

OBSERVATIONS

ON

DOCTOR MACKRILL'S

HISTORY

Charles OF THE *A Wind*

YELLOW FEVER, &c.

BY A GENTLEMAN OF THE FACULTY.

I am well aware, that a weak credulity on facts has injured every Science, but none so materially as Medicine.

MACKRILL'S HISTORY, &c.

BALTIMORE:

PRINTED BY JOHN HAYES, PUBLIC-ALLEY,
NEAR MARKET-STREET.

M,DCC,XCPI.

OBSEEVATIONS

ON

DOCTOR MACRILL'S

HISTORY

OF THE

YELLOW FEVER, &c.

BY A GERMANMAN OF THE FACULTY.

I am well aware, that a weak credulity on this has injured
very much, and none is more so than in Medicine.
MACRILL'S HISTORY, &c.

BALTIMORE:

PRINTED BY JOHN HAYES, PUBLIC-ALLEY,
NEAR MARKET-STREET.

M.DCCCXXV.

DEDICATION.

TO DOCTOR BENJAMIN RUSH.

S I R,

THE writer of the following sheets is entirely unknown to you: His name has perhaps never reached your ear.—Whatever respect I may have shewn you through the course of this little tract, cannot be attributed to any worse motive, than a sincere respect and admiration for your eminent talents, and your private worth.

You are entitled to much more than this sort of expression of respect, on account of the conspicuous share you had in elucidating the nature of a most formidable disease, and introducing a rational mode of treating it.

You

DEDICATION

You are entitled to much more, on account of your illustrious labours for the benefit of mankind.

I BEG you to accept of this little essay towards the right understanding of a very important subject, as a small but cordial tribute of respect.

I am, SIR,

Your very obedient servant,

THE AUTHOR.

Baltimore, Sept. 1, 1796.

ADDRESS

TO THE MEMBERS OF THE COMMITTEE
OF HEALTH OF BALTIMORE.

GENTLEMEN,

I PRESUME to lay before the public, some observations on a publication of Doctor Joseph Mackrill, which lately appeared under the title of "*The History of the Yellow Fever,*" &c.

It appears to me peculiarly proper to address myself to you, who are in a great measure to be considered as the guardians of the public health in this town; more especially as Doctor Mackrill's pamphlet was expressly intended for the perusal of the citizens, whose representatives you are.

To my view it is a matter of considerable moment, that you should be supplied with the means necessary to enable you to form a just estimate of the little tract, which, I presume, was in part at least, offered as a direction for your conduct in the administration of the important trust delegated to you.

CONSIDERING

ADDRESS, &c.

CONSIDERING it as a rule of conduct either for you or the faculty, I cannot but think it a very improper book, and of dangerous tendency—This conviction gave birth to the following observations; and I will venture to declare, that whoever shall think proper to adopt it as their guide of sentiment or practice, will run great risk of committing very serious mischief, and of entailing pain and regret on his reflections.

I HAVE supported my remarks with the strongest evidence that experience can yield;—and surely no evidence can be more safely relied on.

I am, with sincere regard,

Your devoted fellow-citizen,

THE AUTHOR.

OBSERVATIONS, &c.

A MAN who, acting under a sense of duty,* wishes to dedicate his talents and the result of his experience to the good of mankind, even if he over-rates his capacity and the value of his knowledge, is entitled to some commendation on the score of a good intention. If his endeavours should fail of the end he proposes, it is but common good-nature to pardon weakness that is inoffensive and error that is innocent. He, however, who takes upon him to *instruct* in any point important to the welfare of the community, ought to consider that the best *intention* is not of itself a sufficient qualification for a very delicate task; nor will he, if he is prudent, commit his reputation upon the attempt, unless he is satisfied that his instruction is really calculated for the attainment of the object. If he errs, no purity of motive will protect him against the harsh sentence of rigid criticism; but if his errors are likely

* See Dr. Mackrill's History of the Yellow Fever, p. 26.

likely to produce mischief, the apology of a good-meaning ought not and will not save him from censure and reproach.

I am willing to allow the author of "The History of the Yellow Fever," all the indulgence and merit, to which a chastity of design may entitle him. I am willing to concede that his pamphlet is a sincere effusion of an honest heart, warm with gratitude for "the blessings he enjoys as an inhabitant of the republic of peace."* I am ready to allow that he intended it, in the plenitude of zeal, as a contribution to the public good—But zeal and error are often confederates; and I hope I shall be excused for saying, that if that book should unfortunately engage the confidence of those who may peruse it, very serious evils will probably arise from it.

Acting, as I certainly do, under an influence as pure and as disinterested as the author of "The History" can plead, I shall be sorry if any freedom I shall use in commenting on it, should give offence: for I certainly intend none. I have neither the vanity nor the weakness to think myself less obnoxious to mistake than others.

I have undertaken the very unpleasant labour of reviewing Doctor Mackrill's "History of the Yellow Fever," from a persuasion that

* See Dr. Mackrill's Dedication.

that it is less innocent than many may imagine; and that it is of some consequence that right ideas should be entertained of its merits.

Doctor Mackrill has thought proper to address his pamphlet to the citizens of this town and Fell's-point—and it appears both from the dedication and from the first page of the work, that he means it for a subject of their deliberation. Upon this I would observe, that it is on all accounts improper to refer a topick of this kind as an exercise for popular discussion. Of all the subjects of literature or philosophy, those of medicine are what the public are least qualified to investigate. What indubitably proves the great impropriety of this submission to public judgment is, that the very subject of the pamphlet was a matter of warm discussion among the most eminent professional characters of this country; and that to this hour it remains undecided in the minds of many intelligent persons.* The origin, causes, nature and treatment of the Yellow Fever, as it prevailed in Philadelphia in the year 1793, occasioned great diversity of opinion among the faculty of that place, and the obstinacy with which each party maintained its opinion, contributed, not only to the evil itself, but greatly to distract and confound the public mind with fears and doubts. Even “*the vulgar*
B *tongue*”

* See the various publications on this subject, in the Philadelphia newspapers, during the prevalence of the Fever there—also the papers of this town of the summer 1794, and Dr. Rush's account.

tongue" in which the Doctor has delivered "*the fruit of his experience*," can avail little towards rendering the subject intelligible or his book useful to those "*intelligent persons of all descriptions*," to whom he has addressed it. On an occasion like this, the author would have served the public at least as effectually, and have shewn the strength of his philanthropy as eminently, had he condescended to communicate his experience to his "brethren of the faculty," who are unquestionably the most proper persons to be let into the secret—without perplexing the heads of people, who must be lost in the maze of medical disquisition.—I am sorry that any thing in the conduct or language of the pamphlet should afford reason to say, that in divesting himself of his "technical garb," the author has assumed an empirical one. It must, indeed, be confessed, that if a homely dress and "*vulgar tongue*" can render it clear to the description of persons to whom it is addressed, nothing can be more explicit and intelligible than "*The History*," &c.

The author speaks with full assurance, that his "observations may serve to clear away an error which has uniformly created much mischief in the treatment of the Yellow Fever." It would not be surprising if so positive and decided a declaration should operate upon the confidence of those who can have no rule but their faith, for adopting or rejecting opinions, upon points on which they are not at all informed—

formed—and upon which they are, therefore, incompetent to form a rational judgment. It will fully appear in the progress of this investigation, how far this assertion of the author is consistent with fact and experience.

The Doctor acknowledges that his opinion on the subject of the Yellow Fever differs from that of many of the profession; and he extracts “consolation” from a very extraordinary reflection indeed! that if his opinion is erroneous “a conformity to it would be attended with no ill consequences”! Can Doctor Mackrill be serious in advancing such a monstrous hypothesis? Such a declaration is contrary to every rule of sound philosophy, and can never be admitted without manifest danger. It implies an infallibility attributable to no opinion and no practice in medicine. It is peculiarly exceptionable, in-as-much as it tends to lull reflection and doubt on an occasion where both ought to be carefully exercised.

Before I proceed to the examination of the main part of “The History”, I must solicit the reader’s attention to a fact which I conceive to be of considerable import in the case in hand, and which doubtless will have due weight in the minds of those who are to exercise an opinion on the merits of that work. It is this.—Doctor Mackrill has undertaken to instruct us upon a disease, not one case of which did he ever see. I mean the fever which occasioned such cruel devastation in Philadelphia

phia in the year 1793. The Doctor at that time, I believe, resided in the West-Indies. Had he employed his genius in giving a faithful detail of the fever which he *did* see, he would have been laudably and usefully engaged; but in this instance I am free to think, that he has greatly misapplied his time and his talents; especially if it is considered, that we have already an elaborate and accurate account of that disease from the masterly hand of Doctor Rush; that in his "History", Doctor Mackrill has not suggested one new or useful fact, and that he ultimately depends on Doctor Rush's work to supply the deficiencies of his own. A moderate share of reflection would have convinced the Doctor, that the subject he has chosen is not the most happy.—That ambition is humble and modest to excess which can be satisfied with the gleanings of a harvest, every honour of which has been culled by another's hand.

From what I have just said then it is evident, notwithstanding he assures us that his work is "the fruit of his experience," that he must have derived what knowledge he may have acquired of this disease, through other channels than his own observation.

Should I, in this or in any other instance, be so unfortunate as to misrepresent Doctor Mackrill, I very sincerely beg his pardon.

The object of Doctor Mackrill's argument is, to establish three points. *First*, That the
Yellow

Yellow Fever of Philadelphia and that of the West-Indies are different—

Secondly, That the Philadelphia Fever did not originate in the city, but was imported from some of the Islands of the West-Indies—and

Thirdly, That it was the same fever which was brought from Africa to the Island of Grenada.—To this favourite thesis all his reasoning tends.

Let us now attend to the manner of reasoning, and the arguments by which this author endeavours to support these positions.—

“ The disease in question is, perhaps, with accustomed propriety, termed Yellow Fever; but that it is the common Yellow Fever of the West-Indies, I take upon me to deny.”* The extreme imperfection of medical nomenclature is well known to every practitioner, as well as the confusion and uncertainty it occasions in the treatment of diseases. Perhaps these evils are in no instance more fatally experienced, than in that kind which passes under the fashionable but improper name of Yellow Fever. It has of late become the fashion to apply this appellation to such fevers as are attended with a yellow skin. But the epithet is undoubtedly improper, and is productive of great deception. That the yellow tinge of the skin is not a necessary, but an incidental symptom

* History, p. 5.

tom is certain from the authority of the best writers on the disease. Doctor Rush remarked that many cases wanted that appearance entirely;—Doctor Mosely also says, “I have used the word *yellow* in compliance with custom; but I even distrust that name, as the inexperienced may be looking for that appearance, and not find, until it is too late, the disease he has to contend with.”—But nothing marks more strongly the impropriety of the application of the term *yellow*, as the distinctive sign, than the passage above quoted from Doctor Mackrill’s pamphlet, which makes two different Yellow Fevers, requiring very opposite modes of treatment.

Whatever foundation there may be for supposing a difference between the Philadelphia Yellow Fever and that of the West-Indies, the reasoning by which Doctor Mackrill endeavours to establish his opinion is rather singular.

“Convalescents from the Yellow Fever, for many years past, have been constantly in the habit of visiting the shores of the United States, for the change of air; but I never could learn, that it was ever deemed dangerous to take such passengers on board of vessels, the crews of such vessels did not catch the disease, nor was quarantine ever thought of when such vessels arrived—Nay, the very cloaths of such convalescents, which are always known to secrete a vast portion of contagious matter, would have been abundantly sufficient to communicate

cate the disease; but no instance of this can be traced.”*

This is one argument which Doctor Mackrill advances in maintainance of his first proposition. I do not mean to question the veracity of the position; but certainly, if there was not more cogent evidence of its truth than what is contained in the paragraph just quoted, the matter might still wear a very questionable shape. It would have been infinitely more conclusive, had the author proved the impossibility of importing the common West-India Yellow Fever, by shewing that it is not contagious.

Another argument made use of by the Doctor, to prove the dissimilarity of the diseases, is, that the fever which prevailed in Philadelphia, would not bear the same method of treatment which prevails in that of the West-Indies.

Let us take a fair and impartial view of this subject, and see how far this assertion is consonant to fact.

In page 6, we are informed that bark and wine constitute almost the *unicum remedium*—and are what the author calls by the significant epithet of “sheet anchor,” in the “West-India Yellow Fever.”

The nature and causes of this disease have been a subject of a diversity of opinion, as
the

* History, p. 5.

the disease itself has of treatment. It is found described under a variety of appellations, according to the ideas entertained of its nature, its origin, its principal symptoms and other circumstances. Some supposed it a disease new and of recent origin;—some that it was imported;—others that it was only a variety of an indigenous disorder. The diversity of forms and combinations under which it occasionally appeared, contributed to this discordance of sentiment. It is, therefore, not at all surprising that what is called the Yellow Fever, in the West-Indies, should be liable to very incongruous modes of treatment: since its character would be liable to all those incidental varieties which are constantly induced, by the prevalence of the circumstances of climate, vicissitudes of season and weather, and whatever else controuls or changes the character of diseases. We consequently find, that according to the different states of the system, while under the action of the disease, it has, at one time, been treated by such applications as impart vigour to the powers of the body;—and at other times by such as diminish them.—

I shall take upon me to shew that Doctor Mackrill's declaration that "wine and bark are the sheet anchor in the Yellow Fever of the West-Indies"—is not founded either in the nature of the disease, or in the known and general practice of the best practitioners in the West-Indies.—I shall also shew, by the most undeniable authority, that it was the practice

to

to treat the Yellow Fever of the West-Indies on the same principle, and most minutely in the same manner in which that of Philadelphia was treated, many years before the date which the Doctor has affixed to his African Fever.—

It is pretty evident from the expression before quoted from Doctor Mackrill's book, that he supposes the West-India Yellow Fever to be a disease in which high debility is the urgent symptom—Under this notion he prescribes bark and wine, as the *sine qua non*. This opinion is indeed not new:—it has been considered in the same light by many others, who were not well acquainted with the disease—whose judgment was captive to preconceived notions; from whose mind even experience could not eradicate obstinate prejudice. But this doctrine is totally dissonant from the opinions held by the best reputed modern Physicians of the islands. That it is so, I shall proceed to shew by such authority as cannot be doubted.

Doctor Mosely, in his treatise on the Endemial Causus, or Yellow Fever of the West-Indies, says,—“ The truth is, that this disease is in the highest degree possible an *inflammatory* one, accompanied with such symptoms, *in a greater extent*, as attend all inflammatory fevers, and most strikingly the reverse of any disease that is putrid, or of one continued exacerbation.” (See Mosely on Trop. Dis. p. 414.)

How shall we reconcile this character of the Yellow Fever, with Doctor Mackrill's declaration, that wine and bark are the "sheet anchor" in it? Can any man seriously believe that these are proper remedies in an inflammatory fever? Let the reader compare the character of the West-India Yellow Fever given by Doctor Mosely, with Doctor Rush's history and treatment of the Philadelphia Fever. Whatever difference of opinion there may be, with respect to the theory of the diseases, one point is certainly agreed on, that there was in both cases, an excess of action, and that a reduction of it by the most prompt and active evacuations was necessary for the safety of the patient.

I shall proceed to shew by further facts, that the West-India Yellow Fever and that of Philadelphia accord in every essential circumstance of symptoms and treatment.

Doctor Mosely (page 417) remarks, that "subjects most likely to be attacked by the Endemial Causus (Yellow Fever) are the florid, the gross, the plethoric;—that sort of strong, full, youthful people with tense fibres, who in England (to use a vulgarism) are said to resemble the picture of health. In short, so are all persons who are of an inflammatory diathesis, and do not perspire freely."—

This observation confirms the opinion of its being a disease in which excessive action prevails;

vails; and, what is very worthy of remark is, that even writers who esteem the Yellow Fever to be of what is commonly called the putrid kind, acknowledge the necessity of powerful evacuating applications in the first stage.

In forming a judgment of the genuine character of this disease and in assigning its proper place in nosology, more confidence is to be placed in this uniform method of treatment, than in the names which have been arbitrarily imposed upon it.—*Hillary* calls it the *Putrid Bilious Fever*;—and yet his own account of it contradicts the propriety of the appellation; and the method which he recommends for its cure, is sufficient to satisfy us, that the name he affixed to it was the offspring of a misapprehension of its nature.

That the reader may be satisfied upon what foundation I alledge this, I shall subjoin one or two quotations from that author. Speaking of the intentions of cure, he lays down the following rules.

“ 1st.—To moderate the too great and rapid motion of the fluids, and abate the too great heat and violence of the fever, in the first two days, as safely and as much as we can.”

“ 2dly.—To evacuate and carry out of the body, as much of the putrid bile, and those putrid humours, as expeditiously and as safely as we possibly can.”

“ And

“ And 3dly. To put a stop to the putrescent disposition of the fluids, and prevent the gangrenes from coming on, by suitable antiseptics.”*

The manner in which he proposes to fulfil the first indication is by *blood-letting*. “ Wherefore bleeding in the beginning of the first stage of this disorder, either to a greater or less quantity, accordingly as the following symptoms and circumstances indicate, *is always absolutely necessary.*”¶ Again.—“ For which reasons, when I have been called in time (which is too seldom the case) I generally order 12, 14, 16, 18, or 20 ounces of blood to be taken away on the first or second day.”¶ This bleeding he repeated, “ if the patient’s pulse rises after the first bleeding,” &c.—This is a fact well worthy of attention—The pulse, according to this writer’s acknowledgment, sometimes rises after bleeding largely. No proof can be more explicit that this disease is not referable to those commonly called putrid—and no demonstration can be clearer, that bark and wine would produce pernicious effects in its early stage. “ If it be denominated a *Putrid Bilious Fever,*” says Doctor Mosely, “ what person, in treating a putrid fever, would think of large and repeated bleeding in the beginning? If it were a Putrid Bilious Fever, such practice would certainly be improper; therefore surely the term must also have an injurious tendency.”

I

* Hillary on the Diseases of Barbadoes, p. 156.

¶ *Idem*, p. 157.

I might here introduce a multitude of quotations from the same writer,* in confirmation of this truth; but as I intend in this place only to prove that the treatment of the Yellow Fever is not, by any means new, I shall content myself with one or two more extracts from Doctor Mosely's work.

“ It is unnecessary to fill many pages with a long catalogue of prescriptions and medicines, in the treatment of this fever, for it is comprised in a few words, and almost as few medicines; and requires only care and attention that those moments do not slip away, that the occasion is for ever lost, when

Bleeding	Diaphoretics
Purging	Blisters, and
Baths	Bark—

ought to have been timely used for the salvation of the patient's life; and that afterwards they are not untimely employed for his destruction.”†

“ Bleeding must then be performed, and must be repeated every six or eight hours, or whenever the exacerbations come on, while the heat, fulness of pulse and pains continue: and if these symptoms be violent and obstinate, and do not abate during the first 36 or 48 hours of the fever, bleeding should be executed, *usque ad animi deliquium*”—that is, till the patient faints!—

The foregoing authorities are sufficient to warrant the opinion, that a difference between the

* Hillary.

† Page 427—28.

the Yellow Fever of Philadelphia and that which commonly prevails in the West-Indies, is more in imagination than in fact. There is indeed every reason to suppose that there is a very near alliance between them. In almost every symptom and in the methods of treatment, there is a striking similarity. I cannot avoid expressing my surprise that this should have escaped the observation of a gentleman whose situation afforded him occasion for ascertaining the matter; and that he should have neglected authorities that are familiar to every practitioner.—When Doctor Mackrill asserts a thing as a fact within his own knowledge, I feel much reluctance and concern to insinuate a doubt of the authenticity of the voucher. But a parallel formed by Doctor Rush's account of the former, on the one side; and that delivered by the best reputed writers of the latter, on the other side, affords a proof of identity, too strong and stubborn to yield to the authority of "The History", &c. From every consideration of the subject, I am abundantly satisfied that Doctor Mackrill has committed an error, in attributing the fatal progress of the Philadelphia Fever to the confounding it with the Yellow Fever of the West-Indies: and I hope I shall be pardoned for dissenting from an assertion, which, indeed, is mere matter of opinion, and not matter of fact. The very reverse of the Doctor's proposition is strictly true:—for it was not until the mode of practice laid down by the best practitioners of the West-Indies, for the treatment

ment of the Endemial Fever of the islands, was adopted in Philadelphia, that the progress of that formidable disease was arrested.—

Other theorists invent something of argument to give colour, substance and validity to their opinions; but our author boldly rests the credit of his upon the weight of bare assertion. I rather consider this as a proof of prudence than temerity—for argument and facts are adverse to his opinion. In page 6, of Doctor Mackrill's "History", we read this very extraordinary sentence:—"no man can pretend to say, that such applications as bark and wine are improper in the West-India Yellow Fever, since they are known to almost every indifferent person in that country, as his sheet anchor." I call this extraordinary:—it is so—since it is totally repugnant to the known practice of the most reputable Physicians of the West-Indies. It is true that the bark is made use of in this disease; but that is only where a perfect intermission has taken place—(see Mosely, p. 444) or, as I presume, where the necessary application of debilitating remedies has produced such a degree of exhaustion, or where the same effect has been occasioned by the continuance of the disease—as requires tonic means for restoring what strength was unavoidably sacrificed. Were it possible to ascertain, by any practicable means, the exact quantum of reduction necessary to restore the natural healthy action of the system, I am very much inclined to think that bark, wine or other restoratives would

would be entirely unnecessary. It is not at all more inconsistent with the opinion of Doctor Mosely and others, respecting the inflammatory nature of the Yellow Fever, that bark and wine are sometimes necessary in the latter stage of it, than it is with a fact familiar to every practitioner, to wit: that bark and wine are not unfrequently found necessary in pleurisy—in which no one ever entertained a suspicion of any thing putrid.

I cannot forbear quoting a passage from the same writer, which will confirm, in a great degree, that Doctor Mackrill is mistaken upon this subject.

Representing the condition of the patient in the advanced stage, he observes—"It is in vain to think of bark and antiseptics, though the approach of sphacelation be evident."* Here then is a case which, *a priori*, one might suppose would urgently require the use of bark; but experience proves that so far from affording any advantage, it aggravates the patient's hazard.

Again—The same respectable author, after having observed on the fallacy of the early remission of symptoms, proceeds to say, "They who unfortunately make any dependance here, desist from further evacuations and proceed to giving bark and cordial nourishment. Every person about the patient is filled with flattering hopes

* Mosely on Tropical Diseases, p. 448.

hopes of his recovery. But the evacuations have been discontinued too soon, and have not been sufficient to extinguish entirely the inflammatory disposition of the disease;—which now aggravated, breaks out, and rages with redoubled violence, and hurries the patient into the second stage of the disease, and then soon out of the world.”¶

In two instances in which, in common cases, the bark is necessarily resorted to, it is found, not only to produce no alleviation of the disease—but that it actually hurries on the fatal scene. In what state—in what circumstances of the disease, I would ask, will Doctor Mackrill prescribe bark? In what manner do its effects in the Yellow Fever, warrant the application of the epithet of “*sheet anchor*”?

Even in the very advanced stage of the disease, when “every symptom and circumstance evidently shew, that a dissolution of the globules and texture of the blood, and a putrescent, colliquative, gangrenescent state of the fluids, now hasten on apace with all their fatal symptoms,”*—even then, when an opportunity and demand for the bark would appear most warrantable, it is inadmissible. Hear what an avowed advocate for the opinion of its being a *Putrid Bilious Fever*, says—“In these circumstances the *Cortex Peruv.* may be thought to be the best and most likely medicine to succeed. I grant that its well known
D efficacy,

¶ *Idem*, p. 449.

* Hillary, p. 163.

efficacy, in preventing or putting a stop to mortifications, promises much: but the misfortune is, that this drug is so disagreeable to most palates, and the stomachs of the sick in this disease are so much affected, and so weak and so subject to reject every thing, even the most pleasant and innocent, that they can rarely take it in any shape, and still fewer can retain it when they have got it down, &c.” ¶

I am really at a loss to conceive upon what authority the bark can be considered as the “sheet anchor” in the Yellow Fever.

It is sufficiently clear from the antecedent considerations, and will appear still more evident from what will hereafter be shewn, that Doctor Mackrill’s denial of the affinity of the Philadelphia Yellow Fever to that of the West-Indies, is founded in misconception and error.—It would seem as if the Doctor imagined that similar diseases could not exist in distant places, unless one was derived from the other by contagion:—for his principal argument against the affinity of the two fevers is, that the common West-India Yellow Fever cannot be communicated by contagion, and therefore the Yellow Fever of Philadelphia could not be the same kind of disease which commonly goes by that name in the West-Indies. If Doctor Mackrill had attempted to prove his position by asserting, that a pleurisy resembling the pleurisy of European countries could

could not occur in America, because pleurisy in Europe is not contagious, the proof would have discovered at least as much of sagacity as the proposition.—The error lies in supposing, what is contrary to the observation of every man in the least versed in medical matters, that remote situations cannot be in circumstances so much alike, as to originate similar diseases. Such an inference is certainly deducible from Doctor Mackrill's mode of reasoning. It would be a prostration of argument to attempt a refutation of such a doctrine.

I pass on to the consideration of the Doctor's account of a new species of Yellow Fever, which he calls the *African Fever*. He informs us, that “a large *slave* ship belonging to Simmon and Hankey, of London, had just before arrived from Africa, on board of which, a few days after her arrival, a most alarming species of Yellow Fever made its appearance—every white man on board died of the disease. It was soon communicated to the inhabitants, and great numbers fell victims to its rapacity, particularly those who had lately arrived from Europe, hardly one of whom survived it. It seems the contagion had remained inert during the passage of the ship from Africa; but no sooner did she arrive than it burst forth with all its horrors, for here it found, in a great measure, its own proper *nidus*, its own nutritious *pabulum*, in the close heated atmosphere so frequently prevailing in Grenada.”

It

It must strike the mind of the reader of this part of Doctor Mackrill's pamphlet, if he is qualified to judge at all, as a most extraordinary circumstance, and entirely out of the common law of contagions, that a fever so uncommonly malignant and destructive, did not make its appearance, until some days *after* the ship had arrived! That a contagion so virulent generated and imbibed in Africa, should have remained dormant, torpid, inactive during the voyage from thence to Grenada, is scarcely reconcileable to the known operation of contagious matter on board of ships. Can any man seriously suppose that such a contagion, existing among the people on board could have a more propitious *nidus*, than among a number of wretches, crowded together in the foul and noisome hold of a Guinea-man, and compelled to respire an atmosphere hot, close and contaminated? Is it not beyond the comprehension of the understanding, how a contagion so characterized should remain inert, until it came into circumstances *less* favourable for its action? Can the atmosphere of the island of Grenada be a more "nutritious *pabulum*" for such an infection than the foul and corrupt air in the confined and nasty hold of a slave-ship, replete with filth and impurity and poisonous exhalation?

I have so far proceeded upon a concession that the Hankey was really a slave-ship—I do not consider it an affair of material moment whether she was or not.—But the truth is, that
she

she was not—There were none on board but adventurers from Great-Britain.—It is scarcely worth while to censure this want of precision—although a writer who professes himself an historian ought to be faithful even in trifles.

But setting aside reasoning upon this subject, I shall make it appear that Doctor Mackrill has not only unaccountably misstated the fact—but that he is totally wrong in every circumstance which he alledges relative to the origination, nature and treatment of the fever brought by the Hankey.

Doctor Mackrill tells us, that “a few days after her arrival, a most alarming species of Yellow Fever made its appearance,” &c.

Here are two propositions on which I think it proper to submit a few remarks: First, that the disease made its appearance *after* the arrival of the ship at Grenada:—Secondly, that it was a species of the Yellow Fever.—On points of fact the mind must be determined by the validity of the evidence by which they are supported. To some it may perhaps appear a trivial consideration whether the disease appeared before or after the arrival. But surely a work which professes to be a history ought to be correct in all its facts—and should not be vitiated by false allegations, when truth may be attained without the labour of research. By detecting its errors, I shall shew how extremely defective and imperfect Doctor Mackrill's

rill's pamphlet is, even in the historical part; and if those errors are found to multiply in every page of that performance, they afford an apology for indulging our skepticism with regard to the whole—If he is wrong in the statement of facts of which he says he has had personal cognizance, we are certainly excusable for not subscribing to the validity of his *opinions*.—But I shall leave nothing to the determination of conjecture.

To explain this matter as it is, I will just cursorily observe, that the Hankey was employed to transport a number of adventurers from Great-Britain to Africa, with a view to the establishment of a colony there. Circumstances rendered the project abortive, and compelled the people, to the number of two hundred and upwards, to confine themselves on board, where they continued near twelve months, exposed to all the discomforts of imprisonment, and to those prolifick sources of disease, heat, wet, filth, unwholesome air, and bad food. In this melancholy condition, cheerless and despondent, while they yet remained at the island of Boullam, this dreadful fever added its destructive ravages to the disasters of the unfortunate people. Many were destroyed by it. From Boullam, after suffering incredible difficulty and distress, they reached St. Jago. There they communicated the disease to the crews of two ships of war. From thence the Hankey sailed for Grenada.* Nothing

* See Chisholm on the Malignant Pestilential Fever of Boullam, p. 85, 6, 7, &c.

Nothing more need be said on this topick, to satisfy the reader that Doctor Mackrill's account of the time and manner of its appearance is at least inaccurate.—How far the second proposition, to wit, that this was a species of Yellow Fever, is true, will appear in the sequel. I shall proceed to the consideration of a subject of more immediate importance.

Doctor Mackrill is of opinion and very confidently asserts, that the Yellow Fever of Philadelphia was the very same with the malignant fever brought by the Hankey from Boulam. It would have contributed to the elucidation of this point, as well as to the satisfaction of his readers, had he accompanied his assertion with a correct portrait of the Boulam Fever, that by comparing the features of one with the other, we might have judged of their similitude.—I shall supply in some measure this deficiency of “The History”, &c. and then leave the ingenious and candid reader to determine for himself.

“The patient,” says Doctor Chisholm, “without any previous complaint, suddenly becomes giddy; he loses his eye-sight; every thing seems to move round him with inconceivable velocity; he falls down almost insensible, and in that state remains half an hour or upwards. During this paroxysm the body feels cold, and is overspread with cold sweat which issues from every pore in astonishing abundance. On his recovery the cold goes off,

off, and is instantly succeeded by intense heat, and quick, small, hard pulse; the head aches dreadfully, particularly the forehead and siniput, which is generally accompanied with pain in the right side and at the præcordia. The last, however, has never been acute, and may rather be called oppression than pain. The eyes are much inflamed, watery, protruded and wildly rolling; the face much flushed; much heat is felt at the pit of the stomach, and that organ seems considerably affected by the *nausea* and frequent retching and vomiting, which then come on. The patient soon after complains of intolerable pain in the small of the back and in the calves of his legs; but the last appears to be the most violent. During twelve, eighteen, twenty-four or thirty-six hours these symptoms continue increasing, except the quickness and hardness of the pulse, which do not change materially during that time, and are then succeeded by general coldness, cold sweat, a greater or less degree of coma and delirium, or a state very much resembling intoxication. Life, in this state is lengthened out to sixty or ninety hours from the first attack. A short interval of reason then takes place; the patient considers himself better, and is for a moment flattered with a prospect of recovery; but a fit as sudden and unexpected as the first comes on, during which he foams at the mouth, rolls his eyes dreadfully and throws out and pulls back his extremities in violent and quick alternate succession. In general the patient expires in this fit; but
some

some have recovered from it, and continued rational a few hours longer, when a second fit has carried them off. This has been generally the progress of the disease, in its worst form; and indeed there have not been many deviations from it: The principal of these were, the general symptoms coming on, without any preceding convulsion. The patient has been in some instances comatose from the very commencement of the disease; others have had the disease ushered in by a frequent succession of short convulsive fits, and it has afterwards been marked with constant delirium and cold clammy sweat, without any intervening heat of surface, &c. The disease too, in a few cases, has seized the patient in the manner most other fevers come on; that is, with shivering and a sense of cold. The most constant symptoms, and consequently those which distinguished the disease, were the uncommon suddenness of the attack, the remarkably acute pain in the loins and calves of the legs; the watery, inflamed and rolling eye; the flushing of the face; the tendency of the coma from the onset; the peculiarity of the delirium attending; and the pain confined to the forehead, seldom extending to the temples or even the vertex.*

I need not point out to the intelligent reader the obvious difference between the symptoms above described, and those which constituted the fever of Philadelphia. If there is any
 E parity

* Chisholm's Essay, &c. p. 105.

parity in some of the symptoms of the two fevers, it would be absurd, on that account, to conclude that the diseases were the same; for the difference between the leading circumstances, those which governed the treatment, is so plain and so great, as to mark a distinction beyond a doubt.

To elucidate this subject and enable the reader to form a fair and accurate determination, I shall proceed to exhibit a comparative view of the circumstances of the two diseases in which they disagreed. For this purpose I shall avail myself of the best authorities for my guide; Doctor Rush's account of the Bilious Yellow Fever of Philadelphia—and Doctor Chisholm's essay on the Malignant Pestilential Fever of the Hankey.—From those evidences let the public judge.

There is a very remarkable difference observable in the mode of attack. The Yellow Fever of Philadelphia was ushered in by certain premonitory symptoms. "The precursors, or premonitory signs of this fever," says Doctor Rush, "were, costiveness, a dull pain in the right side, defect of appetite, flatulency, perverted taste, heat in the stomach, giddiness or pain in the head, a dull, watery, brilliant, yellow or red eye, dim and imperfect vision, a hoarseness, or slight sore throat, low spirits or unusual vivacity, a moisture on the hands, a disposition to sweat at nights, or after moderate exercise, or a sudden suppression of night sweats."

sweats.”—In the Malignant Pestilential Fever of Boullam there was no premonition:—The attack was sudden and “without any previous complaint.”

I beg leave to place in opposition to this, the accounts of the indigenous Yellow Fever of the West-Indies. I wish to impress upon the reader’s mind the corresponding circumstances of this and the Yellow Fever of Philadelphia, in order that he may judge how far Doctor Mackrill’s denial of their identity may be just.

Doctor Mosely observes, “When a new comer is seized with a sudden loss of strength, and a desire of changing, for rest, in every position, without finding it in any, those symptoms which constitute the Endemial Caulus may be expected. This is of great consequence to be understood, and to be well remembered.”* From hence, then, it appears that the approach of the West-India Yellow Fever is indicated by premonitory signs, as well as that of Philadelphia. The knowledge of this fact, in both instances, derives its importance from hence, that preventative means may be timely used. By an early attention to the premonitory signs, and by a judicious application of the proper defensive remedies, the disease has often been prevented. But in the Boullam Fever the attack was made with so little previous intimation, that no preventative could be applied.† In this respect, therefore, the Philadelphia

* Page 418.

† See Chisholm, p. 149.

delphia and the West-India Yellow Fever agree—and in the same circumstance they differ from the Boullam Fever.*

Another circumstance in which these diseases differed was, in the subjects most obnoxious to it. The fever of Boullam invaded strangers and natives indiscriminately. It is a notorious fact, that strangers, especially those who come from climates exterior to the tropics, are infinitely the most liable to it. But the Philadelphia Fever was almost entirely confined to the residents of the city. "The refugees from the West-Indies," says Doctor Rush, "universally escaped it. This was not the case with the natives of France who had been settled in the city." (p. 94.)

Here we may discern a strong affinity between the West-India and Philadelphia Yellow Fever. The inhabitants of the city who were unaccustomed to the circumstances which gave birth to the disease, were the only subjects of it: The West-Indians, who were habituated to them, escaped.

I consider this fact of sufficient importance to merit attention. It is well known that the natives of the West-Indies, and those who by long residence are inured to the climate, are rarely subject to the Yellow Fever, unless it is provoked by intemperance or great irregularity. Strangers

* See Rush, p. 36, 37. Chisholm, 105, 107. Deveze on the Epid. Dif. of Philad. p. 40:

Strangers who go from what are called temperate latitudes—those in which the changes of the seasons are strongly marked, are very liable to become its victims.—Now, an inhabitant of a temperate climate who, after a cold winter, is exposed to a summer, which, with respect to heat and other conditions, resembles the climate within the tropics, will be in a situation very exactly similar, with respect to the causes of disease, with one who goes from the one climate to the other, and consequently will be more obnoxious to the diseases of such a situation, than one inured to it. This I take to be the true interpretation of the fact which I have quoted from Doctor Rush.

Hæmorrhage was a symptom common to the Boullam Fever, and the Philadelphia Yellow Fever. The very opposite effects produced by it upon the health of the patients in the former and the last, afford another very strong presumption against their identity. In the Boullam Fever, we are told, profuse bleeding happened from various parts of the body—from “the nostrils, mouth, anus, urethra, sometimes from the canthi of the eyes.”—Doctor Chisholm expressly declares that “hæmorrhage has occurred in this disease much oftener and more profusely, and has been attended with more dangerous consequences than in any other, the scurvy perhaps excepted, that I have met with.”* Contrast this with the effects of the same symptom on the fever of Philadelphia,

* Page 120.

ladelphia, as recorded by Doctor Rush. This gentleman informs us that “ hæmorrhages belong to the symptoms of this fever, as they appeared in the sanguiferous system.” The discharges were often in unusual quantity, and they took place universally, and from various parts of the body—But so far was this from being attended with the same hazard as in the Boullam Fever, that the perfect relief which they uniformly afforded, had a considerable share in inducing him to adopt the practice of copious bleeding.* The Doctor mentions the case of a young woman, labouring under the fever, who had been bled. In the morning after, she lay weltering in blood, which had flowed, in the night, from the orifice in the vein. Every symptom of the disease disappeared.—Can any thing be more decisive?

We find the same symptom often occurring in the common West-India Yellow Fever. I wish this to have its full weight in the reader’s mind—and shall therefore transcribe Doctor Mosely’s observation on it.

“ In the early part of the disease,” (the common Yellow Fever of the West-Indies) “ spontaneous hæmorrhage is always critical, and should never be suppressed.”¶ Hence then it appears, that in the Yellow Fever of the West-Indies and that which occurred in Philadelphia, spontaneous bleeding is always attended with remarkable benefit; while in the Boulam

* Rush’s Account, p. 157, 279. ¶ Mosely, p. 432—3.

lam Fever it never failed to destroy the patient.—

That the Boullam Fever was the effect of a most active contagion, I make not the least question—It is very proper to remark, that its action took place generally in a very short time after its application to the system, and that it was never postponed beyond the fourth day after exposure to it. That the Yellow Fever of Philadelphia was really contagious as has been generally supposed, is a position, in my opinion, very far from being unquestionable. There are, in fact, very good reasons for doubting it. I do not mean, however, at present to make it a subject of disquisition:—but, whether it was contagious or not, there was a very observable difference in the latter, in which many instances occurred of persons in whom the seeds of the disease were evidently in a state of germination for a considerable time before the symptoms disclosed themselves in the form of disease. Doctor Rush was himself an instance of this—There were many also in whom a disposition to the disease was distinctly legible in the countenance and functions of the body, although that disposition proceeded no further than a threat.

Another distinguishing circumstance was a symptom which occurred in the Boullam Fever, entirely unnoticed and unknown in the Yellow Fever of Philadelphia: This was an extraordinary affection of a part of the genital system.

system. As this symptom has in it something singular, I shall transcribe Doctor Chisholm's account of it, for the satisfaction of the reader.

“ About the end of the second day, the patient begins to complain of a violent pain in his testicles ; on questioning him, he says he feels a contraction of the spermatic chord, and is sensible of a drawing up of the testicles toward the abdominal ring. On examination they appear very much lessened in size, are drawn up considerably toward the abdomen ; and the scrotum appears at the same time remarkably flaccid and empty. The surface of the scrotum becomes soon after very painful, and an excoriation takes place, chiefly at the most descending part, from which a considerable quantity of very offensive purulent matter issues. At the same time a similar discharge from the urethra takes place ; which ceases with the disease, when the event is favourable, or becomes ichorous or bloody and insufferably foetid, when death is the consequence. In cases which terminate favourably the whole scrotum in a few days is covered with a coat of hardened pus, which, in the convalescent state, comes away very easily by means of a warm bath. The thickness of this coat may be about a fourth of a line, and when separated, resembles much moistened parchment. In fatal cases the affection of the scrotum always terminates in gangrene, a few hours before death.”*

If

* Chisholm, p. 122.

If I mistake not, I believe no such appearance was observed in the fever of Philadelphia. In the Boullam Fever it was considered as a critical affection.†

There was another singular symptom which contributed to form the barrier between the two diseases. This also was peculiar to the Boullam Fever—and I quote it from the same respectable authority.

“ A suppression of urine is by no means an uncommon symptom in the bilious remittents of the country; and in general it is a circumstance that often occurs in fevers of a synochus or typhus character: But in the Malignant Pestilential Fever it is particularly remarkable for its coming on early, its duration, and the cause which seems to produce it.” This cause is a thickening of the coats of the bladder—and for an exact account of this appearance the author refers to a paper of Doctor Gelchrist, in the third volume of the Edinburgh Literary Essays. He then proceeds to say, “ Here I shall only observe that the suppression is accompanied by violent pain above the os pubis; a scalding in the urethra; a sense of fulness, without any visible turgescence in the region of the pubes; a considerable contraction and contortion of the penis; and the urine is generally of a very deep red colour; sometimes brownish; sometimes green; very frequently bloody; and in a few instances much

F

inclining

† *Ibid.*

inclining to black, and of an oily consistence. The smell of the urine was generally offensive in the highest degree."

The dissections of subjects who were victims of the Philadelphia Fever, exhibited no such appearance; and if I do not err, no affection similar to the one above recited, was noticed.

It is unnecessary to usurp more of the reader's time, in pointing out all the symptoms in which these diseases essentially varied. A careful comparison of the two authorities I have quoted will give him the fullest satisfaction.

I now proceed to other proofs, which will more than any thing else, tend to place this subject in a clear and unquestionable point of view.

An agreement in some symptoms can be no argument of an identity of diseases, which differ in the most essential symptoms—In truth there are numerous symptoms common to almost all fevers of a highly malignant nature. But whatever similitude may have appeared between the fevers which form the scope of these remarks, to a superficial inquirer, their disparity, I apprehend, is most evidently and explicitly indicated by the very different effects of the same treatment on them.

If ever two fevers could be called by the same name, the Yellow Fever of the West-Indies

dies and that of Philadelphia surely might. There is a remarkable coincidence of their symptoms; and, what is a stronger confirmation, if possible, they required exactly the same method of treatment.

We are told by Doctor Mackrill that the Boullam Fever required to be treated, with some trifling variation, by the same means which so wonderfully succeeded in Philadelphia.—I submit with pain and reluctance to the offensive employment of pursuing this writer from error to error;—let me not be charged with a violation of civility towards him, in complying with what I conceive to be and intend as a respectful regard to the welfare of the inhabitants, by disproving his assertions which are not founded in fact and observation. To the impartial judgment of the candid and qualified reader, I submit with confidence.—To render this subject as plain and evident as possible, I shall lay before the view, a comparative sketch of the applications employed in each disease, with their effects.

The failure of the remedies which were at first employed in the Yellow Fever of Philadelphia, under a presumption of its putrid nature, lead to an investigation of the causes of the disappointment. A just reflection on the actual state of the system under the disease; and most probably a fortunate comparison between its symptoms and those of the West-India Yellow Fever, induced Doctor Rush to
adopt

adopt a plan of practice which, though opposed and vilified by some, justified its adoption by its admirable success. I shall consider these remedies, as far as may be necessary to my purpose, in the order in which they are arranged by Doctor Rush.—

First, *Purging*.—For this purpose Doctor Rush made use of the most active materials, and the evacuations produced were often surprisingly profuse. “Doctor Say probably owes his life to three and twenty stools procured by a dose of *Calomel* and *Gamboge*”! The effects of the operation of purgatives were always admirably salutary, although administered on many occasions where, to one less confident and experienced, it would have appeared desperate. The effects were, to raise the pulse when low,—and to reduce it when too high; to revive and strengthen the patient; to abate the fever; to produce sweat; to check vomiting; to remove obstructions in the lymphatic system: to prevent the yellow skin.—Never was practice more bold, free and successful, if we except his next principal remedy,

Now, let us oppose to this the method of evacuation practiced in the Boullam Fever.—Doctor Chisholm found purging necessary in that disease:—but mark!—he was under a necessity of exercising extreme caution and very mild means, lest he should “anticipate the fatal issue of the disease, by inducing an extreme degree

degree of debility"!—What was his purgative? Not the drastic composition of Jalap or Gamboge and Calomel; but "an ounce and an half of salts and two grains of tartarized Antimony, dissolved in a pound and an half of water"! Of this solution a wine-glass full was given occasionally, until gentle evacuation was effected!

This marks a strong and evident difference in the diseases. One of them bore and required profuse and reiterated purging; the other demanded the greatest nicety and caution in the management of this article!

The second principal remedy of Doctor Rush was *Blood-letting*.

It was neither by *intuition*, nor by *instinct** that this eminent practitioner was directed to the application of those remedies. From the exercise of his *reason* he was induced to adopt them. It was an induction drawn from a careful consideration of the constituent symptoms of the disease, and a comparison with others whose characters it resembled. The ill success which attended the method of treatment used under the idea which at first prevailed, that it was either the common Bilious Remittent or a Putrid Fever, led to a minute inquiry into its peculiar characteristics; and a reference to reason and research into authorities for a happier method of cure. It was doubtless from
the

* See Mackrill's History, &c. p. 10, 11.

the exact resemblance discovered between it and the Yellow Fever of the West-Indies, that the treatment by evacuation was introduced. I cannot resist this occasion of transcribing a passage from Doctor Rush's account of the Philadelphia Fever, which will at once contribute to support what I have just said, and also to shew that he thought very differently both from Doctor Mackrill and his prototype in practice, Doctor Stevens, with regard to the proper mode of treating the West-India Yellow Fever. Doctor Stevens, as well as Doctor Mackrill, considered bark and wine as the "*sheet anchor*" in that disease. Now, hear what Doctor Rush intimates upon that subject. "If I should survive my present labours I hope to prove that Doctor Stevens's theory of the disease in the West-Indies, is as erroneous, as the practice he has recommended has been fatal in Philadelphia."* But to return—

It is no way surprizing that when bleeding was first proposed as a remedy, it met with violent opposition; but certainly no practice was ever more justified by its event, and perhaps none ever carried to so great excess, with such beneficial consequences. "I drew," says Doctor Rush, "from many persons seventy and eighty ounces of blood in five days; and from a few a much larger quantity. Mr. Grubbe lost by ten bleedings an hundred ounces; Mr. George about the same quantity in five bleedings; Mr. Peter Mierke one hundred and fourteen

* Rush's Account, &c. p. 224.

fourteen ounces in five days;" and "Children, and even old people bore the loss of blood much more in this fever, than in *common inflammatory fevers*."†

Let us now contrast with this account, the effects of this remedy in the Boullam Fever.

"It has generally been recommended to draw some blood before other means are used, at the beginning of malignant and pestilential fevers. In the present instance, the ardent heat of the surface, the oppressed hard pulse, the pain in the side, the oppression at the præcordia, the head-ach and throbbing in the temples, seemed strongly to indicate the use of bleeding. Very little experience, however, was sufficient to shew the impropriety of it; and instructed by repeated examples of its hurtful effects, I very soon laid aside all thoughts of lessening the inflammatory state by means of it. Although the blood, drawn in the cases wherein this remedy was employed, was remarkably florid, and always threw up an inflammatory crust of greater or less thickness, and although the pains seemed to undergo a temporary mitigation, yet the consequence at the expiration of a few hours WAS ALWAYS FATAL!"*

Let it be observed here, that the objections to blood-letting in this fever, did not arise from conclusions *a priori*, supported by a stubborn determination against the adoption of a remedy

† *Idem*, p. 272.

* Chisholm, page 155.

dy which had proved itself beneficial: not from ungenerous jealousy—or party rancour, or personal opposition;—but from the events of experiments actually and honestly made; where the practice was invited by the aspect of the symptoms.

Is there a mind so fastidious as to require more coercive testimony than this? Is there an understanding so prone to names—so submissive to the feeble authority of unsupported assertion—or so perverse in error, that it cannot or will not distinguish the most evident dissimilarity of those diseases? If these circumstances do not sufficiently establish their difference, I ask, what can constitute a distinction? If the Philadelphia and Boullam Fevers have the near alliance attributed by Doctor Mackrill, I will be bold to say, that there is no manner of difference at all in fevers.

I will only add one more argument to shew that the Yellow Fever of Philadelphia and the Boullam Fever could not be the same, and which will remove every difficulty on this subject, that is not conceived in mistake and maintained by incorrigible pertinacity and prejudice.

The Boullam Fever was on all hands allowed to be generated from human contagion, and was propagated by human contagion. It is evident, then, that this fever could spread itself only by the agency of the same principle.

It

It was found that its influence did not extend to any considerable distance. But the fever which prevailed in Philadelphia in the year 1793, was undoubtedly generated in the city: It has been proved so by the strongest positive and negative evidence.—Many of the Physicians of Philadelphia, it is true, entertained a contrary opinion;—but Doctor Rush, in my humble conception, has removed every ground of question upon the matter. The College of Physicians, indeed, asserted its propagation by importation in a style and manner unbecoming the character of so grave a body:—They asserted it upon the slender evidence of conjecture; when called on for their proof, they had none—when it was demanded of them in what place, when and by what means was it imported, no satisfactory answer was given. It never was traced to any vessel; and what confirms this opinion is, that Doctor Hutchinson, whose duty it was to inspect foreign vessels, was very decided that it was not imported. He certainly would not have accorded with Doctor Rush in opinion, that it had its origin in the local condition and circumstances of the city, had he had any reasonable ground for attributing it to foreign introduction.

I have thus proved by the evidence of clear and decisive *facts*, that the Philadelphia Yellow Fever was not, as Doctor Mackrill has asserted, the Boullam Fever propagated from the West-Indies.

I now proceed to offer a few observations upon the practice as proposed by Doctor Mackrill.

If in informing us what he thinks ought to be done in the Boullam Fever, he means to explain what he would do if a Yellow Fever were to arise in this place, I must take the liberty to say, that such a proposal is inconsistent with sound reason. When he pretends to tell us what ought to be done in the treatment of a disease which has not existence, surely the Doctor forgets that acute diseases are very apt to alter their characters essentially, from the influence, force and combination of those local circumstances which invariably create the characters and peculiarities of such diseases, and that they will consequently demand a method of management arising out of the character which they may assume, and not from their nosological title. By our author's own acknowledgment, Doctor Stevens experienced the truth of this axiom.

I will take upon me to say that a Physician who makes the *name* under which a disease may pass, the rule of his practice, is a very improper person to take charge of health and life—and very undeserving of confidence.—It was not until the ordinary remedies in bilious complaints failed and did mischief, that Doctor Rush adopted his bold, decisive and effectual practice.—But that gentleman, amid all the cordial joy with which his heart was elated in
the

the possession of the means of triumphing over so formidable a foe, had too much modesty and too good an understanding to propose that practice as an invariable guide in every future disease that might be called *Yellow Fever*.

Doctor Mackrill denies that the Philadelphia Yellow Fever was the same as that which commonly prevails in the West-Indies of the same name; but he will have it to be the Boulam Fever imported from the West-Indies. This fever, which raged in the island of Grenada, with uncommon malignity, was infectious in the most extreme degree. Doctor Mackrill tells us that he saw and treated patients in that fever. According to his ideas of that disease he lays down a plan of cure, and that plan he proposes as a rule for practice in the Yellow Fever, as it is called, of this country. In the year 1794 a Bilious Fever prevailed in this town, to which the favourite appellation of *Yellow Fever* was appropriated. This fever was in no instance contagious. Many indeed were destroyed by it;—and it is well known that the method practiced by Doctor Rush in the Philadelphia Fever, and which was adopted here by some, did not succeed so admirably as it did under the direction of that gentleman in the Philadelphia Fever. This fact will contribute to shew how dangerous and useless it is to fit names and remedies together.

Doctor Mackrill introduces this subject by remarking, that a Physician ought to regulate
his

his conduct in the treatment of diseases, according to the indications of nature. There is no rule of practice more substantially founded than this: But it requires some judgment in a Physician to discriminate between actions which essentially constitute the disease, and operations instituted by nature for relieving the morbid affections. I find that it requires even more caution and discernment than I imagined. "The History of the Yellow Fever," &c. exhibits but too evident an example of this observation.

"The physician acting either as the servant or consultor of nature, is attentive to her operations; if the pulse are tense and quick, he will observe the countenance flushed and the eyes turgid with blood; if nature is left to herself under these circumstances, hemorrhagy is generally induced; loss of blood is then the indication she plainly points out."*

Had the author studied for the most unapt example to elucidate his principle, he could not have succeeded better than in the unfortunate instance just quoted!

Let it be remembered, that the author is treating of the Boullam Fever, or as he calls it, the African Fever. How does this precept of Doctor Mackrill apply to the phænomena of that fever, as delivered by Doctor Chisholm? This writer, who certainly took much pains

* Mackrill's History, &c. p. 17, 18.

pains to study the peculiarities of the Boullam Fever, tells us in the plainest language that the pulse was hard; the eyes always so much inflamed as to constitute one of the symptoms; and that the disease was attended with frequent and copious hæmorrhages—but so far was this symptom from inviting the use of the lancet, that Doctor Chisholm affirms, that it was invariably attended with more dangerous consequences, than in any disease that occurred to his notice, the scurvy excepted! Is hæmorrhage in such a case, an indication for blood-letting?

Now, the case was exactly the reverse in the Philadelphia Fever. In *that* hæmorrhage was a frequent symptom, but was always attended with salutary effects; and here it was a fair indication of nature. Indeed it was from observing the efficacy of spontaneous bleeding that Doctor Rush was led to make trial of blood-letting as a remedy.

In a former part of this essay, it was shewn that bleeding in the Boullam Fever invariably accelerated the fatal termination of the disease.

It is very singular, but has the appearance of fact, that Doctor Mackrill has imbibed his ideas of a fever which, he says, he saw and treated, not from observations actually made on its phænomena; but from an account of a fever which he did *not* see!—I beg his pardon if I misrepresent him.

It

It is very proper to observe that Doctor Mackrill in his pamphlet has sketched out the plan of practice, which he either did pursue or would have adopted in the *Boullam Fever*: for it must be held in mind, that he was never concerned in a case of the Yellow Fever of Philadelphia.—Either we must discredit the authority of Doctor Chisholm, or we must suppose that the author of “The History of the Yellow Fever,” &c. did, or would have done a great deal of mischief, had he treated the Boullam Fever, according to his precepts.

This author insists upon bleeding as a most necessary preliminary—and next to it purging. In this dictate we perceive nothing of that cautious reserve and hesitancy in the administration of these remedies, which Doctor Chisholm found so necessary.—*Bleed*—and if the pulse is tense and quick, yet low with a degree of hardness, *bleed on*, until the pulse becomes soft and moderate! These are nearly the author’s words.*

I beg the reader to recollect what Doctor Chisholm says upon this subject.† He tells us that not only the hardness and quickness of the pulse seemed to indicate bleeding as the proper remedy, but that the propriety of its use was apparently corroborated by several other symptoms: Yet it was found by fatal experience,
that

* Mackrill’s History, p. 19. † Besides the place already referred to in Doctor Chisholm’s Essay, see p. 158 of the same author.

that the patients upon whom the experiment was made, became in a very few hours the victims of a treacherous indication.—Blood-letting was, therefore, early expunged from the catalogue of remedies for the Boullam Fever.

From hence it is sufficiently plain, that if Doctor Mackrill's fancy of the identity of the Philadelphia and Boullam Fevers was strictly true, his scheme of treatment is little calculated for its relief.

After bleeding, Doctor Mackrill orders a smart purgative; thirty grains of Jalap, with as much Cream of Tartar!

Active purging was so far from being considered safe or salutary, in that disease, by Doctor Chisholm, that he found it necessary to act with much circumspection in the exhibition of it—lest he should irrecoverably sink his patient.

Read Doctor Mackrill's pamphlet, and one would imagine that there were few symptoms to engage the attention of the Physician, and those few so plain and easy, that the symptom and the remedy succeeded of course—that the disease, in fact, was to be cured by *instinct** rather than by judgment: As if to bleed and to purge could be the only acts to be done, and these once exhibited, the Physician might slumber over the disease! Doctor Chisholm certainly

* See page 10, 11, of "The History", &c.

certainly experienced very differently.—He found the exercise of the closest attention, the nicest discrimination and the most mature judgment necessary in his treatment of the Boullam Fever.* In proof of this, besides what has been said on the article of blood-letting, I shall mention, that he found his patients could not bear the evacuation of purging, but under great caution. This is confirmed by the very trifling dose which he was obliged to employ—to wit—about one drachm of salts with about one twelfth part of a grain of Tartar Emetic.

If then there is truth in Doctor Chisholm's account, not only bleeding, but active purging also were inadmissible.

But neither bleeding nor purging, which Doctor Mackrill so strenuously recommends, was the remedy that the practitioners of Grenada found the most availing. A happy experience taught them the advantages derivable from Mercury, given in such doses and applied in such a way as to excite the most speedy *salivation*.† It secured more from the deadly effects of the Boullam Fever, than all else that ingenuity could suggest. Of this Doctor Mackrill does not intimate an idea.

I again beg the reader to bear in his recollection, that this author is laying down a course of treatment for his *African Fever*, and that it is

* See Chisholm, p. 158. † See Dr. Chisholm's Essay, &c. p. 158, &c.

is only an unfounded, mistaken imagination of his, that the Philadelphia Yellow Fever, and indeed every other Yellow Fever that may hereafter occur, must be derived from the Boullam Fever.

It is unnecessary to make any further observation on Doctor Mackrill's therapeuticks in the Yellow Fever;—having said and proved enough, by the soundest testimony, to convince any mind qualified to determine, that in every article of the HISTORY, NATURE and CURE of the fever of which he professed to treat, he has unfortunately been extremely *deficient, erroneous and imperfect*.

The evil which would be likely to arise from the doctrines contained in the subject of the foregoing observations is, that a wrong direction is given for the means proper for preventing the rise of contagious and other dangerous endemics among us. It is of the highest consequence that right ideas should be held on this point, because the methods of prevention will necessarily be regulated by the opinion that may prevail of the manner of their origin.

That diseases of a malignant, infectious nature may be imported from abroad; and that every necessary precaution should be used to prevent the propagation of imported contagion, no man in his senses would deny—But he is little acquainted with the causes of diseases,

H

who

who supposes that *importation* is the only channel through which they may be introduced—or the only danger to be provided against. Such causes unhappily exist among us, and there is no object which ought more sedulously to engage the attention of the inhabitants of large towns especially, than the removal or correction of them.

The causes existing among us, which are apt to excite dangerous endemics, are filth and foul air. To these may be added intemperance in living, and, with respect to the condition of the system, vices of the non-naturals. It behoves those to whom is intrusted the duty of providing for the public health, to have a strict regard to the removal of every source of putrefaction—to have the streets and other public parts of the town purified of all filth—to have all the receptacles of stagnant water filled up—and as far as possible, to induce the inhabitants to cleanse and ventilate their houses perpetually.

It is no part of the design of this work to enlarge upon this topick—and I wave the subject the more readily here, because I propose, at no distant day perhaps, to undertake a full and free discussion of it in another work.

Of all the tasks or duties in which a man can engage, that of criticism is the most invidious. If the subject of his remark demands the exercise of a free and honest power, plain-dealing

dealing will expose him to the hazard of offending the feelings of vanity and self-sufficiency:—his conduct will be most likely imputed to mean, malicious and dishonourable motives: Pride, irritated and angry, with calumny for its auxiliary, will arm itself against him.

Under a prospect so unfavourable, it requires some courage, supported by a strong confidence in the propriety of the purpose, to venture on a scene which promises little more satisfaction than what may arise from the consciousness of fulfilling a duty.

Nothing but a full persuasion of the dangerous tendency of the publication, upon the merits of which I have animadverted, and a hope that, by exposing it in a proper point of view, I might render a benefit to the inhabitants of this town, could have induced me to undertake the ungrateful employment. I should even have considered the little tract in question, as perfectly innocent, notwithstanding its multitude of errors, had it been confined to the eye of those who are capable of judging fairly of its value; had it not been avowedly intended for the perusal of the *citizens* to whom it is addressed. Nor should I have conceived it worthy of a serious reply, if there was not good reason for apprehending, from the industry and method used to disseminate it among the inhabitants, that its errors might be adopted as facts and truths, and that a remissness
of

of attention to the proper means of preservation against the rise and propagation of the disease, of which it professes to treat, be very likely to happen.

to mean, a tedious and disagreeable, with a view to the security, will still be against

THE

Under a pretext of unavailability, it requires some course, supported by a strong confidence in the propriety of the purpose, to turn on a scene. *S. I. N. I. S.* In this more distinction than what may arise from the commonness of following a duty.

Nothing but a self-reliance, the danger of our tendency of the public mind, the one of which have advanced, and the hope that, by exposing it in a proper point of view, I might render a benefit to the community of this town, could have induced me to undertake the present self-employment. I should have been content the little that I have

Medical Pamphlet

multitude of errors, but it has been compared to the eye of those who are capable of judging fairly of its value; had it not been avowedly intended for the perusal of the public, to whom it is addressed. I should have been content with a copy of a letter to the public, and a good reason for approaching, from the industry and wisdom used to distinguish it among the multitude, that its spirit might be taken as a lesson and truth, and that a remembrance of

