accompanying this present pestilence was of the worst kind, both on account of

and especiallly cherries and *grapes*, which were at so low a price that the common people surfeited with them;" which, in regard to grapes at least would not have happened in this country without a summer of more than common heat. It is, indeed, mentioned as such, distinctly, by Mr. R. Hooke, in a letter to the Honourable Robert Boyle, dated July 8th, 1665, in which, after noticing the adjournment of the Royal Society by reason of the plague, he says, "I cannot, from any information I can learn of it, judge what its cause should be, but it seems to proceed only from infection or contagion, and that not *catched*, but by some *near approach* to some infected person or stuff; nor can I at all imagine it to be in the air, though there is one thing which is very different from what is usual *in other hot summers*, and that is a very great scarcity of flies and insects." See Boyle's works, (1772) vol. vi. page 501. And in regard to the stagnation of the atmosphere, and want of rain, Dr. Edward Baynard, (Physician in Bath) in page 252 of Sir John Floyer's "Ancient ψυχρολουσια revived," (printed in 1702) writers, "I was at Chiswick, and sometimes in London, in the time of the great plague in the year 1665, and I very well remember," "during the time of the plague, there was such a *general calm* and *serenity of weather*, as if *wind* and *rain* also had been banished the realm, for many weeks together, I could not observe the least breath of wind, not enough to stir a weathercock or fane; if any, it was southerly." That there was an unusual *drought* in that year is farther manifested, at page 256 of the History of this Plague, by the following observation: "It pleased God to send a very plentiful year of corn and fruit, but not of hay or grass; by which means bread was cheap, by reason of the plenty of corn; flesh was cheap, by reason of the scarcity of grass; but butter and cheese were dear for the same reason; and hay, in the market just beyond White Chapel Bars, was sold at 4*l* per load." In several places of the same work, (supposed to have been written by Defoe) particularly at pages 145 and 146, the weather, in that summer, is stated to be "*very hot*" And in the then state of London and its vicinity, this hot and dry weather might well be expected to occasion more than the ordinary proportion of intermitting and remitting, but not of typhus, fevers.

its state and periods; sometimes *imitating a quotidian, at others a tertian;* sometimes seeming to retreat, and at others attacking again, with redoubled fury. There was never (he adds) a total cessation, but sometimes a remission for an hour or two, although every exacerbation was worse than the former." All this is very similar to an epidemic marsh remittent, and as *unlike typhus fever as possible.* The same author, in a letter addressed to a person of quality, and subjoined to his Vindiciæ Medicinæ & Medicorum, printed in London, ann. 1666, after mentioning the irregularity of the fits, or paroxysms, of the fever, in those who were ill of the plague, adds, that "they seemed most to resemble *a double tertian;*" and that "in many, when the *virulency* was expelled and spent, these fits did *keep* and *observe their types,* and became either *pure or bastard tertians."*—Or, in other words, that after the contagion of the plague had ceased to *operate* in the body, the influence of marsh miasmata, previously imbibed, continued to produce its usual effects.

In regard to the treatment of the disease, I shall offer but a few observations. It appears to have been a very early and general opinion among the physicians of this and other neighbouring countries, that those who were attacked by the plague, small pox, and other contagious diseases, had imbibed a *morbid poison,* and that it *was necessary, above all things, to assist nature in expelling that poison* from the body, and this *principally* by *sweating,* which Morton called, "*regiam viam*" or the king's highway. In "certain rules, directions, or advertisements, for this time of pestilential contagion," "first published for the behoofe of the city of London," in "the visitation of 1603," "by Francis Herring, Doctor in Physick, and Fellow of the College of Physicians," and re-published, upon the recurrence of the plague in 1625, *copious sweatings* were directed to be excited by strong sudorifics, with warm beds and bed clothes, "so soon as any of them ("the poorer sort of people") apprehend themselves to be taken with the plague," and these were to be *repeated every eight hours;* and they were to "*continue this course for four or five days;*" and whilst sweating, it was enjoined *not to* "*let them rest, or sleep.*"* The same opinions, or modes of

* To discover the motive for this *strange* injunction, we must recollect that patients in the plague are often comatose, or morbidly disposed to sleep; and as those who were so affected had commonly died, it was conceived that the poison of the disease was enabled to exert its pernicious influence, more powerfully in a sleeping than in a waking state; and, therefore, that it was of the greatest importance to hinder sleep, especially while attempts were making to *dislodge the enemy by sweating*, and also when nature was supposed to have endeavoured to produce a similar effect by buboes. In an old work entitled, "De la maniere de preserver de la pestilence, et d' en guerir, par Benoit Textor, Medecin," printed at Lyons, in 1551, the author, at p. 130, makes this observation: "C'est un accident de grande importance, et perpetuel ou inseperable de cette maladie, que *le long et profond dormir*, contre lequel, pour cette cause, il est necessaire de soigneusment *batailler*, tant s' en fault qu' on le doive mepriser." And he afterwards explains himself more on this subject, at page 150, in these words: "Quand le bubon and le charbon sortent, le dormir est fort dommageable, d' autant qu' il retire au dedans la malice du venin. Et neantmoins c'est alors que les malades y sont plus enclins, et qu' il y ha plus à faire à les empescher de cela." "Or en ce cas quand ces enflures se font, veu que par un tel moyen nature se efforce de poulser ceste matiere aux parties exterieures, alors pour luy ayder, le *veiller* est requis au malade, s' il le fut onques. A cela on taschera par paroles recreatives, par jeux, par bruits, par crys. *On criera bien hault aux aureilles du malade mesmes, par voix aigue*, on *sonnera des bassins*, et dautres choses aupres deluy, on cornera, on *frappera avec des bastons*, on *ouvrira et fermera les portes*, ou quelque coffre et armoirè à *lestourdie*, on *usera de pinsemens rudes*, de *ligatures fortes* es *extremitez comme es doigts*, ce quon appelle bailler le moyne, on *ployera doloureusement ces parties*, on *luy tirera les cheveux, la barbe, et principalement les poilz des parties honteuses*, on *luy tirera bien fort le nez et les aureilles*, ou *luy ouvrira les yeux par force*, ou y *jettera du vinaigre*, on *les gratignera asprement*, on *le frappera*, on le scourra, on l'exposera a la lumiere, *on le tourmentera en toute maniere, ou rovandera en la maison* ou procedera prudemment par toutes ces façons de faire selon le personnage. D'autre part pource que par le trop veiller les esperits vitaux se dissipent, dont souvent s'ensuit grande debilitation, pour eviter ce danger, si les malades demeurent trop longuement sans pouvoir dormir, ou y pourvoyera, &c." Certainly nothing but the most extravagant apprehensions of danger, from sleeping in the plague, could have induced a physician of good character seriously to advise such violent, extraordinary, and indecent means to produce watchfulness, knowing as he did how much it contributed to exhaust the powers of life.

treatment were adopted in the "advice set downe by the College of Physicians, by his Majesties speciall command," which was printed in 1630, along "with certaine statutes" concerning the plague in that year. By this it was directed "that there be good fires kept *in*, and *about* the *visited* houses, and their neighbours;" and "to make fires rather in pannes, to remove about the chambers, than in chimneys, the better to correct the ayre of the houses." After which directions are given for the repeated administration of the most *powerful sudorifics*, upon the ground of opinions delivered in the following sentence, viz.—"For as much as the cause of the plague, standeth rather *in poison*, than in any *putrefaction of humours, as other agues doe, the chiefest way* is to *move sweatings*, and to *defend the heart* by some cordial thing.

On the 13th May, 1665, the College of Physicians were required by "a Committee" of the Privy Council, appointed by the king, "for prevention of the spreading of the infection of the plague," to inspect the "Rules given by the Physicians of former times, and imprinted for the public benefit," and to make such alterations as they should "find the (then) present times and occasions to require and to cause such their directions to be as speedily prepared and printed as possible;" and the College in their answer or address to the said Committee, on the 25th of May, signified that they had done as was required of them. And among the directions then published by the College, after the mention of bleeding, purging, and vomiting, they say "*these three great remedies rarely have place in the plague*, but *are generally dangerous, and most of all purging*, by any strong medicines; and are therefore not to be used, but upon some extraordinary urgent indicant, or just occasion, and with the greatest caution, which only an able physician can judge of." They afterwards deliver it as their opinion, that "the poison is best expelled by sweating, provoked by posset ale," "and London treacle" to the quantity of ℥ij mixed; the Patient to "be put to bed to sweat well covered in a blanket, *without his shirt, for twenty-four hours*, every fifth hour *renewing his cordial*, but in half the quantity" first taken, "between whiles refreshing him with posset drink, oatmeal caudle, or thin broths, made jelly wise, or hartshorn jelly," and, if necessary, *warm bricks*, wetted with vinegar, and wrapped in flannels, were to be put to his feet, and care was to be taken that he "*sleep not till the sweat be over.*" Blisters were at the same time to be applied, "behind the ears, about the wrists, near the armpits, on the insides of thighs, and near the groin," to "*draw forth the venom.*" The buboes, or swellings of the lymphatic glands, were to be "always *drawn forth*, and *ripened*, and *broke with all speed.*"

I have mentioned these facts to illustrate the motives, as well as the means, by which persons, were as I fear, often sweated to death in the plague, small pox, miliary fever, and above all, in the *sweating sickness*,* and with so little suspicion of

* I may at some future time endeavor to dispel the obscurity in which the cause and nature of the sweating sickness seem to be involved, but at present I shall only observe that the efforts to produce sweat, and the mischiefs thereby occasioned in this disease, were probably greater even than in the *plague*; and to throw some light upon the effects of that treatment in the latter disease, I venture to introduce a paragraph, which with others, I have extracted from a curious manuscript, part of Sir Hans Sloane's Library, No 349, now in the British Museum It is intituled "Λοιμογραφια, or an Experimental relation of what happened remarkble in the last Plague in the City of London," by "William Boghurst, Apothecary," &c. He was also the author of Londinologia, sive Londini Encomium MS. also in the British Museum; and his epitaph, at Meereworth, states, that he "was an honest just man, skilful in his profession, and in the Greek and Latin tongues, delighting in the study of antiquity," &c.

In chapter 21, at page 121, when going to treat of the cure of the plague, Mr. Boghurst says, "Before I begin this, I must needs say something concerning a doubt which hangs in my mind, which I have hinted at a little once or twice before, viz. whether *strong sweats often repeated* be an authentical, canonical rule, which will serve for all sorts of people or cures. I wish somebody of more skill would resolve the doubt: that which makes me doubt and *stick* concerning this, is that I have seen so many this year of strong, lean, raw-boned nervous, (sinewy) people, of much spirit and little humour, that have been very *laudably* sweated, and *scarce one in twenty* the mischief, from a violent excitement and expenditure of the living power, produced by stimulating cordials, heated rooms, excessive covering, deprivation of sleep, &c. that all who escaped death under such treatment, were supposed to have been saved by it.

Opinions equally erroneous were entertained of the nature and treatment of buboes, and exanthematous eruptions, which, being considered as critical eruptions, *though often occurring at the commencement* of the disease, it was thought necessary to bring them as speedily as possible to a state of suppuration, by the most stimulant applications, in order, as was imagined, to extract the morbid poison, and even to employ scarification or excision for the carbuncles. Sydenham was indeed so fully convinced, that buboes and carbuncles were intended by nature to produce salutary evacuations, that he considered an attempt to discharge the morbid poison by artificial sweating, as an endeavour to force it into *other outlets*, than those which nature had selected for this purpose. It seems highly probable, that the buboes which accompany the plague, are no more the effect of an effort of nature to evacuate morbid matter, than those which occur in the venereal disease, or, after the introduction of variolous contagion by inoculation; and it has been proved, in a multitude of instances, that no harm has resulted from the spontaneous resolution or dispersion of pestilential buboes without suppuration. Such instances have been often seen, and attested, by the most accurate and respectable writers on this disease; and though it may be proper to pro-

*of them have lived*, but dyed all within two or three days; and the *sooner*, by how much *more freely* they sweated, and *were worst* after *sweating*; being much more subject to lightness of head, staggering, faintness, bleeding at the nose, quinzies in the throat; and some had the *tokens* come out presently, which made me desist from much sweating such persons, and then I had many patients lusty men, who lived and *never sweat at all*, and are living to testify the same; and I observed that they that sweated freely of their own accord *seldom lived*" The spontaneous sweatings here mentioned were doubtless the effect of a *morbid predisposition* to that discharge, connected with debility, which frequently occurs in this disease, though in many patients, who have the plague, there is an *unusual dryness of skin*, and indisposition to perspire.

mote their suppuration by emollient, and moderately stimulant cataplasms, &c. where a natural disposition to that issue is evident; there can, I think, be no danger in favouring their dispersion, or resolution, by the usual means, when we observe a spontaneous tendency to such a termination.* I know that the sudden retrocession of buboes, previous to suppuration, and whilst other symptoms indicating danger, subsist unabated, is often followed by death. But this mortality is not in such cases produced by any change in the buboe itself, or by the retention of any matter which ought to be discharged, but by such an extreme diminution of the living power, or other injurious effects of the disease, as is incompatible with the continuation of a suppurating process, and also with the patient's recovery; and therefore, this retrocession is to be considered not as the cause of death, but as an indication, and consequence of that condition of the patient, from which death necessarily resulted; and on the other hand, when these glandular swellings rise, and suppurate favourably, they indicate such a state of the living power, and of the system, as is likely to overcome the disease, without the supposed benefit of an evacuation of morbid poison by that suppuration. The same reasoning appears applicable to carbuncles, though in their gangrenous state, and when not surrounded by concentric inflamed rings, they require hot stimulant applications, and afterwards such as will promote a suppuration, and a separation of the carbonaceous crust.†

* Mr. Boghurst, at p 141 of the before mentioned MS. says, "Though many buboes after they came to be very big, never come to break at all, but sink away again, and by degrees wear quite away; yet doth the patient *grow well,* and *continue so, notwithstanding their not breaking:* yet you must not make it *your design* to *repel, divert,* or *discuss them,* but only to suppurate them; and if you see them not more forward, or digest so fast as is convenient, then you may apply a vesicatory, just underneath them, that so the *pernicious* ichorous matter may have vent; and these blistering plasters had better success always, than *cupping,* or *burning,* or *potential cauteries.*"

† Boghurst mentions, at p. 146 of his MS. that in the year 1665, some persons used to make incisions round each carbuncle, and apply vinegar and salt to the fresh wound; "but to what purpose, (says he) I know not unless they delighted to torment people; for

In regard to the treatment of the disease, generally, I have little to offer; and, until we know more of the ways and means by which nature endeavours to overcome it, I am afraid we can do but little for her assistance, otherwise than by restraining all violent and dangerous symptoms, all excessive and debilitating evacuations, and supporting, *when necessary*, the powers of life, by a *moderate* use of wine, æther, opium, volatile alkali, and Peruvian bark. The instances of persons who have strangely recovered from the plague, after having wandered alone about the country, particularly in Egypt, exposed to *cold* and *wet*,* seem to indicate, that even the most moderate sweating is, at best, useless: but on the other hand, the unsuccessful trials made by Dr. Price, of the cold bath, afford no encouragement to repeat such applications to the surface. In some few cases, where the disease occurs in the vigorous and robust, and is accompanied with highly inflammatory symptoms, bleeding might, I think, prove beneficially, if employed within a few hours from the attack; though in general, very bad effects appear to have resulted from this evacuation. Mild emetics are said, in some cases, to have proved beneficial, given at the very beginning of the disease.

Mercurial preparations were tried in Egypt, and probably thought *not* to have been previously employed in any other country for the plague. Diemerbroeck, however, mentions, at p. 268, their having been long before employed by Ambrose Paré, who bestowed some commendations upon them, (lib. 21, cap. 29,) with what justice, I will not decide. But during the plague at Marseilles, 1720, *Diedier* states, that he employed mercurial frictions, and pushed them as far as possible, "ausi loin qu'on les peut porter," *without producing any good effect*. See Traité de la Peste, p. 522.

it put them to as much pain as if they had been on the rack." He adds that a Frenchman, after cutting round the carbuncles, used to pluck out the eschar with pincers, before it was "*ripe*" for a separation.

* Desgenettes mentions two remarkable cases of this kind, at p. 149, 150, and they are not the only ones which have been well attested.

Orræus also asserts, p. 154, that he had employed mercurials in various forms and doses, both *internally* and *externally*, and that they *did no good*. "Nec minimam utilitatem præstarunt." Pugnet, likewise, as he states, employed the same medicine, both internally and externally, without the smallest benefit, even in the cases for which it seemed most suitable, excepting a few instances where buboes were degenerating into cold indolent tumours, and appeared to be improved by applying mercurial ointment along the course of the lymphatics leading to them. He adds, that it did not obviate infection, as several persons caught the plague, whilst under salivation for venereal complaints. Sotira also declares, that he employed salivation in three persons ill of the plague, and that two of them died. Dr. Price, who probably tried salivation to a greater extent than any other person in Egypt, seems to have formed a better opinion of its effects; and for the same fallacious reasons which have induced many persons to consider it as beneficial in yellow fever, viz. the recovery of some patients, in whom he was able to excite a salivation, and the deaths of *all* those in whom he attempted to excite it, without success. He admitted, however, in a conversation with me, that he had always found it extremely difficult to affect the salivary glands of persons under the plague, and was never able to do it by mercurial inunctions alone: and when I observed to him that a considerable time would be necessary to produce salivation by the means which he had employed: i. e. unction, and calomel internally, and that it seemed probable, that those who lived long enough to be salivated in this way, must have previously passed over the dangerous part of the disease, and have been therefore likely to recover, if no salivation had taken place;—he admitted that my observation was probably just, and that the deaths of those in whom a salivation was not produced, had *always taken place before the time* which was commonly required to affect the salivary glands. I have, therefore, no difficulty in believing, that the supposed benefit from salivation in the plague, depends upon fallacy similar to that respecting yellow fever, which I have stated between pages 74 and 80.

In regard to the frictions with oil, which were strongly recommended by Mr. Baldwin, I am afraid that but little benefit is to be expected from them in this disease; like mercury, they appeared to be an old and discarded remedy, reproduced as a new one. Benoit Textor, in the publication lately mentioned, so long ago as 1551, says at p. 33, in regard to sweating for the plague:

"Celuy qui ha besoing de suer doit estre fort frotté par tout le corps, principalement avec huile de camomille, ou avec camomile & *huile* ou avec l'herbe dite nepeta, &c."—"Que le personnage se face bien couvrir & chauffer des quarreaux ou des briques aux piedz, ou appliquer dessouz les aixelles & es aines, vessies remplies d'aue chaude." Riverius also recommended frictions over the whole body, with oil warmed, night and morning, in the plague. See Prax. Med. lib. xvii. cap. 1. These frictions were, however, tried extensively by the French physicians in Egypt, and with very little, if any, benefit; though in a few cases they seemed to give temporary ease and relief. Pugnet, indeed, says they were not only useless but caused anxiety and disturbance to the sick, that of 15 patients to whom these frictions were applied under the French physician Carrié, one recovered with difficulty, and all the rest died; and that where they seemed to do good, the disease was always mild. See p. 261. With so much reason to doubt of their utility, there is a strong objection to their use; arising from the very great danger of communicating the disease to the unfortunate person by whose *hands* they may be applied, and thus destroying many lives, without much probability of saving one.

END OF THE FIFTH AND LAST PART.

# APPENDIX.

## No. I.

The following is Dr. Physic's communication, which I promised at p. 24 to insert in this appendix: viz.—

"Some Observations on the Black Vomit, communicated by Dr. P. S. Physic, of Philadelphia, to Dr. Millar." Extracted from the 5th volume of the New-York Medical Repository, 1802,—page 129.

"Having, in the years 1798 and 1799 had frequent opportunities of dissecting the bodies of persons who died of the yellow fever in the City Hospital, I had thoughts of publishing a circumstantial detail of the several appearances in each case. On perusing, however, the descriptions given by late authors, I find but little to add to them, except some observations respecting the *black vomit*, which do not appear to have been particularly noticed.

"The common opinion *was*, and for any thing I know to the contrary *is*, at the present time, that this black matter is poured out by the liver. The dark-coloured appearance of the bile in its accumulated state, approaching more to the colour of the black vomit, than any other secreted fluid, would very readily induce a person to conclude that they were the same, if he did not compare and examine them carefully; and, likewise, attend to several other circumstances."

By such an examination the following differences have been observed.—

"First,—If the darkest-coloured bile be spread thinly over a white surface, such as the skin, it loses the colour it had in its accumulated state, and appears of a yellowish-green colour; but, if the black vomit be treated in the same way, it retains its black, or dark-brown appearance.

"Secondly,—The bile in the gall bladder has its common bitter taste, but the black vomit is insipid, or nearly so. This fact has been ascertained by several persons; and among others, by the late Dr. S. Cooper. I have inquired of a number of patients just after they had vomited it, they almost all declared it to have no taste; and the organs of taste were proved to be perfect in these people, by trying whether they could distinguish between different tastes. It occasionally happens, that violent efforts made in vomiting, will force some bile out of the gall bladder into the stomach, and then the black vomit will have a bitter taste like bile. Dr. Cooper twice found it intensely bitter owing to this circumstance, which, however, is a rare occurrence.

"Thirdly,—The black vomit differs very much from any mixture that can be made of the dark-coloured bile, with any of the fluids found in the stomach, or intestines. If bile be mixed with the mucus of the stomach, or, if some of it be added to the black vomit, it mixes with these uniformly, and imparts a yellowish-green tinge to them. The nearest resemblance to black vomit that could be made, was, by mixing some of the mucus of the stomach, a little blood, and some bile, together; but the difference was still very obvious.

"Fourthly,—The stomach has been found full of black vomit, when, in the same subject, the fluid in the gall bladder and biliary ducts, was very different from it in its colour and appearance. I have found the gall bladder filled with a fluid of a brick-dust colour; in some others it contained a fluid of a light green colour; and, in others, a transparent and colourless fluid, resembling the white of an egg, only that it was of a thinner consistence. In some instances, again, a purulent-coloured fluid was found in them. Some of the same kind of fluid which the gall-bladder contained in these last-mentioned instances, was generally found in the duodenum; the stomach in the same body containing black vomit.

"Fifthly,—The pylorus, in several instances, has been found closely contracted, and yet, the stomach contained black matter.

"The above observations have appeared to me to overthrow the idea of the black vomit being secreted by the liver. The question, however, still remains;—from whence is it derived? I believe it to be a secretion from the inflamed vessels of the stomach and intestines;—and for the following reasons:—

"First,—It is found in these viscera, when it cannot be detected in any other organ, or cavity connected with them.

"Secondly,—It is often found of so thick a consistence, that it does not mix with the fluids of the stomach: in such cases, it adheres to its inside, forming a black coat of considerable thickness; and, when it is once scratched off, it cannot be made to adhere again in the same manner.

"I have, in one instance, observed this black substance in two almost circular patches, each about two inches in diameter, adhering to the stomach; and all the other parts being free from it. In this case, there was no black matter loose in the cavity of the stomach nor intestines. On scraping it off, the spots which had been covered by it, were found inflamed, and these spots only. Now, it can hardly be possible for this black substance to have got into such a particular situation, had it been secreted by the liver; and some of it, in that case, would have been observed in the gall bladder, gall ducts, or duodenum.

"It must not be conjectured, that the black vomit irritated the stomach, and produced the inflammation: on the contrary,—Dr. May frequently repeated an experiment, which proved it to be very bland. He dropped it into his eyes, and never experienced any more inconvenience from it than if water had been used. When the hands have been irritated in dissecting, which once occurred to myself, I beileve it has arisen from some acrid substance having been swallowed by the patient just before death, as elixir vitriol, volatile alkali, &c.

"Fourthly,—I have seen the inside of the inflamed stomach as black as the black vomit, resembling it in colour exactly. In most of these cases, no black matter was found in the cavity of the stomach. The vessels only which were inflamed were distended with it. This colour differs very much from the dark purple of a part in a state of gangrene; and I never observed any putridity attending it. This blackness has, in some stomachs, been universal, in some in spots only; the other spots being in a state of high inflammation, giving the inside of the stomach a chequered appearance. These spots, in one instance, were seen resembling each other in shape and figure exactly; and were, in every respect alike, except in colour; the one being red, the other black. Here some of the inflamed vessels only had gone into the act of forming black matter, but did not excrete it.

"The secretion of black vomit appears to be one of the most common modes in which violent inflammation of the stomach has a disposition to terminate. Death, however, in general, takes place before it entirely disappears. I have seen many cases, which shew that the inflammation is diminished by the secretion;—of which, it will be sufficient to mention the following. On opening a stomach, one-half of it was coated with adhering black matter, while the other half was free from it; on scraping it off clean, and comparing the part underneath with the other half of the stomach which had not secreted any black matter, the difference in the degree of inflammation was very striking, being much the least in the part which had been covered with the black substance.

"In some cases, where the vomiting of black matter had been considerable in quantity, or continued for several days, the inflammation was found very faint indeed; and in some, the inside of the stomach appeared as if covered over with a vast number of small glands, like mucuous folicles crowded together."

The author had also promised at p. 40, to give "the substance of another Memoir concerning the Black Vomit, written by Dr. Isaac Cathrall, of Philadelphia; which, however, by reason of the room it would necessarily occupy, the editor of the latter parts of this volume has been forced to omit; preserving, however, the following extracts, from an inaugural Dissertation on Malignant Fever, by Dr. Stubbins Ffirth; (now, or late, of Philadelphia,) relating to the appearances on the dissections of a considerable number of persons who had died of the yellow fever: viz.—

"The brain was generally found in a diseased state, the meninges being considerably inflamed, the dura mater being sometimes agglutinated to the pia mater, in consequence of the increased action of the arteries thereof. The blood vessels were turgid with blood, appearing as though they had been injected; the substance of the brain was harder and firmer than usual; the ventricles frequently contained water, sometimes to the amount of several ounces; in some cases, the rupture of a small vessel had taken place, and an effusion of blood was found between the pia and dura mater.

"The stomach was *always* found diseased; great inflammation being observable throughout, and erosions of the villous coat frequent, nay, in a number of cases, whole portions thereof, of the size of a dollar, were detached, and found floating in the black vomit. The blood vessels were, in general, very much distended; and, in one case, their smaller extremities filled with a fluid similar to the black vomit in appearance, taste, and smell. This inflammation was frequently continued to the small intestines; the duodenum was the most affected, but the jejunium and ilium also suffered a part; nay, the large intestines by no means escaped free, for I have often found them considerably inflamed.

"The spleen and pancreas were generally found in a healthy state; the kidnies were also generally sound; but the bladder was, in a number of cases, inflamed; and in some so contracted, that the cavity would not hold four ounces.

"The liver was generally, I might say almost always found in a healthy and natural state."—"I do not find amongst my papers any evidence of its having been diseased, except in three of the patients that I examined, and, in two of them it had been of a chronic nature." "The *gall bladder* was always found in a healthy state, containing its usual quantity of bile, and of a natural colour. I have preserved specimens of black vomit, and of bile, taken from the same patient, shewing the difference, which is obvious from first sight. From every circumstance, I feel myself authorised to, and I do positively, assert, that black vomit is *not* an altered secretion of the liver; is *not* changed bile; and does not come from the liver, whatever others may assert to the contrary." To prove this, he says, 1st.—"It is never found in the gall bladder, the hepatic, or the cystic ducts, or the ductus choledochus communis." 2ndly.—The bile is found natural in the gall bladder, when the stomach is distended with black vomit." 3rdly.—"I have found the stomach distended with *black* vomit, when the pylorus valve completely obstructed all passage from the duodenum to the stomach, or vice versa; at the same time, the liver was perfectly free from disease, and the bile in the gall bladder natural in colour, taste, and consistence." 4thly.—"I have seen arteries of the stomach distended with a fluid similar to black vomit, and not to be distinguished from it by any means whatever: a portion of the villous coat of the stomach separated from its adhesion to the others, and the space filled with black vomit, poured forth by the termination of the small arteries." See Dr. Cox's Medical Museum, vol. 1st. p. 116—118.

In some few cases of mortality from the yellow fever, the stomach, after death, is said to have been found without marks of inflammation. But I do not recollect that any of these were described so particularly as they ought to have been, or with such explanations as could enable us to ascertain either the accuracy of the observers, or the causes of such deviations, if real, from the condition in which, according to my best information, that viscus has been seen in at least 49 out of 50, of the bodies of persons dead of yellow fever, which have been examined. In support of this observation, I could adduce numerous proofs; but thinking them unnecessary, I shall only subjoin a few lines from an official statement, published in the Moniteur "du 17 nivose an xi," (7th of January, 1803,) concerning the death, &c. of the Captain General Leclerc, (brother-in-law of the then First Consul of France,) at St. Domingo, who is therein declared to have been attacked with fever "le 8 Brumaire an xi."—"Et le medecin a declaré ce meme jour, que c'etoit *la maladie de Siam*, *dans toute son intensité*," &c.—"Le 10, le *vomissement* a éte *plus frequent*, & est *devenu noir*." During the vomitings, it is stated that the skin appeared *black*, with a *yellow tinge*; and that blood escaped by the eyes. He died in the night between the 10th and 11th, "Brumaire." This statement is signed "Peyre, Medecin en Chef." The medical officers who examined the body after death, declared that they had found "*l'estomach extrêmement phlogosé*, la tunique interne *sphacelée*, et *enduite d'une* humeur *noirâtre* et *visqueuse*." (The stomach very much inflamed; its inner coat sphacelated, and covered with a blackish viscid fluid.)

We are informed by Dr. Caldwell that "the existence of *intro-susceptio intestinalis*, was the only actual discovery made by the knife of the anatomist, during the epidemic (at Philadelphia) in 1805. This affection was confined to the small intestines, and was found to exist in several cases of the disease. I believe, (says Dr. Caldwell,) the discovery was first made in private practice, by Dr. Stuart, and afterwards by Dr. Parish, at the City Hospital."—"The course of the intro-susception was always from above, downwards the upper portion of the intestines being the receiver, and the lower portion the received." Dr. Caldwell's Essay on the Pestilential or Yellow Fever at Philadelphia, in 1805, p. 179.

Probably these intro-susceptions were produced by violent strainings to vomit. How frequently they happen in cases where the disease proves mortal, will deserve, as far as possible, to be ascertained.

# APPENDIX

## NO II.

THE author, at p. 98, after giving decisive proofs "that if putrefying animal matters are not completely harmless, they are, at least, innocent of the charge of producing contagious fever," has referred those who might desire farther evidence on this point, to his second appendix, which was intended to contain "rather a redundancy, than a deficiency of such proofs." But as the present volume is already extended beyond the author's expectation, and as the facts allotted for this appendix appear almost superfluous after those formerly stated, the editor ventures to omit by much the greater part thereof.

The following statement is extracted from a letter written to the author by Mr. Lawrence, Anatomical Demonstrator at St. Bartholomew's Hospital; whose character, talents, and professional acquirements, have already, at an early part of his life, greatly and justly advanced him in the road to eminence. It was dated February 21, 1809.

"In a constant attendance at the dissecting room of St. Bartholomew's hospital for more than ten years, I have never seen any illness produced by the closest attention to anatomical pursuits, except such as might be expected to follow from a similar confinement and application to any other employment. When it is considered that most of the students come from the country, and that many spend much time in dissection, being employed also in writing, reading, &c. during the rest of the day; it will not be a matter of surprise that their health should occasionally suffer: but the indisposition has never appeared to derive any peculiar character from the exposure of the subject to putrid effluvia. Of course, you will except from this observation, the effects which may arise from the absorption of noxious matter from wounds received in dissection. It has not appeared to me, that ill consequences of that description, follow more frequently from the dissection of the most putrid, than from that of recent bodies. The following particulars will afford the most complete proof, that the exhalations from decomposing animal substances are not necessarily injurious to the human body. John Gilmore, together with his wife, and two sons, lived for ten years in a room under the anatomical buildings of St. Bartholomew's. The whole family slept, as well as spent the day, in this apartment, which received a very small quantity of light, in consequence of its single window opening against a high wall. The room was at the end of a passage, in which several tubs containing bones in a state of maceration were generally placed, and with which other divisions of the cellars communicated, containing large excavations for receiving the refuse of the anatomical rooms. The latter were not separated from the general passage by any door.

"The animal matters thrown into the receptacles just mentioned, are, I believe, converted into adipocire, and the fetor is consequently not so offensive as if they went through the putrefactive process; but the whole place was constantly filled with a close cadaverous smell, very disagreeable to any persons who went down from the fresh air. During the whole day, Gilmore was employed about the dissecting room, in removing the offals, in cleaning macerated bones; in short, in an almost constant handling of the most putrid matters. He always enjoyed good health, was fat, and possessed very great bodily strength. He left his situation in consequence of an apoplectic attack, and died lately, at the age of 69, after two other similar affections. His wife survives, enjoying a good state of health. Neither of his sons appears to have suffered from any unwholesomeness of their abode. They are both hearty and strong, although they have been employed some years in attending the dissecting room. But the whole family left the cellar soon after the father's first attack."

During the time that our very numerous fleet of transports lay in the bay of Aboukir, many bodies of sailors who had either died, or had been drowned, were washed on the shore, where they remained unburied, exposed to the heat of the sun. In riding to Rosetta, it was necessary to keep along the shore; and I passed 18 or 20 corpses in this situation. They were in various states of putrefaction; but the stench from them all was offensive in the highest degree, and often extended to more than 100 yards. My curiosity led me to approach close to most of them, that I might examine the changes they had undergone. Some were swelled up to an enormous size, and the skin seemed so distended, that it appeared ready to burst. These were often of a dark-brown colour; some had not yet come to that state; others had passed it; and the skin having burst in several places, the air had escaped, and they had become more or less desiccated, and of a black colour. Every person who had occasion to pass from the camp to Rosetta, was obliged to come within reach of the vapours emitted by these bodies. There were orderly dragoons constantly passing; yet, neither myself nor any one else, so far as I could learn, was attacked with fever, in consequence of our exposure to these vapours; and my professional situation would probably have enabled me to learn if any such consequence had followed.

Orræus Descriptio Pestis, &c. p. 47. After stating that, towards the decline of the plague in *Moscow*, in February, 1772, the College of Health received information "hinc inde in domibus emortuis & infectis . . . . . cadavera clanculum inhumata vel aliter occultata reperiri;"—and that they ordered all the houses to be searched, offered 20 roubles to informers, "et quæ (cadavera) in locis spatiosis non sat profunde inhumata fuerunt, eorum sepulchra terra multa contegere, cætera vero *nuda* reperta in *cæmeteria transportare*." He says, "Hac ratione circiter *mille* cadavera in *habitationibus*, ipsis reperta fuerunt. *Notabile* omino fuit *neminem* ex *vespillonibus*, vel *aliis* in *negotio hoc* periculoso *versantibus infectum*, nedum *morbo aliquo* *corruptum fuisse*, quamvis tanta ab omni infectione incolumitas vix ac ne vix quidem sperari posse videbatur."

The good health commonly enjoyed by tallow chandlers and soap makers, is now too well known to require any evidence from me in confirmation of it, notwithstanding the very offensive and putrid exhalations to which they, and particularly the former, are exposed. Glue and catgut makers are exposed to vapours equally corrupt and disagreeable. When riding on the Uxbridge road, near the 4th milestone, on the 14th of August, 1810, I was assailed by an offensive smell of putrid animal matters, which I soon discovered to have come from a set of works near the road, employed in the making of glue; adjoining to which were several huts belonging to the labourers and their families, most of whom I saw, and they all, both adults and children, had the appearance, and, as I was informed, the enjoyment of good health. I have heard the same of catgut manufacturers.

In the Edinburgh Medical and Surgical Journal of October 1, 1810, may be seen an account, given by Dr. Chisholm, of a manufactory (of which I had some knowledge from the time of its first establishment,) at *Conham*, near Bristol, destined for the conversion of animal flesh into a substance resembling spermaceti, by cutting up dead horses, asses, dogs, &c. and putting their muscular parts into boxes with holes for the admission of water, and afterwards placing them in pits filled with water, while the entrails and useless parts of many hundreds of carcasses, were left to putrify on the surface of the ground. And it appears from Dr. Chisholm's statement, as well as from other information which was given to me on the subject, that though the effluvia of these putrefying animal matters were highly offensive to the overseer of this manufactory, and to the workmen employed under him, as well as to others within their reach, no injury was done by them to the health of any person, during the two years in which these operations were continued.

In regard to the morbid effects supposed to result from the putrefaction of fish, they appear, so far at least as regards fever, to have had no existence, but what was derived from the indiscriminating credulity of such writers as Forestus. That a large whale was formerly cast ashore, and suffered to putrefy on the sea coast, near Egmont, in North Holland, (a place nearly surrounded by marshy or low grounds,) I am willing to believe; but that the fever which is said by Forestus (tome 1, lib. 6) to have followed that event, was produced by the *whale*, rather than by marsh miasms, I cannot believe; because whales have not been found capable of producing such effects in later times,* and because fevers from marsh effluvia constantly fall under our observation.

* See the account given by Dr. Gordon of a whale which putrefyed *very harmlessly* at the island of Santa Cruz, published in the appendix to Dr. Chisholm's Letter to Dr. Haygarth, p. 251—253. The same appendix contains an account of the putrefaction of 1,000 barrels of salted beef, at the same island, which were finally ordered to be thrown into the sea: and were thus disposed of without having occasioned sickness to any person in the house, store, or neighborhood where this putrefaction had taken place and subsisted.

About the year 1788, a whale was stranded on the coast of France, near Havre de Grace, and M. Baussard, in an account of it, published in Rozier's Journal de Physique, for March, 1789, says, "Pendant que j' etois occupé a dissequer ce gross animal, une Lueur Phosphorique exhaloit de l'interieur de son corps, et une odeur tres fætide de la tete." "Les exhalaisons m'ont occasionné des inflammations aux narines, et a la gorge et certains parties huileuses de la tete m'ont mis les mains dans un etat pitoyable."

No mention is, however, made by M. Baussard, of any febrile affection occasioned either to himself or any other person by the putrefaction of this fish; and that no such affections do, in fact, result from that cause was farther proved by the information which I obtained on the 2d of October, 1807, at the Greenland Dock, where the late proprietor, Mr. Ritchie, (who had just sold his property to Sir Charles Price and his associates for 35,000*l.*) informed me that for a considerable time all the Greenland ships had been used to boil their blubber at this place, for which purpose five coppers, with proper coolers, &c. had been erected. Mr. Ritchie had lived more than 50 years in the neighbourhood of this dock, was well acquainted with the boiling process, and assured me, repeatedly, that though the blubber is often in a very offensive state, emitting an highly putrid smell, neither himself, nor his people, nor the crews of the Greenland ships, who perform the whole boiling, &c. nor the neighbours, have ever, to his knowledge, suffered in their healths from that operation; that his people, and himself, have always been healthy, and that he believes no crews are more healthy than those of the Greenland ships. This account was confirmed by the master of a Greenland ship then in the dock, who said he had been employed in the whale fishery for the last 22 years, excepting one year, and had been used to boil down the blubber for 16 or 18 years of that time. He said besides, that the Greenland ships, on their return home, often smell very offensively to strangers, though to themselves the stench is imperceptible; that the casks in which they carry out their water are those in which they have brought home the blubber; and that the water generally is found extremely offensive for some hours after the bung is taken out; in which state, however, the men are accustomed to drink it; and, that notwithstanding all this, he does not conceive that any men are more healthy than the crews of those ships. That the stench from the blubber is universally admitted to be greatest when it is boiling; and that these effluvia, so far from being at all unhealthy, are, on the contrary, reckoned so wholesome that it is very common for sick persons to come to the copper, as soon as they rise from their bed, and to hold their heads over the steam, as close as they can.

Mr. Ritchie informed me, that what remained of the blubber, after the boiling was finished, was now very commonly bought for agricultural purposes; that it was usually taken away by the purchasers just after the boiling, and was allowed to lie by a certain time, till it was in a proper state to be used as manure; when it was laid upon the ground and found to be very useful.

The use of fish as *manure* is no new invention:* herrings, pilchards, and mackarel, have been long employed for this purpose in those parts of Great Britain where they are caught in the greatest abundance, and so are the various species of mollusca. In some parts of Cambridgeshire, &c. a small fresh-water fish called stickle-back, (gastyrosteus aculeatus, Lin.) becomes so plentiful, that, leaving their native ditches, they form vast shoals in the rivers, and being caught in nets, or baskets, are strewed over the ground, in the proportion of twenty bushels per acre. No morbid effect, however, so far as I can discover, has ever been known to result from the putrefaction of fish, or other animal matters employed in this way, though fevers ought to have resulted from it, if producible by the natural decomposition of animal substances.

Putrid human excrement seems equally incapable of producing fever. A nightman, who had been extensively employed for thirty years in this metropolis, assured me, that though his labourers frequently fell into asphyxia, or *died off*, as he called it, they had always recovered on being brought into the open air; that no fever had ever ensued from such accidents, nor, as he believed, from this kind of occupation; that sometimes, from intemperance, and getting cold, they had feverish indispositions, but not more so than other labourers; and that, when steady and sober, he thought them remarkably healthy; that their eyes were sometimes affected, so as to produce temporary blindness, from which, however, they commonly recovered in a few days; and that this, with asphyxia, were the only disorders to which he considered them as particularly liable from the nature of their occupation.

The receptacles of human ordure belonging to the great hotels in Paris, being commonly very capacious, and very seldom emptied, those mephitic exhalations which here produce asphyxia, and are there called "*plomb*," seem to be highly concentrated, in these receptacles, because they produce death to the nightmen not unfrequently, but, in other respects, their effects resemble those which are produced in this metropolis, as may be seen by a report, entitled, "Observations sur les fosses d'aisances et moyens de prevenir les inconveniens de leur vuidange," par M. M. Laborie, Cadet de Vaux, et Parmentier, (who had been employed by the French government to ascertain facts on this subject) published in the Journal de Physique, &c. An. 1778, p. 444.—See also Ramazzini de morb. artific. cap. xiii.

* In the "New English Canaan, containing an abstract of New England," written by Thomas Morton, "upon ten years knowledge and experiment of the Country," and printed at Amsterdam Ann. 1639, are these passages; viz. (P 86,) "The Coast aboundeth with such multitudes of codd, that the inhabitants of New England doe dunge their grounds with codd." P. 89. "There is a fish (by some called shadds, by some allizes) that at the spring of the year passe up the rivers to spawn in the ponds, and are taken in such multitudes in every river that hath a pond at the end, that the inhabitants dunge their ground with them. You may see in one towneship a hundred acres together set with these fish, every acre taking 1000 of them: and an acre thus dressed will produce and yeald so much corne as 3 acres without fish." This practice has been mentioned by several other writers.

# APPENDIX

## NO. III.

The purpose of this appendix has been stated, at pages 108 and 109 of this volume. It will contain a faithful abstract of Mr. Holwell's narrative of his own sufferings, and those of his unfortunate companions in the *black-hole*, at Calcutta, and more especially of the facts connected with the supposed production of fever by the crowding and suffocation which occurred in that situation.*

Fort William, at Calcutta, had been surrendered to the *Suba* of Bengal, in the afternoon of the 20th of June; and, between 7 and 8 o'clock of the same evening, Mr. Holwell, who had then become chief in council, with the other civil and military officers of the India company, their servants and soldiers, amounting in all to 146 persons, were forcibly driven into a prison, called the black-hole, which was a *cube of about eighteen feet*, "shut up by dead walls on the east and south, (the only quarters from which the wind could reach them) by a wall and door to the north, and open only to the west, by two windows *strongly barred*, from which they could scarcely receive *any the least* circulation of air." In this state, these unfortunate persons, previously exhausted by fatiguing exertions to defend the fort, and with only standing room, (i. e. $26\frac{1}{2}$ inches by 12 inches, to each person, upon the average) in a very sultry night, soon fell into an excessive perspiration, which was followed by extreme thirst; and this became the more insupportable, as their bodies were more and more deprived of moisture.

To gain more room, they stript off all their clothes; and, to relieve the fatigue of standing upright, they all sat down on their hams, or rose up at given signals: but they were so wedged up while sitting down, that it required considerable efforts to rise, and several who were too weak to make such efforts, were either trodden to death or suffocated. "Urinous effluvia" soon pervaded the interior of the prison, which at last became very powerful, and, to use Mr. Holwell's words, "affected them as if they were forcibly held with their heads over a bowl full of strong volatile spirit of hartshorn, until suffocating." (p. 26.) In the mean time, also, the atmosphere was gradually more and more vitiated; so that (p. 15,) "before 9 o'clock every man's thirst grew intolerable, and respiration difficult. In this distressing situation, the prisoners cried loudly for water; and when water was at length brought

* See "A genuine Narrative of the deplorable Deaths of the English Gentlemen, and others, who were suffocated in the *Black Hole*, in Fort William, at Calcutta, in the kingdom of Bengal, in the night succeeding the 20th June, 1756. In a Letter to a Friend, by J. Z. Holwell, Esq. London, printed for A. Millar, in the Strand, 1758."

by some of the guards, with such eagerness did they struggle to get it, that not only the greatest part of the water handed in hats through the bars of the prison was spilt before it reached any one's lips, but many were trampled down and suffocated, while others, particularly those who stood near the windows, were pressed to death. It was soon, however, discovered, that draughts of water were of little service towards quenching a thirst produced and kept up by such causes; they now, therefore, became clamorous for *air*, and endeavoured to force the door of the prison; but finding their attempts vain, and, preferring an immediate death to the lingering extinction which they apprehended as their doom, they grew outrageous, and abused (p. 25,) the Suba and his officers, and their own guards, by the most opprobrious names, "to provoke the latter to fire in upon them; every man that could, rushing tumultuously towards the window, with eager hopes of meeting the first shot. Then a general prayer to heaven to hasten the approach of the flames to the right and left of us, and put a period to our misery." After such violent exertions, they whose strength and spirits were quite exhausted, laid themselves down, and expired quietly upon some of their companions; others, who had yet some strength left, made a last effort for the windows, and several succeeded by leaping and scrambling over the backs and heads of those in the first ranks, and got hold of the bars, from which there was no removing them afterwards. In this manner, which is more easily conceived than described, was the remainder of the night passed; and when, at the dawn of day, an order was brought for their release, only 23 persons remained alive out of 146, and these were so weak that it took more than 20 minutes to remove the dead piled up against the door, so as to procure a passage out for one at a time. Of all those who survived, Mr. Holwell probably suffered the most in the course of that night, and certainly he had the narrowest escape from death; being one of the first who entered the prison, he had placed himself at a window, and continued there more than three hours, until his "legs were almost broken with the weight against them," and he was "at last so pressed and wedged up by those who were clinging to the bars over him, as to be deprived of all motion." Unable to endure this torment, he begged the people about him, "as the last instance of their regard," "to make way that he might retire from the window to die in quiet," and with difficulty he passed through them to the platform on the opposite side of the room. Here his distresses rapidly augmented, and, "in less than ten minutes, he was seized with a pain in the chest, and palpitation of the heart, both in the most exquisite degree." "He retained his senses, however, and not willing to bear any longer so much pain, without attempting to obtain the relief which he knew fresh air alone would afford, he pushed towards a window, and by an effort of double the strength he had ever before possessed, he found means to seize one of the bars, and to fix himself in the second rank of those who were standing at it.— The relief now felt was counterbalanced by new evils; for as others, in like manner, climbed and strove to get air, he presently had to sustain the weight of a heavy man, whose knees were pressing on his back, with the body resting on his head; a Dutch serjeant had also seated himself on his left shoulder, and a black soldier on his right, all which nothing could have enabled him long to support, but the props and pressure equally supporting him all around."—— In this position did he remain from half after 11 till about 2 o'clock, when, being exhausted by the repeated exertions he had made to dislodge these incumbrances, and finding that he must either quit the window, or sink were he was, he resolved on the former, having truly, for the sake of others, suffered infinitely more for life, than the best of it is worth. With great labour he forced his way from the window, (several, in the inner ranks, appearing to him dead, while standing, and only prevented, by the throng about them, from falling) and having gained the platform a second time, he lay down on it, and in a short time lost all sensation. Here he remained until near six o'clock in the morning, when the Suba, having been informed of the suffocation of most of the prisoners, sent to inquire if Mr. Holwell had survived: and being told that there was an appearance of life remaining, and that he might recover if the door were opened very soon, an order came instantly for the release of the prisoners. In the mean time, Mr. Holwell had been brought to the window, where he revived in a few minutes, and was shortly after restored to his senses. As we have no information respecting the circumstances and consequences of this transaction, excepting that which was published by Mr. Holwell, and as his account of those consequences of necessity relates principally to his own case, (for he was immediately separated from all but three of the survivors,) I have thought it proper to describe minutely his sufferings, that the reader might the better understand and decide, how far Dr. White, (in the Philosophical Transactions, vol. lxviii.) and Dr. P. A. Wilson, (in his treatise on febrile diseases, vol. i. p. 407) were justly intitled to adduce this transaction as an instance and proof of the generation, of "a most malignant and infectious fever," (these are Dr. White's words) by "the crowding together of a number of men in camps, hospitals, jails," &c. It is true that Mr. Holwell, at p. 31 of his narrative, mentions his belief that very few of his companions had retained their senses during the time in which he was senseless, or if they did, that they "lost them soon after they came into the open air, by the fever they carried out with them:" he also mentions, at p. 34, that when taken out of prison he found himself in "a high putrid fever," and so weak as "not to be able to stand;" and he informs us, at p. 38, that in the ensuing night, he was "covered from head to foot with large painful biles," which he considered as "the first symptom of his recovery; for, until these appeared, his fever did not leave him." During the next night, his three companions, also, "broke out in biles all over their bodies; a happy circumstance, (he adds) which, as I afterwards learned, *attended every one who came out of the black hole.*" From these expressions, it has been pretended that the atmosphere of the prison being contaminated by the respiration, and perspiration, of so many persons, and by "the *intolerable stench arising from the dead bodies*," (as is mentioned by Mr. Holwell, at p. 32) had generated contagion, and that this had produced in all the survivors that *formal disease* which is properly denominated *fever*. But, on a close examination of Mr. Holwell's statement, this will not be found to have happened, even though *suffocation*, from a want of the vital part of the atmosphere, breathed by these unfortunate persons, had been superadded to the other evils of their situation, and had irrecoverably destroyed nearly six in seven of their whole number; for, with vital air enough to obviate suffocation, crowding, with all its consequences, would not have caused any of their deaths. That they should have lost their senses by being suddenly brought into a *pure*, open atmosphere, can surprize no one; such a transition might well produce even greater changes upon men in their situation, without the aid, or existence, of fever, properly so called. And it appears evidently, from Mr. Holwell's statement, that he, and probably the others, very speedily recovered their senses: for he being carried immediately before the *Suba*, (or viceroy) who had heard or suspected that his prisoners had buried or concealed money and valuable effects, in the Fort, and wished, by violent threats, to extort a confession thereof from Mr. Holwell, the latter manifested such presence of mind, and power of reasoning, as could not have been expected of one who had been so recently brought back to life, even if no temporary suspension of intellect, or any other disorder than debility, had occurred. We are informed, however, that the Suba, giving no credit to Mr. Holwell's denials, ordered him to remain confined under the care of Mhir Muddon, general of his household troops, and he was consequently removed that morning, with his three companions, in a common carriage of the country, drawn by oxen, to the camp, "above three miles from the fort:" and soon after loaded with fetters; and it was *here* that he passed the night, and was covered with biles, which put an end to his supposed fever, *in less than* 24 *hours from its commencement*. That Mr. Holwell, who had suffered so greatly, and in so many ways, during the preceding night, should have found himself exhausted, disordered, and feverish, the following day, appears very natural; and perhaps it was not unnatural that using the word *fever* in a *loose*, and *popular* sense, he should describe himself as having been in one. But that his disorder was not strictly a fever, and much less what has been understood by the term of *putrid fever*, will, I am confident, be readily admitted by every physician of ordinary candour and discernment. Indeed, Mr. Holwell may be presumed to have had no other reason for giving the name of "*putrid*" to the febrile commotion which took place in him, than a supposition that any disorder occasioned by the heated and putrid atmosphere of the black hole, must necessarily partake of putridity. But if a putrid fever had really occurred, it is not credible that he could have been removed above three miles, exposed to the unclouded sun in that climate, and yet relieved from this fever in less than twenty-four hours. It appears also (at p. 38) that Mr. Holwell and his companions were *marched* back to Calcutta the next morning "in their *fetters*, under the *scorching* beams of an *intensely hot* sun," which certainly would require exertions too great for men, of whom *one* had been in a putrid fever but a few hours before, while the others were not then relieved from that with which they were said to have been attacked, and which only left them upon the occurrence of biles in the evening, after this fatiguing march. And certainly if Mr. Holwell's disorder, and that, of his companions, had amounted to fever properly so called, such fatigue, if, against all credibility, they were capable of enduring it, must have been followed by consequences very different from any which appear to have been produced. Two days after this march, these gentlemen were embarked in a large *open boat*, and sent as prisoners up the river, to *Muxadabad*, then the capital of Bengal; this voyage lasted 13 days, and, during the whole of it, they were exposed to one regular succession of heavy rain or intense sunshine," with no defence against either; and their only food during most of the time, was rice and the muddy water of the river: they were besides "so distressed for room, that they could not stir without bruising their own or each others biles;" moreover Mr. Holwell, on a particular occasion, was forced out of the boat, and made to walk in the scorching sun about noon, more than a mile and a half from the river; "his legs running in a stream of blood, from the irritation of his irons, and himself ready to drop at every step from excessive faintness, and unspeakable pain." "By this cruel travel" he became so exhausted, that his guards were forced to *carry* him part of the way back, and support him the rest of it. All these sufferings, however, did not produce fever. Mr. Holwell, indeed, tells us, (p. 47) that five or six days after this, "he was attacked by a fever on the night of his arrival" at Muxadabad, attended with considerable inflammation of his leg and thigh; yet he adds that "all terminated the next night by a *regular fit* of *the gout* in his *right foot* and *ankle*;" a sufficient indication of the laxity and incorrectness with which Mr. Holwell has used the term *fever*.

Such are the facts relating to this *singular* transaction, and to me they prove decidedly, *not that febrile contagion may be generated*, by crowding many persons into a small space, without sufficient ventilation, *but the reverse*. For certainly the few who were recovered from the *combined* efforts of crowding and suffocation, escaped with less of indisposition than could have been reasonably expected, considering the cruel treatment which some, and probably all of them, afterwards sustained: and it cannot be fairly pretended, that this indisposition amounted, in a single individual, to that disease which is strictly denominated fever; much less to *contagious* fever, of which, indeed, there was not the slightest vestige or appearance. In none of the instances where crowding has been supposed to have generated febrile contagion, was it ever carried to the extent of *suffocating*, I will not say the greater, but even a small part of those subjected to it: and as in this case, (where, out of 146 persons, 123 were thus deprived of life) neither contagion, nor even a fever, without contagion, was produced among the survivors, notwithstanding the concentrated exhalations from so many dead bodies, running speedily into putrefaction, (as their various excretions had done before) we may consider this as a most decisive proof that febrile *contagion* is not capable of being generated by causes of this description.

# APPENDIX.

## No. IV.

The subject of this Appendix, viz.—The *Black Assize at Oxford*, anno 1577, and that of the *Old Bailey*, anno 1750, are mentioned at pages 110 and 111.

Though I have taken considerable pains to throw light upon these remarkable events, the former, I fear, must always remain in great obscurity. The best account of it which I have been able to find, is that given in "The History and Antiquities of the University of Oxford," by Anthony A. Wood, M. A. of Merton College, first published in English, from the original MS. in the Bodleian Library, by John Gutch, A. M. printed at Oxford, &c. 1796. At p. 188 of the 2nd volume of this work, we are informed by the author, that at this time, i. e. the 19th-20th of Queen Elizabeth,—"lived in Oxford a certain book-binder, named Rowland Jencks, who, in his familiar discourse, would not only rail against the commonwealth, but the religion now established, and sincerely by the generality in the university embraced:" that "he made it his chief employment to vilify the government now settled; profane God's word; speak evilly of the ministers, &c."—"In this course of life, he continuing for some time, taking glory, as it were, in it, the university to whom the said person belonged, (because privileged) took cognizance of him and his actions;" and "a convocation of doctors, regents, and non-regents being held, May 1st, it was ordered that he should be seized on, and sent to London to be examined by the Chancellor of the University, and the Queen's Council."—Which was done. "But after he had been examined at London, he was sent to Oxford again to be committed to prison, and stand to a trial the next assizes following," &c.

"The assizes, therefore, being come, which began the 4th of July, and continued two days after, in the *Court-House* at the *Castle yard;* the said Jencks was arraigned, and condemned, in the presence of *a great number of people*, to *lose his ears. Judgment being passed*, and *the prisoner taken away*, there arose such an *infectious damp*, or *breath* among the people, that *many there present*, to *the apprehensions of most men*, were *there smothered;* and others so deeply infected, that they lived not many hours after." Here Mr. Wood introduces an old ditty written upon that event, and printed in black letter, in which death is made to boast of his feats on that occasion. "The persons (continues Mr. Wood,) that then died, and were infected by the said *damp*, when sentence was passed, were Sir Robert Bell, Chief Baron of the Exchequer; Sir

Nicholas Barham, Sergeant at Law; both stiff enemies to the Roman Catholic religion;—Sir Robert Doiley, High-Sheriff; Hart, his Under-Sheriff; Sir William Babington, Knight; with five Justices of the Peace;" and a considerable number of gentlemen who are named, "besides most of the jury, with many others that *died in a day or two after.* Above 600 sickened in one night, as a physician* that now lived at Oxford attesteth; and the day after, the infectious air being carried into the next village,† sickened there an hundred more.

"The 15th, 16th, and 17th days of July sickened also‡ above 300 persons, and within 12 days space died an hundred scholars, besides many citizens. The number of persons that died in five weeks space, namely, from the 6th of July to the 12th of August (for no longer did this violent infection continue,) were 300 in Oxford, and 200 and odd in other places, so that the whole number that died at that time were 510 persons, of whom many *bled till they expired.* The time, without doubt, was very calamitous and full of sorrow; some leaving their beds, occasioned by the rage of their disease and pain, would beat their keepers or nurses, and drive them from their presence; others, like madmen, would run about the streets, markets, lanes, and other places; some again would leap headlong into deep waters."—"The physicians fled,—not to avoid trouble which came more and more upon them, but to save themselves and theirs.|| The doctors and heads of houses, all, almost to a man, fled, and not any college or hall was there, but had some taken away by this infection." Those who thus died, he says, "were troubled with a most vehement pain of the head and stomach, vexed with the *phrenzy*, deprived of their understanding, memory, sight, hearing, &c."—"At the time of their deaths, they would be very strong and vigorous, but if they escaped it they were to the contrary."§—"That which is most admired is, that

* Dr. George Ethryg is the physician here alluded to. He practiced at that time in Oxford, and in the second book of his "Hypomnemata quædam in aliquot Libros Pauli Æginatæ," &c. printed in London, anno 1580, he states, that on the night in which the disease first made its appearance, abont 600 were attacked by it, and in the next night 100 more in the neighbouring villages.

† The carrying of the air to the neighbouring villages, seems to be a mistake. Those who sickened in them, had all been exposed to the cause of the disease, whatever it was, in the Court House, or in the Castle, as is stated in the register of Merton College, of which an extract was published in the Philosophical Transactions, vol. 50, p. 699—702, and in which, at p. 701, are these words:—"Nam *illi solum* et hic et *alibi* decumbent ægroti qui en *castro* et *guilda*, quam apellant *aula* quinto et sexto hujus mensis adsunt."

‡ This is also stated in the register of Merton College.

|| This seems to have been an unnecessary, as well as culpable desertion of duty by the physicians; the disease having never manifested any contagious property, whatever apprehensions might have been entertained on that subject at first. Besides the fact asserted in the Register of Merton College, that those only sickened who were present, &c. on the 5th and 6th of July, and that no medical attendants, nor visitors were attacked, Mr. John Stow, in his Chronicle of England, (London. 1631) after saying that "there died in Oxford 300 persons, and in other places 200 and odd, from the 6th of July to the 12th of August," adds, "after which died not any of that sickness, *for one of them infected not another.*"

§ This account of the disease is in exact conformity with the Register of Merton College.

no women were taken away by it, or poor people, or such that *administered physic*, or *any that came to visit.** But as the physicians were ignorant of the causes, so also of the cures of the disease." He adds, "many supposed that the cause of this infection proceeded from the nasty and pestilential smell of the prisoners when they came out of the jail, of whom, two or three being overcome with it, died a few days before the assize began, as a note† written in these times testifieth. If so be, that was the cause, (he adds) why then were none destroyed at the *first* appearance of the said prisoners, which was the 5th of July, when, as 'tis generally said,‡ none died till after sentence was passed, which was the day following? Certainly, we cannot to the contrary but think, that the said smell or stench, was more violent the first time when the prisoners appeared, than when they had received air several times before." Here, however, he introduces the observation of Bacon, Lord Verulam, mentioned at p. 109 of this volume. He afterwards informs us, that "some thought this Oxford mortality was the same that Leonard Fuschius calls *Sudor Anglicus*, (sweating sickness,) which began first in England, anno 1485, 1st Henry VII." &c.: but this Mr. Wood thinks "not likely, because the nature of that disease was almost quite different from the other." He adds, "Some again have thought, and do think, that it was devised by the Roman catholics, who used the *art magic* in the design; and that also, as a certain note‖ witnesseth, it sprung "ex artificionis *Diabolicis*, et plane *papisticis* flatibus e *Lovaniensi* barathro excitatis, et ad "nos scelestissime, et clam emissis." This notion, however, he rejects, as well as that of *Sanderus*, who (de Schismate Angl. lib. iii.) calls it "*ingens miraculum*," and a just judgment on the cruelty of the judge, for sentencing the book-binder to lose his ears.§

The most important, and at the same time the most obscure part of this account, is that which relates to the circumstances and means connected with the production of this disease. Stow, after mentioning the arraignment and condemnation of Jencks, "for his seditious tongue," says, "At *which time* there arose amidst the people such a damp, that almost all were *smothered*; very few escaped that were not taken *at that instant*;—the jurors died presently," &c. Annals of England—imprinted at London, by Ralfe Newberry, 1601.

* This is also stated in the Register of Merton College; and Stow also asserts, (Loc. Citat.) that no woman or child died of the disease; in which he is supported by Sir Richard Baker, in his chronicle of the Kings of England, p 353, and by Camden, in his Annal Reginæ, Elizabeth, 1577, (Ed. Th. Hearnii, 1717,) p. 316,—vol. 2.

† This is mentioned by the author from the entry in the Register of Merton College, to which he belonged; and this register, with which he was well acquainted, appears to have been his only authority for stating, that *two or three of the prisoners had died before the assize began*, as I cannot find any thing of that sort mentioned by any other writer.

‡ This is stated by Camden, and I believe, by Hollingshed, to whom I cannot now conveniently refer.

‖ The author here alludes to the Register of Merton College, and proceeds to copy from it the words placed between inverted commas.

§ Mr. Wood informs us, that after "Rowland Jencks had suffered the sentence passed upon him, he went to Donay, (in Flanders) and there became *baker* to the English College of Seculars, and lived to be a very old man."

Camden, (Loc. Citat.) after mentioning the bringing up of Jencks for judgment, adds, "*venenoso* et *pestilenti halitu, sive fœdore incarceratorum, sive ex solo* ita correpti sunt, plerique omnes qui aderant ut," &c. And sir Richard Baker, at the page lately quoted, after mentioning the same proceedings against Jencks, adds, "Suddenly they were surprised with a pestilential savour, whether rising from the noisome smell of the prisoners, or from the *damp ground*,* is uncertain; but all that were then present, almost every one, *within forty hours died*, except women and children."

The most probable *meaning* of all these accounts would seem to be, that about the time when sentence was passed on the prisoners, a noxious vapour, in some degree perceptible by the senses, and proceeding either from the prisoners, or the earth, had been suddenly diffused through the hall, and that in consequence thereof, a great part of those who were present had been almost immediately attacked, and that many died within a few hours.

There is, however, no cause of disease with which I am acquainted, whose effects would have been such as are here described. Pestilential contagion cannot be suspected, because that would have required *contact*, and because the symptoms of the disease were not like those of the plague, nor was it contagious. And there is as little reason to suspect the contagion of typhus or jail fever, (especially at that season of the year,) there being no instance recorded, or known, of its producing disease so suddenly, nor of that disease when produced, terminating so speedily in death. Nor were the symptoms such as occur in jail fevers:† nor does the contagion of that fever spare women,

* This expression, and that of Camden, seem to point at marsh effluvia, which, at that season of the year, would be more likely to occasion disease, than typhus contagion, and in a shorter space of time, and chiefly upon vigorous men; probably, also, the situation of the place was suitable for their production. The old Shire Hall, in which sentence was passed on Rowland Jencks, was placed in the *yard* of Oxford Castle, (once deemed impregnable,) which stood on the west side of the town, at a small distance from the river *Isis*, whose banks, especially at that time, were low. The prison was also within the castle, at about 200 yards distance from the hall, and consisted of a multangular tower, called St. George's. (on the west side of the castle,) together with an adjoining church, which also bore the name of St George, and two square rooms, all connected one with the other, and made the common gaol for the county, by a statute in the reign of Henry the 3d. See Grose's Antiquities of England, vol iv. p. 182-3: also, King's Vestiges of Oxford Castle, p. 28. In the appendix to Thomas Hearne's Preface to Gulielmi Neubrigensis Historia, &c. p 88, is a print representing the catsle of Oxford, and on the other side of the river is a mount, at the foot of which, are the ruins of an old building, which are thus described in a note to the plate: viz.—"Reliquiæ domûs in quâ *assizæ* olium tene bantur, donec *ob pestem* subitaneam, ad alium civitatis locum regnante *Elizabethâ* transferre placuit." But though I think marsh miasmata a more probable cause of the disease in question than typhus contagion, I am far from believing that they would have produced effects such as are said to have occurred at this *black assize*.

† In addition to, and confirmation of the account of the symptoms of this disease, as stated by Mr Wood, from the Register of Merton College, I will here subjoin that which was given to Mr. Bernard Gilpin, who after founding a grammar school at Houghton le Spring, in Durham, educated several young men there, and afterwards maintained them at the University of Oxford, which brought him into a correspondence with their college tutors, one of whom wrote a letter in Latin, giving an account of the disease in question of which an extract translated, has been preserved in "The Life of Bernard Gilpin, by William Gilpin, M. A." 2nd edition, London, 1763, p. 120. The translated account of this *disease* is in these words, viz.—"Those who are seized with it are in the utmost torment; their bowels are burnt up; they call earnestly for

children, and *poor people*, as the cause of this disease is stated to have done, (but on the contrary:)—nor do the stoutest and most robust soonest perish by it, as the Register of Merton College declares to have happened in this disease. ("Et ut quisque fortissimus, ita citissime moritur.")

Whether the facts connected with the production and nature of this disease have been misrepresented, or, whether it proceeded from a cause which has ceased to operate in later times, I leave for the decision of others.

The sickness which I have mentioned at p. 110, as occurring at Exeter, almost nine years after, viz. in 1586, appears to have been the *true jail distemper*, according to the account given of it by Hollingshead, vol. ii. p. 1547, where he says, "At the assizes kept at the citie of Exeter, the 14th daie of March, in the eight and twentieth year of her majestie's reigne, before Sir Edmund Anderson, Knight, Lord Chief Justice of the Common Pleas, and Serjeant Floredaie, one of the Barons of the Exchequer, Justices of the Assize in the county of Devon and Exon, there happened a very sudden and strange sickness, first among the prisoners in the jail of the castle of Exon, and then dispersed (upon their triall) amongst sundrie other persons."—"This sickness was very sharp for the time, and few escaped which at the first were infected therewith. It was contagious and infectious, but not so violent as commonlie the pestilence is; neither doth there appeare anie outwarde *bleer* or *sore*."—"The origin and cause thereof, diverse men are of diverse judgments. Some did impute it, and were of the mind that it proceeded from the contagion of a gaole, which, by reason of the close aire and filthie stinke, the prisoners newly come out of a fresh aire into the same, are, in a short time, for the most part, infected therewith; and this is commonlie called *gaole sickness*, and *many die thereof*." He then proceeds to relate the case of thirty-eight Portuguese seamen, who had been some time before taken at sea, coming with fish from Newfoundland, and "cast into the *deep pit* and *stinking dungeon*" of the "gaole of the castle of Exon;" and "had no change of apparell," but being left to lie "upon the ground, without succour or reliefe, were soon infected;" (probably by the contagion previously existing in this dungeon,) "and all, for the most part, were sicke, and some of them died, and some of them was (were) *distracted*; and this sickness verie soon after dispersed itselfe among all the residue of the prisoners in the gaole, of which disease many of them died, but all brought to great extremities, and verie hardlie es-

drink; they cannot bear the touch of clothes; they intreat the standers by to throw cold water upon them; sometimes they are quite mad; rise upon their keepers; run naked out of houses; and often endeavour to put an end to their lives The physicians are confounded, declaring they have met with nothing similar, either in their reading or practice. Yet many of them give this distemper a name, though they have done nothing to shew that they are at all acquainted with its nature. The greater part of them I am told, have now left the town, either out of fear for themselves, or, conscious that they can do no good This dreadful distemper is now generally attributed to some jail infection, brought into court at the assizes: for it is remarkable, that the first infected were those only who had been there. Few women or old men have died. God be thanked, the rage of this pestilence is now much abated. It is still among us, in some degree, but its effects appear every day weaker."

caped. These men, when they were brought before the foresaid justices for their triall, manie of them were so weake and sicke, that they were not able to go nor stand, but were carried from the gaole to the place of judgment, some upon hand-barrows, and some between men leading them, &c. He adds, that these miserable men were brought in at "one end of the hall, near to the judge's seat," where their wretched condition excited general commiseration, and particularly that of the chief justice, "who, upon this occasion, *tooke a better order* for keeping all prisoners thenceforth in the gaole," &c. He adds, "And howsoever the matter fell out, and by what occasion it happened, an infection followed upon manie, and a great number of such as were there in the court, and especially such as were nearest to them, were soonest infected. And albeit the infection was not then perceived, because everie man departed, as he thought, in as good health as he came thither; yet, the same by little and little, so crept into such upon whom the infection was seizoned, that after a few daies, and at their homecoming to their own houses, they felt the violence of this pestilent sickness, wherein more died that were infected, than escaped." He then gives the names of some of the principal persons who were thus cut off; among whom were Serjeant Floridaie, one of the judges; three knights; and several esquires and justices of the peace; and many constables, reeves, tythingmen, &c. In this case, the disease appears to have been propagated by those who were first attacked, so that "it was dispersed throughout all the whole shire." And, on this subject the author makes an important, and I believe just observation, concerning the time which the infection remained *dormant in the system*: viz.—"*It resteth for the most* part, *fourteene daies and upwards*, by a *secret infection*, before it *breake out into his force* and *violence*." All this is *very unlike* the disease from which the black assize at Oxford obtained that appellation.

I am next, according to the promise made at p. 111 of this volume, to enter upon an examination of the facts relating to the supposed propagation of typhus or jail fever, at the Spring sessions of 1750, in the *Old Bailey*, by contagion from *Newgate*.

I have already mentioned that Sir John Pringle's Observations on "Hospital and Jayl Fevers," were published (probably written also,) immediately after, and *to take advantage* of the sickness which then occurred. In his dedication of that publication to Dr. Mead, he says, "Whilst I was revising the notes I had made on the Diseases most incident to an Army, the jayl distemper having broke out in such a manner, as to alarm the town, I thought I could not comply more seasonably with your desire of having them published, than by communicating *at present*, that part of my observations which related to this disease." He says afterwards, that he the more willingly embraces this occasion of writing, "as *at this time every body is inclined to listen to the subject*," &c.; adding, "I am *certain*, that however rarely our jayls produce such visible noxious effects, they are often one of the more *insidious* sources of slow and malignant fevers, which generally prevail in large and crowded cities. Thus, in the late case of infection:—from the *quantity of the contagious matter*, the closeness of the air, and crowds of people to render its corruption more quick, a distemper arose so suddenly, and was so violent, general, and fatal, that every body now refers it to *its true cause;* whereas, if the number of malefactors had been fewer, the multitude less, and the air freer, so few would have been seized, and that with fevers of a slow and less alarming kind, that the cause might have been intirely overlooked."

By thus confidently representing the Old Bailey sickness as the product of jail infection, Sir John Pringle obviously secured a very general attention to his publication, and gave it unusual importance *at that time.* But I think too highly of his probity, to magine that he would, from such considerations, intentionally mislead the public; and, therefore, I am the more surprised, that he should have thus assumed to know, and decide upon, "the *true cause*" of this disease, and upon "the *quantity* of contagious matter" supposed to produce it, without having seen any one of the persons who had been attacked by the disease; and without having, in the smallest degree, ascertained the existence even of a single atom of contagious matter, at that time, in Newgate.

But, though Sir John Pringle, in this his first publication, confined himself to general, and gratuitous assumptions, respecting this transaction, he afterwards, in his work on the Diseases of the Army, first published in 1752, gave a statement of what he considered as the principal facts relating to it; and, as this statement seems to be the foundation of almost all that has been since written, and believed by others on this subject, I shall here subjoin it, from the seventh edition of that work, which contains, I believe, his latest additions and alterations; introducing, in the form of notes, such corrections and explanations as seem necessary. The statement is at p. 330, and seq. in these words, viz.—"In the year 1750, on the 11th of May,* the sessions began at the Old Bailey, and continued for some days; in which time, there were more criminals tried, and a greater multitude present in the court, than usual. The hall in the Old Bailey, was a room of only about 30 feet square.† Now, whether the air was most tainted from the bar, by some of the prisoners, *then ill of the jail distemper*, or, by

*The sessions began, as is proved by all the diurnal, and monthly publications of that time, on the 25th of April, *old style,* and ended on the 30th. Sir John Pringle intended here to designate the time according to the *new* style, which was afterwards established by law, as is evident, by his having annexed the letters N. S. to the 11th of May, in his account given in the *first* edition of his volume on the Diseases of the Army. But even this left him in an error of five days. The Universal Magazine for April, 1750, contains the following passage:—"Wednesday, the 25th, the sessions began at the Old Bailey; when 23 prisoners were tried;" &c. and it added, that on "*Thursday,*" the 26th, *ten* persons were tried; and among them "Captain Edward Clark, for shooting Captain Thomas Innes, in a duel, in Hyde Park," March the 12th. These gentlemen were captains in the royal navy, and Captain Clark's trial exciting great curiosity, produced a very crowded court, and lasted a great part of the day; so that only nine other prisoners were tried And it was on this day, the 26th, as will be seen presently, that the cause of the disease, whatever it may have been, was applied to the persons who afterwards sickened, and who appear to have been all then present, and *only then.* There was, indeed, so far as I can discover, no trial during the sessions of that month, which excited any great curiosity, or occasioned any crowd; nor will Sir John Pringle's reasonings or descriptions apply to any other day.

† According to the measurement of Mr. Dance, Clerk of the Works to the city of London, this hall was 40 *feet* in length, and 31 *feet* in breadth.

the *general uncleanliness* of such persons, is uncertain;* but it is probable that both causes concurred. And, we may easily conceive, how much the air might have been vitiated by the foul steams of the Bail-dock, and of the *two rooms* opening into the court, in which the prisoners were the whole day crowded together, till they were brought out to be tried.† It appeared, afterwards, that those places had not been cleaned for some years. The poisonous quality of the air was aggravated by the heat and closeness of the court, and by the perspirable matter of a number of people, of all sorts, penned up for the most part of the day, without breathing the free air, or receiving any refreshment.‡ The bench consisted of six persons, whereof four died, together with two or three of the counsel, one of the under-sheriffs, several of the Middlesex Jury, and others present, to the amount of above forty; without making allowance for those of a lower rank, whose death may not have been heard of; and without including any that did not *sicken within a fortnight* after the sessions.

"*It was said*,‖ that this fever, in the beginning, had an inflammatory appearance, but that after large evacuations the pulse sunk, and was not to be raised by blisters, nor cordials; and the patients soon became delirious. Some had petechiæ, and all that were seized with the fever died, excepting two or three at most. Some escaped without a fever, by a *looseness* coming on, and which was easily cured. How

* This question, of all others, ought not to have been left, and much less represented, as being in a state of uncertainty; since all our reasonings and conclusions will almost exclusively depend upon it. Sir John Pringle certainly had no good reason to suppose that any person in Newgate was "*then ill of the jail distemper*, and much less *any of the prisoners at the bar;* and his language is therefore fallacious. In regard to "the general uncleanliness of such persons," I shall indeed wonder if any one, after reading this volume, can believe it to have been capable of producing the disease under consideration.

† In a note to this passage, Sir John Pringle mentions his having heard, that about 100 prisoners were tried at these sessions, and he supposes that they "were *all* kept" in "the bail-dock, and the two rooms opening into the court,"—"as long as the court sat,"—which is, I believe, an error: for though care is taken, that a sufficient number of prisoners shall always be in readiness for trial, the whole number is not, according to my information, brought up *at once*, when it is known that they cannot all be tried in one day. Sir John Pringle seems also to have formed erroneous ideas of the situation of the bail-dock, &c. It will be seen by the plan of the Sessions House, or *Justice Hall*, in the Old Bailey, between pages 438 and 439, that the bail-dock is placed *without*, and in *front* of the house, and is a kind of *vaulted cell* or *dungeon*, *below the surface* of the *court yard*, where the prisoners are kept, until called *successively* into court, a *few minutes before* their respective trials are expected to commence. And while in the bail-dock, they must be too far removed, and too much separated from the hall, to infect the court, even if they were all labouring under jail fever. In regard to the "two rooms opening into the court," and which he supposes to have been *crowded* with prisoners, they must have been merely partitions within the bail-dock, to separate the men from the women.

‡ The heat and closeness of the court, with the quantity of perspirable matter from those who were "penned up in" it, for a few hours, will bear no sort of comparison with the state of the *Black Hole* at Calcutta, where we have seen that nothing like contagion, or a regular, and much less *a mortal fever* was produced.

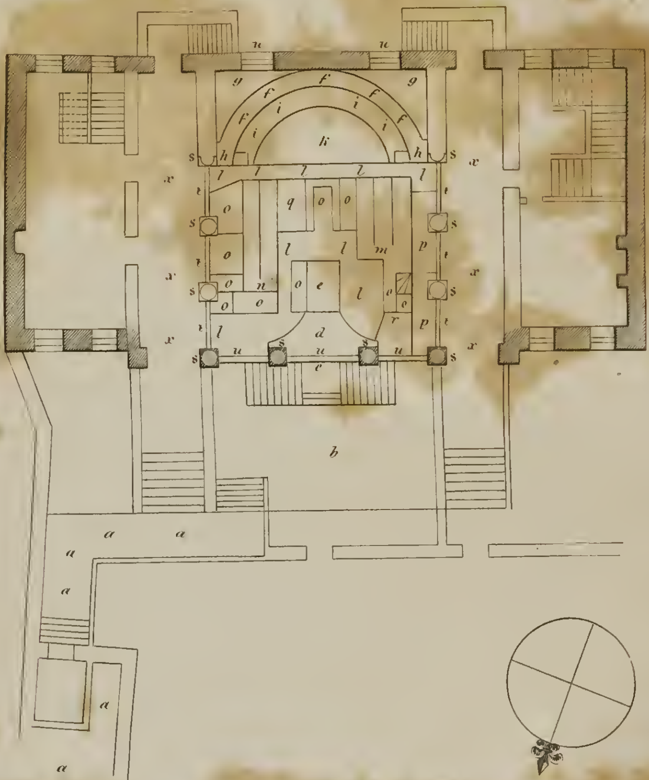
‖ *This*, and other expressions, demonstrate that Sir John Pringle did not attend a single person labouring under this fever;—for, if he had done so, it would not have been necessary for him to recur to hearsay; and he would certainly have given us a *distinct account* of the *symptoms*, especially as all those who did attend in this sickness, had strangely neglected this duty; so that we have nothing but *loose* hearsays on a point of great interest and importance.

far this sickness spread among the nurses, and other attendants on the sick, is not known."*

Sir John Pringle has mentioned in a note, that the persons on the bench of the judges were, " The lord mayor, three of the judges, one of the aldermen, and the recorder. Of these (he adds) died Sir Samuel Pennant, Lord Mayor; Sir Thomas Abney, and Baron Clark, Judges; and Sir Daniel Lambert, Alderman. It was remarkable that the lord chief justice, and the recorder, who sat on the lord mayor's *right* hand, escaped, whilst he himself, with the rest of the bench on his left, were seized with the infection; that the Middlesex Jury, on the left side of the court lost many, whilst the London Jury opposite to them received no harm; and that of the whole multitude, but one or two, or at most, a small number of those who were on the Lord Mayor's right hand, were taken ill. Some, unacquainted with the dangerous nature of putrid effluvia, have ascribed both this circumstance, and the sickness in general, to a *cold taken by opening a window*, by by which a *stream* of air was directed to the side of the court on the Lord Mayor's left hand.† But it is to be observed, that the window

* I am sorry to observe, that there is an appearance of disingenuousness in this expression. The author, in his first edition, had said, " This sickness, as far as appears, *spread no farther*; which was perhaps owing to the season, and to the weather, at that time *cold*, from *northerly* winds." A circumstance, however, which would have favored its spreading, had the disease been a jail fever. In the 5th edition, (printed in 1765,) the author says, " This sickness, as far as was known, spread no farther; there being at that time *no disposition in the air*, nor other circumstances to propagate the infection." Here it is manifest, that he was convinced no person had taken the disease from any of those who were first attacked, and that he was anxious to *obviate* the inference suggested by that circumstance, that the disease was not the jail fever, by saying *what he could not have known*, that there was then "*no disposition in the air*," &c. when, in fact, the unusual coldness of the season, was suited " to propagate the infection." Whether Sir John Pringle was convinced of the insufficiency of these allegations, to explain why the disease *had not been communicated* by any of the sick, I know not; but he afterwards thought proper, in his 7th edition, printed in 1774, to *suppress* these allegations, and represent it as being uncertain, whether the disease had, or had not, been communicated by a contagious quality, or rather as being uncertain *how far* it had been "spread &c.; affording room for a belief that it had in fact been spread among the nurses," &c. though the *extent* of its spreading was unknown. Twenty four years had then elapsed, and with the dispositions which were manifested by Sir John Pringle on this subject, it may be safely inferred, that he had never in all that time been able to gain any information, rendering it probable that the fever under consideration, had been propagated in a single instance by contagion. For a single well-ascertained fact of this nature, would have answered his purpose, by proving that the disease was a jail fever, and have rendered unnecessary all his other laborious and ineffectual endeavors to attain this object.

† In the Gentleman's Magazine for May, 1750, (p. 235,) the following paragraph relating to this subject may be found, viz :—" As there was no uncommon sickness among the prisoners *that came to the bar*, or in Newgate, some attentive observers do not suppose it to be a case of infection. But that the court being so much *heated* by the crowds which came to hear the extraordinary trials of Captain Clark and others, and there being a *very cold piercing East Wind* to attack the sweating persons, *when they came out of court*, and many of them dining in public, and drinking perhaps a glass extraordinary, to comfort them after fatigue, a fatal fever might ensue." The writer of this paragraph, though he properly ascribes the fever to a piercing cold East wind, seems not to have been sufficiently informed of a *most important fact*, connected with the place and manner in which this wind was applied, so as to produce disease :—I mean the *opening of a large window* in front, and on the *left* hand of the court, at the North-West corner of the building, one of the *three* distinguished by the letters N, N, N, in the annexed plan. This was done in consequence of the *great heat* and *want of air*, produced by the *crowded* state of the hall, during Captain Clark's trial. It appears, that before this expedient was resorted to, the court, as well as the audience, had been greatly incommoded and overheat-




*Plan of the Old Bailey, as it existed previously to its destruction during the riots in 1780. copied exactly from the original Drawing in the possession of George Dance Esq. Architect or Clerk of the Works, to the City of London, by whom this Copy was given to the Author. NB The divisions within the Court-Hall are marked according to its present state; but the Author has been informed by Mr. Dance that these do not differ, or, at least, not materially, from those which existed in the former Building.*

## EXPLANATION of the PLAN of the OLD BAILEY.

*a* *Covered passage, by which the prisoners are brought from Newgate to the Old Bailey.*

*b* *Bail Dock, in which the Prisoners are kept till they are called into Court.*

c *Small Door, (under the Window) by which Prisoners enter the Court from the Bail-Dock, preparatory to their trial.*

*d* *Place where they stand before they go into the Prisoners box.*

*e* *Box for Prisoners under trial.*

*f* *Bench for the Lord Mayor, Judges and Aldermen.*

*g* *Places of retirement from that Bench, taken from the corners of the room. They are about seven feet high, and that on the right of the Court is left open at top.*

*h* *Boxes for the Sheriffs.*

*i* *Bench for the Counsel.*

*k* *Table for Do.*

*l* *Spaces serving as passages.*

*m* *Box for the Middlesex Jury.*

*n* *Box occasionally used for the London Jury.*

*o* *Boxes or Seats for Officers, Clerks, &c. of the Court.*

*p* *Box for Law-Students, &c.*

*q* *Box for the Witnesses.*

r *Standing place for persons who attend to prosecute or give evidence.*

s *Columns.*

*t* *Doors opening into different parts of the Court.*

*u* *Windows, of which the three facing the Judges are large Sash-Windows.*

*x* *Passage outside of the Court, over which are the Galleries for Strangers who come to hear the Trials.*

was at the furthest end of the room from the bench, though the judges suffered most. Nor could the *kind of fever*, nor the mortality attending it, be attributed to a cold; it is therefore probable, that the air from the window, directed the putrid streams to that part of the court above-mentioned. Indeed, it must be granted, that septic particles passing into the blood, become more active and fatal, if the infected person catches cold, or, by any accident suffers a stoppage of perspiration, or of any of the other discharges of excrementitious and noxious matter.

Before I particularly notice the latter part of the preceding note, I think it proper to introduce some other testimonies on this subject.

In Foster's Reports and Cases on Crown Law, (printed at Oxford, 1762, folio,) I find, at p. 72, the following statements, viz:—

" At the Old Bailey sessions in April, 1750, one Mr. Clark was brought to his trial; and, it being a case of great expectation, the court, and all the passages to it were extremely crowded, &c.

" Many people who were in court at this time, were sensibly affected with a very noisome smell; and, it appeared soon afterwards, upon an enquiry ordered by the Court of Aldermen,* that the whole prison of Newgate, and all the passages leading thence into the court, were in a very filthy condition, and had long been so.

" What made these circumstances to be at all attended to, was, that within a week, or ten days at most, after the session, many people who were present at *Mr. Clark's trial*, were seized with a fever of the malignant kind; and few who were seized recovered.

" The symptoms were much alike in all the patients; and, in less than six weeks time, the distemper entirely ceased.

" It ought to be remembered, that at the time this disaster happened, *there was no sickness in the gaol more than is common in such places;* this circumstance, *which distinguisheth this from most of the cases of the like kind* which we have heard of, suggesteth a very proper caution:—*Not to presume too far upon the health of the gaol, barely because the gaol fever is not among the prisoners.*"

In addition to the preceding statement, it will be proper for me to introduce the testimony of the Rev. Stephen Hales, D. D and F. R. S. whose character as a divine, and philosopher, needs no encomium from me. He attended as a witness to Captain Clark's character upon the trial in question; and, in the second part of his Treatise on Ventilators, (printed in 1750,) at p. 161, after quoting a considera-

ed; and, upon the opening of the window, a stream of cold air forcibly rushed in, and passed straight forward *along that side of the hall* which was nearest to the street, and on the left hand of the Lord Mayor. And the mischief done, or sickness produced, appears to have been confined to those who were placed in the direction of this stream of *cold air*; which, *therefore, contained and conveyed the morbid influence, whatever it was, that occasioned the fever.* This important conclusion, my readers will, I hope, *bear in mind,* till I can resume this topic.

* This mention of the Court of Aldermen made me wish to have the books of that court examined; and a gentleman, who by his office, had access to them, obligingly looked them over for the whole time between 1745, and 1770, without being able to find any entry respecting this subject, or the health of the prisoners, except some orders for the payment of money, for putting up and working a ventilator at Newgate, which was done in consequence of the sickness in question, under the direction of the Rev. Dr. Hales and Sir John Pringle.

ble part of Sir John Pringle's account of it, he adds, "As the most putrid vapours are the most subtile and volatile, so I observed them to be in the court, at the Old Bailey, May the 11th, (erroneously adopting Sir John Pringle's date) 1750, when I was obliged to be there, and found the *smell* of the air in the gallery on the right side of the court, sensibly more offensive than below, when I was called down among the crowd to give evidence. And accordingly, those who were situated *highest* in the court, as the *Lord Mayor*, *Judges*, *Middlesex Jury*, and those in the gallery on the *left* hand of the court, were *chiefly infected with the fatal contagion ;* on which (left) side a *wide* sash-window facing the Judges *was open ; at which* an *Easterly* wind *entered*, which might *blow down* the *most venomous* vapour which was *near the ceiling*,* and *condense*, in some degree, and check the subtile infectious vapour, *heated* by a *crowded* court for *many hours*, from *ascending* so fast from among those on the bench, and in the left hand gallery: whereas, on the *right hand*, *where no window was open*, the same heat might cause the envenomed vapour to ascend quicker to the lofty ceiling; as it is well known such vapours constantly do in rooms full of crowded assemblies."

Assisted by these statements and explanations, I will now proceed to overthrow the *baseless* fabrics, to erect which so much labour and ingenuity have been wasted

Those who ascribed the fever in question to jail infection, must have supposed, and ought to have proved, either that the prisoners by whom the contagion was said to have been diffused in the court, were then actually labouring under jail fever, and capable of generating its contagion, or, that this contagion existed so copiously in the place whence they had been brought into the court, (Newgate) as to infect their clothes sufficiently to enable them to infect others at the distance of 25 feet, which seems to have been the space between the Lord Mayor's seat, and the box allotted to the prisoners on trial. (See the plan.) But it is not known that any of the prisoners who were brought into court had any sort of febrile indisposition, nor is it known that any case of jail fever then existed, or had recently existed, even in Newgate. Indeed, the contrary seems to have been generally admitted and believed, and with great reason; because, after so many persons were attacked with fever, in consequence of their attendance at the Old Bailey, on the 26th of April, so much attention was excited and directed to the state of Newgate, that if any thing resembling jail fever had been found in a single case, it would have been reported and made known; instead of which, we only are told that upon an examination,

* It is truly wonderful that Dr Hales, who, on other occasions, and particularly at p. 156, has justly observed, that the "cooler heavier external air," by rushing into a warm room, displaces, and forces upwards the lighter, should here employ so much ingenuity to deceive himself, and reconcile his own facts with Sir John Pringle's suppositions. Let any one pour quicksilver into a tube containing spirit of wine, and see whether the former, when it enters at the top, will remain *uppermost*, and *press down the latter*. His reasoning concerning what happened on the right side of the court, is equally fallacious. He says the air on that side, and especially in the gallery, was "sensibly more offensive ;" and, therefore the "infectious vapour" ought to have been there most abundant ; and yet all the mischief was done on the opposite side.

it was found that the prison, "and the passages leading thence into the court, were in a filthy condition." It has been asserted, however, that though there was then no case of jail fever in Newgate, there might be such a remnant or accumulation of febrile contagion formerly generated there, as to contaminate the clothes of the prisoners, and enable them to infect the court. without being themselves made sick by that concentrated infection, which, even when diluted and diffused, was still powerful enough to cause the deaths of persons at a considerable distance; and such assertions have been adopted and repeated under the sanction even of great medical authorities: and the better to account for this wonderful *immunity* in these distributors of death, it has been pretended, that persons might gradually acquire the habit of bearing *unharmed* the impressions of that contagion, which has proved so destructive to others; and, therefore, that we are not to conclude, that febrile contagion did not exist in a very concentrated and virulent form in Newgate, from the circumstance of its not having occasioned sickness to any person who was confined therein. In my judgment, however, these assertions and reasonings, as applied to the contagion of typhus fever, have no foundation in truth. All our experience proves, that the longer persons are exposed to the action of this contagion, the more, certainly, will they be attacked by the fever: that the escapes of medical men are entirely owing to their remaining but for a short space within its reach at any *one time:* that persons who may resist its impressions for a day, are not likely to resist them for a week; and that those who resist them for a week, will rarely continue to do it for a fortnight:—I mean in places where the contagion is not dispersed and rendered innocuous by frequent changes of the atmosphere. But even if the assertions which I am now controverting were true, they would not answer the purpose for which they were employed, in regard to the events of the Old Bailey sessions. Sir John Pringle, in a note to p. 330, mentions what is indeed generally known, that "it has been the custom some days before every sessions, to remove all the malefactors from other gaols into that of Newgate." And this appears to have been done previously to the sessions in April, 1750; and as the persons thus removed could not have gradually acquired the supposed habit and power of bearing the contagion alleged to have been at that time accumulated in Newgate, they must have been as liable to its morbid influence, as those who were said to have been infected by it at the Old Bailey, and infinitely more so, because they must have been exposed to it at the *source*, and in its most concentrated form; whilst the latter could only have received it at a second hand, when diffused from the clothes of prisoners, and *greatly diluted*. And as none of the persons so brought into Newgate appear to have been attacked with jail fever, either before or after the sessions, there is the strongest reason to believe that *no such contagion at that time existed in Newgate*, &c. consequently that *none could have been carried thence* to infect the court at the Old Bailey.

But if we were to suppose (for the sake of argument) that the jail fever did at that time prevail in Newgate, and to as great an extent as it was ever known to have done, there would still be good reason to conclude that it did not occasion the fever which was consequent upon Capt. Clark's trial.

In a note to p. 438, I found sufficient ground for concluding, that whatever the cause of that fever might be, it must have been contained in and applied by the *stream of cold air* which entered by the open window, and reached the persons who, being placed in that particular direction, were afterwards exclusively attacked with the disease. This was also the avowed opinion of Sir John Pringle and Dr. Hales. But if we suppose the jail infection to have been brought into court by Capt. Clark, (a strange supposition considering his rank in life, and the cleanly *decency at least*, with which he must have been clothed) *that contagion* would have been much more likely to operate previously to the opening of the window, when, as there was little or no circulation of air, it would have been less diluted; but if it had operated at that time, its operation would *not* have been directed and limited to those particular persons on whom the cold easterly wind afterwards blew, but to those who were nearest to him on all sides; and we may therefore presume, that the fever in question was not occasioned by any application of jail infection, previous to the opening of the window, and certainly it *could not have been produced by that cause after the admission of such a strong current of air*, as must by its *quantity and coldness* have so much diluted, elevated, and dispersed the contagion, as to render it harmless, even if, in addition to Captain Clark, *scores* of prisoners had been in court.*

Another insuperable objection to the supposed production of this fever by contagion arises from the space between the bench on which the judges were seated, (where the cause of the disease proved mortal to four out of six then present) and the box wherein the Prisoners are placed when under trial, and which is the nearest approach they are ever allowed to make towards that bench. This distance, as I have already mentioned, and as will appear by the engraved plan of the hall, is about twenty-five feet; and we are warranted by all the experience of modern times in believing, that the contagion of jail or typhus fever, from a person actually under the disease in its worst form, will not produce fever in other persons at the distance of three yards, in a room of moderate dimensions, where the air was not previously infected.†

* Had there been any contagion in Newgate capable of infecting the court at the Old Bailey, we might have justly expected that it would have done so on the first day, when more than twice as many prisoners were tried, and when no window appears to have been opened, to dilute and disperse the infection. It is not, however, pretended that the jail infection operated at any time during the sessions, excepting the day when a stream of cold air was admitted, *sufficient of itself to produce the fever*, without any aid from contagion, and *sufficient also to render contagion harmless, even if it had been present.*

† Dr. Haygarth, in his letter to Dr. Percival, on the Prevention of infectious fevers (Bath, 1801,) says, page 8, "In 1777, I began to ascertain, by *clinical* observations, according to what law the *variolous* infection, and in 1780, according to what law the febrile infection is propagated. I found that the pernicious effects of the variolous miasm were limited to a very narrow sphere. In the open air, and in moderate cases, I discovered that the infectious distance does *not exceed half a yard*. Hence it is probable, that even when the distemper is malignant, the infectious influence extends but a few yards from the poison. I soon, also, discovered that *the contagion of fevers was confined to a much narrower sphere*."

With this knowledge of the very limited action of the contagion of jail or typhus fever, *when present*, and with so many valid reasons for believing that *none was present*, at the sessions, the 26th of April, 1750, we are certainly bound to ascribe the fever which was a consequence of that session, to some other cause; and none presents itself so obviously, and with so many probabilities, as that which Sir J. Pringle thought proper to reject; I mean the sudden admission of a continued stream of cold air, impelled forcibly by the external wind, upon persons who had previously been greatly overheated, and were consequently in that state which renders a sudden and copious application of cold, either externally, as on that occasion, or internally by large draughts of spring or iced water, in very hot weather, or by the eating of iced creams, &c. in particular circumstances, so often productive of *a mortal disease*. Even Dr. Hales was aware of danger from a free admission of fresh air by ventilation, as may be seen at p. 144, 153, and 155 of the work lately quoted, and at the last of these passages, after representing "it as a matter of great importance to use means to change the air in crowded rooms, he adds, that this must be done by a constant *gentle succession* of fresh air;" which "must not be let in at open full windows, especially in *cold* weather;" and this injunction he repeats in other places. Almost all the circumstances which are known to render the application of cold hurtful, seem to have cooperated at the Old Bailey, during Capt. Clark's trial, particularly the length of time in which its application was continued; its being applied by a wind, or *current* of air; its being a transition or sudden and considerable change from heat to cold; and its being applied *partially* to a particular part of the body, while the rest was kept in greater warmth than usual: we know but little of the causes which might have assisted to produce debility on that occasion, and thus to render the impressions from cold more injurious; excepting that of *fasting*, which, from the duration of the trial, must have been unusually prolonged; nor are we acquainted with what might have happened after the court had adjourned, to increase the morbid influence of the cold,

In a report made by the committee of the dispensary at Newcastle, published in Dr. Clark's collection of papers respecting fever wards, (Newcastle, 1802) the following paragraph occurs, at page 12, viz "The most malignant fever does not render the atmosphere infectious farther than a *few feet from the patient, or from the contagion preserved in clothes, furniture*, &c. and *daily observation confirms, that a person must remain a considerable time within the sphere of infection to receive it*; for physicians and surgeons, who avoid the current of the patient's breath, and the effluvia arising from his body *within the bed-curtains*, do not receive the contagion in their ordinary visits; and they never convey it to others; the infectious effluvia, received in their apparel, being speedily rendered innoxious by being diluted with pure air." In a report, also, from the institution for the cure and prevention of contagious fever in the metropolis, May 5th, 1805, it is stated, that "the house occupied for the purposes of the institution stands in the midst of a row, in contact with dwelling houses on both sides; that 420 patients" (with typhus fever) "have been received into it," &c "yet the neighbourhood has continued altogether free from the disease," &c. After which the following observation is added, viz. "Were not the *general prejudice* on this subject strong, this fact, indeed, might have been clearly anticipated. For if, as we have *learnt from experience*, contagions, diluted by the free admission of air, are not communicated from room to room in a house *nor even from bed to bed in the wards of an hospital*, it scarcely required a positive experiment to prove, that houses even in contact, were not liable to infect each other."

which had been already applied. We know enough, however, to make it probable, in the *highest* degree, that this was the cause of the fever which ensued, and proved mortal to so many persons. Sir John Pringle has, indeed, delivered an opposite opinion, but on *grounds* which, in my judgment, have little solidity; he observes, in the note lately quoted, that the window which admitted the cold air, "was at the farthest end of the room from the bench, though the judges suffered most;" it should, however, be remembered, that this window was *much higher* than the heads of any of those who were on the left hand side of the court, and, consequently, that the stream of cold air passed harmlessly over those who were nearest to the window, and, gradually descending by its superior gravity, went uninterruptedly, and with full force, to the judges, (on that side) who being most elevated, were most exposed to its impressions, though *farthest from the window;* a circumstance which Sir John Pringle did not think of any importance, though immediately after he stated it as probable that *the air from the window directed the putrid streams to that part of the court where the judges were seated*." Certainly, if the current from the window was sufficient to convey the supposed putrid or infectious matters to the judges, it must have been sufficient also to communicate the *effects* of its own diminished temperature or *coldness*.

In regard to his other ground, viz. that neither "the kind of fever, nor the mortality attending it," could "be attributed to a cold;" it may be answered that they are much less attributable to jail infection. Unfortunately, we know but little of the "*kind* of fever" then produced; a circumstance for which Sir John Pringle himself is blameable: for, though he appears never to have seen a case of it, he might easily have procured, from other physicians, a *sufficient* account of its symptoms, (which Mr. Foster states to have been much alike in all the patients,) and have enabled us to judge how far they were similar to those which he has described as belonging to jail fever. But without doing this, he admits that "it was said that this fever in the beginning had an *inflammatory* appearance," which is exactly that of a fever from cold, and the very *reverse of a jail fever*. In opposition, however, to this admission, he adds, "that after *large evacuations* the pulse *sunk*, and was not to be raised again by blisters, nor cordials, and the patients soon became delirious." It will not, I presume, be expected, that I should undertake to account for particular effects, loosely stated on the ground of hearsay, without any communication of other facts and circumstances, which, if known, might probably remove all difficulty and obscurity respecting them. I need, therefore, only observe, that I do not consider it as necessary, that fevers produced by the sudden application of cold, should, in all circumstances, ages, and constitutions, bear *large* (probably excessive) evacuations, without a sinking of the pulse, or without subsequent delirium. And in regard to *petechiæ*, which were reported to have been observed in some few cases, I need only refer to p. 97 of the Appendix to Sir John Pringle's work, where he notices, "the *undetermined meaning* of the word Petechiæ;" adding, its "ambiguity is such, that I must regret my having at all used the term." The author had before said, in the preceding page, that even these spots which he had called petechiæ, though sometimes accompanying the jail fever, had no "title to characterise that disorder."

Sir John Pringle also mentions that some escaped without a fever, by a "looseness coming on, which was easily cured." This fact I consider as *eminently indicative of morbid affection from the application of cold*, which is *often* observed to take *that course*; and by doing so, to obviate the occurrence of fever, and other worse consequences: but I have never known such an escape from typhus fever.—On the contrary, I believe that where a sufficient dose of that contagion has been imbibed, the supervention of diarrhœa from any cause, would render its operation in producing fever, more *speedy* and *certain*, by inducing debility.

It is much to be regretted, that neither Sir John Pringle, nor any other writer within my knowledge, has stated the precise interval between the trial of Captain Clark, and the commencement of fever in any one of the persons then present. He only mentions that none were taken into his account who did not sicken within the first fortnight after the sessions. With this latitude, there might have been barely time for the production of fever by jail infection. But, though this was the longest interval, they might, for any thing which he says to the contrary, have all sickened within half the time, and that, in my judgment, would have afforded the most decisive proof of a different febrile cause; and this probably was the fact, at least in some of the cases; though the extract which I have given from Foster's Reports, &c. is almost as vague on this point, as Sir John Pringle's statement; for it only asserts, "that within a week, or ten days at *most*, after the session, the persons in question were seized with a fever," &c.*

In regard to the mortality from this fever, which Sir John Pringle thinks *too great for a fever from cold;* he surely ought to have recollected, that this objection would apply with *greater force* to his supposition, that the fever had *originated from contagion;* for the most concentrated or virulent jail infection ever known in this country, did not, so far as I can recollect, produce a fourth part so many deaths among an equal number of sick; and it therefore must be incredible,

* We are left in equal uncertainty concerning the time which the disease had subsisted even in a single case, before its fatal termination. We are only informed by the London, Gentleman's, and Universal Magazines, for May, 1750, of the days on which most of the persons died, with intimations of their having been at the preceding Old Bailey sessions. The London Magazine mentions Sir Daniel Lambert, and one or two others, as having died on the 13th of May, and other deaths as occurring on nearly all the succeeding days, until the 21st. The Obituary of the Gentleman's Magazine states, "Sir Daniel Lambert, knight and alderman," to have died on the 13th, "of *a violent fever*," which certainly is *not a description of typhus.*

Dr. Adams (misled by Sir John Pringle, in regard to the commencement of the sessions,) states in his Inquiry into the Laws of Epidemics, (p. 12 and 13) that on the 11th of May, 1750, the prisoners at the Old Bailey were sufficiently in health to attend their trials; yet, from the effluvia they brought with them *on the 13th, died one magistrate*, on the 14th the undersheriff, on the 17th one judge," &c. Here Dr. Adams manifestly believed, and intended that others should believe, that the effluvia supposed to have been thus brought into court on the 11th, had not only produced a violent disease, but that this disease had also proved fatal, *all in the space of* 48 *hours* from the time of its communication. A monstrous error: it being probable, that jail infection never produced disease and death in so few weeks as the number of days in which the doctor supposes all this to have happened.

that such unexampled mischief should have been occasioned, where the febrile contagion, supposing it to have existed, *was too weak to produce disease*, even in those who were said to have brought it into court, or *in Newgate*, whence it was said to have been derived. And, though the mortality in question was greater than I should have expected, from a fever produced by the sudden application of cold, yet, so many things are capable of increasing and aggravating the morbid effects of that cause, particularly by inducing local and mortal inflammation in some important organ or viscus, that it is much less surprising that a fever so produced should occasion an unprecedented mortality than it would have been, if so many deaths had resulted from a jail or typhus fever.

I have not insisted on the non-communication of the disease by any of those persons who sickened after being at Captain Clark's trial, because that fact, though it increases the probabilities, does not afford any decisive evidence that the fever was not a typhus, as most of the sick were above the ordinary class, and may be supposed to have occupied apartments so large, that the propagation of the fever was not a necessary consequence.

# APPENDIX.

## No. V.

The author, at p. 119, intimated an intention of inserting in this Appendix certain *extracts* from the "Memoires sur les Hospitaux de Paris, par M. Tenon," respecting the Hotel Dieu of Paris; but the editor *finds it necessary* to refer those to whom these extracts might have proved interesting, to M. Tenon's original work, particularly to the preface, and to pages 135, 138, 141, 163, 165, 194, 197, 198, 199, 207, 208, 209, 223, and 287.

# APPENDIX.

## No. VI.

The author, at p. 151, has referred his readers to this Appendix, "for Proofs" of the "Exemption of the inhabitants of (or adjoining to) Peat Bogs from Intermitting Fevers." The same necessity which compelled the editor to omit what was intended to constitute the preceding Appendix, (i. e. the unusual number of pages to which the volume has already extended) has determined him to refer the reader to Dr. Jameson's Mineralogy of the Scottish Isles, vol. 2, p, 127; and to the Essay on Peat, by the Rev. Dr. Walker, Professor of Natural History, in the University of Edinburgh,—published in the Transactions of the Highland Society of Scotland, vol. 2, p. 23. The author had moreover intended to place in this Appendix certain MS. communications stating the non-occurrence of marsh fevers at Strabane, and some other places in Ireland, adjoining to Peat Bogs, except in persons who had previously been exposed to marsh miasms, by residing in other situations. In such persons, particularly one from the state of Maryland, intermitting fever is stated to have occurred some months after leaving America, and to have proved very obstinate.

# APPENDIX.

## NO. VII.

Having explained the purpose of this Appendix at pages 203 and 204, and meaning to leave no doubt subsisting in the mind of any person qualified to decide on the credibility of Dr. Chisholm's account of the supposed generation of a new Pestilential Fever on board the ship Hankey, on the Coast of Africa, I beg leave to premise a short, but faithful statement of the transactions which are supposed to have been connected with that *monstrous production*, founded principally upon the *African Memoranda* of that very intelligent and meritorious officer, Captain Beaver, of the Royal Navy;* and upon authentic documents, published in Wadstrom's Essay on Colonization.

Several gentlemen having associated for the establishment of a colony at the island of Bulama, near the mouth of Rio Grande, on the Coast of Africa, and obtained the approbation of government, a number of colonists and labourers were engaged, and embarked in two chartered vessels, viz. the Calypso, of 298 tons, and the Hankey, of 260 tons, besides a sloop of 34 tons, purchased by the association; all of which, with sufficient stock of provisions and stores, sailed from Spithead, on the 12th of April, 1792: the Calypso having on board 149 settlers, or colonists, consisting of men, women, and children; the Hankey 120; and the sloop 6; making in the whole 275. These vessels were, however, separated in a few days, by a storm, and the Calypso having touched, on the 3rd of May, at Teneriffe, and on the 12th at Goree, proceeded to Bulama, and on the 25th of that month anchored in a harbour, at the western extremity of the island, where the governor of the new colony, Mr. Dalrymple, determined to wait for the arrival of the Hankey, on board of which was the investment for purchasing the island, and trading with the natives. But in the mean time, he imprudently landed, with some others, to explore the island; while, as Captain Beaver informs us, (at page 46 of his African Memoranda,) several of the colonists erected small huts and tents on the shore; parties wandered wherever they pleased in the day, and returned to the ship, or not, as they thought proper, in the evening. In short, nothing," adds he, " could be more irregular, or improper than their conduct. In this disorderly state, they were on the 3rd of June surprised and attacked by a party of Bijugas, a warlike tribe of Africans, inhabiting the neighbouring island of *Canabac*, (to whom Bulama then belonged) who killed five men and one woman, wounded four men, and carried off four women and three children. This disaster, which had resulted from a mistaken notion in the natives, that the colonists had come to dispossess them by force of their territory, caused so much alarm among those who had escaped, that they re-embarked immediately, and sailed on the 5th for Bissao, a Portuguese settlement on the coast, at a small distance. But on their way thither, finding the Hankey was just arrived in the Bijuga channels, they joined the latter ship, and on the 8th of June, both ships, with the sloop, proceeded to the road of Bissao, where they anchored. By this time, as Captain Beaver informs us, (p. 58. " the Calypso had many persons on board ill of *fever*, though none had yet died of it;" besides which, he adds, " nothing was heard but mutual reproaches from the people of the Calypso. The colonists accused the members of the council in that ship, of a want of attention to their comfort and accommodation: (those in the Hankey having fared better than themselves, by procuring supplies of fresh provisions at Teneriffe and St. Jago.) They were tired out with the length of the voyage; irritated by the loss of their

* At p. 94 of his Letter to Dr Haygarth, Dr. Chisholm, after mentioning Captain Beaver's publication, describes it as " a most complete and circumstantial narrative of the whole of their proceedings;" (those of the Hankey and the Bulama colonists,) and as executed " with a candour and naiveté, which stamps it with the seal of truth."

friends, in the recent attack of the natives, and the disappointment of their hopes. On the junction of the ships, these discontents were speedily communicated to the colonists on board the Hankey: Captain Beaver, who had made his passage in that ship, says, that he had "left her on the 5th of June, a quiet, clean, healthy, and orderly ship;" but at his return, on the 7th, he found her "a noisy, dirty, disorderly ship; the colonists dissatisfied and dispirited."

During their stay at Bissao, Captain Beaver mentions the fever to have appeared on board the Hankey; and that on the 21st of June it "still continued in both ships, but only one person had yet fallen a victim to it in the Calypso, and none in the Hankey." (Page 60.) Soon after the arrival of the ships at that settlement, agents were sent to the island of Canabac for the purpose of ransoming the women and children taken prisoners at Bulama, which was accomplished on the 19th of June; and the willingness of the Bijugas to sell the latter island having been ascertained, the ships quitted Bissao on the 21st of June, and anchored on the 27th in a fine harbour, in the East Channel, between Bulama and the Biafara shore, opposite to the spot on which the settlement was subsequently fixed; having previously dispatched Captain Beaver with Mr. Dobbin to Canabac, where they speedily concluded a treaty, by which the island of Bulama was purchased for 473 bars, worth from 120 to 140 pounds sterling. In the mean time, however, the discontents had increased among the colonists; "the major part of whom," Captain Beaver describes (in a letter addressed to the Trustees of the Bulama Association in London, dated from Bulama, the 22d of November, 1792, and printed at page 300 of the second part of Wadstrom's Essay on Colonization,) as being "drunken, lazy, dishonest, and impatient;" and the setting in of the rainy season, which began on the 4th of June, with the prospect of great fatigues and privations to be undergone during that season, had probably contributed not a little thereto. In this state of mind, a very considerable number of them resolved to abandon the colony, notwithstanding the success of the negotiation with the Bijugas. Accordingly, sixteen of them separated almost immediately; and others, to the number of 147, (among whom were the governor, Mr. Dalrymple, and the lieutenant-governor, Mr. Young,) sailed from the Bulama in the Calypso, on the 19th of July, for Sierra Leone, and thence to London, where they arrived in the middle of November, excepting about 40 who died from sickness. The state of both ships, in regard to health on the day of the Calypso's departure from Bulama, is thus mentioned by Captain Beaver, at p. 80 of African Memoranda;—"19th of July. The fever had hitherto continued in both ships; the Calypso having buried three who had died of it since we had left Bissao, and sailed with many sick. The Hankey had buried three also, and had now two colonists, and three of the ship's company labouring under that disorder."

Upon the departure of the Calypso, with governor Dalrymple, &c. the remaining colonists, consisting of 48 men, 13 women, and 25 children, elected captain Beaver to be governor, and his conduct, amidst the numerous difficulties and distresses by which he was continually surrounded, until compelled to abandon the island on the 29th of November, in the following year, appears to have been in all respects highly commendable. The first and principal undertaking of the new colonists, was that of building a block-house for their protection and accommodation, upon the summit of a hill near the harbour, which was then "covered with a thick forest." See p. 93. But, having no sort of accommodation on shore, they were constrained to live on board the Hankey; and the better to defend themselves from the rains, &c. they erected a covering to the ship's deck. Their only shelter, on the island, was "a little tool-house," and as the heavy rains made it impossible to dress their victuals on shore, the working people were landed on the island at day-light every morning, and brought on board to breakfast, at 8 o'clock; carried back to their work at nine; brought on board to dine at noon; again relanded at 2, P. M. to resume their labours; and, finally, brought on board to sleep at sun-set. In this manner they continued to work through the rainy season, (which ended about the 15th of October,) and afterwards until the beginning of November; when the time being expired for which the Hankey had been chartered, it became necessary to land the colonists with all their stores, to enable the ship to prepare for her departure, although the block-house could not be finished until the month of February.

On the 23d of November, the Hankey sailed from Bulama, having on board sixteen of the colonists; viz. 7 men, 4 women, and 5 children, besides her crew. At this time the colony had sustained a loss, subsequently to the Calypso's departure, of 48 persons by death, of whom 23 are stated to have died of fever; one by "fever and flux," an 14 by other specified diseases, hurts, and accidents. On the 26th of November, the Hankey anchored at Bissao, and sailed thence for England on the 3d of December, but in the night of the 4th she grounded on a sand bank near the island of Warang, which made it necessary to send a part of her crew in the pinance (an open boat) to Bissao for assistance; and these men, after hard rowing for two days and nights, returned with a schooner and long boat; and with their help the ship was again made to float: but on this day "all the people who came from Bissao in the pinance were taken ill." On the 13th the Hankey was brought back to Bissao, to refit, but *finally* sailed thence on the 21st of December, and anchored in St. Francis's Bay, at St. Jago, on the 26th of that month; where I shall leave her at present, that I may enter upon an inquiry into the causes and nature of the fever which had been so prevalent, while she remained at Bulama;—an inquiry which, as marsh fevers *alone* have hitherto been found to prevail in that part of Africa, nothing but Dr. Chisholm's confident, though unwarrantable statements, could have made necessary.

It has been already mentioned that the Calypso had arrived at Bulama fourteen days before she rejoined the Hankey in the Bijuga channels in which interval, the colonists (tired of their confinement on shipboard, and believing they had now reached the land of promise, had wandered about that island in the most unrestrained and imprudent manner, sometimes sleeping on shore, and otherwise exposing themselves most incautiously to the action of marsh miasmata,* until they were surprised, and partly cut off, by the Bijugas. Those who have lived between the tropics, or who have read of the numerous instances of mortal fevers which have been there produced by sleeping a single night on shore, will not be surprized that after such conduct, it should have been found, as is mentioned by Capt. Beaver, that on the 8th of June "the Calypso had many persons ill of fever :" nor that this fever should have been, as will be incontestably proved, the common marsh, intermitting or remitting fever of the *coast*, and that the same fever should have appeared in the Hankey some time after her arrival at *Bissao ;* a place where this "*coast fever*" is known to prevail in the rainy seasons, (which had then recently begun) and where during

* Dr. Chisholm, at p. 103 of his letter to D. Haygarth, says, "In every instance of great mortality on the coast of Guinea, and in many other countries of the old and new continents within the tropics, (and there are *many dreadful ones given by Dr. Lind and others*) the cause has evidently been *marsh miasmata*, or an "inland impure atmosphere, loaded with stinking sulphureous mists." Yet, in direct opposition to this assertion, he immediately after, endeavors to maintain that the mortality on board the Calypso and Hankey did not result from this cause, but from one which certainly never did produce "great mortality" between the tropics ; I mean febrile contagion ; and for this purpose, he alleges that Bulama, "is every where surrounded by sea, is no where marshy, gradually rises to a moderate elevation immediately from its shores, is blessed with abundance of running water, and with a soil *rich* and *prolific*, affording ample pasturage to innumerable wild animals." Had it however suited Dr. Chisholm's purpose, I am persuaded that, even supposing this description to be accurate, (which I do not believe) he would, notwithstanding, have readily discovered sufficient sources of marsh miasms at Bulama. Its being surrounded by sea is no obstacle to their production ; witness the islands of St. Thomas, Batavia, St Lucia, St. Domingo, and scores of others, which are extremely unhealthy ; and in regard to its "*rich* and *prolific*" soil, Bulama, in this respect, only resembles the country along the banks of the *Guadalquiver*, in Andalusia, the *Terra di Lovoro*, near Naples, and other places already mentioned, as being eminently productive of marsh fevers. Governor Dalrymple, and the council, in their joint letter to the trustees of the Bulama association, say, "the island, we learn from the gentlemen who have explored it, has extensive savannahs, of *a deep black mould*." See Wadstrom's Essay on Colonization, 2d part, p. 144. And governor Dalrymple, in a separate letter, quoted in the preceding page, says, "the north end of the island, is one continued savannah, covered with *long grass*, with a few trees interspersed, but without any rocks or stones." He adds, "the soil of this plain is *deep* and *rich*" Now, it is utterly impossible, that such a soil, in such circumstances, should not be greatly productive of morbific exhalations, in that climate, whenever (by showers or otherwise) there is sufficient moisture for their extrication, and besides these savannahs, it is incredible that the shores of such an island, and especially its harbours, should not in a multitude of places be fitted to produce and give out marsh effluvia copiously : there not being, so far as I can discover, one situation in that part of Africa where a ship can anchor near the shore and remain even for a week, during the rainy season, without some of her crew being soon after attacked by marsh fever. Such, however, was Dr. Chisholm's anxiety to persuade others that in this respect, Bulama differed from every part of the coast, that (at p. 118 of vol. i. of his Essay) he has introduced a quotation from the "Voyage de Chevalier des Marchais (en Guinée and isles voisines)" which, besides its being otherwise of little importance, relates entirely to the islands in the river of Sierra Leone, and not in the smallest degree to Bulama, which is in the Rio Grande, and distant more than 200 miles. He makes a similar mistake by quoting Dr. Lind, who has not once mentioned the islands in Rio Grande. I may add that Dr. Chisholm's anxiety on this subject has also led him to adduce the testimony of Mr Paiba, (which admitting it to be correct as far as his knowledge extended, amounts to very little) though in many other parts of his writings Dr. Chisholm earnestly endeavors to *impeach* Mr. Paiba's *veracity*, and as I think, with very little reason ; for though his communications to Dr. Smith, were not all correct, in regard to dates and numbers, (he relating them from memory only, and after an interval of four years) his statements in other respects have been generally confirmed by other evidence, and he has no where made such *groundless* misrepresentations, as those of Dr. Chisholm, nor indeed any which manifest an intention to mislead.

her stay of 14 days, it may be presumed that the drunken, dissipated, and vicious characters mentioned by Captain Beaver, as being a majority of the colonists, would have freely indulged their dissolute propensities,* in aid of the morbid influence of mash miasms; but though these vessels afterwards proceeded to the harbour, adjoining to the spot destined for the settlement at Bulama; yet by reason of the intended departure of the adventurers, very little work was done on shore during this interval; and the fever, *being void of* contagion, did not spread in the ships; as only five persons were ill of it on board the Hankey, when the Calypso quitted Bulama on the 19th July. After this time, however, all the remaining colonists, able to work, were *constantly* employed in laborious occupations on shore, for 9 hours daily, and were often caught, as is mentioned by Capt. Beaver, in very heavy showers of rain, than which few things can be more dangerous in that climate; and they must also have been greatly exposed to the marsh miasmata constantly emitted in that rainy and sickly season. These causes, joined to their general despondency, and various hardships, produced, as might well be expected, such an increase of sickness, that, as Captain Beaver mentions at p. 137, "every person employed about the block-house was ill and unable to work" on the 17th of September.

Dr. Chisholm has, in various places, stated his opinions of the causes of this fever, though with some variety, and often with no little obscurity: that statement, however, which seems to have obtained his last correction, may be found in his letter to Dr. Haygarth, in which, at p. 103, after asking, "what were the causes of this fever and mortality," he says, "*all* the causes which generate infectious or pestilential fever, is the obvious answer." Now, though this answer may satisfy some of those who believe that there are such causes, it is far from satisfying me, who (for many reasons already stated) have no such belief; especially, as though assuming the presence of *all* these supposed causes, he has omitted to designate, and establish the existence of any one of them. He has, indeed, attempted to prove that the fever in question was infectious, and for this purpose (at p. 99 of the letter just quoted) has availed himself of a passage in captain Beaver's African Memoranda, (p. 54) where the latter, describing the transactions of the Hankey and Calypso, after their rejunction on the 7th of June, says, "the fever from which the Hankey was still free, had already made its appearance in the former ship; and, instead of separating the infected from the well, and taking any steps to prevent the spreading of that dangerous disease, by prohibiting any unnecessary intercourse between the two ships, the whole time, since the arrival of the Calypso, had been taken up in the constant interchange of visits: nay, the affected themselves, the very persons who *had the fe-*

* Wadstrom, page 311, gives the following extract of a letter from Charles Drake, Esq. "We left the remains of several of our people at Balama; but I know of none whose decease might not be accounted for, by *their being addicted to drink rum.*"

Id page 113, Lieut. governor Young, in his "Return of the deaths of the Bulama Adventurers," states that, "of the nine persons who died at Bulama, not one contracted his fever there, *but all of them at Bissao*, except those who brought their disease from England. Of the remaining number, many caught the fever at Sierra Leone, through intemperance," &c.

*ver on them at the time*, had been *actually on board the Hankey*,* and the consequence was, that many days did not elapse before the fever made its appearance in that ship also." That captain Beaver, when he wrote this passage, (which probably was about the time when the transactions happened,†) did suppose *the fever* in question to have been communicated from the Calypso to the Hankey, is evident. He seems, however, to have thought so, from an erroneous notion (which has prevailed, in a great degree, even among medical men, and which captain Beaver might well have adopted) that all fevers were contagious, and that, *as a matter of course*, such a communication of fever must have resulted from the intercourse just described. But there is good reason to believe, that he did not long retain this notion, but was soon convinced, by *personal observation*, that contagion had no influence in spreading the fever, (especially as, even on the 19th of July, only two of the Hankey's crew, and three colonists, had any fever,) or occasioning the mortality which resulted from it; for he does not afterwards (according to my best recollection and belief,) even *hint* at the operation or existence of any such cause; but wholly *omits it*, in the various parts of his journal and official letters, in which he endeavours to account for the sickness and deaths, either of the colonists or of the Hankey's crew;‡ nor did he, when he afterwards be-

* This fact indicates, what will soon be proved, that the fever in question must have been a *mild* marsh fever, probably an *intermittent*, for under such contagious fevers as Dr. Chisholm supposes the Hankey to have drought to Granada, patients do not pass their time in going from one ship to another, to make or return visits.

† Capt. Beaver, at p. v. of his preface, says, " most of the circumstances *preceding the 19th of* July were written by myself, as the circumstances occurred;" and he professes generally to have published his journal just as he wrote it, in order to exhibit the impressions on his mind, at the moment when each part was written.

‡ To establish the truth of this allegation, the following proofs will suffice, viz. in a letter from Capt. Beaver to the Bulama trustees, dated November 22d, 1792, (see African Memoranda, p. 292) he writes, " *the great mortality must certainly be attributed to the great labour and fatigue* attendant on those who first attempted to settle a colony, *and to the necessity we were reduced to of working in the rains*, in order to have a fort to defend, and a house to cover us. With little strength, we found it necessary to work from morn to night, except when the *rains poured like torrents*, and by these we were *often caught* when going in the boats either on board, or on shore." Afterwards, he observes, (African Memorada, p. 495,) " had we carried out the frame and materials necessary for the erection of a large house, it might have been finished, in at most one month; but " as *all* the timber, which I *built* with, *was growing* at the time of our arrival, it was February in the following year, before I had a room to put my head in. The being exposed during the whole of that time to either the *rains* or the *sun* must certainly have been a great cause of our mortality." He had before observed, (African Memoranda, p. 367) that the mortality of the colonists at Bulama, "though in *some measure* certainly *to be attributed to the climate*, was much more to the adventitious circumstances which have been already noticed; and I am inclined (adds he) to think that, independent of its having really *been the most unhealthy season* of the year, independent of our hard labour, and great exposure during that inclement season, *much of our very great mortality may be attributed to the uncommon depression of spirits* which our situation produced, on the minds of most of our colonists; and I verily believe that I should have died too, if I had ever suffered my mind to be so subdued. But how far this despondency may have contributed to our mortality, must be left to the decision of physicians." Upon this passage, Dr. Chisholm (at p. 105 of his letter) says, "Were I to hazard an opinion, I should be inclined to say that it contributed as a powerful *predisposing* cause to the action of *infection*, which had already *accumulated in their bodies*, like the *electric fluid* in the Leyden phial, and required only this excitement to *destroy* at a *single* discharge." But surely Dr. Chisholm, before he ventured upon this extravagant and most inapplicable comparison, before he assumed this *excessive accumulation* of some undescribed febrile contagion,

came governor of the colony, (if it may be so called) adopt, in the smallest degree, any of those measures, for "separating the infected from the well," and for "preventing the spreading" of the fever, which he had at first supposed to be of high importance; and which he certainly would not have neglected, with so much humanity and vigilance as he has manifested on all occasions, had he still retained his former opinion. Capt. Beaver was, in truth, so far from believing Dr. Chisholm's allegations respecting the Hankey, that he has, on various occasions, given those allegations the most decisive contradiction: witness among others, the following passage, at p. 305 of his African Memoranda, viz. "the mortality which took place in the island of Bulama, and on board the Hankey, after her departure from it, was in this country called the plague, or Bulama fever, by those who were inimical to the success of our enterprise; and such serious representations were made on the subject, as produced an order from the privy council to sink that ship, though on further inquiry it was not carried into

should at least have proved the existence of a *few particles of it*, either in the Calypso and Hankey or upon the island of Bulama. In regard to the former, Dr. Chisholm has admitted (what might otherwise be readily proved) that when these ships sailed, and during the voyage out, the crews and settlers were all healthy;" and that "no suspicion whatever can be entertained of the existence of latent infection among them." See his Essay on the malignant pestilential fever, &c. vol. 1. p. 103, et seq. It follows, therefore, that if the fever in question was produced by contagion, generated in either of these ships, it must have been after her arrival on the coast of Africa, and that the *guilty* ship must have been the *Calypso*, since, according to Dr. Chisholm's *last new opinion* (adopted to take advantage of Captain Beaver's error) the infection was *communicated from her to the Hankey*. But I have already, in different places, adduced so many facts decisively proving that filth, crowding, and all the other causes supposed to be capable of generating febrile contagion, do not produce that effect, when in cold or temperate climates, and in circumstances which ought to render those causes *highly prolific*, if they were capable of such generation, that I shall not be here expected to enter upon a particular refutation of Dr. Chisholm's unsupported and most improbable suppositions, respecting the Calypso and Hankey; especially as it has been already proved that the high temperature between the tropics, is so unfavourable, I need not say to the generation, but to the *existence*, of febrile contagion, that even when it happens to be brought into that *temperature* it cannot *subsist, much less propagate itself.* Dr Chisholm's inconsistencies on this subject are truly ridiculous. In his Essay, at p. 95 of vol. 1. he asserts, that "the state of the atmosphere between the tropics does *not* seem to *admit the generation of a high degree of infection*," and at p. 189 of his letter to Dr. Haygarth he says, "one universally admitted fact, I mean in the West Indies, incontestably proves that heat and filth do not render shipping "the most dangerous of all human habitations." The fact (adds he) is this, that the shipping frequenting the different ports of the West India islands before 1793, were almost uniformly and remarkably healthy" Yet, in the very same letter, he twice adopts and repeats what he had previously declared in his letter to Dr E. H. Smith, that "a fever (on board the Hankey) proceeding originally, perhaps, from the inclemency of the season, and the circumstances of the situation of the adventurers, had become, by confinement, filth, consequent impurity of air, and depression of spirits, a true *jail fever*, or *fever of infection heightened* to almost *pestilential* violence." *A jail fever between the tropics ! ! !* and a jail fever *heightened to almost pestilential violence*," in an atmosphere which he had before mentioned as *not admitting* "the generation *of a high degree of infection ! !*"—And least these contradictions should be thought too few, he tells us, at p. 217 and 218 of the very same letter to Haygarth, that (instead of jail fever) the disease imported by the Hankey to Grenada was a "nova pestis, *a peculiar, original foreign pestilence, recently generated, and utterly unknown before*," &c. (*see p.* 201 *of this volume*) Had he told us that the supposed causes of this new plague, had *generated Elephants* at Bulama, I should have thought the *tale* less improbable, because these animals have been frequently seen on that island, while neither plague nor pestilential fever ever was nor as I believe ever will be.

effect; and the ship was restored to the owners, after their having sustained very considerable loss, by the *industry* with which certain interested people kept up the report of the malignity of the distemper, which it was said that ship brought home, and *for which there was not the shadow of a foundation.*" Again, in a note to p. 192, after noticing the report of the pestilential fever or plague, supposed to have been carried by the Hankey "from Bulama to Grenada," captain Beaver adds, "this report was for a considerable time believed; the Hankey was sent to Stangate Creek to perform quarantine, and orders were given for sinking the ship and cargo; however, on examination, *the falsehood* and *malignity* of this report being *proved*, this order was confined to the Bulama baggage only."

In addition to these facts I shall now adduce the most decisive evidence, to prove that the fever which prevailed in the Calypso and Hankey, and among the adventurers to Bulama, was the common marsh fever of the western coast of Africa; and for this purpose I shall first quote the testimony of Lieutenant-Governor Young, who, in his "*return*" of the deaths of the Bulama adventurers, and in allusion to the fever, which produced so many of them, observes concerning it, that "*the coast fever is* of the *intermitting kind* and *not infectious.*" [See Wadstrom, page 313.] This gentleman's competency to form a judgment on the subject will scarcely be doubted, after reading the following character of him by Capt. Beaver, at p. 82 of African Memoranda: "Young, next to the governor in the council, was a man in mind and information inferior to none I have ever had the happiness to know. I respected, I loved him: and never was in his company without leaving it both wiser and better, from his knowledge and virtues:" and he must have had very sufficient opportunities of observing the intermitting nature of the fever, and its non-infectious quality, both at Bulama and on his passage in the Calypso to England, during which about forty of the persons on board are stated to have died, mostly of this disorder. Mr. Paiba's testimony on this subject, as communicated to Dr. E. H. Smith, and published in the New-York Medical Repository, vol. 1, p. 463, is in these words: "Concerning the sickness which carried off the colonists, both at Bulama and Sierre Leone, and on the home passage of the Calypso, it may be remarked, once for all, that it was by no means of one kind, as the readers of Dr. Chisholm would be led to suppose. Few, if any, escaped, altogether, some had regular intermittent fever, (which is the *fever of the coast,*) of various continuance, from a week to several months; others had a violent fever, which terminated favourably or fatally, in one, two, three, four, five, or six days; or which lingered out after its first violence as many weeks;* some had diarrhœa,

* The "*violent* fever" here mentioned may, in many cases, have been a marsh fever, *aggravated* by some other of the well known causes of fever, such as being caught by heavy rains, drunkenness, fatiguing exercise, or labour in the sun, &c. and in a few it may have been produced by *these latter causes* only, though it seems difficult to believe that any person, who had been even but a few weeks on the island, could have so far escaped the influence of marsh miasms, as that a fever in him should not, in some degree have resulted from their influence.

and dysentery; and others fell martyrs to the indiscreet use of opium and spirits, as preservatives."

But to render superfluous all other evidence on this subject, I will here adduce that of Dr. Winterbottom, who, by appointment of the Sierra Leone company, was physician to that colony, when the Calypso arrived there, with Governor Dalrymple, and the discontentd colonists from Bulama; and consequently had abundant opportunity of becoming well acquainted with the fever which prevailed among them, and which was supposed to have *been communicated from that ship to the Hankey*,† and undoubtedly the testimony of a physician so impartial, and so respectable by his character, and his general as well as professional knowledge will be deemed conclusive on this subject. It was given at p. 16 of the 2d volume of his account of the native Africans, where he says, "the fever which carried off so many of the settlers at Bulama, *precisely resembled the endemial remittent fever of Sierra Leone*, a sketch of which, at some future opportunity, may perhaps be laid before the public; but the fever described by Dr. Chisholm" (meaning the malignant pestilential fever, so called by him, and supposed to have been brought by the Hankey to Grenada) "differs so essentially from that which occurred at Sierra Leone, that it cannot be recognized as the same disease." Afterwards, in the same page, Dr. Winterbottom corrects one of Dr. Chisholm's errors in regard to the Hankey, by stating that she "had no communication whatever with Sierra Leone; adding, "the other vessel, the Calypso, after leaving Bulama, called for refreshment at Sierra Leone, where she remained about *six weeks*, during which time upwards of *forty* of the crew and passengers *died of the remittent fever*, though *unattended with any appearance of peculiar malignity*." Against such testimony, the unsupported assertions and suppositions of Dr. Chisholm, who never was on the coast of Africa, nor personally acquainted with any of the facts in question, must be of *no value*.

I might here, therefore, dismiss this part of my discussion, did not a regard for truth compel me to notice Capt. Beaver's account of his own fever, in order to expose and correct a very important, and seemingly a very culpable, misrepresentation, which Dr. Chisholm has made concerning it. This account may be found at p. 161 of his African Memoranda in these words: "The letter which I wrote to the trustees by the Hankey was, I think, dated Nov. 23, 1792. It was written during that and the two preceding days, in those intervals when I had the full possession of my senses, and was able to apply myself for a short time to writing, for long I could not; and each of the first two of those days, as well as for several before, I was *delirious*, generally from about 10 A. M. till 2 or 3 P. M. and this was the case in almost all the severe attacks of the fever which I afterwards had. This was owing to the excessive heat between those hours; for I invariably got better as the sun declined, and never experienced the violent raging of the fever till the sun had again acquired power on the follow-

† Though the fever was not communicated by one ship to the other, it was manifestly the same in both, and derived from one common source—marsh effluvia.

ing day." Page 161, African Memoranda. See likewise his other description, at page 173—4, and at page 114—121;* from all which his disorder appears to have been one of those *marsh*, or intermitting fevers, the paroxysms of which are often accompanied by delirium, at least between the tropics; and it could not, therefore, possess any contagious property, even in Dr. Chisholm's opinion, as delivered in various parts of his writings; and particularly in the first volume of his Essay, &c. p. 299, in these words, viz. "The true uncombined *yellow remitting fever*, deriving its origin from the *miasmata of marshes*, and the various exhalations from putrid vegetable substances, confined humidity, and stagnant water, is *not contagious;* nor can it be proved to be so in any instance, of *Torrid Zone at least*." But in direct opposition to this opinion, he makes the following unaccountable statement, at p. 102 of his letter to Dr. Haygarth, viz. "The fever" (at Bulama) "appears to have been *direfully contagious;* for in one instance, particularly, *three* persons who were near his" (captain Beaver's) "bed, during his own illness, from which he almost miraculously recovered, *received the infection from his person, and died soon after*." And for proofs in support of this allegation, Dr. Chisholm refers to pages 171 and 172 of the African Memoranda, where indeed, mention is made of captain Beaver's illness, and of the deaths of three persons in two succeeding days, but without any fact, or circumstance, indicative of the existence of any thing like contagion, or of any connexion between captain Beaver's disorder and the deaths in question. The only passages relating to this allegation, which occur in the pages referred to by Dr. Chisholm, are the following, viz.

"Thursday, December 13, 1792. "Very ill; delirious part of the day. In the evening, after having somewhat recovered my deranged senses, sent for Messrs. Fielder and Hood, the only subscribers able to move; made my will, and gave them advice how to act in case of my death."

"Friday, 14. Died of a fever, and were buried, both Mr. and Mrs. Freeman. This couple I married on the 4th of last month. They were both taken ill, about *ten minutes after the ceremony* was performed, *and have been so ever since.* They both died this morning, within ten minutes of each other, and were both buried in the same grave. Myself a great deal better in the morning, but delirious great part of the afternoon."

"Saturday, 15th. Died and was buried this evening. Mr. Fielder. This is the man who, two days ago, made my will, and whom I thought likely to be my successor. He was young and brave—fit to

* Capt. Beaver, at p. 354, states himself to have "had *seven separate attacks* of the fever;" a circumstance which even if most of them should be considered merely as relapses, must render it very unlike Dr. Chisholm's malignant pestilential fever, which he represents as derived from the same source, or contagion, as that which he supposes to have produced the fever of Captain Beaver; for the doctor asserts, that, "by a general law of the peculiar contagion" of his pestilential fever, "those once attacked and recovered are exempted from being affected by it." See letter to Dr. Haygarth, p. 180 My readers will not, I hope, conclude from the mention which I sometimes make of this supposed "malignant pestilential fever," that I believe such a fever to have ever existed, or that the fever so called was any thing else than a modification, or variety, at most, of the common *non-contagious* yellow fever.

draw a lion's tooth." These being the only deaths to which Dr. Chisholm's reference can be applied, or *extended*, we must necessarily conclude that these were the three persons in his contemplation, when, to prove that captain Beaver's fever was *direfully* contagious, he ventured to assert, that in one particular instance, (leaving us to suppose many others probable,) three persons, who had been near captain Beaver's bed, &c. received the infection from his person, and died soon after." Now, from the antecedent parts of captain Beaver's journal, it appears that the fever, under which he laboured on the 13th of December, 1792, had only commenced on the 9th of that month, 34 days after Mr. and Mrs. Freeman were attacked with that fever which occasioned, and lasted until their deaths. It is true, indeed, that captain Beaver had been attacked by fever some weeks before, but this was not until five days after the illness of Mr. and Mrs. Freeman had commenced, i. e. until the 9th of November; and as previous to that day, captain Beaver had been well for about six weeks, Dr. Chisholm could not, with any degree of truth, pretend that they had received febrile *infection from his person*, and died *soon* after; especially as their deaths only happened after forty days; a protraction of disease, which I am persuaded Dr. Chisholm has never mentioned, as occurring in his malignant pestilential fever.

In regard to Mr. Fielder, no mention is made in the pages referred to by Dr. Chisholm; nor, so far as I can recollect, in any other, of the disorder which caused his death; nor are we entitled from any thing within my knowledge, to conclude that he died of fever. But if this did cause his death, it could not have been produced by contagion from captain Beaver's person, at the time of making his will; because even if his Beaver's) fever had been contagious, we have the strongest reason to believe, that no febrile contagion has ever produced disease and death within forty-eight hours from the time of its being received. There is, therefore, nothing which I can discover to *excuse*, and much *less to justify*, Dr. Chisholm in the serious *liberty* which he appears to have taken with the *truth*, on this interesting subject; a liberty which is the more extraordinary, because as the facts which have been so strangely misrepresented, were distinctly and permanently stated in print, there appears to have been no room for any mistake; and, unfortunately for Dr. Chisholm, the impressions resulting from this incident will not be weakened by others, which must hereafter fall under our notice.

After this digression, it will be proper for us to return to those events which more immediately relate to the crew of the Hankey, of whom it has been already mentioned that *three* were ill of fever, when the Calypso sailed on the 19th of July. But during the subsequent part of the rainy season, they remained comparatively healthy; probably because they were much less exposed than the settlers to the causes of *fever on shore*. For if the fever had been contagious, as Dr. Chisholm pretends, the seamen, by remaining *constantly on board with the sick*, would probably have been infected sooner, and in greater numbers, than the colonists who were labouring on shore. But when the Hankey began to prepare for her return to Europe, and her captain, (as is stated in captain Beaver's Journal, p. 135,) "wanting ballast, and being unable to procure any of *stone*, determined to ballast her with wood;"—"all the men he could spare" were sent "on shore to cut it;" which is notoriously a most *dangerous* employment between the tropics. It is also mentioned, in captain Beaver's journal, under the date of October 31, that "all this day, by permission of captain Cox, the Hankey's crew have been stowing away our goods in the store-room" on shore. And they are stated to have been employed in this service on the three following days. Again captain Beaver states, "4th of November.—Four of the sailors came on shore to *cut logs for us*;" i. e the settlers,—see p. 159. And besides these, they were sent upon another dangerous employment, that of watering the ship; by all which they must in that situation have imbibed marsh miasms, sufficient to account for their subsequent sickness, aided by other manifest causes. After leaving Bulama on the 23d of November, the Hankey went to the unwholesome settlement at Bissao, and remained there for a week, during which, it may be presumed, that the sailors run into their customary irregularities; and with these, added to the other causes of disease, it cannot be thought extraordinary, that "three of the crew were taken ill of fever," on the 3d of December, when the ship sailed from Bissao. Her getting aground, and the other events which followed until she reached St. Jago, have been already mentioned. On this subject, captain Beaver has made the following observations, at p. 192 of African Memoranda, viz.—"When the Hankey left Bulama, *not one* of her crew *had been buried*, although so many of the colonists had; however, a few days afterwards she became very sickly: and this was most likely increased by the extraordinary *labour*, consequent on the ship's running a-ground on the 4th of December, in the Bijuga channel; in which situation she remained until the 9th; and the boat having been sent about 90 miles to Bissao for assistance, I find noted in the Hankey's Log-Book, on the day of her return, which was the 8th, "all the people which came from Bissao in the pinnace taken ill." He adds, "this was, in all probability, owing to their great *fatigue*, and *exposure to the sun in the day*, and *the dews in the night*." Here it will be observed, that though Dr. Chisholm would have us believe that all the sickness and mortality which occurred on board the Hankey about this, as well as at other times, were produced by that "*direful*" contagion, which was *imagined* by him, Captain Beaver had entirely discarded all belief of it, and has ascribed events to their usual and natural causes, upon this, as well as former occasions.

I have already mentioned the Hankey's arrival in the Bay of St, Francis, at St. Jago, on the 26th of December, whence she removed, and came to anchor at Port Praya, in the same island, on the 4th of January, 1793; having then lost by deaths, subsequently to her departure from Bulama, eight of her crew, with five men, three women, and two children, colonists, who had been taken on board, I believe, all in a sickly condition.*

* Mr. Paiba states, that all the colonists who embarked at Bulama, to return in the Hankey, excepting himself, Mrs Paiba, and another woman, were *unwell*; as might indeed be supposed from preceding events. That "before the Hankey put to sea, all the bedding of the sick was thrown overboard, or destroyed; and the ship was washed

When the Hankey had been a week at Port Praya, the Charon, ship of war, Commodore Dodd, arrived there from England, an event which was made the foundation of a most extraordinary misrepresentation by Dr. Chisholm, in his first publication, respecting what he called the "Malignant Pestilential Fever," "*from Boullam*," &c. The following is Dr. Chisholm's statement, (p. 87) viz.—

"With much difficulty they (the Hankey's people,) arrived at St. Jago, where they fortunately found the Charon and Scorpion, ships of war.* Captain Dodd, of the former, humanely rendered them every service in his power; and on leaving them, put two men of each ship on board the Hankey. With this aid, they proceeded to the West Indies; a voyage to England being impracticable in their wretched state. On the third day after leaving St. Jago, the men they procured from the ships of war, were seized with the fever, which had carried off three fourths of those on board the Hankey, at Boullam; and having no assistance, two of the four died: the remaining two were put on shore *here*, in the most wretched state possible. Captain Dodd, on his arrival at Barbadoes, from the coast of Africa, was ordered by Admiral Gardner to convoy the homeward-bound fleet of merchantmen. In the execution of his orders, he came to Grenada on the 27th of May, and hearing of the mischief which the Hankey had been the cause of, mentioned that several of the Charon's and Scorpion's people were sent on board the Hankey at St. Jago, to repair her rigging, &c.; that from this circumstance, and the communication which his barge's crew had with that ship, the pestilence was brought on board both ships: and that of the Charon's crew thirty died; and of the Scorpion's crew about fifteen. The Hankey arrived at the Port of St. George, on the 19th of February, in the most distressed situation; and for a few days lay in the bay, but was afterwards brought into the Carenage."

By the publication of such *apparently decisive*, though *fictitious*, instances and proofs of a most powerful and destructive contagion, on board the Hankey, joined to others of *equal value*, which were stated by Dr. Chisholm to have occurred after her arrival at Grenada, we cannot wonder that many persons were so far misled, as to believe in the generation and importation of a new and "direfully" contagious fever by that ship; for at first the Hankey, and not the Calypso, was represented as the parent of this monstrous and dreadful production. Fortunately for the cause of truth, the falsehoods regarding the Charon and Scorpion, were detected, and laudably exposed by Dr. Trotter, who happened to be then surgeon to the Vengeance, ship of the line, one of Admiral Gardner's squadron, under whose protection

from stem to stern, both above and below, with salt water, and then with vinegar and water, and the purification was completed, by thoroughly fumigating her with tar, pitch, and gun-powder; and that this purification was repeated at Bissao; and he ascribes the eight deaths, which occurred subsequently to the ship's getting a-ground, principally to the terror, confusion, great fatigue, &c. occasioned by that event. See New York Medical Repository, vol 1, p. 466.

* The Charon did not arrive until the 15th of January, a week after the Hankey's arrival: and Scorpion only entered Port Praya on the 24th, one day after the Charon had left it.

the homeward-bound West India summer fleet (of 1793) was then returning to England. He informs us, at p. 327 of the first vol. of his Medicina Nautica, that on the 22d of August, a ship, one of the fleet, "lost her fore-mast in a squall of wind, and received other damage, when the Admiral made the signal for the Vengeance to take her in tow. The ship proved to be the Hankey from Grenada and Bulama. Captain Thompson sent carpenters on board, with the necessary stores to assist in repairing her losses: they remained for three or four days, *but no sickness followed*, &c. Dr. Trotter afterwards mentions, how from this, and other circumstances, he was induced to make inquiries concerning Dr. Chisholm's account of the consequences of the Hankey's intercourse with the Charon and Scorpion, particularly from Captain Dodd, who "had his *broad pendant*" in the former ship, and of Mr. Smithers, who was her surgeon: from them, says Dr. Trotter, I have copied the following narrative of their transactions with the Hankey, viz.—

"When the squadron, under Commodore Dodd, came to St. Jago, in 1793, the Hankey lay there, in great distress for want of hands, having buried above one hundred persons,* men, women, and children, from the time she had been at Bulama. The fever was now overcome; Mr. Smithers saw two men who had lately recovered. He prescribed to the master, who was ill of a venereal complaint, and for which he left him some mercurials, with directions how to use them; at the same time he left a quantity of bark. The Charon and Scorpion sent two men each to assist in navigating her to the West Indies. The Hankey at this port was *cleansed*, *washed with vinegar*, *and fumigated*. *No fever* appeared in either of the men of war in consequence of this communication; they arrived at Grenada *in perfect health*," &c. Dr. Trotter adds, "It is probable from these facts, that the Hankey did not import the infection that produced the Grenada fever."—It is also doubtful, how the effects left in the Hankey, could produce the fever, for the bedding was thrown away, and what clothing remained had been aired, and probably had scarcely been in contact with the body after being sick. Mr. Smithers was examined before the (lieutenant) governor of Grenada on the the subject, and gave his opinion decidedly, that the Hankey did not communicate this fever to the colonists."

From this statement, and other proofs, it has been unquestionably ascertained, that every part of Dr. Chisholm's account, which asserts the communication of any disease from the Hankey to the Charon and Scorpion, was a *mischievous falsehood*, *fabricated without the smallest foundation*, or particle of truth; since the latter ship did not lose a single man during her whole voyage, and the Charon lost only *four*, from causes described by Dr. Trotter, and wholly foreign to the Hankey. Indeed, two of these four did not belong to the Charon, and one of them was a black prisoner, sent to England to be tried for murder. But what is Dr. Chisholm's apology, what his *atonement*, for having *asserted* and *published* these falsehoods? Why, truly, in the preface to his second edition, after noticing Dr. Trotter's publication on this

* This number is stated generally, and probably from imperfect recollection. The deaths amounted to sixty, and no more; of these nine were seamen.

subject, he says, p. xxii. "On further inquiry, I find I have been *in-correct** in my statement of the circumstances of the *interview* (an *interview* between ships!) which the Charon had with the Hankey at St. Jago;" and then, as if not willing to retract on the evidence of Commodore Dodd and Mr. Smithers, or without seeming to do it spontaneously from other evidence, (which we are to suppose an anxious regard for truth had induced him to procure) he mentions his having been "*politely favoured*" "with the perusal of a Log-book, kept" by "a lieutenant of the Charon, during the voyage in question;" in which he says, "I found that no sickness took place on board that ship in consequence of the interview." And then he adds, (perhaps as a compliment to Commodore Dodd and Mr. Smithers,) that "a *Log-book* is *unquestionable evidence*, and, *therefore*, I have *suppressed* what I have advanced on this affair," *(a great favour truly!)* "*on the authority of the late Mr. Home:*" adding, "but why Mr. Home should mention this as a fact communicated to him by Commodore Dodd, I can assign no reason:" nor indeed can any other person, as I believe. But before we employ our time in assigning reasons or motives for supposed events, it is always best to inquire whether they have *really happened*. The question seems to be,—Whether, in truth, Mr. Home did inform Dr. Chisholm that Commodore Dodd had told him these falsehoods? This being a question of no small importance; the affirmative is not to be assumed with Dr. Chisholm's seeming *levity* and *unconcern*. For the assumption is nothing less than fixing upon the character (as I believe unblemished) of a gentleman now dead, and unable to justify himself, the stigma of having invented and propagated the most groundness (and in no small degree injurious) falsehoods;† for no person will believe that Commodore Dodd, without any discoverable motive, should have invented and reported what he manifested so much readiness in contradicting, and in authorizing Dr. Trotter to contradict, and what every man on board the Charon and Scorpion, as well as himself, knew to be false.

Had Dr. Chisholm in stating these untruths, named his authority, as he ought to have done, and had the person named admitted himself to have been the informer, Dr. Chisholm's veracity would not have been impeached, whatever might have been thought of his *discretion*, in publishing such reports. But instead of this, he ventured to *assert* as from, or, within *his own knowledge*, that Commodore, or Captain Dodd, on coming to Grenada, and hearing of the mischief caused by the Hankey, had "*mentioned*" the falsehoods in question; and by this *unqualified* assertion, he had made himself *responsible* for its truth, and liable to be considered as its *author*. I do not mean to decide that Mr. Home *did not* give Dr. Chisholm such information, as he pre-

* When the principal facts in a statement are true, and the lesser circumstances, or incidents *only*, are imperfectly or erroneously stated, the author of such a relation may describe himself as having been "*incorrect*;" but when all the *parts* for which a statement was made, are completely unfounded, and without *any intermixture of truth*, other and stronger terms can alone be proper. I should not have made this observation, if Dr. Chisholm had not so often, and with very little reason, applied the harshest and most offensive language to others in his writings.

† Mr. Home, I believe, lost his life by the hands of assassins; and I will not co-operate in stabbing his reputation, when he is no longer able to defend it.

tends; but on a question of this nature, I feel it to be my duty to consider and weigh probabilities; and, in doing this, I recollect that Dr. Chisholm is, at best, an interested witness, and, therefore, cannot be received as one, on this subject; that his own character is in jeopardy; that when making his groundless statement, he, in a note to the very page which contains it, has mentioned Mr. Home, as giving him information respecting Captain Coxe's refusal to destroy the effects of the Bulama adventurers, unless indemnified; but has not even hinted at Mr. Home as having mentioned the other supposed facts, which had just before employed his thoughts and his pen; and which, therefore, it would have been most *natural* and *proper* for him to have done, if they really had been stated on that gentleman's authority. And I recollect also, that while I have no reason to suspect Mr. Home of having ever, either wilfully or incautiously propagated untruths, Dr. Chisholm appears to have done this in but too many instances;* and, therefore, the balance of probabilities, in my

* Some of these instances have been already mentioned, and others will soon fall under our notice. But in the mean time, there is one so analogous to the matter in discussion, that I cannot avoid noticing it here, especially as it proves, that instead of becoming more cautious and attentive to facts, as an *atonement* for the untruths published respecting the Charon and Scorpion; Dr. Chisholm, subsequently to his retraction of them, did not scruple to assert, and publish others equally void of truth, and for the same mischievous purpose of proving his *Nova Pestis* to be *direfully contagious*.

The following are his own words, at p. 320, of vol. ii. of his Essay, viz :—" When visited this island, (St. Thomas's) in November, 1796, an accident furnished me with an opportunity of informing myself relative to the history of the malignant pestilential fever as it appeared there in 1793,4,5, and at that time. The history was indeed a melancholy, but it was also an instructive one An eminent merchant, M. C. G. Fleicker, with whom I had been acquainted at St Croix, requested me to visit a valuable young German gentleman of this house, of the name of Schmaler, who had arrived from Hamburgh only about ten days before, and at this time unhappily laboured under a fatal attack of this most dreadful malady. In Mr. Fleicker's house, the malignant pestilential fever had very frequently made its appearance during and since 1793, and except in one instance, the captain of a Hamburg ship, always fatally. No means, at least none sufficient for the eradication of the infection, had been employed on the death of the unfortunate sick, consequently the chambers, which were successively occupied by strangers from Europe, became a never-failing seminium of the pestilential contagion. A very few days after his arrival, Mr Schmaler felt its influence," &c. This misrepresentation, to call it by the mildest name, being made known to I. F. Eckard, Esq. Danish vice-consul, at Philadelphia, that gentleman wrote a letter to Dr Mease of that city, in which after giving a copy of it, he adds the following observations, viz.

" Dr. Chisholm, no doubt alludes, in the above paragraph to Mr. C. G. Fleicker, who resided at St. Thomas's, but who had not, at the period of Dr. Chisholm's visit any regular establishment in the island, but acted as an assistant to my house, of which Mr. Schmaler was clerk. There being a great intimacy between Mr. Fleicker and myself, he often in my absence, was authorised to superintend my concerns, and this was the case at the time Mr. Schmaler died. I was, however, at home when he arrived from Europe, and returned soon after his death

" More young men had died in my house, from 1793 to 1796, and even later, than perhaps in any other in town; because more had come out to me from Europe than to other merchants. Their deaths, however, could not have been occasioned by the contagion remaining in the chambers of the house, as Dr. Chisholm supposes; for the cases took place at remote periods, in different houses; I having changed my dwelling in 1795 — Neither could their deaths have been occasioned by the contagion remaining in the bedding, for the beds and bedding of those who died of a putrid fever in my house *were never used again*. Further, according to the best of my recollection, two persons were never ill of the fever at any time, in the same chamber, in either of my houses; in both of which I had four or five rooms appropriated for clerks: besides many persons slept in those chambers without any inconvenience. If Dr. Chisholm's account were correct, my house must have been a lazaretto, for those supposed pestiferous chamjudgment, is very unfavourable to him. And in any event, a writer who will adopt, and give his utmost sanction to unfounded reports, when they happen to suit his own purpose, must not expect that even his facts, when he happens to state them, will be believed, without other authority than his own.

It will be recollected, that the testimony of Commodore Dodd and Mr. Smithers, did not extend to any transactions on board the Hankey, subsequently to her leaving St. Jago, and, therefore, a *part* of Dr. Chisholm's statement was left uncontradicted by them, though since proved to have been false by the Hankey's Log-book. The part in question is as follows, viz :—That "on the *third* day after leaving St. Jago, the men procured from the ships of war were seized with the fever, which (as he pretends) had carried off three-fourths of those on board the Hankey, at Boullam, and having no assistance, two of the four died : the remaining two (he adds) were put on shore here, (at Grenada) in the most wretched state possible." The object of this part of Dr. Chisholm's mis-statement, like that of the former, is to prove the existence of this "direful" contagion on board the Hankey : but it seems to be as completely destitute of truth as the other. The Log-book, indeed, mentions that Samuel Hodge, one of the seamen sent from the Charon, on the 23d of January, died on the 4th of February ; but Mr. Pabia, a passenger in the Hankey, says this man "was *unwell* when he came on board,"—though "able at that time to do duty ;"—"that he grew more and more unwell as they proceeded." That "Captain Coxe, who was still *unwell* when the Hankey left St. Jago, *recovered his health*, before they reached the West

bers were almost always occupied ; and I can assure him, that commonly a whole year, and sometimes a longer period, passed without any one of my family being sick of fever. It is, moreover, incorrect, that all those persons died who had been sick of the pestilential fever during, and since 1793, except the Hamburgh captain ; and also, 'that after the two first years of the introduction of this fever, the inhabitants, without exception, whether Creoles or foreigners, equally suffered.' The truth is, that many Europeans and Americans recovered, both before and after the time of Dr. Chisholm's visit to St. Thomas's, and the fever never spread to the inhabitants at large, but was confined to persons recently arrived from Northern climates, and to those on board the vessels in the harbour ; nor was there any apprehension of contagion, except among the shipping. I never heard of a single instance of any person who had resided for some years in the island being afflicted with the malignant fever. A residence of nearly twenty years in the island enables me to speak positively as to this fact.

"I have not the honour of Dr. Chisholm's personal acquaintance, but as he was so polite as to visit Mr. Schmaler in my absence, I feel myself obliged to him, and I am sorry I have been under the necessity of correcting his mistatements. He mentions Mr. Jennings and Dr. Tucker as his acquaintances at St. Thomas's, and to these gentlemen, as well as to Mr. Fleicker, I refer for corroboration of any part of my statement if required.

I am, &c.

(*Signed,*) J. F. Eckard.

Philadelphia, Feb. 1, 1804.

This letter has now been published *seven* years, (in the 1st vol. of the 2nd Hexade of the New York Medical Repository, (p. 337) of which work Dr. Chisholm appears to be an *attentive* reader,) but he has carefully avoided all notice of it, not pretending (so far as I can discover,) to have been misled by even a *dead* person ; though his letter to Dr. Haygarth must have afforded him a most inviting and suitable opportunity for giving, *if it had been possible*, some satisfactory explanation concerning these his gross misrepresentations.

Indies, (though he afterwards had a return of his disorder,)* and *all the others were perfectly well*, (notwithstanding the hard duty they had to perform,) *and continued so*." See New York Medical Repository, vol. 1, p. 469.

For these latter untruths, Dr. Chisholm has never, so far as I can discover, pretended to have had Mr. Home's, or any other person's authority; and, though Dr. Trotter's publication ought to have convinced him that they were, at least, very improbable, he confidently *re-published* them as *matter of fact*, in his second edition; and has not, in any subsequent publication, either retracted, or apologized for them, though the truth had been forced upon his notice by the Hankey's Log-Book.

After a passage of 19 days, the Hankey arrived at Barbadoes, and there, Mr. Paiba states that, during a great part of two days, her captain, passengers, and crew, communicated freely with the inhabitants, without causing disease in any of them: they did the same afterwards at St. Vincent's; and anchored at Grenada on the 19th of February, according to Log-book time, but in ordinary language, in the afternoon of the 18th. The next day, several paragraphs appeared in the St. George's Gazette, mentioning the Hankey's arrival "from the island of Bulama, on the coast of Africa;" and "that she and another vessel had carried out to that settlement upwards of 300 adventurers, of whom one-third had not survived her departure from the settlement," &c. In addition to these exaggerations, it was asserted, as "*from good authority*, that of the whole number the two ships carried out, only ten were living when she (the Hankey) took her departure."

It is not wonderful that such gross mis-statements should have created apprehensions of danger from supposed contagion in the Hankey, among the inhabitants of St. George's, and have also excited and given a *bias* to Dr. Chisholm's industry on this subject. It happened, also, that a state of weather then existed, and had existed some weeks before, at Grenada, very unlike that of other years, and better suited for a copious production of marsh miasmata. This fact appears from an account which Dr. Chisholm has himself published, in the introduc-

* This appears to have been an *intermittent*, of which Captain Coxe is stated to have had several returns, as commonly happens to those who have been much exposed to marsh miasms. Mr. Paiba states distinctly, that while the Hankey was at St. Jago, there was no sickness on board of her, but "debility, and slight intermittents;" that "her crew and passengers mixed without suspicion, and with perfect freedom with the inhabitants of Port Praya, and received them on board, where they had a number of entertainments, of which the governor of the island and several of the principal people partook," without so much as a suspicion of any sickness "being excited by it." He adds "Indeed no sickness prevailed at St. Jago during the Hankey's stay, excepting the common ague and fever of the place." That during the ten days which the Charon remained at Port Praya, it was the commodore's "custom to send his barge every morning to the Hankey, for Mr. and Mrs. Paiba, who usually spent the day with him, and returned in the evening.—That Captain Coxe was several times on board the Charon; and both Mr. Smithers and his *mate* visited the Hankey, and two of the Charon's seamen were employed great part of one day about the Hankey's rigging. And finally, that Commodore Dodd had so little apprehension that any person belonging to the Hankey would be liable to infect others, that he gave Mr. and Mrs. Paiba a letter of recommendation to a gentleman at Grenada; and in consequence of it, they were invited to reside, and did reside at this gentleman's plantation, while they remained on the island. See New-York Medical Repository, vol. p. 468. 470.

tion to his Essay, of "the changes which took place in each month of the years 1784, 1785, 1786, and 1793," and from his "table of the highest, lowest, and medium height of the thermometer during that time;" "from all which it results that in 1793, the months of January and February were generally rainy, which he notices as "*an uncommon circumstance;* and that the heat in those months was uncommonly great; the thermometer, at noon, rising to 88, and 89°, and which is 3 and 4 degrees higher than it was during the same months in any of the preceding years; and seven degrees higher than in 1785. And in the three following months there appears to have been such an intermixture, or alternation of showers and of *hot sunshine*, as commonly renders marsh fevers prevalent in places liable to them.

Dr. James Clarke, also, in his treatise of the yellow fever, which prevailed in the *same*, and following years, in the neighbouring island of Dominica, says, p. 49, "from the month of January to the 15th of June, (1793) when this fever first broke out, the weather was extremely *calm*, and *much hotter than usual* in this and the *neighbouring* islands. There was little rain (he adds) till the 15th of October," at which time "this fever became less violent here; and about the beginning of November it ceased altogether." In the next page he observes, that in June, July, August, and September, Fahrenheit's thermometer "generally rose to 88° or 90° and sometimes to 92 degrees, between the hours of two and four o'clock, P. M." Again, at p. 51, he says, "the heat for some months before, and during the continuance of this fever in the island, especially in the night time, was *almost insupportable.*"

Whether the very early appearance of the yellow fever in most of the West Indian islands, in 1793, resulted from this unusual state of the weather alone, or whether there was a co-operation of other causes which were either unknown, or unnoticed, I leave for the consideration of others and content myself with again expressing my belief, that we are not yet acquainted with all the causes which assist either in the production of marsh miasmata, or in rendering their morbid influence more powerful than usual. But we know particularly from what happened at Charleston, in 1732, (see p. 245 and 319,) and at other places to be hereafter mentioned, that the occurrence of yellow fever, as an epidemic, may be accelerated several months by particular conditions of the atmosphere: and as Grenada is situated several degrees southward of Dominica, and must therefore have sooner felt the influence of the sun in its approaches to the northern tropic, we shall not be surprized that the fever became prevalent soonest at the former of these islands.

In regard to the supposed communication of febrile, or pestilential contagion, from the Hankey, subsequently to her arrival at Grenada, Dr. Chisholm asserts, in the first volume of his Essay, (p. 121,) that "a Capt. Remington, an intimate acquaintance of Capt. Coxe, was the first person who visited the Hankey after her arrival in St. George's Bay. This person (says Dr. Chisholm,) went on board of her in the *evening after she anchored*, and remained three days, at the end of which time he left St. Georges, and proceeded in a drogher, or coasting vessel, to Grenville Bay, where his ship, the Adventure, lay. He was seized with malignant pestilential fever on the passage; and the *violence of the symptoms increased so rapidly as on the third day to put an end to his existence.*" In regard to this transaction, Mr. Paiba not only contradicts Dr. Chisholm concerning the time of Capt. Remington's visit to the Hankey, (which it seems now difficult to ascertain) but he asserts that this Captain "had been all day and night coming from Grenville Bay, and had been wet through." "That he slept on board in his clothes, and went in an open boat* the *next* day, back to his ship; enough (adds he,) to kill any one in that climate." See Medical Repository, vol. i. p. 471. On this point, Dr. Chisholm, in his 2d edition, (p. 122,) says, "That this person had fatigued himself, and had even slept in wet clothes, might have happened; but does this prove any thing further than a greater predisposition of his body to be acted on by infection?" Yes, if true, it proves that Capt. Remington had been exposed to causes sufficient to produce a mortal fever, *without any infection*, as *thousands have experienced;* especially in that climate. Dr. Chisholm, however, objects to Mr. Paiba's statement, because it was made after he had seen Dr. Chisholm's Essay. But would the Dr. have us believe that truth is not admissible, if brought forward to correct particular misrepresentations? On this subject, however, Dr. Chisholm has referred us to "Dr. John Stuart, an eminent practitioner, who attended him (Remington) at Grenville, when he arrived there." And as I have the pleasure of knowing Dr. Stuart (who, satisfied with the produce of his estate, has since relinquished his profession and title) and entertain great respect for his character, as well as the utmost reliance on his candour and veracity, I shall most readily admit every thing stated as matter of fact by that gentleman, only regretting that his statement on this subject is not more comprehensive.

It is contained in the New-York Evening Post, of Tuesday, November 26, 1805, in a letter to Dr. Hosack, to which Dr. Chisholm has referred in his printed letter to Dr. Haygarth) and in it Dr. Stuart mentions his going, in the month of March, 1793,† on board the ship Adventure, then lying in Grenville harbour, to visit the carpenter, who was under his care, and then adds, "While there, Captain Remington arrived from St. George's by sea: he had come round in a drogher, and had had heavy squalls, with rain in his passage to windward. He then complained of being feverish, and seemed low spirited; he had heat of skin, his pulse full, and under 100; head-ache, pain in his back

* The *droghers*, at Grenada, are not properly open boats; but the space they afford, as a protection from rain, under the deck or half deck, is so *very close*, *hot*, and confined, if my recollection be accurate, that most people rather than avail themselves of it for any length of time, would probably allow themselves to get wet.

† Unfortunately, Dr. Stuart has not mentioned the day of the month when this happened; but he has mentioned enough to prove, that Dr. Chisholm must have erred considerably in regard to the *time* when Capt. Remington went on board the Hankey. For if it had been, as he asserts, upon the *evening of her arrival* at St. George's, this would have been on the 18th of February, and supposing him to have remained on board 3 days, and not *one* night only, as Mr Paiba asserts, he must, notwithstanding, have set out on his return to Grenville Bay, upon the 21st of February, and supposing him to have employed two other days in making the passage, though it is commonly done in less than one, still he would have reached his ship not in March, as Dr. Stuart mentions him to have done, but on the 23d of February.

and limbs, and over his whole body. These symptoms I imputed *to cold* caught in his passage up, and accordingly took eight ounces of blood from him, which unexpectedly neither exhibited the buffy coats nor the coagulum, any degree of contraction, nor consequent separation of serum. He took an emetic of ipecacuanha in the evening, and a dose of Glauber's salts the following morning. During three days I continued to visit him, his pulse did not exceed 100, nor was the heat of skin considerable; he took occasionally small doses of antimonial wine, with the addition of laudanum at bed time, and made free use of tepid drinks. At the end of that time, I was under the necessity of putting him in charge of a neighbouring practitioner, having a call to the other side of the island. On leaving him, *I certainly did not entertain any idea of his being in danger;* I was, however, forcibly struck with, and could not well account for, an uncommon degree of despondency of mind that was then present, and it was not possible to remove the impression that he was to die; nor was I the less surprised, on going to Grenville some days after, to be told of his death, and more especially to hear of that event having been preceded by hæmorrhage from his nose, stomach, mouth, and urinary bladder. On this occasion, while in conversation with some gentlemen on the fate of this unfortunate man, I could help noticing the malignancy of the case, and the difference in the train of symptoms, from what I had ever witnessed to take place in the worst case of our endemic fever. But a few minutes had elapsed when a gentleman arrived from St. George's; I had no sooner mentioned Capt. R.'s death to him, and my surprise thereat, when he instantly replied, it was none to him, for that Capt. R. had eat and slept on board the Hankey, during several days that he was in town."

If my readers will compare this account with that given by Dr. Chisholm, they will, I presume, be forcibly impressed by the important deviations from the truth which occur in the latter, and by the evidence which it affords of his inexcusable carelessness about facts which might have been so easily ascertained, if he either did not know, and was at all solicitous for the truth. It appears from Dr. Stuart's statement of Capt. Remington's case, that, instead of that "*violence of the symptoms*," and that rapid increase of their violence, which, "on the *third day*, put an end to his existence," (as Dr. Chisholm asserts) the symptoms were all so very moderate, that when Dr. Stuart left his patient, at the *end of three days*, he was not even suspected to be in any danger, and, according to the best information which I have been able to procure, Capt. Remington did not die until the 8th day of his illness.*

In regard to the cause of Capt. Remington's fever, Dr. Stuart's account of it accords, in my judgment, much better with that of Mr. Paiba than of Dr. Chisholm. Whether Capt. Remington got wet in going to St. George's, or returning thence to Grenville harbour, or both, Dr Stuart avowedly, and with reason, considered his disorder as proceeding from cold. And though he appears to have been surprised

* The editor remembers distinctly to have heard Dr. Stuart mention, that it was on Sunday that he first prescribed for captain Remington, that he took leave of him on Wednesday; that his death happened on the following Sunday.

at its fatal termination, I cannot persuade myself that it is so unusual in that climate for fevers produced by such a cause, to end in death, and even with hæmorrhage from different parts, as fairly to authorize a belief that some more malignant cause must have co-operated; and, perhaps, if Dr. Stuart had never heard of the groundless reports concerning the Hankey, he might not have suspected any such co-operation: though after all that appears to have been told him of that ship, his doing so was, I think, very natural. But as my readers will soon be convinced, if they are not so already, that no infection was, or could have been communicated from the Hankey, and as Capt. Remington's fever manifested no contagious property, even in the narrow space of a ship's cabin, and when as there was no suspicion of infection, no precaution would have been used to guard against it, we have, I think, the strongest presumptive, as well as negative evidence, that it did not proceed from, or possess any contagious influence. Indeed, if the Hankey had abounded in contagion, it must have been altogether incredible that it could have produced disease so speedily; and if the getting wet, and sleeping in wet clothes should not be deemed a sufficient cause for the disease and death of Capt. Remington, another, and that infinitely more probable, in my opinion, than a new pestilence, naturally presents itself to those who recollect, that besides the influence of marsh miasmata in the bay and carenage at St. George's, his own ship lay in Grenville bay or harbour, which Dr. Chisholm (at p. 290 of his letter to Dr. Haygarth) has particularly designated as one of the places which, in an uncommon degree, expose ships lying therein "to the *malignant influence of marsh miasmata*."

Dr. Chisholm tells us (Essay, p. 122,) that "the crew of the Defiance, of Blythe Port, near Newcastle, were the *next* who suffered by visiting this ship; (the Hankey) the mate, boatswain, and four sailors, went on board the *day after* her arrival;* the mate remained either on

* Dr. Chisholm apparently was anxious to lose no time. He made captain Remington go on board the Hankey the very evening of her arrival; though Mr. Paiba says it was nearly a month after, which it might have been, consistently with Dr. Stuart's statement; and consistently with that statement, he could not have gone on board in much less than a week: and, therefore, if the people of the *Defiance* went on board the day after the Hankey's arrival, and were *immediately* seized with the fever, they, and not captain Remington, must have been the first "who suffered." And, as they are said to have all died in *three days*, excepting the mate, whose disorder was slight, and, therefore of no long duration; and *it is not pretended that any other of the crew took the fever from them*; it ought, according to this account, to have ceased entirely on board the Defiance, before the end of February. But by another statement, at p. 202 of the same volume, we are told that "about the *end of March*, 1793, the Herberts, Captain Brown, sailed from the Port of St. George, Grenada, for Glasgow. In working the ship out of the harbour, (continues Dr. Chisholm,) Captain Brown was obliged to send five of his men on board the Defiance, of Blythe Port, to fasten a *warping line*. At this time, (the *end of March*) the malignant pestilential fever *raged* on board the Defiance." It always *rages* in Dr. Chisholms accounts. He adds, "The next day after the Herberts sailed, the five men were seized with the disease, and *three* of them died." Here the Doctor's favorite number, *three*, is applied to the men who died, and not the day or days on which the termination happened. Now, what are we to believe amidst these contradictions? Did this fever rage on board the Defiance, about the 20th of February, according to the first statement, or, about the *end* of March, according to the second? In regard to the latter story, nothing less than Dr. Chisholm's credulity could produce it belief. If five men were sent in a boat along side the Defiance, with a warping-line, the *end* of that line might easily have been thrown on board of the Defiance, and fastened by one of her crew, and is often, and as ought to have been done, where a pestilential

deck, or in the cabin, but the rest went below, and staid all night there. *All* of them were *immediately seized* with the fever, and *died in three* days." Here Dr. Chisholm seems to have been either totally regardless of truth, or completely infatuated, if he *believed* this account to be *true*. What, *five* persons *immediately seized* with a *contagious fever!* Did any species of contagion ever produce disease *immediately?* The plague itself does not act with such celerity. Moreover, these five persons all died exactly "in *three* days." Did ever contagion act with such *deadly* and *exact uniformity?* Differences of age, constitution, susceptibility, &c. were, it seems, completely inefficient. And here I cannot help noticing Dr. Chisholm's singular predilection for the exact term of *three* days. The four seamen obtained by the Hankey from the ships of war at St. Jago, are stated to have been *all* seized "on the *third day*" after she sailed, "with the fever," which probably neither of them ever had; and Captain Remington is made to *die of it* on the *third day*, though Dr. Stuart did not then believe him to be in any danger; and again, the five men from the Defiance, are all represented as having died in *three* days, and a sixth, (the mate) as having only escaped with a lighter disease, because, by remaining on *deck* or *in the cabin*, he was "less exposed to the virulence of the infection." All this, however, rests upon Dr. Chisholm's *unsupported* authority, the value of which, I must leave others to estimate, having no means, except those furnished by Dr. Chisholm, even to ascertain whether the ship Defiance ever existed.

After this, Dr. Chisholm tells us, (p. 123) that "The crew of the ship Baillies, from the same imprudent civility, or curiosity, were the *next* who suffered."* "These (he adds) communicated the infec-

fever was *raging*: and, at most, if this was not done, it could only have been necessary for *one* man to jump on board, for a single minute. No stay could have been allowed, as the men must, at that time, have been wanted immediately on board their own ship. To suppose, in such *circumstances*, that they could *all* have been infected, and that this infection should have operated so as to produce disease in them all the very next day, is completely inadmissible, because no contagion yet known, is capable of being transmitted so *far*, and in so short a space; and also of producing disease within 24 hours after being transmitted; and, therefore, if five men belonging to the Herberts, were attacked by the supposed malignant pestilential fever, they must have *previously* been exposed to the causes which had produced it in others, independently of any communication with the Defiance.

* It is here remarkable, that Dr. Chisholm makes no mention of the nature, or extent of the suffering supposed to have been thus brought upon the crew of the Baillies. He might have told us, at least, how many had been attacked by the supposed *new fever*, on board that ship; and I certainly regret that he did not. He gives us, indeed, four supposed cases of that fever, which fell under the management of his partner, Mr. Campbell, at a time when Dr. Chisholm himself was under salivation for hepatitis, and probably before he had seen any case of this fever. One of these without date, but said to have been the first which occurred in Mr. Campbell's practice, was that of the carpenter of the ship Charlotte, of London, which is not mentioned by Dr. Chisholm as having had any communication with the Hankey: and the second is the case of "*John*," a sailor on board the Baillies. And this was probably the first case of fever which occurred on board that ship; for, as the partnership of Chisholm and Campbell appears to have had her sick, it is not likely that any other practitioner was employed. Mr. Campbell first saw John on the 21st of March, 1793, on which day he first resolved to make trial of mercury, and this in John's fever; being, as he says, "fully satisfied that this disease was the *malignant fever*, which prevailed at that time." Now, if Dr. Chisholm's account of the commencement of this fever was correct, how could the malignant fever be then *prevalent?* Captain Remington had sickened some time in March, and for any thing

tion to the ships nearest to them, and it gradually spread from those nearest the mouth of the Carenage, where the Hankey for some time lay, to those at the bottom of it; not one escaping, in succession, whatever means the captain took to prevent it." An effect which, if correctly stated, is much more like that of marsh miasms than of contagion.

This statement, however, is too general for a particular examination, and being, like the former, without any support, I can have no reliance on its accuracy. That a violent marsh or yellow fever did at that time, or soon after, prevail among the shipping at the Carenage, at Grenada, (as it had often done there, and in the like situations, long before the supposed generation of Dr. Chisholm's new pestilence,) I believe; and I also believe, that this fever (which, *as usual, began* among the seamen) soon after appeared in some of the inhabitants on shore, but not as Dr. Chisholm supposes from contagion. We have seen that the Calypso, who is supposed by Dr. Chisholm, to have *infected* the Hankey, and whose fever consequently must have been the *same*, was not suspected at Sierra Leone to have communicated any disease there, nor to have had any other than a *marsh fever on board.* And we have seen that she returned thence to England, after suffering a mortality, greater in proportion to her stay in Africa, than that of the Hankey; and it never was even suspected, that either her crew, or her surviving passengers, with all their bedding and effects, and those of the *dead* also, (though landed and dispersed in Great Britain) had introduced any sort of contagion, there being fortunately no person *here*, to excite groundless alarms, by inventing and propagating falsehoods *as in Grenada.* In regard to the Hankey, we have every reason to believe that no other than marsh fevers, or fevers from cold, fatigue, intemperance, and other similar causes, and had occurred on board of her during the whole voyage. It appears, moreover,

that appears to the contrary, *subsequently to John;* and also at a *remote part of the island.* The Baillies is stated by Dr Chisholm to have been the second ship which communicated and suffered by communication with the Hankey; and, from his second statement, quoted in the preceding note, she appears to have been the first. But supposing the Defiance to have been the first, the sickening of a part of her crew alone, could not justify Mr. Campbell in stating this to have been a *prevalent fever*, nor that it was one in which he was "well convinced the common mode of practice was by no means successful." To have acquired this conviction, he ought to have had a considerable number of cases, or have known that many cases of this *new* disease had occurred to others, and terminated unfavourably under "the common mode of practice in fever." But such cases could not have happened, according to Dr Chisholm's statement, from a communication with the Hankey; and it therefore seems probable, that what Mr. Paiba has asserted in the New-York Medical Repository, vol. 1, p 47, may be true, viz that this "sickness was universally *known to be in the town of St. George, when captain Remington visited the ship*," the Hackney. And Mr. Campbell's first case, that of the carpenter of the Charlotte, before mentioned, seems to prove that at least in this instance, the supposed new fever was not derived from the Hankey. Mr. Campbell mentions a second case on board the Charlotte, that of Mr. Taylor, the mate, but without any date; and he also mentions a second case on board the Baillies, that of "*Stephen*," which occurred on the 10th of April: and these are the only ones mentioned to have occurred on board that ship, either by Mr. Campbell or Dr. Chisholm. It seems remarkable, also, that Mr Campbell does not even hint so much as a suspicion, that either of his patients had communicated with, or derived his fever from the Hankey; and to me it seems probable, that cases of this supposed *new* fever had appeared at St. George's before the Hankey, though not much noticed, as indeed they were not likely to be, while sporadic, and few in number.

that she was *three times* cleansed and fumigated, viz. at Bulama, Bissao, and St. Jago; and at the latter of these places, (at least) she must have been free from any contagion; for otherwise, her unrestrained intercourse with the inhabitants, would have excited disease in some of them ; which it did not, nor in any person on board the Charon or Scorpion, nor in the four sailors obtained from those ships by Captain Coxe; who, during the subsequent part of the voyage, could not have escaped it, if any had existed, capable of producing such wonderful effects as Dr. Chisholm has supposed, in Grenada.

It appears *certain*, that every body on board the Hankey was well, when she arrived at Grenada, and continued so during her stay there, and also during the whole of her voyage to England, which is probably more than could be said of any other ship then at Grenada ; so that even if it were ascertained that an importation of febrile infection had then taken place, every other ship ought to have been suspected of it *before the Hankey*, coming, as she did, from a place where there is good reason to believe no contagious fever had ever existed. Dr. Chisholm, indeed, does not pretend that the infection supposed to have been derived from the Hankey, had emanated from any person actually on board when she arrived at Grenada, but from the baggage and effects of the deceased colonists: to this, however, there are *insuperable* objections, some of which will be stated in the subjoined note ;* besides others, arising from the fact of its having been found

* It appears from Mr. Smither's account, that the baggage and effects in question consisted of clothing, which "had been aired," and probably never in "contact with the body of any person," after that person became "sick ;" though as there was no contagious sickness among the colonists, that circumstance seems to be of but little importance.

In regard to the *bedding*, Mr. Smithers confirms Mr Paiba's assertion, that it was thrown away at Bulama. But whatever the effects brought from Bulama may have been, they were carefully preserved by Captain Coxe, and "*delivered up*" *at Stangate Creek*, on the 18th of October, 1793, as is proved by the Hankey's Log Book, (though Dr Chisholm has asserted, without any foundation, that they were thrown overboard at Grenville Bay ;) and therefore, it will be easy for me to place Dr. Chisholm in a most embarrassing *dilemma*. Assuming, as he does, that the Bulama baggage was the vehicle, receptacle, or *fomites*, whence the Hankey's supposed contagion issued, he must necessarily conclude, either that this contagion was most powerful at first, and that it daily became weaker and weaker, by constant evaporation, and diffusion through the atmosphere; or, he must conclude, according to the common opinion in regard to fomites, that, like *vermin*, it possessed the power of multiplying itself, and of becoming daily more powerful and destructive. Should he adopt the first of these conclusions, it will be *necessary*, though *impossible*, for him to explain how this baggage could retain enough of contagion, to produce such wonderful and unexampled effects at *Grenada*, though when in its stronger and more *accumulated* state, it was too weak to excite disease in Mr Smithers or his mate, or any other person at St. Jago, or in any of the seamen during the passage thence ; and if he should prefer the latter conclusion, it will be equally necessary, and equally impossible for him to explain, why, if this supposed contagion had become so *virulent* and powerful, as is pretended, at Grenada, it should, with its power of farther *multiplying* and *increasing itself*, have been found perfectly inert, and harmless, during the passage to England *from* Grenada, not only to the six seamen received by Captain Coxe from the commodore at *that island*, (on the 18th of July,) but also to the carpenters sent on board the Hankey, from the *Vengeance* ship of war, (on the 22d of August,) and who remained three or four days on board ? The contagion, by this time, ought certainly to have become "*direfully*" active; and the carpenters, as well as the six seamen lately mentioned, must have been much more exposed to it, and for a much longer time, than any of those, who according to Dr. Chisholm, so suddenly became its victims at Grenada. And, on either supposition that such effects are absolutely incapable of exciting Dr. Chisholm's supposed malignant (or any other) fever, after having been used by hundreds ill, and dead of it. He, as is well known, and his adherents at *Philadelphia*, *insist most strongly*, that the yellow fever which prevailed as an epidemic in that city, in 1793, had been imported from the West Indies, and that it was truly the *malignant pestilential fever*, supposed to have been derived from the Hankey; and it is *solely upon this identity*, that they found their assertions and belief of its being *contagious*. It is also known, that during the prevalence of this fever in that year, an hospital was opened at Bush-Hill, near Philadelphia, and appropriated exclusively to the reception of patients ill of it; who, in great numbers, were placed under the care of M. *Deveze*, as was mentioned by me, at p, 258. And it is also well known, that when the fever had ceased, as usual, at the approach of winter, all the bedding, blankets, clothes, and other effects, which had been used by the sick, were sold to the Agents of the French government, and employed for their sick soldiers and seamen, without having been washed, fumigated, or even aired, and without its having been ever suspected that any disease was produced by these effects. See Dr. Valentin's Traite de la Fievre Jaune, &c. p. 92 and 93.*

Those who are acquainted with the town and neighbourhood of St. George, in Grenada, will readily believe, that it is *much more likely*

it is incumbent on him to shew how, with this amazing activity at Grenada, Mr. Paiba and his family were able to *land* there, with all their effects, including a quantity of goods, which with the Bulama baggage, made the whole of the Hankey's lading, and which were stored at the house of Mr. Napier, in St. George's, without even the suspicion of having infected any person?

To account for the wonderful escape of Mr. and Mrs. Paiba and their servant, from that supposed infection, which is said to have rendered the slightest communication with the Hankey so deadly to others, Dr. Chisholm asks, vol, 1, p. 131, "What connexion had Mr. Paiba and his family with the packages of clothes and bedding, from which emanated the infectious aura, productive of almost" (he might have omitted the almost,) "unexampled mischief?" To this inapt question, I answer, that their connexion, during nearly three months, which they *lived* on board the Hankey, after her departure from Bulama, must undoubtedly have been much greater with the packages in question, than Captain Remington's or any of the seamen, whom curiosity or civility brought for a short time from other ships, and who may be presumed never to have even *seen* them; and, it is difficult to conceive, how those packages, even if they had been infected, could have done the mischief *supposed*. It is not pretended that they were opened, or even removed by Captain Coxe; who, *faithful to his trust*, and convinced of their *innocency*, had determined to carry them to England, and could, therefore, have no motive to open or shew them to any one. In short, there never was, in my opinion, a more *inconsistent*, *improbable*, and *incredible tale*, *imagined*, than this, which is, I believe, *now* sufficiently refuted.

* The following are Dr. Valentin's words, viz:—"Le vêtemens, les fournitures des lits, qui avaient servi a des personnes infectées, ou *mortes* de la fiévre jaune, et qui ont passé a d'autres, sans avoir même été aérés, lavés, ou parfumés, ne la leur ont pas transmise. On en a eu *un exemple frappant*, dans les fournitures qui ont été vendus, de l'hospital de *Bush-Hill*, immediatement après l'épidémie de 1793, par le comité de Santé de Philadelphie, aux Agens de la Républiqe Française, pour les militaires malades comme l'a *rapporté mon ami Deveze auquel ils ont été confiés*." Indeed, thousands of proofs might be adduced of the innocency of clothing, &c after having been used in North America, by persons labouring under that very fever, which Dr. Chisholm supposes to have been imported to Grenada by the Hankey, and afterwards *transported* to different parts of the Continent, Spain, &c. And, therefore, all his assertions of the emanation of contagion from the Bulama baggage, must be unworthy of a moment's consideration.

to produce violent marsh or yellow fevers, than Bulama, where the greatest heat felt by the colonists, appears not to have exceeded 84 degrees; and nothing but the aggravated and fallacious reports spread upon the Hankey's arrival, could ever have suggested the idea of looking to the latter of these islands as the source of what Dr. Chisholm mistook for a new pestilence. Dr. Lind mentions, at p. 180 of his work on the Diseases of Hot Climates, that a "*malignant* sickness in the islands of Grenada and the Grenadines, proved very fatal to the English, who, upon the peace, in 1763, first went over to settle there." In a work, printed at Paris, in 1805, under the title of Observations sur la Fievre Jaune, &c. par J. B. Le Blond, Medicin Naturaliste, &c. (who formerly practised physic in Grenada,) the author mentions the yellow fever as having occurred frequently at that island, and with great mortality to strangers; adding, that "its malignity was shocking, in consequence of the unhealthy *exhalations from* the *port* and *town of St. George*. (See Medical Repository, vol. 10, p. 73.) And Dr. John Hunter, at p. 307 of his work on the Diseases of the Army, says, "The town of St. George lies *low*, and *there is marshy ground in the neighbourhood;* the troops, in such situations, have always been unhealthy," &c. These accounts are, however, too general; and therefore, I will add, from my own recollection, that this town is situated at the foot of a steep and lofty hill; (Mount Cardigan) and on both sides of a *ridge* descending from its summit towards, and into the sea, and forming what is called the *Carenage*, or harbour, at the bottom of a capacious bay. This Carenage is a long inlet or arm of the sea, in which vessels are *land-locked*, (except a small opening at the south-west, whence the wind rarely, if ever blows,) and are moored close to the wharfs; and here, both the atmosphere and water are commonly stagnant, in a considerable degree: the shore on this side of the ridge or town, is remarkably *low*, and crowded with houses and other buildings, reaching close to the wharfs; most of them small, built of wood, and filled with low and profligate inhabitants. And in addition to all these circumstances, obviously favourable to the production of yellow fever, there is a large *offensive marsh at the east end of the Carenage*, over which marsh, the wind commonly blew upon this inlet, and the greatest part of the town, during the months in which the supposed *new* fever became prevalent. But I would not have it understood, that those only were attacked with the fever who *lived* within the reach of marsh miasms. Mankind exercise their locomotive powers; and when these miasms are not brought to their habitations, they often go on business, or pleasure, to situations in which this cause of disease abounds. In this way, persons commonly residing or stationed on Richmond Hill, and Mount Cardigan, were sometimes attacked by the fever, though much less frequently, and with less violence, than others. Dr. Chisholm considered these attacks as the effect and proof of contagion; but the late Dr. J. Hunter had, nearly ten years before, thought more justly on this subject; for, at the page lately quoted, after mentioning the salubrity of these elevations, he added, "But in order that troops may enjoy the full benefit of such situations, care should be taken that they be *not permitted to go down to the low grounds*, for if they are, *they will infallibly carry fevers up* *with them.*" Dr. Lind had, indeed, previously convinced himself of this truth; for, at p. 217 of his work on the Diseases of Hot Climates, he observes, "that in several places, mentioned as a secure refuge from sickness," (by marsh effluvia,) "there are instances of persons being seized with the diseases of the adjacent country. The *yellow fever* has been known to seize persons in the garrison of *Monk's Hill*, in Antigua! But inferences (he adds) drawn from a few uncommon cases, have no force against observation and experience."—"unless the garrison of Monk's Hill had been apprised of the danger of sleeping in unwholesome places, and had cautiously avoided sleeping out of the garrison, the question cannot be fully determined, whether persons who never slept out of Monk's Hill, were attacked with the diseases of the adjacent country? It is my opinion, they seldom or never would.

"I mentioned the affair to a person who had resided long at English Harbour, in Antigua, and he informed me, that he had known some of the garrison on Monk's Hill to have had the yellow fever. I desired he would endeavour to recollect the circumstances of their being taken ill, and whether they had slept any nights preceding their illness in the *low* grounds, or in English Harbour. It immediately occurred to him, that when he was seized with the yellow fever, there were at the same time, two officers belonging to the garrison at Monk's Hill, labouring under it, who had both been seized with it early in a morning, after sleeping the two preceding nights at English Harbour. Upon beginning to consider the great danger of sleeping in unhealthy places, (with which he was before entirely unacquainted,) he recollected that most of the people in Monk's Hill, who had been seized with this fever, were taken ill after sleeping on the low grounds; it being a *common custom* among the officers of that garrison, to sleep in the house at English Harbour where they had dined and supped." I need hardly mention that English Harbour notoriously abounds in marsh miasmata. The application of these facts will be obvious.

Dr. Chisholm has laboured, as the *foundation* of his whole superstructure, to create an essential difference between his supposed *new* fever or pestilence, and the yellow fever, which had previously existed in the West Indies and North America; and for this purpose has employed the most strained, though unavailing efforts to *identify it with* the *true plague*; and he concludes a chapter on this subject by declaring, (vol. 1, p. 322,) that "We must consider the fever of Grenada, in the years 1793, 1794, and 1795, as *truly pestilential*, and differing from the *plague*, *strictly* so called, only in not *always* exhibiting those symptoms that are said to be peculiar to that malady." Surely, when Dr. Chisholm committed himself so unfortunately, he could not have recollected that the plague had never existed between the tropics. My readers, I presume, will not expect that I should employ their time, or my own, so unprofitably as I must do, if I were seriously to refute a conclusion so extravagant, after the facts recently stated in my chapter on the plague. Indeed, Dr. Chisholm has not appeared to be satisfied with his own opinion on this subject, for, without disclaiming, he has strangely deviated from it in several instances; sometimes representing his supposed *new* malignant fever, as resulting from "the *united action* of *pestilential contagion*, and the *miasmata of marshes*, and other direct causes of the yellow remitting fever," &c. See vol. 1, p. 208. And at other times, after laying aside his pestilential contagion, he substitutes that of typhus or jail fever, and represents *this* as having caused not only his supposed *new* fever at Grenada, and in other parts of America, but also that typhus which prevailed among the troops under General White, at Cork,* and afterwards in the passage thence to Barbadoes, in 1796, as I have mentioned at p. 344; thus deriving from *one* cause, *two fevers*, as different from each other as any within my knowledge. Dr. Chisholm seems, however, as little disposed to *adhere* to his jail infection, as to that of the true plague; for, in his letter to Dr. Haygarth, which is the most recent publication of his sentiments on this subject, we find these words, at pages 117, 118, viz:—

"To conclude, let the unbiassed reader now become *umpire*, and *decide* whether, or not, a '*nova pestis*, a *peculiar*, *original*, *foreign* '*pestilence*, *recently generated*, *and utterly unknown before*, endued 'with *a new* and *distinct character*, possessing *new powers of devas*'*tation*, and capable of *propagating itself by contagion throughout* '*the world*,' was introduced by the *Hankey*, on the 19*th of February*, 1793, *into Grenada*." He adds, "The *demonstration of my senses*, and the clearest perception of my mind, *assure me it* was a NOVA *pestis*, to which the observation of Thucydides is truly applicable; for never before had so dreadful a pestilence occurred in the West Indies, nor such a destruction of the human race been recorded."

Here it is remarkable, that though Dr. Chisholm declares the Grenada fever of 1793, to have been a *new*, *peculiar*, *original* pestilence, *never known until* the 19th of February, 1693; yet in the very next page, he makes it a *very old disease*, by asserting, that if it be compared with the plague of Athens, as described by Thucydides, "we shall find that they bear a *strict affinity to each other*, in their *causes* and *symptoms*;" and that "they seem, indeed, to *have been the same disease*, the *same destructive monster*." But though Dr. Chisholm excites our astonishment, and tries the extent of our credulity, by his accounts;—first of the miraculous creation of a *new* and terrible *pestilence*; and then of the almost as miraculous *revival* of one, which had been for *many ages extinct*, or unknown, he has kindly relieved us from these awful impressions, by stating in a letter to Dr. Davidson, dated Demerary, August 10, 1800, that a fever then prevailed in that colony, whose "features were almost without exception, *precisely those of the malignant pestilential fever of Grenada*, of 1793 and 1794;"—"*fully as fatal*, *as rapid*, and *as insidious*;" and that this *direful* fever *originated* in a ship, which had "arrived about the beginning of July, or end of June, from *Liverpool*, after touching at Surinam."—"The filth on board, (he adds occasioned by a *cargo of horses*, and the extreme neglect of the officers and crew, beggars all description." (See Medical Repository, vol. 5, p. 229.) Here we have Dr. Chisholm's *new pestilence*, *stript by his own hand* of every thing miraculous and wonderful, with which he had previously clothed it, as proceeding from *Africa* and the *Hankey*; and we are told

* See Dr. Chisholm's Essay, &c. vol. 1, p. 203, et seq.

that it was *produced* by a *common vessel*, making a *common passage from Liverpool to the West Indies*, and with *nothing more extraordinary than a cargo of horses*; of which, *thousands* of *cargoes* have at different times, been brought to the West Indies, particularly from New England, as closely stowed at least, and probably with sailors as neglectful, as those of the Liverpool ship, and all, as I believe, without any suspicion of having ever generated a contagious fever. Indeed, the only peculiarities which belong to a ship with horses, are those which occur in *stables* on shore, and which are not found or supposed to produce any morbid effect, much less to have ever generated any species of pestilence, or pestilential fever. Dr. Chisholm mentions in the same letter, that at the request of Drs. *Dunkin* and *Lloyd*, in *Stabroek*, and of Dr. Ord, on the Eastern Coast of Demerary, he had visited a few of the sick with *this fever*, and that he had "*no hesitation in pronouncing it a fever of infection*." No person will, I believe, suspect Dr. Chisholm of any great disposition to *hesitate* in pronouncing; though I have good reason to believe that the gentlemen whom he mentions as having called for his intervention, did hesitate;* and that they were very far from adopting the decision pronounced so readily; having been long acquainted with the *first* of them, and having had opportunities of conversing with him on this topic, several years after the date of Dr. Chisholm's letter. But I am entering unnecessarily into a refutation of Dr. Chisholm's inconsistent, extravagant, and incredible notions, which only *need* to *be stated*, and contrasted, in order to their being *properly estimated*.

Here, then, I might have desisted from any farther observations on this subject, if Dr. Chisholm's supposition of *novelty* in the fever of 1793, at Grenada, had not been countenanced, and in a great degree adopted, by Dr. Stuart, whose opinions I consider as justly intitled to a respectful attention. This gentleman, in his letter to Dr. Hosack, lately mentioned, has stated his reasons for believing, that the fever in question "was specifically distinct from every form of the indigenous bilious remittent," which he had ever before observed. These reasons are,—

1st.—"Because it appeared at a season of the year which I had always found healthy, during 19 years I had resided in the colony." But this, as I have already observed, can decide nothing. The occurrence of yellow fever, principally depends upon the temperature, and other conditions of the atmosphere, and if these conditions take place before the usual time, (as was the case, according to Dr. Chisholm's statement, in 1793,) the fever will be also *precocious*. It has been already mentioned, particularly at p. 245, that the yellow fever, which rarely

* In the month of February, 1805, the editor passed two nights as the *guest* of Dr. or Mr. Ord, (formerly surgeon to the 39th regiment,) when going from Demerary to Berbice, and returning thence; and was, *each* time, *distinctly* told by him, that he had *never seen any fever* in that colony, or on that coast, which he believed to have been *contagious*. The editor has also heard a similar opinion from Mr. Dunkin, lately Garrison Surgeon at Demerary, and now Deputy-Inspector of Army Hospitals, when conversing with him on this subject. Indeed this gentleman's very accurate and judicious report, concerning the prevailing fevers of Demerary, (see Chisholm's Essay, &c. vo. 2nd, p. 203, &c.) shews, that his ideas and reasonings were too well founded for him to have ever adopted opinions like those of Dr. Chisholm, in regard to the existence of contagious fever, in that part of America.

prevails at Charleston before the month of *August*, became very general and mortal there, in the month of *May*, 1732. A like precocity happened at La *Vera Cruz*, in 1802, when, according to a statement by the "*Consulado*" of that place, a mortal sickness, with a black vomiting, began there in *April*, and raged with extraordinary violence until October, causing the deaths of fifteen hundred seamen, and strangers from the interior and higher country. See Medical Repository, vol. 10, p. 296.

2ndly.—"Because it did not *particularly* appear in those situations, where bilious remitting fever usually prevailed, during the unhealthy season of the year." This being *a general* statement, I can only answer it generally, by reminding my readers of what has been already noticed, in different places, that the most violent forms of marsh fever, accompanied with black vomit, &c. commonly require for their production, the co-operation of all, or some of the circumstances or peculiarities of a *large sea port*, or commercial town, accessible to ships or vessels. And, therefore, yellow fever, strictly so called, often does not appear in some marshy situations, which are very liable to bilious remittents.

3rdly.—"Because there was an evident difference in the character and type of the two diseases; there was a *greater* despondency of mind in this fever; the eyes were *more* muddy and inflamed, there was commonly a deep-seated pain in the eye sockets, the motion of the eye balls was attended with uneasiness; the pain in the back and limbs was *greater* than in the bilious fever; the vomiting was not of *so violent* and *straining* a nature, nor was there such evacuations of *bilious* matter.* The black vomit generally occurred at an early period; the *yellowness* was of a *dingy hue*, not of the real *icteric* tinge, accompanying cases of bilious fever. The delirium was, in many instances, of a peculiar nature, and much resembling a state of intoxication; hæmorrhage was more frequent, particularly by urine, and from the stomach and intestines."

This statement, in my apprehension, relates chiefly to the *plus vel minus* of particular symptoms, and presents nothing *new*, or characteristic of a *new* disease: nothing, at least, of any importance, which had not been noticed and described by Towne, Warren, Lining, Moultrie, Hume, Hillary, Mosely, and others, as occurring in the yellow fever, long before the year 1793. Indeed, the differences here alleged, as distinguishing the supposed *new* fever, seem to depend solely upon an increased violence, or aggravation in the *usual* symptoms; and, therefore, never could, according to my conceptions, alter the nature or kind of a disease, or create even a new *species* of it.

I have already, in various places, adduced numerous, as well as *high* authorities, to prove that marsh fevers are liable to all the variations noticed by Dr. Stuart, and therefore I cannot persuade myself that they afford any ground for considering the fever of 1793 as a new disease. Perhaps Dr. Stuart had not sufficiently attended to this fact. He men-

* Dr. Chisholm asserts, vol 1, p. 174, that "the vomiting was, for the most part, *porraceous;* but towards the fatal crisis, always black, and resembling coffee badly boiled." It may be hoped, that he does not consider this as a new symptom, or as the mark of a new disease.

tions, in his letter to Dr. Hosack, that he had for nineteen years exercised his profession at a considerable distance from St. George's, and that his ordinary residence during that time was four miles from even the *small port* of Grenville; consequently his patients must have been in a great degree separated from those circumstances, or causes, which have been mentioned as modifying and aggravating marsh fevers, into that particular form which is called yellow fever; and as this fever had not for many years prevailed with so much violence and exasperation, even in the *large sea-port towns* of the West Indies as it did in 1793, Dr. Stuart might well have been strongly impressed by this aggravation of its symptoms, beyond any thing which he was accustomed to see; and believing as he did the extravagant reports which were circulated respecting the Hankey, he might naturally have been persuaded that the *fever* in question was a *new* disease.

This fever, at Grenada, appears in most cases, and especially those of seamen, (who were the greatest sufferers by it) to have resembled that variety or modification of the disease, to which the French applied the name of *Mal de Siam*, more than a century ago; and in which from a scorbutic taint (as I have supposed, at p. 237) or some other cause, hæmorrhages, black vomitings, and petechiæ, were predominant symptoms; occasioned, undoubtedly, by what has been called a dissolved state *of the blood;* which seems to occur (at least in some degree) before death, in all the more violent forms of yellow fever. And, by attending to this fact, we shall find no difficulty in accounting for the *early* appearance of *black vomiting*, in the fever of 1793, as mentioned by Dr. Stuart, nor for the *dingy hue* which, from *extravasations of blood*, was given to the "*icteric tinge*" of the skin; nor for the more frequent occurrence of hæmorrhage from the urinary bladder, stomach, and intestines. Nor can we be at any loss in explaining why the stomach, in that gangrenous state, which frequently attends the worst cases of the disease, should be less irritable, and less capable of violent strainings to vomit, than it is when less injured, as in the ordinary bilious fever; nor why the *least violent strainings* should produce the *smallest ejections of bile;* nor why exudations of blood into that viscus, should change the colour of the matters ejected, as they had done a century before. In regard to the 4th, 5th, and 6th, reasons mentioned by Dr. Stuart, viz. the fever's not having ever to his knowledge terminated "within a few weeks in an intermittent;" (as happens sometimes to bilious remittents;) its producing a greater degree of weakness; and its not admitting, at least with benefit, of an *early*, bold, and free administration of the bark, I must observe, that, to my understanding, they do not oppose any obstacle to the belief of this disease having been the yellow fever; which seldom changes to an intermittent; always produces extreme debility; and, in its inflammatory stage, before the appearance of a remission, is, I believe, always aggravated by taking the bark.

As Dr. Stuart's reasons, for thinking the fever in question to have been a *new* disease, were stated with candour and precision; and as they have been quoted and adopted by Dr. Chisholm, (at p. 24 of his letter to Dr. Haygarth,) I have chosen to answer them in preference to those which Dr. Chisholm has described with greater prolixity, and with a manifest effort to make this fever resemble the plague. His descriptions appear, also, to have been adapted, almost exclusively, to the *worst* cases; though he admits, that in many persons the fever was mild, and thought to resemble the ordinary bilious remittent. But the worst cases were best suited to his purpose of creating a distinction between these fevers;* he has, however, not only failed to establish any such distinction, but has even mentioned facts which prove that the fever of 1793 could only have been a marsh fever. The following are some of these facts, viz. (like all other marsh fevers it most *readily* and generally attacked *strangers* from *cold* or *temperate climates*, and with the most fatal consequences; sparing all others in different degrees according to their susceptibilities, as explained by me, between pages 177 and 197. At p. 140 of the first volume of his Essay, Dr. Chisholm has given, in regard to this disease, what he calls a "*scale* of its violence, or the gradation it observed with respect to the different classes of inhabitants;" which accords entirely with what I have repeatedly mentioned of the effects of marsh fever, upon similar descriptions of persons, in other situations. The highest place in this scale, is occupied by "sailors, more especially the robust and young; those least accustomed to the climate, and those most given to drinking new rum." The *next*, by "soldiers, more especially recruits lately from Europe, and the most intemperate." After these come "white males in general lately arrived, more especially young men

* Dr. Chisholm seems principally to *rest* the supposed *novelty* of the fever under consideration, and its character, upon a sort of petechiæ, or efflorescense, resembling *patches of red* or *livid spots*;" (vol. 1, p. 58) upon a "dinginess, or peculiar mixture of *livid and a dirty yellow*," (mentioned by Dr. Stuart) which he (Chisholm) whimsically ascribes to (a nonenity) "the *action of the matter of infection*;" (vol. 1, p 76) and upon the *frequency and profusion of hæmorrhages*; (p. 166) but all these symptoms obviously result from a dissolved state of the blood, which as well as the symptoms themselves, have been long noticed by almost every writer on the yellow fever. Dr. Moseley (p. 456) observes of it, that, in "the last stage," "the interior surfaces of the body are all *oozing* out blood, into their cavities; every excretion is corrupted blood;" "and internal hæmorrhage becomes general." Hillary says (p 151). ' in the latter stage of this fever, the blood is so attenuated, and dissolved, that we frequently see it flowing not only out of the nose and mouth, but from the eyes, and even through the very pores of the skin." He mentions, also, "*livid spots* in many parts of the body," and that they are multiplied after death. Dr. Moseley's description of Capt *Mawhood's* case, which occurred before the Hankey existed, affords all the *worst* symptoms of the *worst* cases of the supposed *new* fever of 1793 It was not, indeed, accompanied by Dr. Chisholm's "*dinginess, &c*," which I have mentioned (at the bottom of p. 47) as someties occurring in the disease, and which Dr. David Grant, in describing the yellow fever of Jamaica, calls "a *livid hue tinged yellow*." It is remarkable, however, that though so much importance is now attached to this symptom, Dr. Chisholm, in the first Edition of his Essay, did not mention or even allude to it; but contented himself with stating, as "a principal distinction," between the supposed *new* fever, and the bilious remittent, that in the former, "the yellow suffusion, *seldom* happened; in the latter always." See p. 128 of his first edition. The "dinginess" seems only to have been noticed by Dr. Chisholm, after his return to the West Indies in 1796, when, according to his representation, the malignant pestilential fever had ceased to prevail there, and the common yellow fever had taken its place.

I may here observe, that the fever of 1793, at Grenada, did not differ half so much from the common yellow fever, as it did from the fever which occurred in the Hankey, and among the colonists at Bulama; and it must be very absurd to derive the former from the latter, as its parent, and at the same time pretend that the supposed new fever, with its smaller variations from the common yellow fever, could not have originated from the causes by which it (the yellow fever) is produced.

from Europe." The places of other descriptions of persons, will be readily understood by what I formerly stated of their respective susceptibilities, in regard to marsh miasms: always recollecting that females are less susceptible than males, children less than adults, and negroes least of all. The greater liability, or predisposition to the disease in seamen, than in any other description of strangers, Dr. Chisholm ascribes with reason, to their "violent exercise in the sun, the immoderate use of undiluted rum; bathing in a state of intoxication, and often when violently heated; and the sleeping on deck during the night." (Vol. i, p. 124.) Indeed, these causes *alone* will often produce yellow fever. Dr. Chisholm has, however, suggested another, (p. 302,) as *perhaps* rendering "the disease infinitely more fatal" to them, than to "any other class of men," viz.—a scorbutic taint, which I formerly mentioned, as likely to have occasioned that variety of the disease called by the French mal de Siam. In regard to the military, at Grenada, he says, "that nearly one half of the 45th regiment" were in the barrack near the carenage, and that "*all** the officers and men were successively seized with the disease; but it proved fatal *only* to the *recruits who had lately joined.*" Vol. i. p. 132. The case was similar in the *ordnance* department; of those who had become seasoned, in some degree, to the climate, only five died out of 56; but of *recruits just arrived*, 21 died out of 26; and this with all the advantage of Dr. Chisholm's skill, two months after he had discovered the wonderful benefits of mercurial salivation, in this fever. See vol. i. p. 133.

After these instances of the frequency and violence with which persons newly arrived from *northern climes* were attacked by the disease, (as always happens with marsh fevers exclusively) he proceeds to the other extreme, and mentions field negroes, as being the most exempt from it; though that mixed race called people of colour, seem to have enjoyed this exemption in a very high degree; for Dr. Chisholm tells us, that "the inhabitants of the district of Montserrat," (and a part of St. George's, adjoining the carenage) are almost all *free* people of colour; and among them the disease *never appeared, affecting their own persons;* many of the sick, (he adds) from the shipping, were accommodated in their houses; but a *peculiarity* of *temperament saved* them generally." Vol. i. p. 128. This peculiarity of temperament might, indeed, well save them from the morbid effects of marsh miasms, as it has always been found to do, when they had not *lost it*, by long residence in a cold climate; but negroes and people of colour,

* It is difficult to understand what quantity, or *proportion*, Dr. Chisholm means by "*all.*" He says, at p. 134, vol 1, of his Essay, that "about the middle of June, the disease broke out in the 67th regiment, and among the artifices and labourers on Richmond Hill." "*all* were successively seized with it; but it fell heavier on the officers than the men; several of the former, being young men lately arrived from Europe. He *adds*, that "of about 300 men, at that time the strength of the 57th regiment, only about *sixty* were seized with the symptoms of infection and of that number only *three* died." Here we find that *all* signified only *one-fifth part*, and that the disease proved fatal only to one in twenty. Did Dr. Chisholm form his description of the disease from its general appearance on Richmond Hill? Or did he conclude the disease to be *direfully* contagious, because only 60 out of 300 men took it, and doubtless, by going down the hill, and within the reach of marsh miasmata?

have no *peculiarity* which will protect them from the morbid effects of pestilential and typhus contagions; on the contrary, they are, *as has been proved*, the *first* and *greatest sufferers* by these contagions, and this *single fact would alone overthrow* Dr. Chisholm's whole system.*

In regard to the termination of this epidemic, Dr. Chisholm states, (vol. 1, p. 136) that "from about the middle of September, till the month of February, of the year 1794, the disease seemed to have disappeared every where in Grenada." Why a contagion, with such irresistible power, should have become extinct when there was no want of persons susceptible of it, he has not explained; but, in looking over his account of the weather we find, in vol. 1, p. 90, the following statement, viz. "September, the greatest part of this month *remarkably rainy*, attended frequently with most vivid lightning and tremendous thunder, and *violent squalls*." Of October, he says, "*much rain fell in this month also*," "but not in the *violence of* the last." He adds of November, "five days excepted, the *whole* of this month was *uncommonly rainy*;" "a great deal of thunder and lightning." "December was also very rainy." That these *excessive and long continued rains* (assisted by the salutary effect of violent squalls, thunder, &c.) should have put a stop to the fever, by diluting the marsh miasms, and washing away the materials necessary for their production, will, I am persuaded, be thought highly probable, by those who may have attended to the facts mentioned in the former parts of this work. Concerning its return, in February, 1794, as the Hankey did not then *return*, it might have been expected that Dr. Chisholm would have found some difficulty in giving a suitable explanation, but having before luckily sent the contagion to Philadelphia by some unknown vessel, and from some place, respecting which there are very contradictory accounts, he contrives to have it brought back to Grenada, by the *way of Martinico*, where, after it had been deposited by one vessel, it was taken up by another and left at Grenada. But unluckily, though he was then at Grenada, Dr. Chisholm is unable even to hint at her *name*, or the name of *her captain*; a sort of *inability* which (considering how much greater difficulties he had previously surmounted) I should not have expected; especially as he says the sick belonging to this vessel, and by whom Grenada was the *second* time *infected*, were placed under his "*charge*;" and that he "had no doubt respecting the identity of the disease." Really, I should have believed that the books of himself and his partner, must have contained the names at least of the vessels, to which patients of that description belonged; as they certainly did not bestow medicines

* At p. 302, of his first volume, is the following passage, "why, however it" (i. e. the virus of his supposed contagion) "should operate with *most violence on Europeans just arrived*, and *who had never entered the Torrid Zone* before, is a *singularity* I do not pretend to explain." This *abstinence* from explanation, though unusual in Dr. Chisholm was, I think, *discreet* on the present occasion. Certainly, if his supposed contagion had existed, and manifested this wonderful *predilection* for Europeans, at their first entering the Torrid Zone, it would have been a singularity never observed in any other contagion; and therfore well calculated to discourage him from any attempt to explain. The singularity, however, and the contagion, were but creatures of his own imagination

and attendance gratuitously. I should have, moreover, expected, as Dr. Chisholm *had no doubt of the identity of their disease*, with that which had recently done so much mischief, that its *re-introduction* would have been deemed by him a matter of high importance, and that instead of such unexampled negligence, he would have spared no pains to collect and preserve all possible information on the subject.

I have stated, in a note, at p. 260, that Dr. Chisholm's adherents in Philadelphia, convinced that they could not otherwise establish any distinction between what they represented as his Grenada fever, and the marsh bilious fever of that country, had assumed contagion *as forming its distinguising character;* and Dr. Chisholm has followed their example, doubtless for the same reason. At p. 200, vol. 1, he says, "the most remarkable distinction between the malignant pestilential fever, and the yellow remitting, is contagion;" this assertion he repeats at the next page, and then very properly endeavours to prove the existence of this contagion; but here he unfortunately adduces for his first and principal proofs of it, the absurd story concerning the ship *Herberts*, lately noticed; and his equally absurd notion "that much of the melancholy fate of the army collected at the Cove of Cork, under major-general White, about the end of the year 1795, for St. Domingo, is to be attributed to the infection of the malignant pestilential fever." Thus making yellow fever the parent of *typhus*. These being evidently, in his own estimation, his best proofs and instances of the contagion, which is supposed to *distinguish* the fever of 1793, I shall not be expected to notice the others, after all that I have before written on this subject.

Dr. Stuart, in his letter to Dr. Hosack, mentions of this fever, that "its contagious nature appeared from many instances of men in 1793-1794, going to St. George's on business, and being attacked a few days after their return to the country, with fever; to several of whom it proved fatal." This, however, is only what commonly happens to persons, exposing themselves to the influence of marsh miasmata; and what has been often described, as happening to persons going into towns where the common yellow fever prevailed, long before the Hankey existed; which yellow fever, Dr. Stuart (as well as Dr. Chisholm) believes never to have been contagious. Dr. Stuart, indeed, with laudable candour, makes this addition to what has been just quoted from his letter, viz. "but I must observe that I met with *no instance* in the country, of the disease being communicated to others, either visitors or attendants. It is true that every attention was paid to keep the chamber of the sick well aired, their linen frequently shifted, and when a fatal issue took place, every article of wearing apparel and bedding was *commonly* destroyed." This destruction, however, was probably of no use, as it only took place in fatal cases, and not invariably even in these, as I conclude from the expression "*commonly;*" and as the cases which were not *fatal*, would have equally infected the *undestroyed* clothes and bedding, from which no harm resulted. The airing of the sick apartments might, indeed, have been effectual, against typhus; but not against any such virulent and powerful contagion as the Hankey is supposed to have imported. This part of Dr. Stuart's statement is confirmed by Dr. James Clark, in regard to the fever which began at Dominica, in June, 1793, and which Drs. Stuart and Chisholm suppose to have been the same with that of Grenada, and derived from the same source. Dr. Clark's statement on this point I have already quoted at p. 296, and I may add that as no precautions were there employed to hinder the operation of contagion, I am persuaded that if none had been used at Grenada, the fever would have proved equally incommunicable. Dr. Clark says, (p. 61) "that the common remittent fever, dysentery, and other bilious complaints, had begun to shew themselves previous to the appearance of the yellow fever" in 1793; a plain indication of the prevalent influence of marsh miasms. The same author, at p. 22, makes the following observations concerning the supposed new fever, viz. "I have been informed that it has been considered, by some, as an imported and very infectious disease; but in this island it did not appear to be either imported or infectious. The very few instances which seemed to indicate contagion, I think, may be accounted for on other principles. Some inhabitants, who had been accustomed to breathe a cool healthy air, in high situations in the country, were sometimes attacked after a visit to town in the same manner as new comers from Europe and America, who never had been in the West Indies before; the reason of which will be enquired into hereafter. Those who had *resided long in town, or near the sea side, were not attacked* with it. The *physicians* and *surgeons* who visited the sick, and *the nurses who attended them constantly, were not infected;* nor did there occur a *single instance*, of one of them *being seized with this fever* for these *three* years, that I have remained in the island, since it broke out: although *no* prophylactic, or *precaution of any sort whatever, was made use of to counteract or avoid contagion.* I am, therefore, of opinion, that this terrible disease was not imported into *this* or *any other* of these islands, or into America; but that it was produced from natural causes."* Of Dr. Clark, Dr. Chisholm has stated, at p. 258 of his 2d volume, that he was "a physician of great eminence, whose practice for five and twenty years, in the West Indies, furnished a most ample field for observation and experience;" and it appears that he was not singular in believing that the fever in question was not contagious. For Dr. Chisholm states, (vol. 2, p. 254) that "Dr. Fillan," who "has been an eminent practitioner in Dominica, for near twenty years," "*imagined* he could perceive nothing contagious or infectious about it, i. e. the new fever:" this *negative* exercise of the imagination, however, is at least unusual; persons having much oftener imagined they could perceive what does exist, than the reverse. It appears, indeed, that it was not Dr. Fillan who thus exercised his imagination, but Dr. Chisholm, who thought it most convenient to represent Dr. Fillan as having *only imagined*, when in fact he had *asserted*, that he could perceive nothing contagious, &c. for he immediately subjoins the following sentence, viz. "on questioning the doctor *close-*

* Dr Roberts, Physician to the forces, who was at Grenada during the great mortality there, in 1796, and who, probably, saw more than 1000 cases of the supposed malignant pestilential fever, declares that he could discover no sufficient reason for considering it as a disease different from the common yellow fever of the West Indies, with which he was well acquainted; having been born at Antigua.

*ly* on this subject, *I perceived* that no conclusive or well-founded reasoning could be adduced in support of this *assertion.*" That Dr. Chisholm should not think any reasoning conclusive or well-founded, which had induced another person to *imagine* he could *not* perceive that contagion, which has so long occupied and bewildered his own imagination, I can readily believe; and though I cannot applaud the *candour* of his statement, I am glad that it contains less of misrepresentation than many of those which I have found it necessary to controvert. But though Dr. Chisholm could not, by "closely questioning," hinder Dr. Fillan from *asserting* his inability to discover any appearance of contagion in the supposed *new* fever, he seems, from his own account, to have been a little more successful with Dr. Fillan's "*assistant.*" For the latter, (who is styled, by Dr. Chisholm, an "*eminent practitioner*") after declaring that he had neglected to make any "inquiry so as to establish the source of the disease in *any* instances," is said to have readily "acknowledged, that had an investigation been instituted, with the care and attention the subject merited, *he had reason*" (where did he get this *reason*) "to think that contagion *might* have been detected, as the cause of the disease, *in every instance.*" If Dr. Chisholm has not misrepresented this (probably *young*) gentleman's language, it may be well that his complaisance, or his imagination, did not carry him farther.*

But after all this, Dr. Chisholm, who has repeatedly stated his supposed new fever, or its contagion, to have been carried from Grenada to Dominica, (though without designating any mode, or vehicle, or person, by whom this mischief was perpetrated,) tells us, at p. 256 of his 2nd vol. that "the first appearance of the disease (at Dominica) was in a ship, called the *Providence* of *London.* She arrived (he adds) about the 8th or 9th of June, and the first case of fever appeared about the 13th: many of the crew were attacked immediately after, and died. The fever appeared in the neighbouring ships, and *spread* so *rapidly*, that about the 20th, scarce any were free from it." And *this*, Dr. Chisholm (whose intellectual and perceptive faculties appear *inacessible* to all ideas, which do not accord with his supposed contagion,) considers as a proof, that the disease was communicated from the sick to the well, and from one ship to another, *in succession;* and *all in the space of one week.* A *monstrous supposition!* utterly incompatible with every thing known of the different kinds of contagion; among

* As an illustration of the truth of this remark, I will here mention a statement made by Dr Chisholm (at p. 257, of vol. 2d.) on the information, as he alleges, of Dr. Fillan's assistant It relates to a Dr. Wilson at Dominiea, who resided about 12 miles from town, and who three days after returning to the country from town, where he had visited persons labouring under the prevailing fever, was astonished to perceive a number of *carbuncles*, spreading round his neck, and towards his face and breast. The progress of these was so great, as to threaten a mortification. In this distress, he sent an *express* for Dr. Johnson. Dr. Fillan's assistant "who immediately visited him. By the time he arrived a mortification had actually taken place, and Dr. Wilson died a few hours after, whilst endeavouring to examine the state of his neck in a looking glass. The description of the sores on this patient's neck, says Dr. Chisholm, *left no room to doubt they were pestilential carbuncles.*" Thus has Dr. Chisholm's heated, distempered imagination, created the *true plague* at Dominica; and that it might not be created in vain, he immediately adds that "an immense number of the inhabitants were seized and many died." Is it possible that he can have believed and wished to make others believe, that these persons died of the true plague?

which, there is scarcely *one*, that could have so soon produced disease, *even in two persons successively.* When ships are placed within the reach of those miasms, or local causes of disease, which, at certain times, commonly produce the yellow or other marsh fevers, in the harbours of the West Indies, their crews being acted upon, nearly at the same time, by the morbid exhalations, many persons will naturally fall sick almost simultaneously: and nothing but such a cause, could have produced the effects just mentioned, as having occurred at Dominica. But how Dr. Chisholm expected to reconcile the first appearance of the disease, in a *London* ship, just arrived at Dominica, with his assertion of its having been derived from Grenada, I am unable to conceive.

Believing that it would be not only superfluous, but tiresome to my readers, as well as to myself, if I were to carry this refutation any farther; and presuming, that I have sufficiently demonstrated the fallacy of all those statements and arguments, by which Dr. Chisholm has laboured to prove the fever in question to have been derived from the Hankey; and that there is no good reason for considering it as *specifically different* from the common yellow, and other marsh fevers, which have been proved to be void of contagion, I shall here dismiss the subject; adding only a few observations, to justify the uncourteous and disobliging expressions which I have been sometimes forced to employ, in regard to Dr. Chisholm. Had this gentleman contented himself with stating *fairly* and *correctly*, the facts and reasonings which occasioned, and were by him thought capable of supporting his most extraordinary opinions, I should have left the public to judge of their sufficiency, and have gladly avoided every appearance of controversy with him. But instead of doing that, which would have most benefited the cause of truth, and the interests of mankind, Dr. Chisholm, with great perseverance, and very little temperance or moderation, has laboured to propagate and maintain these opinions, by assertions as positive as they were unwarranted; and by statements of pretended facts, which were destitute of any foundation in truth, and at the same time of such *decisive import*, that a belief of them, must necessarily produce an adoption of the opinions intended to be thus maintained; which opinions, I sincerely believe to be not only erroneous, but likely to obstruct the progress of medical science; perplex and frustrate all endeavours to elucidate the important subject of contagion; and, moreover, to produce all those mischievons consequences which I have mentioned at p. 332—336. Under this conviction, I have thought it my duty, to endeavour, at least, to *rescue* and *vindicate the truth*,* from those fallacious arguments, and confident misrepresentations, with which it has been surrounded and obscured, by Dr. Chisholm; and for this purpose, to contradict, and confute the untruths advanced, (I will not say fabricated,) by him; and demonstrate the little reliance which (either from his extreme want of caution, or excessive anxiety to overcome his opponents,) ought to be placed on his supposed facts and representations, in regard to the origin and nature of the fever of 1793.

* Dr Chisholm, at p. 219 of his 1st vol. has declared, that "the cause of truth is *paramount to all other considerations;*" and has assigned this as his motive, and justification, for contradicting Dr Rush; professing, at the same time, to entertain the highest opinion of "the humanity, the genius, and the professional skill of that gentleman."

For such contradictions and confutations, I have found it necessary to employ *adequate terms;* but I have done it reluctantly, and have invariably preferred the least offensive, as far as I could do so, without weakening my conclusions, or leaving my ideas imperfectly expressed.

This explanation I have thought due to *my own character*, rather than to Dr. Chisholm. For, whilst endeavouring to spare his feelings, I have believed that very few writers were less entitled to indulgence and tenderness in this respect: because, with a few exceptions, he seems to have exercised neither towards those by whom his statements and opinions are controverted. His Letter to Dr. Haygarth abounds in offensive, arrogant, and injurious language, particularly at p. 75, where he designates the most respected and meritorious physicians of New-York and Philadelphia, as " pertinacious theorists."—" *Disturbers* and *destroyers of society:*"—and as deserving " *to be execrated* by their fellow citizens:" and this, only because they had controverted his opinions, respecting the fever of 1793. And again, at p. 159, he has generally accused all who deny the supposed contagion of that fever, of " a *predetermination* to *break down the barriers, which alone can secure mankind* against the *calamities*," arising from " *infection and contagion*," " *universally diffused*, and *universally destructive of the human race*." He had previously, in his preface to the 2nd edition of his Essay, plainly intimated, that the " Medical Staff" of the army, under Sir Ralph Abercrombie, in the West Indies, were chargeable, in a great degree, with the deaths of 13,437 soldiers; and, at p. 243 of that volume, he has accused them of a species of " conduct, which led to the pernicious, indolent, and unscientific habit, of prescribing for the *name* of a disease:"—" prevented the investigation of the principles on which practice, in any particular distemper, should be founded;"—" which tended to the discredit of a mode of treatment of the epidemic, from which alone, success could be expected:" and which was, " *in short, the paramount cause of the mortality which has disgraced* the annals of the West India Medical Staff of the day."* The foundation of these *outrageous* accusations, and the delinquency to which they relate, is nothing more than a belief or pretence, in Dr. Chisholm, that the medical officers of the army in question, did not sufficiently *adopt* and *persevere* in the practice recommended by him, of giving or applying mercury to excite salivation, in the yellow fever; a practice, of which I have delivered my opinion, at p. 73—78; and I will here observe, as one of the physicians to that army, that there was nothing in Dr. Chisholm's situation, which could make it our duty, or even warrant us, to persevere unsuccessfully, in the trial of his strange and unpromising innovation.† He had only been known as a practi-

* The mortality with which the medical officers of Sir Ralph Abercrombie's army are here reproached, was certainly great, and ever to be deplored; but it did not, in any instance, extend to 21 out of 26 patients, as happened to persons under Dr. Chisholm's care, after his *pretended* discovery of an almost certain remedy.

† It is remarkable, that while Dr. Chisholm brings such heavy accusations against the medical officers of the *army*, for not having sufficiently employed mercury to cure the yellow fever, Dr Gillespie, who was then in charge of the *naval* hospital at Fort Royal, in Martinico, declares, "that many of these gentlemen fell *victims* to their implicit faith in mercurial medicines which had been lately supposed so efficacious in epidemics, of a

tioner at Grenada; and when there, could not have pretended to an *equality* with several others, in public estimation. Nor did I find that his boasted success at that island, from the use of mercury in the yellow fever, had been *known* there, until his *printed accounts* of it had *arrived from Europe, nor that it was believed when thus made known.* I feel, however, no resentment against Dr. Chisholm, for the obloquy which he has thus attempted unjustly to throw upon me among others, though I sincerely regret, that his acknowledged talents and industry, were not more judiciously and beneficially employed.

similar nature to those which then reigned." Meaning the Grenada fever, and Dr. Chisholm's account of it. See Observations on the Diseases," &c. "of his majesty's Squadron, on the Leeward Island station," &c. p. 19.

# APPENDIX.

## NO. VIII.

At pages 204 and 205, I have explained the subject and motives of this appendix; and the following is Dr. Blane's statement, (therein mentioned, to the American and Prussian ministers, viz.

"On the 16th of May, 1795, the Thetis and Hussar frigates, captured two French armed ships from Guadaloupe, on the coast of America. One of these had the yellow fever on board, and out of fourteen men sent from the Hussar to take care of her, nine died of this fever before she reached Halifax, on the 28th of the same month, and the five others were sent to the hospital, sick of the same distemper. Part of the prisoners were removed on board of the Hussar, and *though care was taken to select those seemingly in perfect health, the disease spread rapidly in that* ship, so that near *one third* of the whole crew was more or less affected by it.

"This fact carries a conviction of the reality of infection, as irresistible as volumes of argument; and, it further affords matter of important and instructive information, by proving that *the infection may be conveyed by the person or clothes of men in health.*"

Numerous facts and considerations, most of which have been already mentioned, having induced me to think it impossible, that the yellow fever should have in this instance manifested properties, which it had been found not to possess in a multitude of others, I was solicitous to investigate the circumstances of this transaction, as minutely as possible, in order to ascertain the truth, on a point which I have long considered as of high importance to mankind; and I therefore applied to the commissioners of the transport department, for permission to inspect the medical journals of the surgeons of his majesty's ships Thetis and Hussar, for the year 1795, and the correspondence of the surgeons of those ships with the sick and wounded board, for the same period, and was very soon honoured by a letter from Sir Rupert George, Mr. Serle, and Dr. Harness, dated 14th October, 1807, and informing me that "the journals were ready for my inspection, but that there did not appear to have been any letters received from the surgeons of those ships during that period." I lost no time in availing myself of this permission; but as the journals in question did not afford all the desired information, I extended my researches to the navy office; after which, I thought it my duty, fairly to communicate the results of my inquiries to Dr. Blane; and having done so, he thought it proper to write the following letter to Dr. Wilson, now physician to the Plymouth hospital, and formerly surgeon to the Hussar frigate, viz:

"*London*, *Nov.* 17, 1807.

"Sir,

"You may possibly have seen, in some of my writings, that I have adduced as a proof of the infectious nature of the yellow fever, that it spread from the Raison, French prize, to the Hussar frigate, in May, 1795. There is, in my opinion, sufficient proof of this fact from other quarters, but as you were then surgeon of the Hussar, and as I find by my notes, that it was partly on your testimouy that I built, I take the liberty of putting some questions to you on this subject. What leads to this is, that Dr. Bancroft, a respectable medical gentleman of this place, who has paid much attention to the subject of infection, but who is of a different opinion from me on this point, means to controvert my statements of that case, and founds his proofs chiefly on your journal, which he has not only inspected, but has copied it for the months of May and June, 1795, and has very candidly called on me, and shown it me. There is certainly nothing in that part of your journal which can be construed into a proof, that the ship's company of the Hussar had the same fever with the prisoners. But the composition of it bears marks of very great haste; and no description of the symptoms is given, except a reference to the case of Mr. Backhouse, who was not taken ill till a month after the battle, and whose symptoms have nothing characteristic of the yellow fever. I am sure, however, from my notes, that either in some other parts of your journal, or in those accompanying remarks, enjoined by the form of the journal, or in your letters from the hospital, there must have been some further testimony from you on this subject. The purpose of this letter, therefore, is to request you to consult your memory, and your papers, regarding it. You will certainly recollect, whether or not, the men of the Hussar, either on board, or at the hospital, were affected with a fever attended with yellowness of the skin, and coffee-coloured vomiting, and in what numbers; also, whether only the prisoners apparently in health, or both the sick and healthy, were brought on board of your ship? If you should be able to give any satisfactory information, either for, or against, these facts, I should hold myself extremely obliged to you, and I am sure so would Dr. Bancroft, in order that neither of us may be led into error, in so material a point. There is a reference in your journal, to some letter you wrote from the hospital, but as Dr. Bancroft could not find it, it has, no doubt, been mislaid at the office; but it would be a great satisfaction to us, if you should have kept a copy of it. In short, if you will have the kindness to send all the information on this affair, which your memory, or written documents, can supply, and your time will afford, you will greatly oblige

Sir, your most obedient,
and very humble servant,

GIL. BLANE.

"Dr. Wilson, Plymouth Hospital.

"P. S. I ought to have mentioned that Dr. Bancroft has also inspected the muster-books of the Hussar, at the navy office, and finds the prisoners in *sickness as well as health* were brought on board the Hussar, contrary to what I have stated in my letter to the American and Prussian ministers, which you may probably have seen in print."

To this letter the following answer was returned, viz:

"*Royal Navy Hospital*, *Plymouth*, *Nov.* 22, 1807.

"Dear Sir,

"I am much afraid I have lost the journal of the sick kept by me, at the time the yellow fever prevailed on board the Hussar; I have, however, found a copy of the letter, which I transmitted to the sick and hurt board, on that occasion. I have, agreeably to your wishes, *transcribed the greatest part of it*, and *herewith enclose it for your perusal;* if *plain* matter of fact can have any weight, you will there find sufficient to establish fully your observations on the contagious nature of that disease. Should any other testimony be wanting, by a reference to capt. Beresford, who then commanded the Hussar, and now commands the Theseus, further particulars may be obtained.

"Mr. Backhouse was subject to frequent attacks of *chronic rheumatism;* was, in consequence, sent to the hospital at Halifax;—first, on the 27th of June, 1794—and again in November following. Many were afterwards, at different periods, sent for the same complaint, and particularly *two* on the 28*th of May;*—one on the *first*, and another on the 17th of June, 1795.* Their treatment being nearly the same as that of Mr. B. a reference, in order to avoid repetition, was in consequence always made to his case. This, I trust, will explain, why no symptoms, in common with yellow fever, could be found in the cases alluded to, or any thing that could lead one to believe, the Hussar's ship's company to have the same fever with the prisoners.

* This accords with Dr. Wilson's Journal; but in his subsequent letter to Dr. Blane, he admits, that the entry relating to the 17th of June, could not have been correct; "as from the 6th of that month, to the 4th of July following, we (the Husser) had no communication whatever with the hospital." It must consequently follow, that the entry in his journal of another name, as sent to the hospital, the 20th June, must also be incorrect.

"I do not recollect the number of prisoners received on board the Hussar: I only remember, *that the sick were not removed from the Raison; that they remained in the prize until her arrival in port*, and *that it was only those in health that were permitted to come on board of us;* the names of the sick, as well of those in health, were, as is customary in such cases, indiscriminately entered on the books of the Hussar.*

"The number of the Hussar's ship's company affected with the disease, appears to have been 98. The number of deaths on board the Raison, between the 16th and 28th of May, were (was) . . 9

The number of deaths at the hospital, between the 28th of May and 6th of June . . . . . . . . . . . . . . . . 6†

"Total 15

"The above will, I believe, be found a correct statement.

"I am, dear Sir, &c.

"J. WILSON.

"To Dr. Blanc."

* The only way in which I can reconcile this with the opposite testimony, is by supposing, what appears in other respects probable, that there were *no sick on board La Raison.* I found, by the books at the *Navy Office*, that "*head money*" had been paid to the *captors* for 116 prisoners, as having been on board the *Raison*, at the *commencement of the action;* and it appears from the *master-book* of the Hussar, that she received the same full number of 116 men on board, *from* La Raison, on the 16th of May, and that *every one* of these men were delivered from the Hussar to *the Security, prison ship*, at Halifax, on the 28th of May; consequently none had been killed in, or died subsequently to the engagement of the 16th of that month. Their names are *all* stated in the book at full length; and under them is a certificate, signed by John Beresford, captain, and Francis Prior, declaring "to the Commissioners for Victualling his majesty's Navy," "that the before-mentioned prisoners, beginning with the name of A. L. Le San, and ending with that of Anthony Pignon, were *actually victualled* at *two-thirds* allowance, the number of days as specified by the several entries and discharges." These entries and discharges include all the days from the 10th to the 28th of May; and aginst the names is a column, for them when mustered, and in this column the 23d of May is written against each name, or the letters Do. under that date. I inquired particularly of the gentleman at the office, whether it might not be possible that these letters should be affixed to the names of some of the prisoners who had remai ed on board the Raison, and was answered in the *negative;* with an assurance, that every man against whose name those letters were written, must have been actually on board; and that if any man had been sick, so as not to receive his allowance, or absent from the Hussar for a single day, it would have been *noted* in the column, or "*checked*," as they express it: and in confirmation of this, I was shewn several instances of such checking, in the books. It is to be recollected, that the "*two-thirds*" is the *full* allowance to prisoners *in health;* and it would have been *fraudulent* to make a false entry in this respect: and, moreover, it is highly probable, that they must have been in health, because at Halifax they were *all* sent, *not* to the *hospital*, but the prison ship; and none were sent thence to the hospital until three days after; and even at that time, it does not appear that any one sent was ill of fever. That they would have been sent *earlier* from the prison-ship to the hospital, if they had been ill *sooner*, is manifest, because two of the Prevoyante's crew were sent from the prison ship to the hospital, one the 29th of May, and another on the 30th, as appears by the hospital books, and being all prisoners made at one time, and in the same circumstances; the men belonging to the Raison, if any had been *ill* on those days, would *have naturally been sent at the same time.*

† Dr. Wilson has stated and admitted, several times, what the books of the hospital at Halifax prove, that the whole number of men belonging to the Hussar, who were sent to this hospital, in May and June, 1795, amounted only to 12; and, in his Letter to Dr. Blane, of December 20th, 1807, he declares, that six of these were sent back to him, on the 6th of June, and that they all recovered. He also states, that two of the remaining *six*, were cases of rheumatism, in the persons of David Sullivan and Nicholas Martial, who, by Dr. Wilson's Journal, are stated to have been put on

' The following is a copy of the *transcript*, mentioned in the preceding letter, of "the greatest" part of Mr. Wilson's Letter "to the Sick and Hurt Board," viz.—

"On the 16th of May, 1795, his Majesty's ships Thetis and Hussar, cruizing off the Cape of Virginia, fell in with five French armed ships, two of them, after a severe action of about an hour and a half, struck; they had sailed some time before from Guadaloupe, where the yellow fever had prevailed, and had carried off numbers of the French; unfortunately for us, La Raison, the ship we took possession of, had suffered greatly from this disease; several of her crew having died on the passage, and at this time many were found *confined to their beds, in a state of the greatest debility*.* Capt. Beresford, in order to prevent, if possible, the Hussar's ship's company being infected, ordered all the prisoners who had the least appearance of disease, to remain where they were, and those only who appeared to be in a perfect state of health were allowed to come on board of us. Frequent communication was, however, found unavoidable; and, notwithstanding every precaution was taken, I was extremely sorry to find, that before we reached Halifax, we began to experience the sad effects of the contagious nature of this disease. And of fourteen fine, young, healthy seamen, with officers, &c. ordered to conduct the prize into port, five only survived their arrival;† and four of those in so debilitated a state, as to render it necessary to send them without loss of time to the hospital, together with *six* others, that had been affected with the same complaint on board the Hussar. The disease now began to assume so *serious* an appearance, that it was deemed proper to send them on shore the *moment* they *complained*, where *few survived longer than three or four days*.‡

the sick list of the Hussar; one on the 1st, and the other on the 4th, of May: and they continued sick, and were sent to the hospital at Halifax, on the 28th of that month, and entered as cases of *fever* in the hospital books. But as neither of them died, the deaths in the hospital could only have been *four*, and not *six*, as is here stated. It is moreover to be remarked, that though these two men are stated to have been *constantly* on the sick list in *May*, Dr. Wilson, in contradiction to his own Journal, asserts, in his Letter to Dr Blane, of the 20th of December, 1807, "that for some time *previous* to the action of the 16th of May, 1795, *there had not been a man on the Hussar's sick list.*"

' * In the preceding note, I have stated documents and reasons, which make it *impossible* for me, at least, to believe that this can be correct; and this impossibility will be increased, by facts to be adduced hereafter.

† Facts are here equivocally, or obscurely stated;—nine only of the Hussar's crew died on board the *Raison*. We are no where informed, how many officers were put on board that ship, in addition to the 14 seamen. But it appears that no person of that description, either died, or sickened, by going on that service.

‡ Dr. Wilson here admits what may be otherwise sufficiently proved, that *ten* of the Huzzar's crew went to the naval hospital, at Halifax, ill of the supposed yellow fever; none of them, however, as appears by his own Journal, was sent until after two days *illness* Of these ten men, he states in his letter to Dr. Blane, of the 20th December, 1807, that he received back *six*, on the 6th day of June, and that they all recovered. It appears also, by the books of the naval hospital, at Halifax, and by a letter to the commissioners of Sick and hurt seamen, written by Mr. Haliburton, surgeon to that hospital, and dated the 25th of June, 1795, that at the date of this letter, only *three* persons had died there, of the supposed yellow fever; and, in fact, no more than *four* did die, in the *whole*, of that fever, at Halifax; and this fourth man appears, by the books of the hospital to have *relapsed*, and died on the 30th of June. With a knowledge of these facts, I have felt *no little astonishment*, at finding that Dr. Wilson had permitted himself to assure

"The idea of an infectious disease, which had in so short a time proved fatal to so many seamen, alarmed the inhabitants of Halifax and its vicinity to such a degree, that they made application to Captain Roddam Home, commanding his majesty's ship Africa, and then senior officer in port, to order the Hussar, officers, and ship's company, together with those yet alive at the hospital, to be removed to the opposite uninhabited shore: the request was immediately complied with. Tents were erected for their accommodation; and on the morning of the 6th of June, 1795, at day-light, the Hussar was anchored at the place pointed out for that purpose. The following is an extract from the Physical Journal kept by me, during the time the sick were under my care; the *symptoms of those taken ill when on shore*, were observed to be the *same as in those first taken ill* on board the Hussar and Raison.

"On the commencement of the disease, the patients were, in general, costive, and complained of great lassitude, giddiness, sickness at stomach, frequent sighing, cold chills, succeeded by a burning heat all over the body, and affecting the whole of the prima via; a hot dry skin; pain and oppression about the procordia; pain in the head, back, and loins; pulse rather quick and full, with a considerable throbbing of the temporal arteries; *a constant vomiting* of at first a *yellow*, and afterwards, of a *very dark-coloured bilious matter;* eyes red, full, and heavy; thirst great; tongue much furred; teeth incrusted with a dark-coloured gummy substance; a *yellow suffusion of the skin;* great prostration of strength; restlessness; delirium, &c. &c.* The above were

the Commissioners of Sick and Hurt Seamen, that from the very serious nature of the disorder, it had been deemed proper to send them, the sick, on shore the *moment they complained;*" and that there (on shore) "*few survived longer than three or four days.*" Nine of the ten persons in question were sent on shore on the 28th of May; and exactly four weeks after, *three* only of the ten had died. Few, is generally understood, *at most*, to mean the *smaller number*; but here, *more than two-thirds* had survived, not merely three or four days, but as many weeks. In making this computation, I have taken facts most favourably for Dr. Wilson—because, in addition to the *ten* persons here mentioned, *nine* seamen belonging to the Thetis, who were sent on board the Raison, to supply the place of the Huzzar's men, when sick or dead, had also been sent to the naval hospital with the same fever, where they all recovered; and these, I should suppose, ought also to have been taken into Dr. Wilson's consideration, when he thought proper to state that few had survived longer than three or four days.

* Many circumstances convince me, that it is proper to receive this general account of the symptoms of the supposed yellow fever with great caution. Indeed, the imaginations of some persons at Halifax, appear, on that occasion, as at Grenada, in regard to the Hankey, to have acquired a *morbid activity* and *bias*. Even Mr. Halliburton, the superior medical officer of the navy on that station, has, I think, clearly manifested this, in his letter to the Commissioners of Sick and Hurt Seamen, though written on the 25th of June, when all apprehensions of danger had subsided, and *sober* common sense might have been expected to regain its proper ascendency. Among other instances of the effect of his excessive alarm, he states the following, viz—"I went on board the Huzzar with Captain Beresford, had all hands turned up, spoke to the men of the *absolute necessity* of their not concealing their complaints, if they had any; that their *safety depended on an early application.* Six of them came forward, with evident symptoms of the approaching malady, *and although at work*, I recommended it to Capt. Beresford to send them *instantly* to the *suspected tents;* and in the evening when I visited them, I found *three* of them very ill, with quick pulse, vomiting, &c." How much of the illness in these three men depended upon their imaginations, or the imagination of Mr Halliburton, I know not; but if the symptoms of yellow fever had been *so evident* in the six men, as he states, we might expect that he would have found *more than three* of them very ill that evening. And, I will add, moreover, my belief, that if a distempered imagination, or

the symptoms, as they in general occurred in succession. But, Gentlemen, previous to my submitting to your consideration the mode of treatment adopted on this occasion, permit me to make a few observations.

"On the first appearance of acute diseases, in general, the evacuation of the alimentary canal, becomes a primary object; and in no complaint, whatever, is this maxim to be attended to, more than in the disease at present under consideration. In its early stages, I have often experienced the good effect of emetics and cathartics; but when the former were too often repeated, they were found to be productive of the most serious evils; that they not only increased the irritable state of the stomach, but also, that of the hepatic system; and that in consequence, a train of such disagreeable symptoms obtained, as frequently baffled every effort of either medicine or art to get the better of.

"On the first attack of the disease, the patient, if of a full habit of body, was bled, head shaved, a blister applied to the breast, and an emetic, consisting of 2 or 3 grains of tartarized antimony, given; but in order to avoid the bad consequences above alluded to, the emetic was seldom, if ever, repeated; nor was any liquor allowed to be drank during its operation. A considerable quantity of bilious matter was, in general, thrown up; the spasm on the extreme vessels, in some measure, removed, and a stool or two frequently procured. In cases where this last circumstance did not take place, injections were had recourse to, and often without effect; calomel, however, from 10 to 20 grains, seldom failed in procuring some free passages. Could the stomach have borne a sufficient quantity of the kali tartarizat, or any other neutral salt, I am fully of opinion every purpose would have been equally well answered. The pain and oppression about the præcordia, and laborious respiration, were much relieved by the bleeding and blister; and in cases where the vomiting was very severe and painful, saline medicines, combined with camphor, æther, and opium, afforded relief: saline draughts (with opium,) in a state of effervescence, were also of infinite service; and those with whom the vomiting, together with the burning heat of the prima via, continued, by taking three or four grains of nitre, with a few drops of laudanum, in a wine glass of cold water, or lemonade, every hour, although ejected almost as soon as drank, yet, by persevering in its use for eight or ten hours, found the irritation at the stomach, in some measure, removed, and the febrile action considerably abated. Purgatives, whenever they could, with any prospect of effect be given, were administered, and several passages, if possible, daily insured. When, however, the violent heat, and morbid action could not, by these means, be subdued, much benefit was derived from the cold affusion. The manner in which I used it was as follows: The patient was stripped perfectly naked, and if violent, held, whilst sea water from a bucket, was poured

something else, had not overpowered Mr. Halliburton's sober judgment, he never would have thus mentioned *evident symptoms of an approaching yellow fever* in men who were quietly at work, and seemingly had no suspicion of being unwell, until frightened into a belief of their being so. Other physicians often find it difficult to *ascertain* whether a fever will prove to be a yellow fever, even sometime after its actual *commencement*.

on his head, and all over his body, until, if delirious, he came a little rational, which was, in general, in the course of ten or fifteen minutes, (if not delirious, not so long;) the affusion was then discontinued, the patient rubbed dry, with coarse flannel cloths, and put to bed. Sleep, and a gentle diaphoresis, usually followed, and the patient awoke in the course of a few hours, apparently much refreshed, and free from either fever or delirium. On a return of these complaints, the same remedy was again, or again, if necessary, had recourse to, and invariably with the same effect. Tonics, consisting of bark, wine, &c. &c. with a proper nourishing diet, soon perfected the cure; and, I have great satisfaction in adding, that of those who were under my care on shore, amounting in number to 83, I did not lose a single patient.

"J. W."

The preceding communications from Dr. Wilson, afforded me but little information, which was either new or satisfactory. In the hope, however, of obtaining more, I stated, in a letter to Dr. Blane, several new difficulties and objections, which had occurred to my mind on the subject, together with fourteen questions, which I thought it probable Dr. Wilson might be able to answer from memory, (if no written evidence should be in his possession,) and which I conceived likely, and necessary, to ascertain the symptoms and nature of the disease, and supply the want of important facts, which had been omitted in all the former accounts. These questions, together with my letter, were transmitted by Dr. Blane to Dr. Wilson, who, in consequence thereof, wrote an answer, dated December 20, 1807, explaining some contradictions which I had mentioned, between his journal and the books of the naval hospital at Halifax, and recapitulating his former statements, *but without answering any one of my questions*, which his memory, as he declared, did not enable him to do. He moreover intimated, that he thought it "hard to be thus called upon, at this distant period of time, for further proofs of accuracy and fidelity." Having thus failed in my endeavour to procure full information on this subject, nothing remained for me, but to employ, as well as I was able, the little which had been afforded: and as this did not, in any degree, weaken the reasons which had, from the first, made it difficult for me to believe the disease in question to have been the yellow fever, I shall state those reasons, after having premised the following additional facts and explanations, viz:

It appears, from official documents, as well as from the information which was given to me *verbally*, on the 26th of October, 1807, by Mr. Sawers, who was surgeon to the Thetis, in 1795, that in consequence of the sickness and deaths of most of the men sent by the *Hussar* on board the *Raison*, a number of seamen belonging to the Thetis* were also sent on board that ship, to assist in navigating her to Halifax. And in Mr. Sawers' "Medical Journal of his majesty's ship Thetis, from the 13th of October, 1793, to the 14th of October, 1795," the names and ages of *nine* of these men are stated, and against them, the several days, from May 27th, to June 11th, on which they

* Mr. Sawers was not able to recollect the exact number, nor the times when sent on board La Raison. He thought, however, that they might amount to 15 or 16.

were sent to the Naval Hospital, at Halifax; and to these names and dates, the following explanation is annexed, viz:

"These men were sent into Halifax in a French prize vessel. We afterwards learned she had been employed a great part of the war as a *prison* ship, at Guadaloupe, and had been very sickly, and a number of men had died on board of her.* *When they began to move her stores, upon her arrival in Halifax harbour, the men began* to complain, at different times, of pains in their heads, attended with rigors and shiverings, and great lassitude, loss of strength, with violent retchings; the *matter* brought up from the stomach, I was told, had the appearance of *contaminated bile*.† A number of men from his majesty's ship Hussar, were also on board this vessel, several of whom died; and it was remarked, that they were either very *yellow* some time before death, or *turned yellow soon after it*. In short, from what I could learn from the prize-master,‡ and other people on board, it must have been the *yellow* fever.

"I had not an opportunity of attending those men myself, as they were sent to the hospital, agreeably to the dates, immediately upon their complaining. They were accommodated in the out apartments of the hospital, and in tents erected in the fields. I went frequently and saw them. The method of treatment was as follows: strong calomel, or other active purgatives, were given in the beginning, and repeated, whenever an inclination to vomit came on, and after the irritability of the stomach was subdued; bark, serpentaria, decoctions of chamomile, and other tonics, were given freely, with plenty of wine, and nourishment; as also, ripe fruits. In some cases, sluicing with cold water, was administered, with good effects. They *all recovered*, which I suppose was owing to early assistance being given at the hospital."

In conversation, Mr. Sawers told me that the disorder of the nine men belonging to the Thetis was slight, that they were able to walk about, but looked sallow, and had been taken to the hospital rather as a precaution, than from any urgency of sickness. That the Prevoyante had remained three or four months at Guadaloupe, after coming there from France, and had lost some men during that interval, by the yellow fever; but that she was in good condition, and her crew very healthy, at the time of her capture; and that they gave no disorder to the The-

* This expression leaves it uncertain, whether the writer meant that these men had died on board the Raison, during her passage from Gaudaloupe, or whilst she was employed as a prison ship; but the latter seems to have been the fact.

† This comparison does not give me any better idea of the "*matter*."

‡ It appears, by this reference to the *prize-master*, that no medical assistant could have been sent on board La Raison, notwithstanding the great number of her own sick that, as is pretended, were left on board, and the additional sickness and deaths of the men belonging to the Hussar: and if it had not been for the little information here given by Mr. Sawers, we should have had none whatever, respecting the sickness of the nine men belonging to the Hussar, who died before her arrival at Halifax. And even at this time, after all my researches and inquiries, I am utterly ignorant how soon any one of them was attacked after being sent on board; what other symptoms, excepting turning yellow, appeared in any of them; how long the illness lasted, nor when any of them died. If it had been wished to compel a belief of their having all died of yellow fever, by withholding every thing requisite to form any judgment whatever, this extraordinary omission would have been very natural.

us; which ship did not send one man to the hospital after her arrival at Halifax, excepting the nine who were on board La Raison. It appears, however, by official documents, that two of the crew of the Prevoyante, notwithstanding her good and healthy condition, died on the passage to Halifax, viz:—one, a French seaman, on the 25th of May,—and one black seaman, on the 26th; whilst none died of the Raison's crew, though represented as being then in a most sickly and deplorable condition. And it is moreover in proof, that after their arrival at Halifax, the sickness and mortality were much greater among the crew of the Prevoyante, than among that of La Raison.

But as Dr. Wilson has given an account of his proceedings with the 83 sick, of the Hussar's crew, it will be proper to give a like account, in regard to such of the crews of the French ships Prevoyante and Raison, as sickened before the 7th of July, after they had been transferred, on the 28th of May, to the prison ship, *Security*, at Halifax. It appears, from the books of the Naval Hospital there, that on the 31st of May, eight of the crew of La Raison, were sent from the prison ship to the hospital; three of whom died before the 25th of June, and one on the 9th of July; *and these were all the deaths from the crew of that ship.* The other four remained in hospital four months; and it seems probable, that the whole number of eight, had either hectical, or chronic, affections; because Mr. Halliburton, in his letter of the 25th of June, to the Commissioners of Sick and Hurt, giving an account of the *sick French prisoners*, says, "One or two *hectic* patients have died, *but none with this fever:*" meaning the supposed yellow fever:—an observation which applies equally to the crews of the Prevoyante, and makes a detail in regard to them unnecessary. It seems probable, however, that after the crews of these ships had been transferred to the prison ship, they were exposed to the contagion of typhus, (which is, indeed, no uncommon event, in such situations,) though Mr. Halliburton, and Dr. Wilson, imagined it to be the contagion of yellow fever.

Mr. Halliburton, in the letter just mentioned, after giving an account of his, and Dr. Wilson's proceedings, in regard to the Hussar's men, supposed to be labouring under *yellow fever*, adds, "the disease now began to make its appearance on board the prison-ship, and as they *were much crowded*, I represented to Captain Home, now senior officer, that the same precautionary measures should be adopted, which were pursued respecting the Hussar; that the sick must be separated from the sound and healthy, and that all with *suspicious* symptoms should likewise be removed. In consequence of which, a place was hired about six miles from the town, by water, called Kavanagh's island, and all the sick, to the number of 31, were *towed* there, in a boat by themselves, that they might have no communication with the ship's boats that towed them. Ten or a dozen of them were very ill, and *many** of them were afterwards seized with it, who had the

* It appears, by this expression that the disease did not actually occur in all who had the first symptoms upon them; and this circumstance ought, perhaps, to lessen the regret which we must otherwise have felt, at Mr. Halliburton's not having made us as wise as himself, in regard to the nature of these "first symptoms."

*first symptoms upon them*, when removed, *but we have not as yet lost one of them ;* and by pursuing Dr. Rush's mode of treating the Philadelphia fever by bleeding, &c." "has proved very successful, and highly pleasing and satisfactory."

It appears by the books of the hospital, transmitted by Mr. Halliburton, to the sick and hurt office, that this removal of sick prisoners took place on the 15th of June; and that they consisted of 17 men, who had belonged to the Prevoyante, and of 14 who had belonged to the Raison; and certainly there never was a *more harmless yellow fever*, excepting that of the men under Dr. Wilson, nor one *more accommodating ;* for, except one man, who, besides the fever, seems to have had some lingering chronical disorder, they are all stated to have been ill, *exactly fifteen days ;* so that as they had been all towed to the island in *one* boat, they might *all be towed* back in *another*. There is, however, *one important fact* connected with this transaction, and which seems to have been hitherto quite overlooked; and this is, that, among the 17 supposed yellow-fever patients from the Prevoyante, six were blacks, (negroes,) whose names are distinctly entered; and of the 14 from La Raison, three were also *blacks*, though there were only ten of that description among her crew. Now the idea of *nine blacks*, coming *directly* from the *West Indies*, where they might have *bid défiance to the yellow fever*, even when most prevalent as an epidemic, and *taking that disease at Nova Scotia*, a place where, like Great Britain, from its moderate temperature, the yellow fever *never did*, *nor ever will occur*, (at least without a great alteration of its climate,) must appear as ridiculous and absurd as any thing which can be imagined; at least to those who may have read the preceding parts of this volume, or who shall have otherwise acquired competent information on the subject. *This single fact*, in my judgment, would suffice to prove, *indisputably*, that the disease in question, could not have been the yellow fever; because the notorious *insusceptibility* of negroes, *coming directly from a hot climate to a cold one*, would necessarily, according to the *principles* formerly stated, and the uniform experience of more than a century, have been encreased, not diminished; but, on the other hand, their susceptibility of disease, from typhus contagion, by such a transition from heat to cold, would have been augmented, as has been repeatedly observed: and this may explain why so great a proportion of negroes took the (typhus) fever at Halifax. Dr. Blane, in his letter to Baron Jacóbi, asserts, what he has indeed repeatedly mentioned in his other writings, that the yellow fever "has never been known to appear, except either in tropical climates, or in those seasons in the more temperate climates, in which the atmospheric heat has, for some length of time, been equal to the tropical heat, that is, *at*, or *above* 80° of Fahrenheit's thermometer. This (he adds) is a fact incontrovertibly established by observation; for there is no instance either in North America, or Europe, of the yellow fever appearing, except at these degrees of heat, nor of its surviving after it had fallen to a lower degree of temperature." Now, if Dr. Blane, before he allowed himself to adopt and give his sanction to this story, of a communication of the yellow fever, by the crew of La Raison, to those of the Hussar, &c. had exercised his good sense, in reflecting upon the usual and probable temperature of the atmosphere on the coast of New England and Nova Scotia, at that *early season* of the year, he must have been convinced that it never could have there attained, and much less have been "for some length of time," at, or above 80° of Fahrenheit's thermometer. Mr. Sawers told me, on the 26th of October, 1807, that on the passage to Halifax, after the capture of the Frevoyante and Raison, they had encountered a gale of wind from the *north-west*, which rendered the air cool, as it must have done on that coast, where this is the coldest wind; and, indeed, it appears by the length of the passage, (12 days) notwithstanding the aid of the gulf stream, that the winds must have been generally from the north, and the temperate, probably, was during that time much oftener *below* than above 60°: and it is well known that even in South Carolina, except from a rare concurrence of circumstances, the yellow fever does not prevail before the month of July or August; who, then, will believe, that in the *frigid* climate of Nova Scotia, it should have attacked so many persons, about the end of May and beginning of June? and this though it never did before, nor since, appear *there*, even in the hottest months *of the summer*.*

Whatever the *cause* of the fever may have been, it certainly was not derived from, nor first applied to, the crew of the Raison; for no one of them appears to have had it, until near the middle of June; whereas, it is stated in the abstract of Dr. Wilson's Medical Journal, authenticated by his own signature, that *three* of the Hussar's men (viz. W. Atterly, John Lopez, and Jonas Ireland were attacked with it, *in that ship*, on the 25th May, and two others on the 26th; and, notwithstanding all that has been *strangely* asserted to the contrary, the crew of La Raison, if *any faith can be placed in official documents*, were less sickly, and had less mortality among them, than those of either the Hussar or the Prevoyante.

It would be to me a very unpleasant undertaking, were I to enter upon a critical examination of Dr. Wilson's general account of the aggregate symptoms of his supposed yellow fever patients; an account so indiscriminating and deficient that no body can discover from it, whether the ordinary progress of the fever lasted a week or a month; or even whether it was continued, remittent, or intermittent; and though some of the 14 questions forwarded to Dr. Wilson were especially, and pointedly directed to *these important circumstances*, I obtained no answer to them. Whether Dr. Wilson was unwilling or unable *to*

* Dr. Trotter, in the first volume of his Medicini Nautica, has introduced the following atement, at page 357, viz—

"December 3d—most of the ships which have returned this season, (the Autumn of 1796,) from the West Indies, have been sufferers from the yellow fever; yet *the diseases in all of them, uniformly dissappeared as they increased their latitude: at 32° North, no fresh attacks were known.*" "The Dædelus frigate, that arrived at Portsmouth in October, left the islands with this fever on board; so large a number of men and officers were affected, that captain Countess thought it expedient to push for Halifax, to land his sick; but before he reached that port, the disease had taken a favourable turn, and was soon extirpated." If this effect was produced on the coast of America, in the hottest part of the Summer, how can it seem credible, that, in the month of May, and with cold northerly winds, the yellow fever should have made so many "fresh attacks," as were supposed in the Huzzar, so much beyond the 32d degree of North latitude, and even at Halifax itself.

*remember*, is best known to himself; there is, however, one of his symptoms, which I cannot overlook, as the description of it seems to have been intended to obviate all doubt of the fever's having been the true yellow fever: and this is "a constant vomiting of, at first, a yellow, and afterwards of a *very dark-coloured bilious matter*." At that time, the *black vomit*, so called, was thought by many persons, and probably by Dr. Wilson, to consist in a great degree of condensed or dark bile, and this description *seemed* to have been intended to induce a belief that *black vomiting* was a common symptom among his patients. But that I might not fall into a mistake on this particular, one of my questions was directed immediately to it: as another was to the number or proportion of the sick who became yellow;* but these questions, like all the others, were left completely unanswered, and, as I must think uncandidly; because it is incredible that Dr. Wilson should not, at least, have known his own meaning in regard to the "very dark-coloured bilious matter" vomited by his patients; and it was, I think, his duty to remove all doubt concerning it. Certainly, if what is commonly understood by black vomiting had occurred, in even one of his 83 patients, there is the strongest reason to believe that no more than 82 of them would have recovered. And his unexampled success, if there were no other reasons to disbelieve the existence of yellow fever among his patients, would render it absolutely incredible. Some gentlemen, indeed, from whom I should have expected more discernment, have strangely imagined that the recovery of every individual of 83 patients, under Dr. Wilson, might be ascribed to a superiority in his treatment of them, without suspecting that the disease was not the true yellow fever; and Dr. Wilson seems to have derived credit and promotion from this mistake; which, however, is not likely to subsist; experience having, I believe sufficiently proved, that in cases of the true yellow fever, recoveries have not oftener resulted from this, than from other modes of treating that disease: and it will, I think, have been fortunate, *if no mischief has been done*, by an imitation of that part of his practice, which consisted in the giving of *antimonial emetics*.*

* I have already several times remarked, that yellowness of skin is not a characteristic symptom of the yellow fever, nor a rare occurrence in typhus; but, on the contrary, that in some circumstances and situations, (particularly one mentioned by Dr. Lind) it has so frequently accompanied the latter fever, as to have procured for it the name of yellow fever. Dr. Blane has, also, mentioned a similar occurrence on board the Royal Oak, where a *ship-fever*, introduced by six men who came from England in the Anson, and which extended to thirty others, was attended with *yellowness of "the eyes and skin,"* in all that were affected by it, though the disease was very mild in most of the cases. See his work on the diseases of seamen, p 148 and p. 352.

* I have mentioned the danger of giving emetics in yellow fever, at p. 67, &c. and this danger is multiplied in a ten-fold degree, by preferring antimonials. The following is Dr. Moseley's observation, respecting the effects of emetic tartar, or tartarised *Antimony*, employed by Dr. Wilson, *to the extent of three grains*, viz. "How often have I seen and lamented the effects of emetic tartar, given to remove the supposed cause of the treacherous symptom of vomiting! Even in slight degrees of fever, in the West Indies, in young plethoric subjects, newly arrived, the stomach has been some times *destroyed* by it. Instead of removing the irritating sickness in this (yellow) fever, or exciting a diaphoresis, a spasm has been produced in the stomach; incessant vomiting; inflammation; the vessels of the thorax and head have been stifled with blood, and the patient has *vomited away his life*" See his valuable work on topical diseases, &c. p. 435 of the Third Edition.

That the fever which spread among the crew of the Hussar, and in the Security, prison ship, could have been no other than a typhus, I am convinced. It was, indeed, so unusually mild, and devoid of mortality, that we can only account for so many recoveries, even under that fever, by the known effects of a warm temperature (which would have taken place at Halifax in June) in first *moderating*, and soon after producing a *cessation of it*.

Whence the infection of this fever was derived I cannot be expected to explain, destitute as I am of all information respecting the communications which the Hussar may have had with infected persons, or things. Mr. Sawers told me, what indeed must be otherwise highly credible, that ships of war on the Halifax station, were sometimes known to get the contagion of typhus, by pressing seamen, and *emigrants*, out of merchant vessels going from Great Britain and Ireland to the United States, &c. many of the latter being often taken from the lower classes of society, among whom that fever is but too common.

In regard to the fever which produced so much mortality among the men sent by the Hussar, on board the Raison, and of which we know nothing, except that some of them became yellow, either before or after death, it would be presumptuous in me, without more information, to offer any thing but (a very probable) conjecture, that it was occasioned by the *foul air or morbid effluvia*, contained in her *hold*, *or in some other part* which these men had *rummaged*, in search of rum, or wine, or other things which their appetites, or cupidity, might dispose them to plunder, and which, *in a prize*, they might expect to find, perhaps, concealed. In numerous instances, fevers resembling the yellow fever, and *even more generally* and *speedily mortal*, have been manifestly produced by exhalations from the foul ballast, and other decomposing matters, retained in the ships;* and the fact stated in Mr. Sawers's Medical Journal, as lately noticed, in regard to the men belonging to the Thetis, who were sent on board the Raison, viz. that " when they began to *move her stores*, upon her arrival in Halifax harbour, the men began to complain, at different times, of pains in their heads, attended with rigors and shiverings, and great lassitude, loss of

* For *decisive instances* of such fevers, produced by these causes, see the New-York Medical Repository, vol. i. p. 394, vol. ii. p. 327, vol. iv. pages 2, 3, 243, 245, and 353; vol. vi. p 159; vol. vii. pages 86, 87, and 88; and vol. viii p. 71. The first instance in the 4th vol. of this Repository relates to a yellow fever produced by the foul atmosphere of the hold of the America frigate, General Green, which was fatal to twenty persons, and decidedly not contagious; though some of the crew fell sick on board, several weeks after she had been cleansed and freed from the noxious matters producing the noxious atmosphere; a circumstance, for which it then seemed difficult to account, but which, from what I have mentioned in a former part of this work, concerning the time that miasmata often remain inactive, may now be readily explained. See also, for similar instances, Trotter's Medicina Nautica, vol. ii. p 97, &c. Dr Gillispie's volume, page 19 20, 21, and 61; and Valentin's work on yellow fever, from p. 121 to 129. I could easily multiply these quotations, but I will finish with one from Dr Blane, on the diseases of seamen, p 609, viz. "With regard to the effect of putrid exhalations, I need only mention that at the time of the battle of the 12th of April 1782, there was not a sickly ship in our fleet, but many of the officers and men who were sent to take care of the French prizes, were seized with the *yellow fever*; and it was observed, that when at any time the *holds* of these ships, which were full of putrid matter *were stirred, there was an evident increase of these fevers* soon after."

strength, with violent retchings, &c. seems to prove the existence of a noxious atmosphere, or rather of matters capable of emitting noxious effluvia when disturbed; and this might well be expected in a ship which had been long employed as a prison ship, and afterwards sent, probably without being cleaned, to obtain a cargo in the United States, for France. That this fever, or that which afterwards occurred at Halifax, could not have resulted from contagion in the *Raison*, must be evident; because if that had been the case, as it must have been applied to her own crew several weeks before, and for a much longer time than it was to those of the Hussar and Prevoyante, the former would not have been among the *last and least* sufferers by it. Here I will close this appendix, and, with it, this volume.

# A FEW BRIEF NOTES,

BY

JOHN B. DAVIDGE, A.M. M.D.

*&c. &c.*

# NOTES.

*Page* 40.

The matter of black vomit is, notwithstanding the reports of the examinations of bodies dead of the yellow fever, sometimes met with in the gall-bladder, common duct leading to the duodenum, and in the duodenum, in considerable quantities, as well as in the stomach. I have had occasion to observe this fact, and have also been informed by respectable authority, in which the utmost confidence may be placed, both as to dignity of character, and acuteness to observe, that such is the fact. To Dr. Wm. Donaldson, a physician of great circumspection in observation, and extensive professional knowledge, I am indebted on this head.

That the matter of black vomit is the result of morbid secretion, appears to be the opinion of some of the best informed in this city, Baltimore. The hemorrhage occasionally met with is, perhaps, incidental. That the liver is the organ chiefly concerned in the *secretion* of this matter, I am strongly inclined to believe, from its presence in the gall-bladder, and its existence in cases where there has been no effusion of blood, at least, observable.

*Page* 93.

I have taught, from my Chair as Professor, for several years past, the doctrine, which I am much gratified to find advocated by the able pen of Bancroft, that putrid animal matter, simply as such, is never productive of fever. And although in certain habits, sickness at stomach and faintness may occur from the effluvia of putrid animal bodies, yet in regard to the production of regular fever, of whatever type or genus, they are wholly innocuous.

The genius of our learned author has shed much important light on the subject of Typhus, arising from collections of healthy persons in ships, hospitals, or jails. He properly and judiciously rejects all such speculations, and places the affair in its proper attitude. There is no fever that can with propriety be attributed to any such collections of healthy persons. If disease, febrile disease, take place among persons crowded together in ships, or jails, or hospitals, we must look for its cause to some other source, than the recrementitious materials thrown off from the bodies; the cause is from without. When persons die under the circumstances above suggested, it is somewhat as when they die from carbonic acid gas; they are killed negatively. In other words, they die for want of pure, repirable air; not from any poisonous operation of the fluvia. These effluvia, I hold to be, equally with those from putrid dead animal bodies, unproductive of a febrile disease.

*Page* 122.

In the Introduction, I have taken the liberty to prefix to this volume, the reader will have perceived that I have called the attention of the medical philosopher to the important, yet simple fact, that no feverous disease arises from, or with propriety can be referred to, more than some one simple cause, however aided that cause may be by circumstances incidentally disposing the body to be acted on. And that if the yellow fever be derived from marsh miasmata, as is proved by the ingenious and learned author of the present work, and is well known to be a fact, by every man conversant with the subject, it is altogether unphilosophick, if not absurd, to ascribe it to a poison generated in the body. All communicable diseases, so far as our researches extend, have their origin universally from poisons generated in the living body. How contagious diseases came first into operation, whether from climate and conditions of life, or original poisons created by Deity, as the various principles of animal and vegetable lives are, I am not prepared to answer. Much learning and talent have been displayed on both sides.

No disease, that has its origin from a poison or cause not formed by the morbid action of living vessels, is communicable or contagious. And if the yellow fever be, a thing occurring daily, imported from one country to another, it must be in the original materials, and not by diseased bodies, or infected clothes. I touched on this subject in my Sketches, published in 1798.

*Page* 312.

In the year 1797 I was, by a particular combination of circumstances, led into a discussion on the nature and origin of the yellow fever. Matters took such a course, that I deemed it proper, in the beginning of 1798, to collect and arrange into form the scattered and fugitive remarks, that, during the discussion, I had occasionally given to the public, and to publish them in a more serious and formal manner, as the result of my most mature deliberations and judgment.

In this small Treatise, I assumed it, as an ascertained medical axiom, that no marsh disease is a communicable disease; that the yellow fever is a marsh disease; that as such it cannot be communicated by any intercourse of persons; that its diffusion is co-extensive with, and attributable to the marsh effluvium; and that no disease whatever is derivable from more sources than one: every simple effect being the production of a simple cause.

Sir Gilbert Blane, although he has altogether failed in establishing his favourite hypothesis, furnishes an important argument for us to believe that the yellow fever is not found beyond the influence and operation of the marsh effluvia. So well assured is he of this valuable and pertinent fact, that he delivers it as his serious and professional opinion, "the few cases that occurred in the Hussar, after her arrival at Halifax, are to be ascribed to the *inhalation of poison in the warmer latitudes*," (not to the contagion emitted from the diseased bodies on board of ships); "and ventured to assure the Ministers of Russia and Prussia, that their countries had nothing to fear from the importation of this pestilential epidemic."

No point in medical history is better ascertained, or more strongly established, than that all diseases known to be contagious, are certainly more communicable in the colder than in the warmer latitudes. Nor can a stronger or more conclusive argument against the contagious nature of the yellow fever be brought forward than the well-settled fact; it does not communicate in the more cold and humid regions. It is well known, and wholly beyond controversy, that all poisons, animal secretions, are more condensed, and, in proportion to their state of concentration, more active, in the colder than in the warmer latitudes. This holds true in regard to the small-pox, measles, chicken-pox, mumps, &c. &c. and every other contagious disease which acts through the medium of the atmosphere. Contagions that act by immediate contact, as canine madness, siphilis, and perhaps the plague, act equally in the colder or warmer regions. I am writing at present on diseases that communicate at smaller or greater distances, without contact. But to return to the facts of Sir Gilbert.

"It has never," says Sir Gilbert Blane, "shewn itself, in the fisrt instance, but in a sea-port town, and never in the interior of the country, whether island or continent."

In scientific disquisition there is nothing less entitled to consideration, or more unphilosophic, than dogmatical gratuitous assertion. That the yellow fever has never, in the first instance, shewn itself but in a sea-port town, is wholly gratuitous, and in direct opposition to facts from the most respectable authorities. From authorities, which, as they are not professional, and had no particular prepossession to sustain, must be viewed as the more valuable and disinterested.

To aver that to be an uniform, or even a general fact, which has been the object of our personal observation only, is to proceed very far on the principle of the begged question. For although personal observation affords respectable ground on which our author is to repose his opinions, it is proper to take into account the consideration that our individual experience does not embrace in its range the whole field of facts, and may not be laid under those circumstances, as to country and climate, demanded by the subject in discussion. That the first appearance in the interior of the country, in any one instance, of the yellow fever, was not the experience of Sir Gilbert, is a thing very probable. But from such personal negative observation there can be no positive induction that the fever never shews itself, in the first instance, but in a sea-port town. The conclusion is far more than commensurate with the premise, and such as to furnish ample reason to question the ground upon which it is made.

That the yellow fever does not shew itself, except in a sea-port town, involves a full consideration of all the circumstances of its origin. We are led by the position to an examination of all those sources from which it may possibly derive its existence: and also of the various testimonies as to facts. It must be shewn, that the original cause, whatever it may be, *cannot* have place but in a sea-port town. The disease must have derived its origin from some definite cause, and under given circumstances. What can this cause be, and what are those circumstances?

If the cause be human effluvia, it is difficult to conceive the circumstances, in a sea-port town, under which such effluvia can efficiently act, that could not have place in a town in the interior of the country, whether island or continent. I am apprized of the operation of no cause or causes productive of disease, contagious or not contagious, exclusively the production of a sea-port town. Whether human effluvia, or marsh effluvia, or any vegetable poison, be the subject presented to mind, I am still at a loss to conceive why, occasionally, such effluvia or poison might not be furnished, under given circumstances, in an inland as well as a sea-port town.

That the yellow fever does not shew itself, in the first instance, except in a sea-port town, is, *a priori*, indefensible, and, I am convinced, in contravention of well-established facts.

Dr. Miller, of New York, in his excellent Essay on the Yellow Fever, repeats a communication, highly important and valuable, by Mr. Andrew Ellicott, a gentleman of character, both as regards ability to observe, and integrity to communicate such matter, as his enlightened mind might deem worthy of communication and public notice.

"The village of Galliopolis," says this judicious observer, "is a few miles below the great Kanhaway, on the west side of the Ohio river, and situated on a high bank. It is inhabited by a number of miserable French families. Many of the inhabitants, this season, fell victims to the yellow fever. The mortal cases were generally attended with the black vomiting. This disorder certainly originated in the town, and in all probability from the filthiness of the inhabitants, added to an unusual quantity of animal and vegetable putrefaction in a number of small ponds and marshes within the village."

"The fever could not have been taken there from the Atlantic States, as my boat was the first that descended the river after the fall of the waters in the spring. Neither could it have been taken from New Orleans, as there is no communication at that season of the year, up the river, from the latter to the former of those places. Moreover, the distance is so great, that a boat would not have time to ascend the river after the disorder appeared that year in New Orleans before the winter could set in."

"General St. Clair," continues Miller, "who had the advantage of a medical education, and is, moreover, a gentleman of a discriminating mind and distinguished talents, has assured me, that he is well convinced the yellow fever is an endemic complaint, in a large portion of our south-western country, where he resided as governor a number of years."

Here are two interesting facts; one communicated, in a public document, by a gentleman who cannot be supposed to be at all involved in the professional question of contagion; the other by a gentleman who was educated under the old doctrine of contagion. Until 1797, when a fugitive communication or two appeared in the Baltimore newspapers, from my pen, I find nothing but the idea of contagion in any American writer, and it must be presumed that the prepossession and habits of thought with general St. Clair, were in favour of contagion. The circumstances of his observations must have been very decisive in character, for him to embrace new sentiments, and abandon altogether the doctrines he had received in his early years.

With the former of these gentlemen, Mr. Ellicott, there could have been no conceivable reason, or interest, for him to misrepresent a plain fact. He could have had no professional prejudice to overcome; no affair of party to support. In short, there could be nothing to give to his narrative a false or disingenuous colouring, or to his mind a dishonest obliquity. He is not only positive as to the fact, but, by a plain and conclusive course of reasoning, shows that the disease must have derived its origin from the circumstances of the village.

In addition to the above, we are furnished with a fact or two by Dr. Watkins, a man of distinguished talents and acute observation; and whose decision is the more valuable, inasmuch as he was educated in Edinburgh, under the doctrine of contagion, and is to be presumed not to be wholly free from prejudices in favour of the system of contagion.

"There is a village," says the Doctor, "called New-Design, about fifteen miles from the Mississippi, and twenty from St. Louis, containing about forty houses, and two hundred souls. It is on high ground, but surrounded by ponds. In 1797, the yellow fever carried off fifty-seven of the inhabitants, or more than a fourth. No person had arrived at that village from any part of the country where this fever had prevailed, for more than twelve months preceding. Our informant resided in the village at the time, and, having seen the disease in Philadelphia, he declares it to be the same that prevailed at New-Design. He also mentions an Indian village depopulated by the same disease, two or three years before."—Med. Rep. vol. iv. p. 74.

In corroboration of the above communication, Dr. Watkins, during a stay of a few days in Baltimore, visited several cases of yellow fever with me, and gave the assurances that he had seen, in the western country, perhaps alluding to the instances already quoted, a disease accompanied by all the circumstances and phenomena that were present in the yellow fever we were then visiting.

2dly. "No part of the population of the towns where it has broke out has been affected, but such as had some communication with the shipping, directly or indirectly."

What sir Gilbert intends to convey by the term *indirectly*, it is difficult to conceive, since no one fact on the page of faithful medical record is more uniformly true, than that no persons residing remote from the sources of marsh effluvia, have ever received the disease by way of contagion from other persons, who have contracted the fever by being in the more immediate vicinity of such sources.

Why those who may be in the neighbourhood of the shipping, will have the disease, is obvious. The shipping is along the wharves, and the wharves are, for the most part, constructed in, and sometimes of the alluvious soil, the very materials to which the origin of the fever is justly referred. That certain persons, of highly susceptible habits, residing at sites remote from the shipping, may be affected, is not only possible, but probable. It may be readily conceived, and there is respectable record to support the opinion, that by certain currents of air, the poison evolved by the wharves or ponds, or alluvion, may be carried to great distances, and applied to those highly sensitive habits, and produce the disease, while the general population would remain healthy. What given quantity of the poison is adequate to the production of the disease, is not ascertained; and were it, the great varieties of susceptibility remains to be settled. Before a medical philosopher permits himself to admit the existence of contagion, he should be well satisfied that no marsh effluvia are present; since few, or no men are so sceptical at the present, as not to concede that this fever does, in the general, have its origin in poison thrown off by alluvious earth.

Sir Gilbert Blane constantly speaks of this fever, as being present with the shipping. He does not surely mean that it arises from the ships, or the sea-air, or the sea-water. It must be ascribed to something contained in the ships. What can this something be? If it be animal matter—animal matter is found remote from ships, in all its possible conditions. If it be vegetable matter—vegetable matter, under all its changes and revolutions, is met with in places distant from shipping or wharves. If animal and vegetable matter, mixed and combined, the same mixture and combination are discovered in ponds and lakes, and alluvious soils in the interior of the country, whether island or continent. If the reference be to multitudes collected together, and living filthily, the same conditions of life are to be met with on land, in places to which ships can have no access, and under the same latitudes.

"In the year 1798," says sir Gilbert, "I wrote a letter to Mr. Rufus King, minister from the United States of America to the British court; and in the year 1801, another to Baron Jacobs, minister from Russia, for the information of their respective governments. In these letters I laid particular stress on what occurred regarding a French ship, taken in battle on the coast of America, in May, 1795, on board of which, this fever, or its infection, was found, and was communicated to the seamen of the British ship Hussar, by the men *in health*, who were shifted into her from the prize. It is evident, that if it could be proved that this fever is communicable from one ship to another at sea, such a proof of the reality of contagion would be of the nature of an experimentum crucis, there being no possibility of *land* exhalations to account for it; such I then considered, and still consider the facts of this case to be; they were, however, so strongly and so speciously contested by Dr. Bancroft, as greatly to frustrate the impressive effect which my statement was calculated to produce. The reader will be able to judge of the validity of his objections from an annotation at the end of this work. I feel to myself that I was so far from making too much advantage of these facts, that I might and ought to have availed myself of them still more. I might have adduced them as a very striking illustration of the incompatibility of the disease with a certain temperate degree of atmospheric heat, for the range into cool and pure air, in proceeding to Halifax, did in a very short time, first deprive it of its malignity, and then of its infectious nature, so as entirely to extinguish it. The few that were seized after arriving at Halifax, might have imbibed the poison, in the warm latitudes through which they passed. It was on the strength of such facts as these, that in my conferences with the members of the British parliament, and in my correspondence with those of Russia and Prussia, I ventured to assure them that in none of those countries was there any thing to fear from the importation of this pestilential epidemic, which in the end of the last century, and in the beginning of this had so afflicted the West Indies, North America, and Spain, as to excite a general alarm throughout Europe."

" There is another useful remark which I did wrong in omitting in my statement of fourteen men sent from the Hussar to navigate the prize; nine died before reaching Halifax, a passage of twelve days; the other five were sent to the Hospital, where some of them probably died."

In the two paragraphs, just quoted, Sir Gilbert Blane has given what he terms the experimentum crucis. From them I expect to derive such proof as will be satisfactory to all, in any degree acquainted with the nature of the subject, that there is not in the circumstances detailed, the slightest evidence in favour of the hypothesis of contagion.

" It is evident," says Sir Gilbert, " that if it could be proved that this fever is incommunicable from one ship to another at sea, such a proof of the reality of contagion, would be of the nature of an experimentum crucis, there being no possibility of *land* exhalations to account for it."

It has never before been suggested by any writer, that the yellow fever, as to its origin or cause, is attributable to *land* exhalations. I was not aware that *land*, as land, evolved any exhalations to which any fever could be legitimately traced. It is only contended that the yellow fever is properly ascribable to a certain condition of vegetables, whether on land or on board of ship. And, notwithstanding the authority of Sir Gilbert, I am still disposed confidently to believe, that putrid coffee, or cabbages, or potatoes, or flax-seed, &c. &c. would be the same, in nature and effect on board of ship, as on the land. The profession, no doubt, will be amused, if not instructed, to hear from such high and respectable authority, that the yellow fever is derived from *land* exhalations; or that it is necessary for putrid vegetable matter or foul water to be on *land*, to emit the noxious poison from which this fever draws its origin.

Hitherto it has been presumed, and really I thought the opinion rational and modest, that vegetable or animal substances, in a given state of putrefaction and quantity, would, other circumstances being equal, produce the same effects at sea as in harbour. But it appears that the common sense of the world, acting upon the experience of all ages, has been wrong; place and vehicle are every thing!

Perhaps, however, Sir Gilbert, or those who conceive his facts and arguments to be decisive, can furnish the rationale, why a foul ship, as the French prize is acknowledged to be, lying along-side of a clean ship, at sea, could not, as readily and certainly, by poisonous exhalations evolved from her foul materials, produce disease in the seamen of the healthy ship as in harbour.

To my understanding the proximity and relative position of the ships, the exhalations and presence of healthy excitable bodies, the intercourse between the ships, would result in the same on the ocean as at the wharf.

Sir Gilbert says nothing of the relative position of the ships while engaged in battle, or after the French struck her colours to the English ship; not whether they fought in contact, in grappled relation; nothing as to the course of the wind, whether from the foul French ship to the healthy British seamen, nor whether the French ship was boarded after the fight, by way of taking possession of her, by the British seamen; nor whether the very seamen who took possession, in part, at least, were not those, who after the ships set sail for Halifax, became diseased.

An inquiry into the above is of primary importance to me: but to Sir Gilbert and his admirers it may not be so necessary. They are satisfied that no *land* exhalations were present.

As the facts now stand stated, there is no more evidence or probability of contagion, than if I were, with a company of armed men, to approach a wharf, or any other source of foul alluvious ground, engage in battle prove victorious, take possession of the unhealthy district, and, after the delay of some hours, necessarily consumed in making arrangements, within the range of the miasmata, again continue on to my destined position, taking with me a few healthy prisoners, there would be, that my company would be diseased by the *healthy* men taken, and not by the poisonous exhalations which had been inhaled during the contest, and the delay in the neighbourhood of the alluvious ground. And were I to give publicity to the opinion, that the company attacking, most certainly took the disease from the *healthy* men, and not from the foul district, my ignorance, I believe, would meet with little else, from men of science, than pity or contempt.

The cases are parallel as far as regards every thing that can be conceived to be efficient in the production of the disease.

To know how very foul the French ship was, we have only to admit the evidence of Sir Gilbert, "that of the fourteen men that were put on board the prize to navigate her into Halifax, in the course of twelve days nine died, and the remaining five were sent diseased to the Hospital, where probably some died."

Certainly no ship could be more foul; but yet, not a word is said of the medical men, nurses, or patients in the hospital. Was the disease communicated to them? No. Did the *purity* of the air of the Hospital check the contagion? A contagion can act at sea, in the purest air, but cannot be propagated in a Hospital! Had a few of the citizens of Halifax paid a visit to the foul prize, they, in all probability, would not have found the air of such purity, as to have kept them in safety.

Can credulity itself admit that a contagion which, on board of ship, is so virulent, as not to permit a solitary man, out of fourteen, to escape, and yet to become weak and innocuous the instant it is placed within the walls of an Hospital!

In regard to the British ship, the Hussar, we are told that the seamen were infected by the *healthy* taken from the prize ship. Sir Gilbert does not enter into particulars; he does not say *all* her seamen. Yet the phrase, the "healthy men from on board the prize communicated to the seaman of the Hussar," does, by liberal interpretation, mean the whole. Or Sir Gilbert may mean that the healthy men from the prize, communicated the fever to the men of the Hussar who had been on board the prize.

That he does not mean the whole of the men of the Hussar is certain, from what follows. "The few that were seized after arriving at Halifax, might have imbibed the poison in the warmer latitudes through which they passed." And if the few who were seized after arriving at Halifax, imbibed the poison while passing through the warmer latitudes, what certainty is there that the few or many that were seized before her arrival at Halifax, did not derive the poison from the same source—the warmer latitudes. Or from a source far more probable, the exhalations of the prize while engaged in battle with her, and subsequently during the stay in her neighbourhood. The marsh effluvia will evolve its effects in morbid phenomena, after the lapse of eighteen or twenty days, or indeed several months. I write from personal observation and knowledge. See Bancroft, p. 81.

"There have occurred, since the period alluded to, facts equally conclusive, regarding the communication of the disease from one ship to another. It will be enough to specify one or two. 'A French ship of war, the Palinurus, lying at Martinique, severely affected with the yellow fever, was ordered on a cruise, to try the effect of sea-air on the disorder. She fell in with and captured the Carnation, a merchant ship, on her passage from England, part of the crew of which were seized with the fever while at sea. Another French ship of war, in which this fever prevailed, both at St. Domingo, and on her passage to Brest, made prize of a merchant ship from the Mediterranean, off Cape Finisterre, and having, without shifting the persons, sent a party of their own seamen to navigate her, the crew of the prize caught the fever, and almost all died of it. The men having been seized on board of their own ship, makes it a stronger case than the other, in which this circumstance is not mentioned; had they been taken ill on board the capturing ship, it might have been said, that it was from the *exhalations* of the *hold* or *stores*."

In the affair of the Palinurus, the narrative, as far as it goes, amounts to but little. It is not stated, whether the men were changed from the merchant to the foul ship. It is only affirmed, that the Palinurus captured a merchant ship from England, and that a part of the crew were seized with the fever while at sea. It may fairly be presumed that the healthy men were translated to the French ship, for Sir Gilbert appears at this stage of his argument, to be quite sensible of the possibility, perhaps certainty, of the disease being communicated by the "exhalations of the hold and stores." And had the capturing ship sent her men on board the merchant ship, he would have been careful to take advantage of the fact. The case of the Palinurus, therefore, might, with no loss to the argument, have been omitted in this catalogue of imaginary facts. It is altogether without bearing on the subject.

The second case carries with it, at first view, some speciousness and plausibility. It however, conveys us to the old ground, and we have again to inquire, at what distance did the merchant ship remain, after capture, and while she kept with the ship of war, affording facilities for the removal of the men from the ship of war, together with the body and bed-clothes to her own cabin? With the men, provision for their support until they could arrive at Brest, was, doubtless, conveyed from one ship to the other. This provision, possibly, might be vegetable, and this vegetable matter not in the *sweetest* and *soundest* condition. Might I propound the question,—Was this disease at first produced, and afterwards kept up, by this provision? On land, nothing more usually gives origin to fever, than putrid cabbages, potatoes, &c.

Without the least violence to probability, or even fact, it may be admitted, that the two ships lay in convenient relation to each other, side by side, or at a short distance. The two ships, under such circumstances, would of necessity be involved in the same exhalations, which, Sir Gilbert acknowledged, in the other ship, to pour out from the hold and stores very copiously. Twenty minutes may be, equally with twenty days, sufficient time for the poison to make its impression. From the great mortality the poison must have been active and concentrated.

All that is said derives probability and strength from the fact that the capturing was the foul ship. Hence there could be no possible reason why the two ships should keep at a distance.

It is not said that, on the arrival of the ship carrying those, almost all of whom died on their way to Brest, the disease was communicated to any persons of the town or hospital.

Neither Sir Gilbert, nor any of his admirers, will tell us that the air of a city or hospital, is better suited to put a stop to a contagious disease than the pure air of the ocean. While on board of ship, as in the affair at Halifax, the disease operated; but when the men were removed to the hospital, it ceased. Nothing can be more clear and conclusive, than that the poison was in the ship, and not in the persons.

Such are the facts which have afforded cause of great triumph to a writer, in a late number of the Medical Recorder; a book deservedly of extensive circulation, in favour of the opinion, that the yellow fever is a contagious disease. Facts, from which my mind, were they insulated, and the only known facts in relation to the fever, would deduce the opposite conclusion, or surrender its privilege of thinking and reasoning. The assertions of sir Gilbert Blane, and the pretty and impassioned declamations of his friends, being laid aside, there is nothing, as I hope, the reader perceives, in his facts, to induce even a careless inquirer to believe otherwise, than that the yellow fever is of the marsh progeny, and incommunicable. Dr. Bancroft had great reason, indeed, to contest the subject, and weaken the confidence of any Board, in the opinions and conclusions of sir Gilbert.

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It is gratifying to the cultivators of science, and highly beneficial in the promotion of patient and successful research, to see such abilities and professional knowledge, as possessed by Armstrong and Bancroft, blending their powers and influence in recommending the important, and I apprehend defensible doctrine, that the typhus fever is a contagious disease, and referrible to a generick poison alone, in the same manner as small-pox, canine madness, &c. &c.

While I refer the reader to the work of Armstrong, for the facts and the arguments, he will excuse me, if I quote a sentence or two from his valuable pages. His opinions are so pertinent, and so naturally constitute a part of my present subject, I cannot avoid doing myself the pleasure of recording them.

At page 7, says Armstrong, "It strikes me, that to call any species of fever typhus, which has not the contagious *essence*, capable of producing an unequivocal typhus, is equally incorrect in logic as in language. In this essay, therefore, the word typhus shall be limited to the peculiar disease, which is allowed to originate from a *specific contagion*, and which, doubtless, has the power of producing an affection of its own nature, in individuals exposed to its influence."

The typhus is not, so far as my observations have extended, a disease of Maryland, perhaps not of America; at any rate, not south of the New-England states. And since, as Armstrong and Bancroft, and most other enlightened physicians, admit contagion as essential to typhus, (I here refer to the typhus of Britain and Ireland) it must be highly absurd to speak of the typhoid condition of diseases, in regard to those diseases that are not admitted to be contagious in any stage. For surely, no disease can be said to be like another, that is deficient in an essential quality. Hence it appears, how unphilosophic the language is, that states the low and collapsed condition of the body, in remittent bilious fever, synocha of the winter, or pneumonia, to be typhoid. Those diseases are wholly distinct from typhus, in all their stages, cause, and sensible phenomena.

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In my nosology, I have considered the plague as a species of typhus. More extensive reading, and the consultation of the more modern and enlightened authorities, have, however, satisfied me that I was not correct. The plague, upon the authority of those who appear to have had the best opportunity to observe it, is an exanthematous or eruptive disease, and as distinct in nature and character from typhus, as from small-pox, or measles; it is a disease sui generis.

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