A sequel to an essay on the yellow fever; principally intended to prove, by incontestable facts and important documents, that the fever, called bulam, or pestilential, has no existence as a distinct, or a contagious disease.

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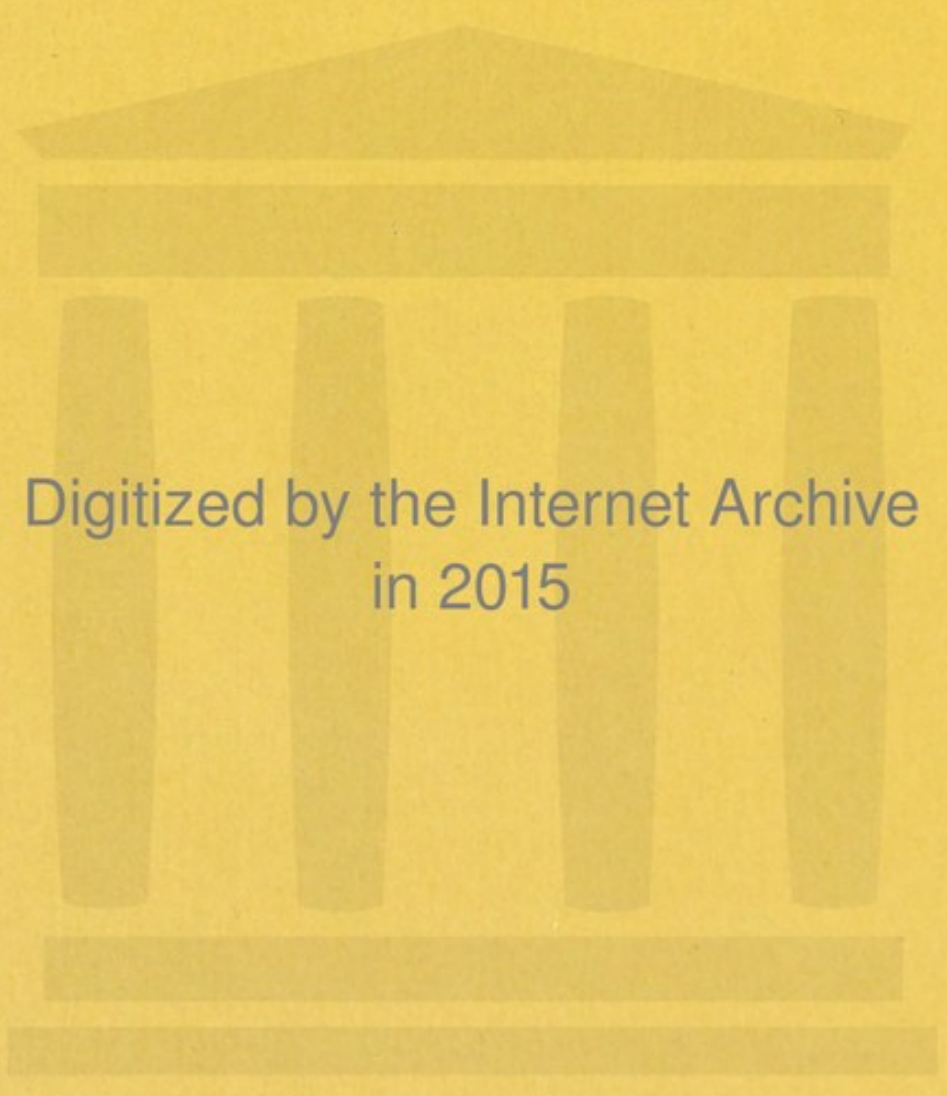
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A

SEQUEL

TO AN

ESSAY

ON

THE YELLOW FEVER;

PRINCIPALLY INTENDED TO PROVE, BY

INCONTESTABLE FACTS AND IMPORTANT DOCUMENTS,

THAT

THE FEVER,

CALLED BULAM, OR PESTILENTIAL,

HAS NO EXISTENCE AS A DISTINCT, OR A CONTAGIOUS DISEASE.

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.

LONDON:

PRINTED FOR J. CALLOW, CROWN COURT, SOHO.

1817.

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A SEQUEL TO AN ESSAY ON THE YELLOW FEVER;

UNIVERSITY OF LONDON

CALLED BULAM, OR PESTILENTIAL.

BY
EDWARD NATHANIEL BANCROFT, M.D.

LONDON:
PRINTED FOR J. CALLOW, CROWN COURT, SOHO.
1817.

INTRODUCTION.

IN the year 1811, I published an Essay on the Yellow Fever, and by facts not to be invalidated, while truth continues to be invariable, proved it to be no other than an aggravated form of that multifarious disease, which is well known to result from the action of those exhalations commonly denominated marsh miasmata, though often extricated from soils and situations which are not marshy; and I also proved that this, like all fevers from that cause, possesses no contagious quality: a fact which Dr. Chisholm had thought proper to admit in regard to the Yellow Fever as it occurred *before* the year 1793, when, as he asserted, a "nova pestis, a peculiar, original, foreign pestilence, recently imported, and utterly unknown before," was brought from Bulam on the coast of Africa by the ship

Hankey, to the island of Grenada. Against this unfounded assertion I thought it my duty to contend in the Seventh Appendix to my Essay, which, so far as I can discover, has rarely, if ever, failed to produce conviction in all, who have read it with unbiassed minds.

Previously to the publication of my Essay, Dr. (then Mr.) Pym had appeared to believe Dr. Chisholm's assertion on this subject, and to think that the recent epidemics in Spain, &c. were not only derived from the contagion supposed to have been imported by the Hankey, but that they were in fact a new and original disease, never before known in the world. But as in my Seventh Appendix I minutely examined the *peculiar* symptoms which Dr. Chisholm had selected, as forming the distinguishing character of his nova pestis, and clearly proved them to be similar to those which had often, and long before, been described as occurring in aggravated cases of the Yellow Fever of the West Indies, Dr. Pym now appears to have so far withdrawn his faith from Dr. Chisholm, as to

admit that "a fever under the name of Yellow Fever, mal de siam, fievre de matelotte," &c. has at different periods prevailed, not only in the West Indies and North America, but in the South of Europe. He contends, however, that, "*before* the year 1793 it had not made its appearance for a considerable time, either in the West Indies or America, and had been unknown in Spain nearly thirty years," or until 1800, when a fever occurred at Cadiz, &c. similar to that which Dr. Chisholm had described "as having been imported into Grenada from the island of Bulam on the coast of Africa:" and he represents the fever lately *revived* at Cadiz, &c. as agreeing so perfectly both in history and feature with that described by Dr. Chisholm, that he has chosen, as he declares, to retain, and call it by, "the name of Bulam Fever, as well with the view of pointing out a *distinct* disease, as of drawing a line of discrimination between it, and the disease called Yellow Fever." By thus supposing that the fever by him called Bulam had become *extinct* both in Europe and America some time previously to 1793, Dr. Pym ac-

commodates his opinion to that which Dr. Chisholm had promulgated concerning the alledged importation into Grenada of a species of *pestilence*, " which, if not then first made known to the world as *an original disease*," was, as he pretends, " the renovation of one that had become extinct," and which was in consequence of this renovation propagated first over the West Indies, &c. and afterwards to Spain : and by this fallacious supposition, together with the use which he has made of the highly improper epithet of Bulam, he gives, as far as he is able, the appearance of truth to one of the most *extravagant*, and, I might say, *pernicious fables*, which have at any time within my recollection, disturbed and deceived mankind.

To effect his purpose of proving that the fever, supposed to have been thus *renovated*, has a *real* existence, Dr. Pym has deemed it necessary to employ the greater part of his work in controverting, or *appearing to controvert*, such of the facts and arguments contained in my Essay, as he did not think it expedient to evade. In his

first page he makes this declaration ; " the principal object which I have in view is to counteract the influence which the opinions of Dr. Bancroft have had on the minds of the *public*, as well as of the profession," &c. Sir James Fellowes also, at page xviii of his Introduction, says, " I feel myself in some measure particularly called upon to advert to one of the latest works on the subject, entitled, " An Essay on the Disease called Yellow Fever," in which the Author has displayed much industry and learning in endeavouring to prove that the pestilential disorder of Spain possessed no contagious quality whatever," &c.

The alledged contagious quality of the Yellow or Bulam Fever, and the exemption said to be obtained by undergoing *one* attack from *all future* attacks of it, having been brought under the consideration of the Lords of His Majesty's most honourable Privy Council by Dr. Pym's work, their Lordships were pleased to call the attention of the President and Fellows of the Royal College of Physicians to this publication,

and to require the opinion of the College upon the two points just mentioned. Unfortunately very few, if any, of the Members of that Body who were then present had any personal knowledge of the disease; and they could therefore only judge of it from the accounts of others which had come to their knowledge; and these accounts led them to suppose that the disease possessed a contagious property. They were induced also, as is stated in their Report, "from the authority of Mr. Pym, Sir James Fellowes, the French and the Spanish Physicians, whose works they had consulted, to believe that a person is not liable to a second attack of the disease. But," they add, "upon a subject of such magnitude, the College cannot venture to give a positive opinion without further experience." These opinions were contained in the first and third paragraphs of the Report of the College, which Dr. Pym has selected, and published (in the London Medical Repository for September, 1816) *omitting* the *second* and *fourth* paragraphs; as he has done *the whole of a second Report* from the College, in these words, viz.

"The President and Fellows of the Royal College of Physicians, have the honour to acknowledge the receipt of a second communication, from the Lords of His Majesty's most honourable Privy Council, on the subject of Mr. Pym's work upon the Bulam Fever.

"The College apprehend that the object of their Lordships' present enquiry will be best satisfied, by stating the reasons upon which they founded the opinion given by them, in the third paragraph of their former Report, viz.

"'That there seems strong reason to believe, from the authority of Mr. Pym, Sir James Fellowes, the French and the Spanish physicians, whose works we have consulted, that a person is not liable to a second attack of this disease. But upon a subject of such magnitude, the College cannot venture to give a positive opinion, without further experience.'

"It is unnecessary for the College to explain to their Lordships the uncertain nature of all medical evidence; but it is proper that their Lordships should be informed, that the history of physic presents numerous instances of recorded facts, which, after having obtained credit at certain periods of time, have, by subsequent investigation and enquiry, fallen into disrepute, or have been disproved.

"It is probable that their Lordships may have been led to a second enquiry by the statement in Mr. Pym's work, that the fact of the non-liability of an individual, who has passed through the Bulam Fever, to receive it a second time, rests upon as good evidence, as the same fact with relation to the small-pox contagion. The College observe upon this statement, that the evidence relative to the small-pox contagion, rests upon the experience of all medical practitioners throughout the known world, and has been confirmed by the testimony of medical writers, from the period of the 7th century, down to the present time. The same is true of the Scarlet Fever and the Measles. The evidence respecting the Cow-pox is founded upon the familiar experience of all medical practitioners throughout Europe, and of all European practitioners in the other quarters of the globe. But the proof of the statement respecting the Bulam Fever rests upon the report of Mr. Pym, Sir James Fellowes, and some of the Spanish physicians, and upon the experience of a short period of time. The College have not intended to throw discredit on these testimonies; on the contrary, in their former Report, they have stated that the evidence adduced renders the fact highly probable. They consider the precautionary arrangements made by Mr. Pym at Gibraltar, as very judicious, and adviseable to be adopted in future. They have also reason to believe, that Mr. Pym is the first author in this country, who has

entered deeply into the investigation of this subject. But in giving their opinion, the College cannot avoid bearing in mind, the fallibility of all medical observation, and the extraordinary nature of the fact, which bears no analogy to the circumstance of those fevers which the Bulam most resembles; and they therefore feel, that it will be impossible for them to give a positive assent to the assertion of the non-liability of the human frame to a second attack of the fever in question, until they shall be able to avail themselves of a more enlarged experience, and the concurring testimony and observation of a greater number of medical practitioners.

"Signed by order of the College,

"CLEMT. HUE, Registrar."

"*College of Physicians*,

"*Nov.* 15, 1815."

As this second report left the subject of the Yellow or Bulam Fever in a considerable degree of uncertainty, the Lords of His Majesty's Privy Council thought it expedient, in a matter of so much importance, to call for further information from the Army Medical Board, and from the Commissioners for the Service of

Transports, Sick and Wounded Seamen, &c. Concerning the communication from the former Board, I am not authorized to give any information; but, concerning that from the latter, I am permitted to say that, after their Lordships had expressed a belief "that the prevalence of the fever above-mentioned in the West Indies and at Gibraltar, must have given to the officers employed in the medical department of the navy many opportunities of observing its effects, and ascertaining its nature, and had requested the said Commissioners to take the steps which seemed to them best for obtaining such facts and opinions as might throw light upon the question," "the Board, upon the suggestion of Dr. Harness (the Medical Commissioner), lost no time in calling upon the different medical officers of the navy, who appeared to them to be likely, from the situations which they either then held, or had formerly served in, to give such information as might afford any elucidation upon this interesting question, and to furnish them with all the facts that might have come to their knowledge either in sup-

port, or in refutation, of Mr. Pym's proposition; adding to those facts any remarks that might have occurred to them. Much time necessarily elapsed before the answers of the medical officers on the different foreign stations could be received. As soon, however, as they were collected, Dr. Harness took to his assistance Dr. Gray, late Physician of Haslar Hospital, and Dr. Tait, late Physician of Yarmouth Hospital: and proceeded to a deliberate review and consideration of the statements received:" and this being finished, the Commissioners for the Service of Transports, &c. caused a concise analysis of those statements to be transmitted for the information of the Lords of the Privy Council, together with the *original Reports*.

In the meantime I had, upon finding that the publications of Dr. Pym and Sir James Fellowes were more immediately directed against mine, determined, notwithstanding my dislike of controversy and my other more than sufficient occupations, to take up my pen in vindication of that which I believe to be the cause of

truth ; and that I might be able to do this more efficaciously and with greater certainty, I solicited a communication of the statements transmitted as before-mentioned by the Transport-Board to His Majesty's most honourable Privy Council; which their Lordships, from an anxious regard to the public welfare, and to the interests of humanity, were pleased to grant. In addition to this favour, of which I entertain a very grateful sense, I am to acknowledge the kindness of Sir James M'Grigor, Director General of the Army Medical Department, who, from a similar regard to the public service and the promotion of science and truth, gave me access to various documents in his office respecting the disease in question, some of which are highly pertinent and important, particularly the communications from Dr. Fergusson, lately Inspector of Hospitals in the Windward Islands (some of which have been since printed), and the answers given by the Medical Officers at Gibraltar in the autumn of 1814 to the questions put to them severally by Deputy Inspector Fraser. With these documents, and the numerous facts which I have

derived from other very respectable sources, I flatter myself that the points in dispute concerning the alledged *distinct* nature, and *contagious* quality, of the Bulam or Yellow Fever, and the peculiarities assigned to it, will, in the judgment of all impartial men, be deemed to be unquestionably and finally decided, in opposition to the doctrines and pretensions of the advocates for contagion.

It will have been seen that Sir James Fellowes, although he does not profess to have connected his opinions with those of Dr. Chisholm, or the fever of Andalusia with that of the Island of Grenada, has nevertheless adopted the term "Pestilential," which had been employed by Dr. Chisholm to designate the latter; a term which to me appears highly objectionable, as having a direct tendency to induce a belief that the *Andalusian* Fever, if not derived from the island of Grenada, is in its nature similar to the true *Pestis* or *Levant Plague:* from which however it differs essentially in almost every respect.

Persuaded therefore as I am that the denominations both of Pestilential and of Bulam, as applied to the fever under consideration, have an immediate and powerful tendency to produce and confirm erroneous and dangerous opinions concerning its origin and nature, I feel that it would not become me to contribute to their general adoption by using them in any instance, in which I can otherwise without circumlocution make myself clearly understood: and therefore I shall generally substitute for these terms those of Ardent, or of Yellow Fever.

It is exceedingly to be lamented that men liberally educated are often found incapable of maintaining their own opinions, and of controverting those of others, even in matters of science, without some feelings of animosity, and some expressions likely to give offence. Solicitous only for *the truth*, and convinced that men with the best intentions frequently think differently respecting it, there is in my judgment nothing more allowable than an *honest* endeavour in each individual to defend his opi-

nions, if it be done or attempted only by facts and *fair* arguments. It was therefore with great and sincere concern that I discovered in both the publications before-mentioned *numerous* instances of *extreme unfairness*, especially towards myself. Some of these, from a due regard to my own character, I have been under the painful necessity of noticing; but in doing this I have endeavoured to lay aside every feeling of irritation, and to avoid every expression tending to produce it, having constantly borne in my mind that the subject under discussion was one of extensive national importance, with which personal feelings ought not to be mixed.

Being about to embark for Jamaica, where I shall probably find no leisure, and shall certainly feel no desire, for a renewed discussion of this subject, I here take my leave of it, in the full persuasion that at least enough will have been done by this Sequel to obtain from medical men so much of unbiassed attention to facts, and so much of candour in reasoning upon them, as must enable the truth to manifest itself and

soon become triumphant: and as this is my sole object, I shall be gratified by the attainment of it, whether the issue be favourable or otherwise to my opinions.

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ERRATA.

Page Line
21—14, *for* laictericia *read* la ictericia.
27— 9, *for* elongations *read* aggregations.
33—12 of note, *for* mihgt *read* might.
111—last but one, *for* 1707 *read* 1797.
115—19, *for* revivication *read* revivification.
117— 7, add inverted commas after the word destroyed.
232—last but one, *for* Dr. Crichton *read* Mr. Neale.

A

SEQUEL

TO THE

Essay on Yellow Fever, &c.

CHAP. I.

THE existence of a *distinct* fever, derived from, and propagated by, a peculiar or specific contagion, having been asserted by Dr. Pym,* and Sir James Fellowes,† as the *foundation* of their respective publications, it is expedient that I should first particularly notice the characteristic symptoms or appearances by which it is assumed, that this fever may be known and distinguished,

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* Observations upon the Bulam Fever, &c. by William Pym, Esq. Deputy Inspector of Hospitals, &c. 1815.

† Reports of the Pestilential Disorder of Andalusia, by Sir James Fellowes, M. D. Inspector of Hospitals.

and that I should also examine the proofs which have been alleged to support its real *entity*.

These symptoms or appearances, as stated by Dr. Pym, together with those which he ascribes to the "Bilious Continued," and the "Bilious Remittent" Fevers (the diseases which, in his opinion, are the most likely to be mistaken for the supposed Contagious Fever), may be found at pages 4 and 5 of his Observations, &c. By this statement, six particular diagnostics are produced, to distinguish the fever, by him called *Bulam*, from the two other fevers just mentioned, which diagnostics regard the following points, viz.

1. The appearances of the eyes.
2. The nature and seat of the "head-ache."
3. The want of remissions.
4. The discolouration of the skin.
5. The duration of the course of the disorder, and
6. The morbid affection of the stomach.

1st. "A peculiar shining or drunken appearance in the eyes." p. 5.

The shining appearance here stated, is chiefly observable at the commencement of the disease, and mostly, as I believe, in those eyes which have become more than

usually irritable by the light; in which cases the secretion of the tears is increased, and a degree of brilliancy thence added to the eyes. This, however, is by no means a *constant* symptom. "The eyes were *sometimes* remarkably brilliant," as Sir James Fellowes stated, at p. 200 of his Reports, "but the vessels of the conjunctiva were full-tinged; at *other* times there was a *muddy cast*, which gave the appearance of a *dull* watery eye." Dr. Pym, likewise, (at p. 59,) describes the eyes in this fever as having "a peculiar *glassy* appearance;" which, according to common usage, implies the reverse of *shining*, the term *glassy* being particularly employed to signify the dull semi-transparent look of the eye *after death*, resembling that of glass, when compared with the sparkling lustre of the diamond.

The "drunken appearance of the eye,"* which Dr. Pym insists much upon, and in 1804 considered as being one of the two "*most* characteristic symptoms of the disease," (see p. 209† of Dr. Burnett's work,) is also

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* "A Practical Account of the Mediterranean Fever, by William Burnett, M. D. Physician of the Fleet." Second Edition. 1816.

† This expression is faulty, because it is not so much by the *eye*, as by the countenance, the speech, and the manner of the in-

objectionable as a diagnostic. That it occurs sometimes, I admit, having myself observed it, and known soldiers labouring under fever, and having this appearance, to have been sent to a guard-house, being supposed drunk; but I have observed it also in Typhus, (I mean the contagious fever of *cold* climates, see p. 506 of my Essay on the Yellow Fever,) and in the Plague; in the latter of which I have good reason to know that it frequently occurs. It is not, therefore, a symptom *peculiar* to the Bulam; nor does it appear to have been even a common one—for, otherwise, it must have been generally noticed by the authors who have treated of that disorder: but I do not find that such a symptom has been mentioned by Dr. Arejula, or any other Spanish physician, or by Sir James Fellowes, Mr. Doughty, or any of the medical officers who made reports on the Epidemic of Gibraltar, although by most of them, the appearances of the eyes seem to have been attentively noted.

Generally, those who have been best acquainted with this fever have mentioned a redness, wateriness, and seeming prominence of the eyes, with turgescence of their vessels, as occurring most frequently in persons la-

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dividual, that intoxication is perceptible. My remarks, therefore, apply to the "drunken appearance" generally.

bouring under it. In my opinion, the appearances of the eyes depend so much on their particular condition in each subject, and on the degree in which the brain is affected, and the blood determined to the head, that they must always be too variable to form any part of the character of this disease.

2d. "The head-ache is excruciating and confined to the orbits and forehead." p. 5. It is to be observed that the seat of the head-ache, as here described, is meant by Dr. Pym to be contrasted with that of the head-ache with which he characterizes his * "bilious continued

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* The term "bilious," as applied to fevers and some other disorders, is so much sanctioned by common use that any objection to it is likely to appear fastidious. The employment of it is founded on the idea of a redundancy of bile in such cases; the secretion of this fluid being supposed, even by medical and physiological writers of great respectability, to be increased in a warm temperature and climate. Although I have felt the weight of those authorities, such increase in the secretion of bile has nevertheless for a long time seemed to me very questionable (as stated at p. 21. of my Essay), and the subsequent observation of six years in Jamaica has not diminished my doubts.

Moreover the term "bilious" is not unimportant; for in almost all the cases of this disease to which it is applied, some treatment or other is adopted to remedy the supposed excess or fault of the bile, and I think that I have sometimes known more or less injury as the consequence.

After this explanation it will be perceived that I mention "bilious fevers" only in compliance with common usage, and not to imply

fever," and which in that " is confined chiefly to the temples." p. 4. Upon this part of Dr. Pym's diagnosis I must remark, from my own experience, that although the head-ache is commonly referred to the forehead and the orbits, it is seldom confined to them, but very frequently extends to the temples, and is sometimes felt even more severely in them than in the forehead: and this accords with the accounts of the Yellow fever given by Dr. Arejula, p. 159 and 170; by Sir James Fellowes, p. 52; by Prof. Palloni and Mr. Short, both quoted by Sir James, the former at p. 204, and the latter at p. 258; by Dr. Tadeo La Fuente, as quoted by Dr. Burnett, p. 490; and by Professor Berthe, p. 78 and 81, and by others. Even in a letter from Mr. Redmond, Surgeon to 54th Regiment, addressed to, and adduced by Dr. Pym, p. 74, the former, in describing the Bulam fever, with which he had become acquainted at Gibraltar in 1804, mentions a " deep seated pain in the orbits and temples," and says nothing of the forehead.—Indeed very few writers on this fever have described the head-ache as *confined* to the forehead and orbits. —Neither is the head-ache always " excruciating." Not unfrequently the pain is not an acute one, but a dull uneasy sensation as from a weight; and Dr. Pym has himself informed us, at p. 233, that, in his fourth form, which

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the existence of any morbid condition of the bile in those fevers, unless it be so expressed.

will generally be considered as the most dangerous of all his forms, " the head-ache is not so severe."

There is besides another circumstance which renders this symptom useless as a diagnostic, I mean its frequent occurrence in remittent fevers; although head-ache is not mentioned among the symptoms which Dr. Pym assigns to that fever.

3. " The Bulam fever has no remissions." p. 5.

Certainly of all Dr. Pym's characteristics, this is the most important, and has, more than any other, the semblance of truth; because, when those miasmata or exhalations from which intermittent and remittent fevers derive their origin, have been matured and concentrated by the action of an * high atmospheric temperature on a fit soil,

* The influence of *high atmospheric temperature* in augmenting the *virulence* of miasmata derived from the earth or from decomposing vegetable and other matters, and in converting intermittents and remittents into continued fevers, has been observed and attested by physicians of great eminence. Instances and proofs of these effects, even in temperate climates, may be found at p. 201, and between pages 293 and 301 of my Essay, where it will be seen that, in summers which were unusually hot, fevers from this cause were aggravated even at Copenhagen, as well as in Holland and Flanders, and in England itself, so as almost to equal in violence the ardent or Yellow fever of the West Indies. It may also be seen at p. 207, that Sir John Pringle has attested this conversion

the fever which they produce will assume the continued form, particularly in the young and robust; especially

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of remitting into *continued* fevers during his Campaigns in the Low Countries; and again at p. 353, 354, that Dr. Moultrie of Charleston attests the production in that city, of a *vehement* and malignant fever in the summer of 1748, when the heat rose to 97° and 98° Fahrenheit, which, upon a sudden and great diminution of this heat was converted to an intermittent. He also mentions that in a former year (1732) when the summer had been equally hot, a most violent and mortal fever was produced, of which many died in two or three days. To these facts and authorities I will only add the testimonies of two medical officers in the West Indies, whose opportunities of witnessing the effects of temperature in regard to fever have been extensive. The first is M. Victor Bally, one of the physicians in the French army under General Le Clerc in St. Domingo, who, at p. 16 of the Journal des Officiers de Santé de St. Domingue (printed in that Island), states that "the heat of the preceding summer (1802) had rendered the deleterious principle, or "cause of the Yellow fever, so active that it often destroyed life in a "few hours" ("qu'elle faisait périr souvent en peu d'heures."); but that "vers le milieu de pluviose, la maladie régnante a fait "beaucoup moins de ravage, et elle a parû, en s'eteignant insen- "siblement, vouloir céder la place aux fièvres intermittentes et ré- "mittentes tant benignes qu'insidieuses." The other is contained in a letter from Dr. Muttlebury, Inspector of Hospitals, dated "Jamaica, 2d February, 1811," and addressed to the Army Medical Board: having in this mentioned the uncommon prevalence of the Yellow Fever in several parts of the Island during the preceding autumn, he says, "The Royal Irish Regiment have suffered severely from "the fever, which at Up Park assumed the *continued*, *remittent* or "*intermittent* form, according to the state of the weather and of the "subject attacked. In the *dry hot* months of July and August, it was "of the *continued* kind; and when the rains set in, it partook more "of the remittent; but the symptoms were often so blended as to "render it difficult to determine to which class the fever belonged.

when the causes which contribute to its production are so extended and exasperated as to render it epidemical. And it is by selecting cases of Miasmatic fever thus changed, and declaring them to be cases of a distinct Contagious fever, that he has been enabled to give an appearance of reality to the disease, on which he has bestowed the arbitrary and fallacious name of Bulam.

That the exasperated cases of miasmatic fever, which have no distinct remissions (and which I continue to think would more properly be designated by the name of causus, or ardent fever, than by the popular one of Yellow fever, see page 5 of my Essay), originate from the same causes, (excepting the difference of excitability in the patient) and are of the same nature with the remittent fever, may be proved by innumerable facts, supported by the most respectable testimonies, and especially by those which establish the *simultaneous*

"In the detachment at Kingston Barracks, a station always unhealthy, the disease was a more distinct remittent, and though it did not so often terminate fatally in the first instance, unless aggravated by circumstances of exposure, fatigue, intoxication, &c. when the black vomit (which did not appear at Up Park) ensued, yet it so undermined the constitution, that diarrhœas, or obstinate intermittents have often followed, and ultimately carried off the patients." The reader is desired here to notice a fact of which I also have very often seen instances, i. e. the occurrence of *black vomit* in aggravated cases of *remittent fever*.

occurrence of both forms of the fever, and their reciprocal *conversions* into each other, at particular places and seasons; together with the invariable appearance of remittents at the same places, both *before* the high atmospheric temperature has operated sufficiently to give them the continued form, and also *after* the effects of this high temperature have ceased to exist.*

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* For facts and authorities proving incontestably, that "bilious" *remittents* commonly precede, accompany, and follow the *ardent* or *Yellow fever*, and that they mutually change their forms and run into each other, see the note to p. 408 of my Essay; also the text of pages 410 and 412, and the note to p. 411.—To these I will add the following, from a much greater number now before me, viz. Dr. John Chichester, who was extensively engaged in the practice of physic, at Charleston, South Carolina, during twelve years, in most of which the ardent fever prevailed in that city, says, in a letter to me, dated Bath, November 3d, 1806; "In the first part of the sickly season at Charleston, say from the 20th June, to the 1st of August, the disease (Yellow Fever) is tolerably manageable; and when it has not been completely subdued within three days, it has for the most part assumed the form of a bilious *remittent*, or what is there called *country* fever. From the first of August to nearly the end of September, the disease is seen in its greatest violence; and it is during that time, principally, that we meet with those cases which so speedily terminate in death." He adds that "from the end of September, until the setting in of cold weather, the disease becomes milder, and is nearly as *manageable* as it was between the 20th of June and 1st of August." Dr. Pinckard after he had become acquainted with the Yellow fever in Guiana, at Martinique, and at St. Domingo, writes in a concluding letter on that subject (p. 468 et seq. of his 2d vol. 2d ed.) that, whether it be denominated a "Seasoning or Yellow fever, or marked by any other

Dr. Pym, and a few who think with him, confidently assert, that the Bulam fever has invariably been produced at Gibraltar by importations of a specific contagion from

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appellation, it is only the common bilious fever of hot climates; and it appears under an intermittent, a remittent, or a *continued* form, according to the soil and situation of the place, or the habit of body, and other circumstances of the person attacked."—"It has happened to myself, to receive newly arrived soldiers at the same time, with this seasoning malady, under all the varieties of its intermittent, remittent, and continued form; and notwithstanding each has been differently attacked, all of them have died in the course only of a few days, with every symptom of the most *malignant* Yellow fever."—He adds, p. 490 of the same volume, that these fevers "run indiscriminately into each other,—a quotidian, or remittent, sometimes becoming a malignant Yellow fever; and a Yellow fever sometimes degenerating into a remittent or intermittent." At page 61 of the same volume, proofs are given of the influence of situation upon the form of the disease. "At Maiconuy and Awbarry, two smaller posts between Mahaica and Berbische, although much sickness prevails, the disease (Yellow fever) assumes an *intermittent* or *remittent* form, and is less fatal; while here (at Mahaica) and at Fort William Frederic, it attacks with all the destructive violence of a *continued* fever." The same gentleman, writing from Port-au-Prince, has stated (vol. ii. 414), that Dr. Jackson "views the disease (Yellow fever) as only a modification, or very high degree of the common fever of the country;" remarking that "it is frequently attended with remissions;" and that if he be called to a patient, at an early stage of the complaint, he is able in most cases "to change its type; and can often convert it into a regular intermittent." This statement corresponds with that by Dr. Jackson himself (at p. of his Outline), where, mentioning the varieties of the Yellow fever, 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

Cadiz, Malaga, and Carthagena; and they consequently must, and do, admit that the epidemical fever of these places is identically the same with that of Gibraltar. It

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a 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, *black vomiting*, purgings of black matter, hæmorrhage from different parts of the body, petechiæ, lividness," &c. That a yellow fever would, by proper treatment at the beginning, be sometimes made to assume the form of an intermittent or remittent, had been previously observed by Dr. Rush, and Dr. Huck (see p. 198 of Sir John Pringle); and on the other hand, Dr. Ramsay, in his Account of the Yellow Fever at Charleston, says "*neglected* intermittents frequently terminated in Yellow Fever." See New York Medical Repository, vol. viii. p. 364.—"Drs. Rush, Caldwell, and eleven other physicians, had, in answer to a call from the Governor of Pennsylvania, stated that "the symptoms of the Bilious and Yellow Fevers are the same in degree,"—and "that the common Bilious and Yellow Fevers often run into each other." It is stated also in the New York Medical Repository, vol. iv. p. 207, 208, that during the summer and autumn of the year 1800, about a hundred cases of *Yellow Fever* occurred in that city, simultaneously and intermixed in the same situations, with *Intermitting* and Bilious *Remitting* Fevers. It is added, that, of these cases of *Yellow Fever*, more than fifty terminated fatally in a few days; many of them "exhibiting the symptoms of yellow skin, *black vomiting*, and stools, hæmorrhages, &c." Dr. Vance, now surgeon to Haslar Hospital, in his Report to the Transport Board, dated 1st January, 1816, states it to be his opinion, "that the *Ardent*, *Yellow* or Bulam Fever, with black vomiting, is only an aggravated form of the Bilious Remittent, and other varieties of Miasmatic Fever;" adding, that, "on a former occasion, on board His Majesty's ship Theseus, on the Jamaica station, about twenty men were taken ill, in whom all these varieties of the disease existed: three died on the fourth, and one

will therefore be proper to examine how far the accounts given of the disorder in those parts of Spain will support Dr. Pym's assertion that it "has no remissions." Of

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on the third day; the rest recovered;" and black vomiting is described among the symptoms in some of the fatal cases. Mr. Brien, surgeon in the Royal Navy, in his Report to the Transport Board, dated 31st December, 1815, says, "The symptoms ascribed by Mr. Pym to the Bulam Fever, excepting the affection of the throat, and symptoms of obscure remission, I have observed prevailing in different patients, in *the ship*, at *the same time;* the more violent symptoms occurring in subjects of a tense muscular fibre, who had been but a short time in the climate, while *remissions* were more observable, according to the length of time the subject has been on the station; but often, more than ordinary attention was required for their detection. From these circumstances, I considered the fever to be of the same nature; the fever being only varied according to the constitution of the patient." I could add many similar testimonies; but that I may not give this note an unnecessary extension, I will only observe, that, not only during my former service in the West Indies, as physician to the army under Sir Ralph Abercromby, but also during the last six years, which I have passed in Jamaica, I have repeatedly seen, as well in my private practice, as among the military, that the three beforementioned varieties of fever have appeared at the same time, and that cases, which had commenced as Remittents, or Intermittents (but more frequently as the former), have occasionally terminated with *black* vomiting, and the other symptoms usually ascribed to the Yellow Fever. Such terminations I have witnessed even in those who had resided some years in the climate: but in young and robust individuals, who had recently arrived from northern latitudes, especially after some excessive indulgence, or other powerful excitement, the disease has generally appeared as a continued fever of short duration, the paroxysm seldom extending beyond seventy-two hours.

such accounts, the one by Dr. Arejula appears to rank high in authority, as well from the important office to which he was appointed during the epidemics in Spain, as because it appears (according to Sir James Fellowes, see p. 51 of his Reports) that "he has seen more of the disease than any of the physicians in that country." On referring therefore to page 171 of his work, we find that the supposed contagious Bulam, or Andalusian pestilential fever, is described as appearing rather as a *remitting* than as a *continued* fever. Sir James Fellowes, who has, in various parts of his Reports, bestowed great commendations on Dr. Arejula, and who, from having selected and transcribed his description of the symptoms of the epidemic, must be presumed to have considered it as the most scientific and faithful within his knowledge, has given the passage to which I now allude at p. 53 of his work, where, in relating the "regular symptoms" of the epidemic (as seen from 1800 to 1804, and not in Cadiz alone, but in other towns also, as Medina Sidonia, Malaga, Antiquera, la Rambla, Espejo, Montilla, Cordova and Granada, see p. 176. chap. 3. of Dr. Arejula), he says "a remission of the above symptoms of the fever, with or without sweat, took place in twenty or twenty-four hours, and an exacerbation on the following day, with a remission or apparent apyrexia on the third, sometimes on the fourth, fifth, and seventh days, although this was rare." Dr. Arejula has also

mentioned at page 200 of his work, where he treats of the cure, that "commonly the remissions were regular, and observed from twenty-four hours to twenty-four hours." Sir James Fellowes again at p. 201, under the head of Observations on the Epidemic of 1804 (which appear to have been chiefly taken from Dr. Gonzales), represents the disorder as having remissions. "This fever in its mildest form *remitted;* sometimes at the end of twenty-four or of forty-eight hours, with relief of the symptoms; and this remission generally appeared after an universal sweat, accompanied with loose bilious stools: the second accession then came on, and ran its course in the same space of time, with a diminution of the symptoms, accompanied by similar critical discharges; and towards the third or fourth day the fever entirely disappeared, when the patient became convalescent." He adds, "when the disorder did not show a tendency to terminate in this way, the above symptoms were more constant and violent the first three days; the remission was more imperfect and obscure, and the restlessness was increased," &c. There is likewise at p. 312-13, a nearly similar account of the Epidemic at Cadiz, from 1800 to 1813, as published by Dr. Flores, to which, as also to the work of Prof. Berthe, p. 89, for the sake of brevity I beg leave to refer the reader; observing only, that at page 307 Sir James Fellowes considers the three Spanish authorities

just quoted, " Drs. Arejula, Gonzales and Flores," to be " the three most eminent physicians in Cadiz, and he believes in Spain."

Dr. Arejula also describes the cessation of the Epidemic in the following words, at p. 227. " The termination of our remittent calenture, in an intermittent, as happened in several persons towards the decline of the malady, was a presage of the speedy extinction of the Epidemic; and this I have noticed every year."

After such unquestionable testimonies, however, it is unnecessary to seek further proofs of the occurrence of remissions in the Epidemics of the cities of Spain, more particularly because they have been frequently observed in Gibraltar itself, as was attested by a considerable number of the medical gentlemen of that garrison, to whom questions were proposed by Deputy Inspector Fraser, respecting the Epidemic of 1814. Of these Mr. Lea, surgeon acting with the 26th regiment, in his official answers to these questions, does not merely assert the recurrence of remissions in the disease, but declares it " to be the true bilious remittent;" and as he states the number of sick who had been under his care, to have been three hundred and fourteen, of whom twenty died, it will not be contended, I presume, that he was unacquainted with the disease, especially as

he declares that he had seen the bilious remittent at other places, particularly in Holland, and at Gibraltar in 1804.

Mr. Weld, surgeon to the 67th regiment, in his answers to the same questions, says in regard to "the diagnostic symptoms at the commencement of the disease," that they were "the general symptoms of bilious remittent fever, occasionally differing in *degree* only." He adds, "I consider this to be the bilious remittent fever —I saw it in Flushing, England, Cadiz, Carthagena, and Gibraltar." Of 127 patients under his care during the Epidemic of that season, he states 27 to have died. Mr. Amiel, acting surgeon to the foreign depôt at Gibraltar in his answer says, "I believe that our Epidemic differs only in degree from the bilious remittent fever; and that the two disorders have one and the same origin; they prevail in the same climates; they attack the same organs of the body; they are chiefly fatal to persons of the same constitution; and the form of a mild remittent is often seen close to the *malignant type* of the Epidemic at the *same time*, and under equal circumstances of exposure; and the types, more or less *continued*, or less *remittent*, are determined by the difference of seasons, constitutions, and by the greater or less virulence of the exciting causes."—In giving an account of the symptoms, he says, in many instances, a *remission* be-

comes sensible on the second day; and the disease is protracted under the *type* of a remittent fever to the seventh, sometimes to the ninth, but seldom later than the eleventh." Mr. Sproule, surgeon of the Royal Artillery, in his answers, says, "I have seen instances wherein it (the Epidemic,) put on a remittent form." He adds, "I saw the Bilious Remittent in this garrison in the years 1806, 1807, 1808, 1809, 1810, 1811 and 1812." "I saw a few cases of the Epidemic in the garrison in 1810." Mr. Humphries, assistant surgeon to the Royal Artillery, in answering Mr. Fraser, says, "I look upon the disease to be the Bilious Remittent, under an aggravated type, from local causes." "I have witnessed the Bilious Remittent at this station." "I have witnessed the disease (the Epidemic,) before this season." "The symptoms, at the commencement of the prevailing fever were milder than in the middle of it, and towards the decline of the season, it again became milder." Mr. Martindale, assistant surgeon to the 67th regiment, in his answers, says, "Early in the season, I observed no *distinct* remission in this fever; but latterly, it has, in *many* instances, put on a *decidedly remittent type*." "I saw the Bilious Remittent in Cadiz and Seville in 1812." Mr. Donnett, surgeon to the navy, and in charge of the Naval Hospital at Gibraltar, in his answers to Mr. Fraser, after describing the violent forms of the Epidemic, adds, that in milder cases, he has *often* observed slight remissions on the

third day; and, the disease has been prolonged to the seventh, and sometimes to the eleventh. The same gentleman, in a letter to the Transport Board, dated 28th December, 1806, described the Yellow Fever which "raged with violence on board His Majesty's ship Melpomene, while off Vera Cruz, from August until the end of October" in that year, "when a few cases of Intermittents appeared amongst those who were in a state of convalescence;" and, in his Report to the Commissioners for Transports, on Dr. Pym's Treatise upon the Bulam Fever, dated 28th July, 1816, referring to that letter, of which he incloses a copy, he declares, that, "having compared the symptoms, progress, and termination of the fever he had observed in Gibraltar," (in 1810 and 1814), with those of the fever which raged on board the Melpomene in August, 1808, in the West Indies, when it assumed the characteristic symptoms of the disease nominated Bulam, he is of opinion that the various forms of the Bilious Fever, from the Remittents of the spring and summer months, to the Yellow or Bulam Fever, which prevailed in the West Indies, at Gibraltar, and in other parts of Spain, are *one and the same disease*, differing only in degree of aggravation."

Believing that these facts and authorities must suffice to refute Dr. Pym's assertion, and to establish the frequent occurrence of *remissions* in the Epidemic Fever of

Gibraltar, Cadiz, and other towns in Spain, and recollecting that, besides these, I shall hereafter have occasion to introduce similar proofs *for other purposes,* I will close my remarks upon this part of his diagnosis with observing that even if it were established (contrary to the fact) that the fever in question "has no remissions," still Dr. Pym, could not, with any propriety, allege the want of remissions as a distinguishing sign of his Bulam Fever, because he has himself admitted, p. 4, that "the Bilious Remittent, in its more violent form, sometimes *proves fatal* on the second or third day, *without evident remission.*" How, then, in this case, is the latter form of fever to be known, as different from the other?

4. "When the Bulam Fever terminates favourably, it is *very rarely* attended with *yellowness of skin;* which, if it does take place, is of a *very pale lemon* colour." p. 5.

In whatever sense the above passage be understood, whether of mild cases only, or generally of cases, which, however severe they may have been, terminate in recovery, it seems to be at direct variance with the diagnostic which Dr. Pym inculcated in his instructions to Mr. Vance, surgeon to the forces (at p. 60), in the following words: "Yellowness of the skin *frqueently* appears in *bad* cases towards the close of the disease; it begins in the eyes, and spreads over the body, but is generally of a lighter colour than in the Bilious Remitting Fever."

But setting the inconsistencies of these two passages aside, and construing them according to their apparent signification, *i. e.* that the yellowness which occurs in the Epidemic Fever, differs from that occurring in other fevers, by being of a light colour, it will be found that, although this may be true in some cases, yet in a great many, the discolouration of the skin is very different. Thus Dr. Arejula, at p. 150, says, that, in the Epidemic of Spain "it is very common for the patient to have the jaundice, or be tinged yellow;" by which he certainly does not mean any "pale lemon colour," but the reverse, as is shown by his remarking, in the same passage, upon "the liveliness of the yellow colour or jaundice" in that fever ("la vivacidad del color amarillo, o laictèricia"), and also by his describing the yellow as being "very remarkable and obvious to the senses" ("tan notable y obvio a los sentidos.") In other places, likewise, as at p. 160 and p. 161, he designates the yellow suffusion by the term "jaundice" ("ictericia"). Sir James Fellowes speaks, in different parts of his Reports on the Fever of Andalusia, of the skin being "very yellow," and, "completely yellow," and, at p. 266, mentions a "*dark yellow* tinge," in "about twenty patients in the worst stage of the disease." Sir Joseph Gilpin, also, in the Edinburgh Medical and Surgical Journal, for July, 1814, p. 312, mentions his having attended different families at Gibraltar, during the Epidemic of 1813, among whom he noticed the skin as

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being "of a *dingy* yellow hue, unlike the *bright* yellow of the Bilious Remitteut." Mr. Donnett also, surgeon to the Naval Hospital in that garrison, represents the yellow suffusion as "generally dark;" and, without descending to particulars, it will be sufficient to state, that, many of the other medical officers at Gibraltar, in their official answers to Deputy Inspector Fraser, have differed considerably from Dr. Pym in regard to this symptom. I could adduce many more authorities equally adverse to this part of his diagnosis, especially among the writers who have described the Yellow Fever in the West Indies, or the United States of America; but I conceive they would be superfluous.

I endeavoured (at pages 38—48 of my Essay,) to explain the cause of this symptom; and, the ample experience I have since had in the fevers of Jamaica, has increased my confidence in the justness of the explanation there offered. I have never seen a yellow suffusion but when there had been some vomiting previously. The tinge of it, however, is not of necessity proportioned, as might at first be supposed, to the duration or the violence of the vomiting; for, as the suffusion results from the compression of the liver, by the violent contractions of the abdominal muscles and diaphragm in the efforts to vomit, during which compression a portion of the bile is forced out of that viscus into the vena cava, as water is forced out

of a wet sponge when squeezed) the shade of the suffusion will depend on the quantity of bile that has been driven into the circulation, and this on the degree of compression which the liver has undergone, which may vary in patients almost ad infinitum. In some, the liver is of a large size, or is so prominently, or otherwise situated, as to admit of its being readily and forcibly compressed; and, in such cases, a moderate degree of vomiting might suffice to produce a considerable suffusion; but in others, that viscus may be comparatively small, or so lodged under the ribs, as to be in a great measure, or wholly, secured against compression; and, in cases of this sort, we can easily conceive (what is not unfrequently observed), that the skin may remain clear of any tinge from bile, although the patient should suffer incessant and violent vomiting. The degree of the suffusion seems, therefore, to be governed by casual circumstances, peculiar to individuals, and is consequently liable to infinite variety.

5. The Bulam Fever "runs its course in from one to five days." p. 5. That it does so frequently, in its most aggravated form, is true; but, sufficient facts have already been stated under the third head, to prove, that, much oftener it continues for a longer time. Dr. Pym has himself described the usual course of the symptoms in the severe cases of the Epidemic of 1804 (which then raged with much greater violence and fatality than in subsequent

years), "which," says he, "put an end to the disease about the fifth or seventh day" (see the account at p. 208 of Dr. Burnett). Dr. Arejula also informs us (p. 161), that, "the patients died about the beginning of the seventh day. This is the ordinary time of their death. Some, however, died in thirty-six hours from the attack, or in two, three, four, or six days, and some in *nine*, *eleven* or *thirteen*; beyond this death seldom happens, though *I have seen such instances.*" Sir James Fellowes, likewise, in reporting the dates of twenty-three deaths, by the Epidemic of 1813, at Cadiz (p. 285), states seven only of them as having occurred on or before the fifth day; and the remaining sixteen, from the sixth to the ninth day. But Dr. Burnett has produced so much additional evidence to the same effect, between pages 414 and 423, that farther proof cannot be requisite to convince the reader that this diagnostic of Dr. Pym * is contrary to fact, and ought, consequently, to be rejected.

Were the duration, however, even as Dr. Pym represents, it could still not serve as a diagnostic from the Re-

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* Dr. Pym, when limiting the duration of the Bulam, "from one to five days," was probably not aware that, of the two diseases with which he identifies that fever, Father Labat has extended the course of the Mal de Siam to thirty-two days (see p. 321 of my Essay), and Dr. Chisholm that of his Nova Pestis to twenty-one days.

mittent Fever; for this, by his own admission, "sometimes proves fatal on the second or third day;" and, according to Dr. Hunter's, in twenty-four hours. See p. 154 of his "Observations on the Diseases of the Army in Jamaica."

6. The Bulam Fever "is attended with a peculiar inflammation of the stomach, which, in most cases that prove fatal, terminates in gangrene, or in a diseased state of the internal or villous coat of that organ, accompanied with a vomiting of matter resembling coffee-grounds." p. 5.

If the chief value of a diagnostic be to enable us to detect the true nature of a disease, whereby we may arrest, or mitigate, its progress, and save the patient, we cannot estimate very highly the utility of the present one, which relates to a state of the stomach that can only be known *after death*, or to a symptom (black vomiting) which is the almost * unerring harbinger of death. But, what

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* I feel that my experience warrants the expression here used, having never known a single recovery among many hundreds of cases of the black vomit which I have seen, and having found but two or three, out of a great number of experienced medical practitioners, whom I have questioned, that affirmed to have witnessed such recovery. Dr. Arejula has indeed stated (at p. 185), that a great many (muchos muchos) who had this symptom recovered perfectly;") and Dr. Pym, although he *now* terms it a "*fatal* symptom," and seems (at p. 268), to consider it as *incurable*, has also related that "the black vomiting has been *cured* in some instances, by only wetting

is particularly striking in this diagnostic is its defectiveness, or obscurity. "A peculiar inflammation of the stomach," is announced by Dr. Pym, as being the most important characteristic of a disorder formidably malignant; yet he leaves his readers wholly to guess *in what its peculiarity consists*. "Peculiar," indeed, must that inflammation of the stomach be, which terminates in *gangrene*, as we are told this does; and it was, therefore, the more incumbent on him to have afforded us some instruction concerning this novel phenomenon, as the medical world have hitherto believed that the only inflammation to which the *internal* membrane* of the stomach and intestines was subject, was the *erythematic* species, which does not terminate in gangrene. Are we to presume that, seeing this peculiar inflammation, he knew not how to describe it? or, ought

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the patient's mouth with drink, and giving repeated injections of cold chicken broth, without salt; and, in one instance, by drinking capillaire and water only:" (see Dr. Burnett, p. 209). I believe, however, that, in most of these supposed recoveries, the patients had not the symptom properly called black vomiting; but that the dark coloured matters which they rejected from the stomach must have been only nourishment or medicines of certain kinds, such as sago or the like with wine, particularly red wine, and very often mixtures with decoction, or infusion, and tincture of Peruvian bark, &c. which always become darker in the stomach, and which I have repeatedly known to have been mistaken for the black vomit.

* The difference between a spontaneous or symptomatic inflammation of the villous coat of these viscera, and the morbid effects ending in sphacelation of the intestines, which are occasioned by a strangulated hernia, are too obvious to require illustration.

we not rather to conclude, especially from his adducing nothing of his own relative to the appearances after death, but only a few quotations from other authors (see pages 220 to 222) that, partaking of the aversion to dissections, which the advocates for contagion in the Yellow Fever have *elsewhere* manifested, he never made any? If he did, his examinations must have been very imperfect; else he would not have mistaken for gangrene what were only elongations of infinite minute vessels, distended with blood, that, perhaps had become darker, after death, or extravasations of blood under or upon the villous coat. Upon this subject, besides referring to pages 22 and 24, and Appendix, No. 1, of my Essay, I need only quote the opinion of an author who is pre-eminent in morbid anatomy, and, indeed, in every professional attainment. In the seventh chapter of Dr. Baillie's work on the Morbid Anatomy of the Human Body, the diseased appearances, after inflammation of the stomach, are thus described: "On opening into the stomach, it is found to be a little thicker at the inflamed part, the inner membrane is very red, from the number of small florid vessels, and there are frequently spots of extravasated blood. It does not often occur that a common inflammation of the stomach proceeds to form pus, or *to terminate in gangrene.*" p. 130.

"The vomiting of matter resembling coffee-grounds," or, "black vomiting," is represented by Dr. Pym, at p. 4,

as " peculiar to the Bulam Fever:" he farther informs us, at p. 4 and 5, that " the Bilious *continued* Fever is *not* attended with black vomiting," and that " the Bilious Remittent Fever," even " in its more violent form," and proving fatal, " is *very rarely, if ever*, attended with the black vomiting." In his instructions to Staff-surgeon Vance, p. 60, he also says, that, " the Bilious Remitting Fever is rarely, if ever, attended with black vomiting."

If the above were said bonâ fide; if at the time Dr. Pym was writing this extraordinary assertion, that, " black vomiting very rarely, *if ever*, occurs in Remittent Fevers," he really believed in its truth, he must have been deficient in medical knowledge to a degree that would deprive every statement of his, on medical subjects, of all weight. But, if it be otherwise, as numbers of the gravest authorities in medicine have recorded or described Remittent Fevers, in which black vomiting was an occasional symptom, as, in later times, Sir John Pringle, Dr. Cleghorn, Dr. John Hunter, Dr. Rush, and a multitude of others, quoted in my Essay, it may be asked, if he could possibly have expected that he should persuade the world, upon his naked contradiction, at once to disbelieve and reject such testimonies, and, with them, the evidence of daily experience in tropical climates?*

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* Dr. Pym constantly denies the " Mediterranean Fever" to be any other than " the Bilious Remitting or Bilious Continued

To evince the occurrence of black vomiting in Remitting Fevers, I lately adduced in addition to my own, the testimonies of Drs. Jackson and Muttlebury; and, I shall hereafter have occasion to prove, by unquestionable evidence, that many mortal cases of fever, accompanied with this symptom, have occurred immediately after, and manifestly as a consequence of, exposure to exhalations arising from the earth, in situations where nothing of the nature of contagion was, or could have been, suspected; because they were either uninhabited, or only inhabited by persons who were in perfect health, and to whom contagious fever had never been known.

In the interim I will observe that Dr. Pym is as little consistent in regard to the period of the disease at which the black vomiting appears, as in regard to most of his other diagnostics: for in one of his accounts of the Bulam Fever, he states it as commonly occurring "about the second day; a few hours after which death generally ensues;" and in another, as commonly occurring after "four or perhaps five days," and occasioning death

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Fever;" (p. 263 et al.) yet Dr. Burnett, in describing the severe form of the first-mentioned disorder in its last stage, says, "the uneasiness about the epigastrium is intolerable, and some patients have complained of a burning sensation, extending upwards to the throat. The vomiting is incessant, often of blood, succeeded in some cases by a matter *resembling coffee grounds.*" p. 9.

" about the fifth or seventh day." (See pages 207 and 217 of Dr. Burnett's work.) According to my own experience, the symptom in question very seldom appears before the fourth day; and those who die of the Ardent Fever, so early as the second or third day, are in general destroyed by the violence* of the affection of the head, and expire with convulsions, or coma, as is described in my Essay, pages 16 and 17.

I shall only add, concerning this black vomiting, that as it is a mortal symptom, never occurring, it may be said, *in those who recover*, and one which is often wanting among those who die, its appearance in this disease must be much rarer even than death; and this circumstance, joined to that of its *not* being " peculiar" to the fever in question, render it very unfit to be produced as a diagnostic thereof.

I abstain from offering any comment on the " livid or putrid appearance of the countenance," mentioned by Dr. Pym at p. 5, as marking the approach of death,

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* Dr. Pym appears to have almost entirely overlooked every case of this description, *i. e.* in which the affection of the *head* was violent; for the occurrence of delirium is noticed by him only in his *third* form of the Bulam Fever, and he is altogether silent as to coma.

(which it is not easy to reconcile with his other representation of the countenance, in the preceding page, as being "bloated,") because I cannot flatter myself with comprehending its meaning, even with his *merry* explanation of it.

7. There are two other symptoms which, although omitted by Dr. Pym, in his description at p. 5, are included among those "for which the Bulam Fever is remarkable," as expressed in his instructions to Staff-surgeon Vance, p. 59. The first of them is "the great quickness of the pulse, being, at the commencement, from 110 to 140:" but in other general descriptions, he has represented the pulse very differently; thus, in his account of the same disease, in 1804, he stated the "pulse from 100 to 130," and in that which he had transmitted to Admiral Sir Richard Keats, he described "the pulse from 100 to 110." See Dr. Burnett's work, pages 206 and 217.

8. The other symptom alluded to, is "the intense and peculiar heat of the skin, which seems to adhere to the hand, after having touched the body of a patient." Now, in the first of Dr. Pym's forms of the Bulam Fever, p. 228, "heat of the skin" is simply noticed as a symptom: in his second form, p. 229, "the skin is burning hot;" and in the third form, p. 233, which "is an aggravation

of all the symptoms of the second," "the heat of the skin is *greater*." How the skin, under disease, can be heated to a higher degree than "*burning hot*," is not quite intelligible. From the preceding descriptions, one might be led to conclude, that an "intense heat of the skin" is, of necessity, a "peculiar" and diagnostic sign of Dr. Pym's fever; yet we learn, at p. 233, that in the fourth form of it, which "is equally fatal with the *third*," "the heat of the skin is very little augmented."

Descriptions of symptoms being simply records of natural events in disease, which stand unalterable, however opinions about them may change, will the confusion, the inconsistencies, and the errors, every where apparent in Dr. Pym's attempts to frame a diagnosis for the Bulam Fever, be deemed very excusable in one who claims merit for discovering peculiarities therein, which had escaped the sagacity and penetration of all other observers?

But without insisting on the fatal force with which such inconsistencies* and errors, must strike at all his pre-

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* In pointing out the inconsistencies and errors of Dr. Pym, in regard to his diagnosis of the Bulam Fever, I have in every instance restricted my proofs, as the reader will doubtless have perceived, to descriptions given either by himself or by respectable authorities who have witnessed the Epidemic in Gibraltar, Cadiz,

tensions, I venture to believe that the facts which have been stated, will be deemed sufficient to manifest the uncertainty and futility of the characteristic symptoms which have been alleged by him to distinguish what is called Bulam Fever, and will therefore abstain from making any addition to them. Dr. Pym indeed acknowledges (p. 5,) that, "in *mild* cases of this last disease, it is impossible to point out any symptoms distinguishing it from attacks of fever from *any* cause; and even in bad cases," he adds, "until the fatal symptoms make their appearance, I may say, (excepting* its prevailing epidemically) that

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or other towns in Spain: my principal motive for which, besides brevity, has been that as the identity of the Epidemic in those parts of the Peninsula is acknowledged by all, even by Dr. Pym, I have conceived it impossible that *their* testimonies could be justly questioned. Numerous, and perhaps better, proofs might often have been adduced from West Indian and American writers; but as against these, Dr. Pym would probably have advanced the objection, however improper, to which he has usually had recourse hitherto, in similar difficulties, viz. that those proofs related to the "*Bilious Remittent*," and *not* to the *Bulam* Fever (of which his rejection of Dr. John Hunter's valuable description is a convincing example), I have relinquished the advantage of them, that I might bring the question about the latter disease to issue, and leave no openings for trifling with the patience of the public on this important subject.

* It is well known, and has been recorded by many and high authorities, (see pages 278 to 281 of my Essay), that fevers, resulting from terrestrial exhalations or malaria have very frequently

it is as difficult to decide positively upon its real nature, as it is in the fever of the small-pox before the appearance of the eruption." But this seeming concession falls very short of the truth; for it has been abundantly proved, that "even in *bad* cases," his fatal symptoms afford no criterion of any fever, either differing from, or worse than, the aggravated forms of a Remittent: and when he tells us (p. 209,) that even Dr. Rush*, with all his experience

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(probably more frequently than fevers from any other cause,) prevailed epidemically; and, therefore, the epidemical existence, which Dr. Pym alleges as a criterion to distinguish his Bulam, when it is not otherwise distinguishable, is not only nugatory, but fallacious, and his adducing it for this purpose, manifests a serious deficiency of information.

* That Dr. Pym should now stand forth to charge Dr. Rush with ignorance and error, in having mistaken another disease for the Yellow Fever, excited some surprise in me, who remembered distinctly that he was once foremost to select and hold up Dr. Rush's publication on the Yellow Fever at Philadelphia in 1793, as a most accurate and perfect description of that disease. It was at a Medical Board convened by the Governor of Gibraltar, in September, 1800, to report their opinion upon the nature of the Epidemic Fever then raging in Cadiz; which the Board pronounced to be the same contagious and malignant disorder that Dr. Rush had described so ably, &c. wherefore they sent a copy of his work concerning that fever, for the information of the physicians of Cadiz. Dr. Pym has alluded to this Board at p. 209, and introduced my name as one of its members; but he has not done me the justice to mention that I there openly stated my dissent from the opinion which the Board gave; for I did not consider the disease to be contagious, and accordingly withheld my signature from their report.

and sagacity, was unable to discriminate between the Bilious Remittent and the Yellow Fever of Philadelphia, and that he actually mistook the former for the latter, in 1794, and this so completely, that the mistake was never, in any succeeding Epidemic, discovered by himself, nor by any other person, (Dr. Pym excepted,) would not this, if true, afford the strongest ground for presuming, not only that there are no means of distinguishing between these forms of fever, but that in fact they are *identically* the same*? Even Dr. Pym asserts (p. 4,) that "the Bilious Remittent assumes such a diversity of forms and symptoms, owing to the constitution of the patient, the heat of the climate, and the degree of concentration of the Marsh Miasmata, that it is impossible, at its commencement, to draw a diagnosis between it and the two others," (viz. the Bulam, and the Bilious Continued Fevers;) why then should he suppose that this multiform

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* This appears to have been decidedly the *ultimate* opinion of Dr. James Lind, as delivered at p. 118 of his "Essay on the Diseases of Europeans in Hot Climates," 5th edition. After stating that, "In the West Indies the Yellow Fever is particularly de-destructive to Europeans," he proceeds thus, "having considered this disease with attention, I am *now* of opinion, that the remarkable dissolution of the blood, the violent hæmorrhages, the black vomit, and the other symptoms which characterize the Yellow Fever, are only accidental appearances in the common fever of the West Indies."

fever should not, when *aggravated*, equally present the more severe symptoms by which he has so unsuccessfully endeavoured to separate and designate his Bulam? Dr. Rush has told us, concerning the Epidemic which produced so much consternation and mortality at Philadelphia in 1793, and which both Drs. Chisholm and Pym consider as the true Bulam, (carried to that city from Grenada,) that it "*assumed all the shapes of Proteus;*" and as this polymorphean property is eminently characteristic of Miasmatic Fevers, we are warranted to conclude, that the Ardent or Yellow Fever is *one of these*, and that Dr. Pym's endeavours to prove the contrary, like his diagnosis of the supposed Bulam, are mere illusions.

The same may also be said of the ultimate diagnosis stated by Sir James Fellowes, relative to his pestilential fever of Andalusia; at p. 402 of his Reports, he says, "the violence and fatality of this fever, the indistinctness of the remissions, the rapidity of its course, and the peculiar debility, which often come on very early, accompanied by a constant propensity to vomit, constituted the striking difference from the Bilious Remittent *common to that country*, during the autumnal months." In thus mentioning the remissions of the fever as being *indistinct*, he contradicts his own statement, at page 53; and in regard to the *rapidity* of the course of this disease, and the *debility* accompanying it, neither of them are greater

than have been frequently observed in the *Remittents*, and sometimes even in the *Intermittents* of hot climates; in the former of which, patients are seen to expire on the *second*, and in the latter, on the *third* or the *fourth* day. It will hereafter be seen from the accounts published by Dr. Arejula, Sir James Fellowes, and even Dr. Pym, that at the commencement of each Epidemic at Cadiz, Gibraltar, Malaga, &c. the medical men at those places have been at least divided in their opinions of its nature; and that sometimes nearly the whole of them have declared it to be no other than the common Remittent of these places respectively; and that it has only been when the causes of the distemper, acting with epidemic force, have affected an unusual number of persons, and with unusual mortality, that a greater proportion of the profession have been led to consider the disease as of a different origin, and of a contagious quality. And here I would ask, if it be credible that causes of such opposite natures, as exhalations from the earth, and a specific contagion, should produce diseases resembling each other so exactly in every particular, as to afford no distinguishing mark or character? Is there any example of such a *similitude* of effects, from such *diversity* of causes? It is undeniable and notorious, that fevers produced by the first of these causes are, by great heat and the co-operation of local circumstances, frequently aggravated to all the violence and mortality of the Ardent or Yellow Fever, and

to an exhibition of similar symptoms; that they generally appear previously, cotemporaneously with, and subsequently to, the latter; that both kinds occasionally *interchange* their forms, and are *converted* into each other; that the Ardent or Yellow Fever never occurs as an Epidemic, but in places where the Bilious Remittent often prevails; and that when it does occur as an Epidemic, in persons not habituated to the climate, the natives and long residents are usually attacked by it* in the milder forms of Remittent Fever: and with a knowledge of these facts, is it not as unreasonable to pretend, that the Ardent Fever is a distinct and peculiar disease, as it would be to assign a *new* name and character to an individual, merely because he had changed one coat for another,

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* Of this fact numerous proofs may be found in my Essay: it has, however, been too long and too generally known to *require* any. And the difference in the fevers with which seasoned and unseasoned persons are attacked, at the same time, and in the same place, can only result from differences in their respective susceptibilities, or re-active powers; and not from any difference in the fever-producing cause. This affords additional reason for disbelieving the presence of contagion, as co-operating to produce the Ardent or Yellow Fever; since, if contagion existed, and acted with sufficient force to render that fever epidemical, among persons unaccustomed to the climate, it is contrary to reason and experience to suppose, that it would ever act so capriciously, as to leave the seasoned inhabitants unaffected by any other than fevers caused by local exhalations, and without its aid.

which he had frequently worn before? Should any of my readers entertain a doubt, sufficient to restrain him from answering this question in the affirmative, I am persuaded that by attending to the facts which will be hereafter sated in their proper places, his doubts will be effectually removed.

(The diseases are very much alike)

CHAP. II.

AT pages 7 and 8 of his observations, Dr. Pym has described certain other "peculiarities," which he conceives to belong to the Bulam Fever, and to *distinguish it from all others*; but if, as I have endeavoured to prove in the preceding chapter, there be no reason to believe in the alleged existence of any such fever, these supposed peculiarities must be as imaginary as the disease itself. One of them, however, that of "its attacking the human frame but once," having been deemed a matter of high importance, and as such brought under the consideration of His Majesty's most honourable Privy Council, in consequence of an application from Dr. Pym, it becomes on this account, requisite to enter upon a particular examination of the evidence alleged to prove its existence.

It was observed so long ago as the year 1695 (see p. 327 of my Essay) that the Ardent or Yellow Fever, then prevailing epidemically at Barbadoes, and by some persons called Kendal's Fever (which Dr. Pym admits to have been the same with the Bulam Fever) attacked *strangers* with much greater violence than others, "all new-comers being generally seized" with it. In succeed-

ing years the like observation was made whenever the Ardent Fever prevailed in that and other parts of the West Indies; or at Charleston in South Carolina. Of this frequent mention is made in the history which I have given of that disease in the volume just mentioned; so that it has been long known, and generally admitted, that persons, who were strangers to an inter-tropical climate, upon their first arrival in the West Indies, or soon after, were at particular times and seasons in great danger of being attacked with the fever in question; while the natives, or those who had resided there several years, in general either escaped wholly, or only suffered from the disease in a milder form; and this partial immunity in the latter, was ascribed to their having been "seasoned to the climate." It was afterwards found that those who recovered from an attack of the fever, obtained the same effect in a shorter time. Concerning the nature of this seasoning, and the powers and dispositions in the human constitution upon which it depends, a copious explanation (and, as I flatter myself, upon correct principles) was given between pages 244 and 275 of my Essay. These principles Dr. Pym attempts (at p. 160), to set aside, without any thing which deserves the name of reasoning, but merely by gratuitous and erroneous assertions, and to substitute for them an inexplicable peculiarity, connected, as he pretends, with that specific contagion to which he ascribes the fever under consideration; for it will be

demonstrated very shortly, that this supposed peculiarity has no other foundation in truth, than the partial or comparative security, which has been long known to exist as an effect of the *seasoning* described in the pages to which I have referred: although he represents it as being similar to that which is acquired by having had the small-pox. Illusive, however, as the supposed discovery will soon appear to be, the honour and merit of making it is disputed: for Sir James Fellowes attributes it to the "native practitioners" in Spain (p. xxiii. of his Reports): but, in opposition to this Dr. Pym says, "*I* made the discovery on the 20th of October, 1804," this being the time at which, as he alleges he found that 122 men belonging to the garrison of Gibraltar, who had previously been in the West Indies, had escaped the epidemic of that year. He adds, "the first Spanish physician that mentioned it was Arejula, and he did not publish until 1806: Sir James Fellowes gives the credit of it to the Spanish physicians generally; no individual one has claimed it. It certainly was not known among them in 1803; and I do not believe it was ascertained in 1804, until after the time that *I discovered* the non-liability of the West Indians," &c. For this, see Dr. Pym's communication in the Edinburgh Medical and Surgical Journal, No. 46.

Professor Berthe, of Montpellier, published his "Précis Historique," &c. in 1802, and in a note (No. 156) at

p. 393, gave an extract from a letter which had been printed, dated from Cadiz, the 6th May, in that year, in which, after stating the apprehensions still entertained of a recurrence of the former "Putrid Epidemic Fever," the writer makes the following observation, viz. "Comme *la petite Vérole*, elle est rarement à craindre, pour ceux qui en ont deja été attaqués." Here then we find that so early as the beginning of May, 1802, the notion existed, that the having suffered one attack of the Epidemic or Yellow Fever secured the patient from a second, in the same manner as persons after having had the small-pox are protected against that disease. By whom this notion (which Mr. Berthe considered as fallacious, and dangerous), was first entertained, or propagated at Cadiz, does not appear; probably it was derived from, and founded upon, the universal experience which I have already mentioned, of the rarity with which the natives and long-residents in the West Indies have suffered any severe attack of the Yellow Fever, and of the security which, from *similar causes*, those persons enjoy who have recovered from one attack of it, so long at least as they continue in the same climate: and this is most likely to have been the reason why no one of the Spanish physicians, although in possession of this notion some years before Dr. Pym dates his claim to the discovery of it, pretended to have been the discoverer. M. Berthe has moreover quoted, at p. 386, part of an official document communicated to him by the Procura-

dor Mayor of Cadiz, and dated the 6th of March, 1801, entitled, " A Relation of the Measures taken by the Ayuntamento of that City, during the Epidemic of the preceding Autumn, to cut short its Progress," &c. in which it is stated, that " towards the decline of the disorder," among other measures, " guards were stationed at the gates to exclude all persons from entering the city, who did not produce certificates of their having already had the fever; as experience had shewn, that those who had previously escaped the disease were, upon returning to Cadiz, immediately seized with it;" and this regulation demonstrates, that the civil authorities there were persuaded, even so far back as the autumn of 1800, that the same individual was not liable to be attacked by the fever a second time. Under a similar impression, Dr. Arejula has, at least, claimed the merit of having taken great pains to publish and make known this supposed security, as appears from his fourth chapter, which begins as follows, viz. " Attentive observation and reflection had taught me, that he who had suffered an attack of the Epidemic Fever, ran no risk of having it a second time. This effect was so general, that I ventured to publish and affirm by notices affixed in the streets, the following among other things, viz. Let all be firmly persuaded, that whoever has suffered the Epidemic malady on the present occasion, or in the year 1800, or has been a long time in South

America, is exempt from this disease; and, in order that he may understand it fully, the same will happen with him as with one who has had the Small Pox, who never fears or thinks of catching the disorder, although he goes where it exists. For which reason, individuals in this fortunate class ought not to shun a risk which has not the least existence, but should lend their assistance with readiness and good-will to their fellow-citizens or relations."* The exact time at which such public notices

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* Although Drs. Arejula and Riseuno mention that "those are exempt from the Epidemic who have once suffered it," it is proper to remark, that their own accounts afford some grounds for questioning the entire correctness of their belief in this respect, as will be seen by the following passages, which, in truth, admit as much as could be expected from those who had committed themselves on the opposite side of the question.

Dr. Arejula, at p. 231, says, "I saw nevertheless two persons in the year 1800, who, having had an obstinate fever with black vomiting in America, were seized with the Epidemic, although lightly. But this does not weaken the general rule." Also at p. 229—"During the Epidemic, individuals have frequently had fever, although they had passed through the *Yellow* Fever: but the latter does not exempt one entirely from every species of fever; and as all diseases, of whatever sort, that happen during the Epidemic, participate more or less of its character and appearance, some physicians believed they could produce examples contrary to what I had published; but, after reconsidering attentively the cases of their patients, they convinced themselves of the truth of my proposition."

Dr. Riseuno has also said (in the account which has been published by Dr. Burnett, p. 242) "I have heard of one or two instances of persons dying with the Yellow Fever, who were con-

were first given, does not appear to have been mentioned; but it might have been done at Medina Sidonia in October, 1801, when the fever prevailed there, and Dr. Arejula was sent by the government with two other physicians, to superintend the medical arrangement which the occasion required; for one of the measures which he states (p. 262) to have been then recommended by him and his colleagues to the Cabildo, or Town Council, was "the procuring of assistants from Cadiz, to be selected from those who *had already had the Epidemic*, in

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fidently asserted to have been previously affected with it; but, if this were the case, they are perhaps solitary instances."

As it appears from what Dr. Arejula has stated at p. 150, and elsewhere, that he had had communications upon this subject with Baron Humboldt, from whom he had, perhaps, derived his principal information, it may gratify some of my readers to know what that enlightened traveller has published thereon, and I therefore subjoin it.

"In tropical situations, a *second* attack of Yellow Fever is *rare;* at Vera Cruz unknown; but in the United States it is *not uncommon*.—It is remarkable that persons born and brought up in Vera Cruz are not *there* subject to the disease. The same is true at Havana, with regard to its inhabitants; and yet the natives of Havana are sometimes attacked with Yellow Fever when they visit Vera Cruz in August and September; while, on the contrary, natives of Vera Cruz have died of Yellow Fever at Havana, Jamaica, and the United States.—The whites and metis," (people of colour) "who inhabit the elevated interior of Mexico, especially the muleteers and recruits, suffer more from Yellow Fever than strangers who *arrive by sea*, probably from experiencing a more sudden change of temperature."—*Political View of New Spain*, vol. ii. p.

order thereby to cut short the progress of the disease." The fever appeared again at Cadiz, and also at Malaga, in 1803, and Dr. Arejula went, by order of his government, to the latter city on the 23d of October. Again, in the following year, the disorder having recurred there, and at Antequera, Montilla, Rambla, &c. he was commissioned to visit them; and he particularly mentions (p. 319) that "at these places, and almost every other, he selected as assistants to the sick, those who had previously undergone the Epidemic:" and as we find that in the year last-mentioned he was at Malaga on the 25th of August, at Antequera on the 28th of September, &c. there can be no room to doubt of his having made the publications in question some time *before Dr. Pym*, according to his own statement, had *his first conception* of the security which he supposed to be derived from having had the Bulam Fever.

But without spending time to ascertain the *author*, I will now proceed to inquire into the *reality* of this supposed discovery; and having done this, we shall be able, at the proper times and places, to form a just estimate of the benefits said by Dr. Pym, to have resulted from it to the nation, and to mankind.

In support of this peculiarity of the Bulam Fever, I find numerous and confident assertions by Dr. Pym, and

those who adhere to his opinion, but nothing like an unequivocal proof; nothing which is not either a manifest misconception, or capable of being well explained by a reference to that state of the constitution which is called Seasoning, or to a similar effect known to result from the attack of *any disease*, by which the vigour, excitability, and inflammatory diathesis,* produced by a cold or temperate climate, is for a time removed. Such is the mention which he makes (at p. 25) of his having requested the Governor to order a parade of all the officers and men (at Gibraltar, in 1804) who had been in the West Indies, at which he adds, "to my astonishment and gratification I found 'a hundred and twenty-two' who had all escaped the Epidemic of that year; and he supposes, without any proof, that, as they had been in the West Indies, they must *all* have had an attack of the Bulam,

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* The difference of the febrile susceptibility in young and vigorous men going to the West Indies from a cold climate, and that of men who have long resided there, may, as Dr. Pinckard has observed, be illustrated by the very different effect produced by the bite of a mosquito on each respectively, which is a painful, and sometimes dangerous, inflammation in the former, while in the latter the bite is scarcely felt or perceived. Another illustration of this difference may be found in that morbid and dangerous excitement which the bodies of vigorous men in cold climates suffer, when, after considerable exposure to severe cold, they suddenly approach, and remain for some time close to, a large fire.

and have been preserved by *that*, and not by the seasoning which they had unquestionably acquired.—Such also is his mention of the 57th Regiment, which arrived at Gibraltar during the prevalence of the malady in 1804, as a reinforcement to the garrison." "It had been," says Dr. Pym, (p. 28) "in the West Indies some time before, and had a considerable number of the men belonging to it when quartered in the Island of Trinidad; and as their service on shore was very much wanted, they were allowed to land, and added to the proofs of the disease *not attacking* a *second* time." To what extent this regiment fared better than other corps we are not told; nor does he pretend by this *vague* statement to have ascertained that *one man* belonging to it had undergone an attack of the Ardent or Yellow Fever; and yet he wishes us to believe that the alleged immunity of this regiment, whatever it may have been, resulted from *former attacks*, exclusively of their seasoning in the very hot and moist station of Trinidad. It is even upon weaker grounds that he adduces the 10th Regiment as a proof of the alleged peculiarity of the Bulam. This regiment, which was at Gibraltar in 1804, had previously served several years in the East Indies; and of this corps he says, "eight officers, who had been in India, were attacked with the fever, and all recovered. Seven officers, who had not been in India, had the disease in so different a form, that five of them died. Four hundred of the

men, who had been in India, were attacked with the disease, of which number only four died; and of forty-eight who had not been in India, sixteen died." Here we have unquestionably a clear illustration and proof of the beneficial effects produced in regard to Yellow Fever, *merely* by *seasoning* in the hot climate of India. For, as Dr. Pym asserts, and has laboured to prove, that the Bulam Fever never appeared there, he cannot maintain that the peculiarity which he has ascribed to it had any operation upon the men of this regiment.

I will not, however, waste the time of my readers by examining any more of such equivocal and unsatisfactory allegations, but will proceed to a statement of facts, sufficient to place this question beyond the possibility of doubt with all reasonable and unbiassed minds.

It will be seen by my introductory pages, that the liability or non-liability of individuals to second attacks of the fever in question, had been made a subject of inquiry by His Majesty's Most Honorable Privy Council, and that, to satisfy their Lordships, the Commissioners for the Service of Transports, and Sick and Wounded Seamen, &c. had requested from the superior medical officers of the navy, a communication of such facts relating to this point, and to the alleged contagion of the Bulam Fever, as had come within their knowledge; and

doubtless the experience and opinions of twenty-four gentlemen, who were supposed, by a competent judge, to be the best qualified, by their particular experience, of any in the naval department, to decide on these points, will be thought highly deserving of attention. I shall now state the results of this experience, with the respective opinions founded upon it.

1. Dr. Weir, Inspector of Naval Hospitals, declares in his Report to the Transport Board, that he has "met with many instances of one individual patient having a second, and indeed repeated, attacks of the Bulam Fever in its mild form; but in all cases where it assumed the malignant form, and was attended with high excitement and delirium, such cases were less susceptible to a second attack." He adds, that "the liability of this fever to a second attack in its mild form, and its less liability to a second attack in the malignant form, are not confined to this Epidemic Fever alone, but extend to *every continued fever* that he has had an opportunity of observing, whether in the West Indies, on the Coast of America, or in Europe."

2. Dr. Blair, late Physician to the Fleet, in his Report, "most positively contradicts that part of the general clause wherein Mr. Pym asserts, that the Bulam Fever never attacks the human frame but once;" and, to sup-

port this denial, he refers to two instances which, as he says, are "so authenticated, that no theory of Mr. Pym can overturn them." Of these, "the first is that of Admiral Lord Hugh Seymour," of which he gives a detailed account, stating his Lordship to have been under his (Dr. Blair's) care at *Surinam*, during "a most severe attack of Yellow Fever, with the most marked and indubitable symptoms." His life was saved with great difficulty; and about two years afterwards, Lord Hugh, being at Jamaica, suffered another attack of fever, which, according to the testimony of Mr. Menzies, surgeon to the Flag ship, and of Dr. Skene, a very eminent physician at Kingston, was most undeniably the fever called Bulam by Mr. Pym, and which proved fatal. Another instance, of which the particulars are stated, occurred in the cockswain of Dr. Blair's boat.

3. Dr. Gillespie, physician in the Royal Navy, in his Report, after mentioning that, while he was stationed at Fort Royal, Martinique, with the medical superintendence of seamen and prisoners of war, between the years 1795 and 1802, he had at least ten thousand patients under his care, a portion of whom were affected with the fever in question, remarks, "I can by no means subscribe to the *opinion of Mr. Pym*, that a person who has once had the Yellow Fever, is not susceptible, by any future exposure to its predisposing causes, to a second or more

attacks of it; an opinion founded, as it appears to me, on the theoretical supposition of the Yellow Fever's being of a specifically contagious nature, resembling Small Pox, Measles, &c. and imported from Bulam in 1793, an opinion which is *diametrically contrary to my experience*, and to the studious investigation which I have for many years bestowed on the subject."

4. Dr. Gray, late Physician to the Fleet in the Mediterranean, says in his Report, "there are many instances on record where Yellow Fever in the West Indies has attacked individuals a second time, and proved fatal, particularly in those who have gone to a northern latitude, and afterwards returned to the West Indies;" and after describing the fever as it occurred with black vomiting, he adds, "I have repeatedly observed relapses to take place in this fever, and have had patients under my care who have been attacked *twice*, particularly those who at first had undergone a mild attack of the disease."

5. Dr. Dickson, Physician to the Fleet in the West Indies, and Inspector of the Naval Department, in his Report, says, concerning second attacks, "I have certainly known many instances of recovery by early and *decided* practice, from what I had every reason to consider as the first stage of Yellow Fever, where the patient has afterwards been attacked, and died of this disease.

I could adduce the names of some officers who died with black vomit, after having been three, four, and five years in the country, who had previously had the fever, which had been happily arrested, but which there was every reason to suppose would otherwise have terminated in the same way. My office as Inspector has been rather to note general occurrences, than individual cases and names; but I consider myself as having seen fair instances of the *recurrence*, as well as of the *relapse*, of the disease. This is a fact, however, which, I should think, may be determined by an appeal to the hospital-books, and, if I mistake not, to the experience of some of the (medical) officers in their own persons."—"But while I am consequently of opinion that the Yellow Fever may attack the human frame a second time, I am fully persuaded, that from the reduction" (of strength), "and the assimilating effect produced by the treatment and by the disease, the constitution will be rendered greatly less susceptible, and, generally speaking, be placed in a state of relative security."*

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* Dr. Dickson has lately published in the Edinburgh Medical and Surgical Journal for January, 1817, some just observations respecting the means by which the susceptibility of the human body for the Yellow Fever is for a time impaired or lost, and afterwards regained; which accord with the remarks contained in my Essay upon this subject.

6. The Report of Dr. Burnett, late Physician to the Fleet in the Mediterranean, is next in order; but, as this gentleman, in the last edition of his valuable work on the fever of that station, between pages 332 and 335, and between pages 467 and 471, produced lists of sixty-three* well-authenticated instances of persons who had undergone second attacks of this fever (besides a considerable number mentioned by other persons), I need not here quote from his Report.

7. Dr. M'Arthur, late Physician to the Naval Hospital at Deal, in his Report, says of the fever under consideration, "I have reason to conclude, from my own experience, that it rarely attacks the human frame more than once, *so long* as the objects of its attacks *continue* in the country, or *do not* commit irregularities. Instances, however, of persons having the fever a *second* time, have been under my own care; but it had invariably succeeded to protracted fits of intemperance. Also *several* cases occurred to me of persons who had had the Yellow Fever, and, subsequently going to Europe, were, after their return to the West Indies, attacked a

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* Some of these were stated in the Official Reports of the Medical Officers at Gibraltar in 1814, and will presently be noticed.

second time.* I now regret that the memoranda of these cases have not been preserved: but as my letter (to the Board) of the 1st of September, 1809, wherein the facts are stated, was hastily written, and never intended to convey my sentiments to the public, I could have no motive for stating them but a conviction of their truth." He adds, " a residence of a few years in the West Indies gives to the European *as great an immunity from the Yellow Fever* as *an attack of it* would have done; the *change* in the constitution which the fever *suddenly* effects, appears to be accomplished more *slowly*, but with equal certainty, by the climate."

8. Dr. Denmark, late Physician to the Fleet in the Mediterranean, after stating in his Report " that there is no difference between the Bulam, Yellow, Bilious Remittent, and Bilious Continued, Fevers, except *in degree*," declares " that one attack is no certain security against subsequent ones, although it may render the body less susceptible, and that relapses are frequent."

9. Dr. Gardiner, late Surgeon to the Naval Hospital at Gibraltar, states " an impression to have been made

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* Similar facts are well known in different parts of the West Indies as well as at Charleston, in South Carolina, to have repeatedly occurred at these several places.

on his mind that those who had experienced the fever in 1804 escaped its influence in 1813; and that the soldiers who had served in the East or West Indies were not so liable to its attacks as those residents who had never quitted the garrison, or had recently arrived from England; but (he adds) it was at the same time reported that a merchant who had long resided in the West Indies, and survived a severe attack of the Yellow Fever in that climate, * fell a victim to the Gibraltar malady in its most aggravated form." He afterwards intimates that he does not think the non-liability to second attacks of the Gibraltar Fever to have been yet satisfactorily proved.

10. Dr. Veitch, principal Surgeon to the Naval Hospital at Antigua, "from 1808 to 1813," in his Report, asserts the liability of the human frame to more than one attack of the Ardent Yellow Fever. He says, "the restoration of vigour and of susceptibility, by returning to Europe, or even by going to sea in the West Indies, will on exposure to the causes reproduce the disease: *many*

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* The merchant whose case is here mentioned, was, as Dr. Burnett has stated at p. 333, Mr. Muir, who had "experienced a most violent attack of Yellow Fever with the worst symptoms of that disease" at Surinam in the year 1812; but, on coming to Gibraltar a little before the commencement of the Epidemic in 1813, "he was among the first attacked with the disease, and died on the fourth day of the attack, with yellow suffusion and black vomit."

instances of this nature came under my observation; and several, even before the ships went out of the harbour. In the Cherub and Thetis some cases of this description proved fatal. It has long been known among people of colour, as well as the long-resident inhabitants, that those who have had this disease are by no means so liable to it as the recently-arrived Europeans, and that, if they continue in this climate, they may be considered, comparatively speaking, as enjoying an exception: but this cannot be compared with the security arising from an attack of the small-pox."—"The Regulus, the first ship in which I visited the West Indies (in 1794) was, after leaving the Cove of Cork, attacked with fever of the Typhoid character, which became pretty general among the ship's company; but the loss was on that occasion inconsiderable, and that fever did appear to have prepared the constitutions of those who were attacked with it, to sustain the influence of a tropical climate; as *not one of those who suffered from Typhus, was attacked with the Ardent Fever** during our stay in the West Indies, which was something more than twelve months." He adds, "an

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* Dr. Veitch, in his inaugural dissertation printed at Edinburgh in 1808, has mentioned that the crew of the Regulus consisted of two hundred and ninety-four men, and that nearly one hundred of them were seized with Typhus; and that, although no one of these was afterwards attacked by the Yellow Fever, a great part of those who had escaped the former were attacked by the latter during their stay in the West Indies.

attack of Dysentery, Remittent, or Intermittent Fever, will as effectually secure the patient from the Yellow Fever as if he had suffered from the disease, and by the same means, that of *lowering* the system." Dr. Veitch also represents Dr. Pym as greatly over-rating the security obtained by having sustained an attack of Yellow Fever, when he compares it with that which results from having undergone the small-pox, measles, &c. and which is *lasting*, moving with the individual through all climates, situations, and changes of the constitution: "but in the Yellow Fever, (says he) the effect may be considered as ephemeral; since the return to a cold climate, in the young and vigorous constitution, will generally reproduce the susceptibility of this disease; and a residence in such a climate will render the *white Creole*, and Black, liable to the Yellow Fever; and hence the promiscuous attacks of whites and blacks by the Fever of Philadelphia." —"I have met with the disease in blacks who had been employed in cold climates. The Creole, who visits Europe at an early period of life, and returns in full manhood, and with the sanguine temperament, often falls a victim to the disease." Of this he gives an afflicting instance which fell under his own observation, in the nephew of Admiral Sir Richard Bickerton.

11. Dr. Vance, one of the Surgeons to the Royal Hospital at Haslar, in his Report, says, "respecting the non-

liability of the human frame to receive the Bulam or Yellow Fever a second time, I think it necessary to state that I consider the Bulam as a variety of the Ardent, Yellow, or Bilious Remittent Fever, and not a disease sui generis: and I entertain no doubt of the liability of the human frame to several attacks of the disease." He then mentions the names of two officers who were under his own care during their first *severe* attacks of this fever, and who were under the care of the gentleman by whom he was succeeded during their second attacks. One of the officers died on the fourth day from the attack with black vomiting. He also subjoins the names of eleven seamen, of whom all suffered *second* attacks, five a *third* attack, and one a *fourth*, giving the dates of each attack, and adds, "the above names have been inserted here out of *many* who have suffered more than one attack; but proofs of this nature, afforded by any individual, are weak in comparison with the existing opinion of the extensive population of the island of Jamaica."

12. Mr. Gregory, late Surgeon to the Naval Hospital at Jamaica, in his Report, says, that during his residence there he has not known "any individual afflicted with the disease a second time;" and, "that it was the general belief among the medical practitioners in Jamaica founded on observation, that no person was subject to the disease a second time while remaining in the country." And, in

regard to the question, whether a person who has had the disease, and afterwards resides some time in Europe and returns again to the West Indies, will be liable, or not, to a second attack, he states that he has "not had sufficient experience to give an opinion."

13. Dr. M'Namara, now Surgeon of the Naval Hospital at Jamaica, says in his Report, "with respect to the non-liability of the human frame to receive the disease known by the name of the Yellow Fever a second time, I beg leave to differ entirely from Mr. Pym; as the experience of nearly eight years in this climate has afforded me many opportunities of observing persons to be attacked with the disease not only a *second*, but a *third* time."

14. Mr. Mortimer, lately Surgeon to the Naval Hospital at Barbadoes, in his Report, states that he was attacked with the Ardent Yellow Fever at Mariegalante in 1801, when it prevailed there with an "intenseness and malignity which has not been exceeded;" and that he was afterwards attacked by the same disease at English Harbour in Antigua" with the full force of "its malign influence." "I had (says he) every symptom save black vomit, and my recovery was deemed next to impossible by those in attendance." He moreover adduces the case of Lieutenant Reddie of His Majesty's ship Gloire, who died in the Naval Hospital at Barbadoes, on the 14th

November, 1809, of a disease which was decidedly the Yellow Fever, with discharges by vomit and stool, " of a brown bloody-looking fluid, several hours before his death," as is recorded in the books at the hospital, where he had previously been, and undergone an attack of the same fever. He farther says, " I have repeatedly known individuals who, having survived one attack, have on a *second* fallen victims ;" and he also relates, in regard to the fever at Mariegalante in 1808, that " of its recurrence in *the same* individual, there were many instances, as well among those continuing in the town as among others removed to different situations in the island."

15. Dr. Kein, Surgeon to the Royal Marine Division at Plymouth, says, in his Report, that he believes " there are very few instances, if any, of the same patient being attacked with the Bulam Fever a second time, although many of those very men were sometime afterwards attacked with the common Remittent Fever of that climate, and some died."

16. Dr. Crichton, Surgeon to the Naval Hospital at Antigua, in his Report, states, " as to the human frame being only liable to one attack (of the Ardent or Yellow Fever,) I cannot hazard an opinion, as I have never been where the disease became so general as to afford me an opportunity of investigating that interesting point. Mr.

Comrie, Surgeon of the Niobe frigate, seems to be well acquainted with the disease in question, and I understand that he has had it a second time;* but, as that ship has left this station, I cannot ascertain the fact."

17. Dr. Magrath, late Surgeon to the Hospital at the Mill Prison, in his Report, mentions, that, " from the result of his observations both in the West Indies and Gibraltar," and the best information he could obtain, " he should be disposed to accede to Mr. Pym's opinion; but with this qualification, that, having once sustained the disease, the constitution is rendered, in a great measure,

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* Mr. Comrie has since published (in the 50th number of the Edinburgh Medical and Surgical Journal,) the results of his experience on board the Raven and Niobe ships of war in the West Indies, and says, in regard to himself, p. 179, " I have *several times* been *severely* attacked with the Ardent Fever." Mr. Comrie, after describing the Fever as it appeared in the Niobe at English Harbour, Antigua, from the middle of September to the latter end of October, 1815, relates at p. 171, that " the captain, and most of the officers, all the warrant-officers, and almost all the midshipmen, and others included in the Report (there given), were *severely* attacked with this fever;" which, however, subsided on the ship's going to sea, and cruizing to windward of all the islands. But it was necessary that she should return to Antigua " to complete her refitting;" and about the latter end of November and through December, of the same year, the disorder recurred with great violence; and Mr. Comrie, having described this second fever, says, at p. 179, " on our passage to Bermuda, the Captain and almost all the officers, and most of the midshipmen were *again* severely attacked with this disease."

unsusceptible of future attacks.—But (adds he), I would not extend the exemption to the positive dogma to which he carries it, when he unequivocally and unreservedly asserts the non-liability of the human frame to receive the infection a second time : for I believe that the multiplied experience of some of our most accurate observers of the nature and qualities of the West India Yellow Fever would serve to *evince, beyond contradiction, its occurrence more than once* in the same subject ; but the subsequent attacks in those, as in Europeans assimilated to a warm climate, have been for the most part comparatively mild, and divested of much of the danger."

18. Mr. John Forbes, late Surgeon of the Venerable, in the West Indies, after stating in his Report, objections to some other " peculiarities" of Dr. Pym, adds, " I have also, on more than one occasion, witnessed patients attacked a second time by the Acute Continued Fever of this country, by whatever appellation it may be known."

19. Mr. M'Leod, Surgeon in the Royal Navy, in his Report delivers his opinion as follows. " Respecting the liability of the human frame to repeated attacks of this fever *I have no doubt:* on the coast of Africa I had it distinctly, shortly after my arrival in that country. In a long passage to the West Indies I had in a great degree recovered my strength, and continued in good health,

until I again (I think about fifteen months after the first,) had a second attack; Mr. Cole, the Master Attendant of Port Royal Dock-yard, died of the second attack. The cases of Lord Hugh Seymour and some others in the recollection of Dr. Blair, Physician to the Fleet, are likewise striking proofs of this liability. Mr. Tobin of Portsmouth Dock-yard has suffered repeated attacks. Certainly second attacks of the disease, where the subject has not left the country, are by no means frequent. But I have no doubt, where he has been removed for a year or two, to England, or some other temperate climate, and recovered the *tone* and bracing of that climate, and returns before an advanced age, he is as liable as ever to the disease, by bringing back with him the original cause, viz. an unassimilated constitution."

20. Mr. E. H. Brien, Surgeon in the Royal Navy, says in his Report, "I do not recollect to have seen the Yellow Fever occur twice in the same subject, nor do I know whether my journals will give any information on that subject; as the crew of His Majesty's ship Franchise, in her first station in the West Indies, were drafted from her previous to her going a second time." He adds, "I have been in the habit of intimacy with the late Dr. Halliday of the Havana, an acute observer and discriminator of the diseases of that climate, and whose abilities I availed myself of in the prevalence of the disease (Yellow

Fever) before alluded to in the Franchise. He asserted that, during an extensive practice of eighteen years among the resident Americans and likewise among the Spaniards, he never saw a person affected with the Yellow Fever a second time, unless he removed to, and resided for five or six months in, a *cold* climate; that he then became as subject to the disease as on his first coming to the climate (of Havana). This, he said, he had opportunities of annually witnessing among the resident Americans, whose business called them for some time in the year to America; that several had fallen victims to the disease on their return to the Havana, who had it *severely* before, on their first coming to that climate."

21. Mr. Richard Tobin, Surgeon to the Royal Navy, in his Report says, "I can bring forward no facts * to

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* Mr. Tobin explains the cause of this by saying, "that, although the (people of the) Arab, of which ship he was Surgeon, had a severe attack of Yellow Fever in the West Indies in 1807, her being ordered home immediately afterwards precluded me from an opportunity of ascertaining the point in question," respecting second attacks. He continues, "we had been stationed on the Coast of Guinea eighteen months previous to our going to the West Indies, and many of the ship's company had the Bilious Remittent Fever while there, which is a strong proof that a 'seasoning' on the coast is no barrier against the dreadful Typhus Icteroides of the West Indies, which, most undoubtedly, is a *different* disease." The reason which seems to have led Mr. Tobin to consider the latter to be a different disease from the former

support or refute the positions advanced by Mr. Pym:" he adds, that, "he has seen relapses from Yellow Fever occur and terminate fatally."

22. Mr. John Niell, Surgeon in the Royal Navy, in his Report, speaking of the supposed immunity from a second attack of Yellow Fever says, "I am inclined to think that such an immunity results more from age and length of residence in a tropical climate than from any protection an attack of such disease affords."

23. Mr. Donnet, Surgeon of the Royal Navy, does not

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is shown by the succeeding paragraph of his Report, in which he calls the Typhus Icteroides by the English term of "the *Continued* Yellow Fever," and then adds in a parenthesis, "I say *continued*, because I never knew it *remit*." I do not besitate to believe that remissions were not observed in the very aggravated fever which Mr. Tobin then witnessed for the first and the last time in the West Indies (of which farther mention will be made in the next chapter): that the Bulam or Yellow Fever, does, however, *frequently remit* has been already, and will again be, abundantly proved; and it will equally be proved that the fevers on the Coast of Africa are not *different* from, but *identically the same* with, those of the West Indies; indeed this has been long and well-attested. And if these fevers be *not* different, the severe cases which Mr. Tobin treated in the West Indies in 1807, must obviously have been *second attacks* in those of his patients who had formerly had the fever of the African Coast. Concerning the latter disease, see Dr. Schotte's Description of it at Senegal; and also Dr. Robertson's Physical Journal on board His Majesty's ship Rainbow.—Part 1. cap. 1 and 2.

deliver any opinion, in his Report to the Transport Board, concerning second attacks of the fever in question; but, having inclosed a copy of his answers to the questions of Deputy Inspector Fraser (relative to the Epidemic Fever of 1814 at Gibraltar) I find that he there expressed himself on this subject as follows, viz. "I am of opinion that those who have had the Epidemic once are not liable to be attacked a second time; but the case I subjoin (adds he) is, I think an exception."* This was the case of John Lewis, Commissioner's cockswain, who was seized "with the characteristic symptoms of the Epidemic on the 12th of September," and "was attacked, on the 6th of November following, with similar symptoms, which proceeded in the same manner."

24. The last of the before-mentioned communications

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* It is proper to observe that, although Mr. Donnet is silent on the subject of second attacks in his Report, he has nevertheless detailed the particulars of a *second* attack of the Bulam Fever in the case of Mrs. Vaughan, who, as Dr. Pym has testified, "had the disease in 1804." Mr. Donnet mentions her as "the first person he witnessed at Gibraltar labouring under that disease in its aggravated form" in the Epidemic of 1810; he visited her on the evening of the 11th of October," and she died the next morning with black vomiting, &c. He adds, "the fatal symptoms manifested in Mrs. Vaughan's case having appeared to me similar to those which had attended the Epidemic Fevers of Cadiz, Malaga, and Gibraltar, I thought it incumbent on me to acquaint Dr. Pym, then Principal Medical Officer of the Garrison, of the circumstance."

is that of Mr. Whitmarsh, late Assistant Surgeon to the Naval Hospital at Gibraltar, who therein makes the following statement. "I know of no decided instance of a person going through the disease in 1814 who had, the year or years previous, been a subject of its attack. It was an opinion, without I believe one exception, from every medical man on the Rock, that a person, having gone through the disease once, could not again come under its grasp. I was firmly *prejudiced* to believe so: but reasoning on the subject has made me suppose the idea *fallacious*, and that a future period will convince the medical world that the common Bilious Remittent of hot climates, which more or less appears towards the autumn of every year along the whole southern coast of Spain, is in truth, the *same* fever, but a milder grade of what is held forth as the devastating Bulam."

By a review of the preceding extracts it will be seen that, of eight gentlemen who have filled the office of Inspector of Naval Hospitals, or that of Physician to the Fleet, or both, *all* either attest their knowledge or their belief of the frequent occurrence of second attacks from the fever under consideration; that of six gentlemen who have executed the office of Surgeon to one or more of the Naval Hospitals in the West Indies or at Gibraltar three decidedly maintain and give instances of the occurrence of second attacks in the Ardent or Yellow Fever; one, Dr. Gardiner, seems doubtful, but mentions one instance, and ultimately

concludes, that the non-liability to second attacks has not been satisfactorily proved; another, Dr. Gregory, whose residence and experience in the West Indies have been, as appears from his statement, limited, has not known any instance of a second attack, and states it as a general opinion among medical practitioners in Jamaica that an individual was not liable to a second attack while he remained in the climate;—an opinion, however, to which a multitude of exceptions have been attested;—and the third, Dr. Crichton, from want of experience, does not hazard an opinion, but refers to the case of Mr. Comrie; and that of the remaining gentlemen, ten in number, two, Mr. Brien and Mr. Tobin, having no sufficient experience, abstain also from giving any opinion; one, Dr. Kein, seems to think that where he has known second attacks, one of them has been from the Bilious Remittent, while the rest either decidedly attest second attacks, or admit such explanations of Dr. Pym's peculiarity as make it accord with the mere effect of what has long been known under the name of *seasoning*.

In addition to this very respectable mass of *cogent* evidence against the existence of any peculiarity, which operates as a security from second attacks of the fever in question, it is stated (in the answer from the Commissioners of Transports, &c. to the Lords of the Privy Council) that Dr. Harness, the Medical Member of that Board, with the assistance of Dr. Gray, late Physician to Haslar

Hospital, and of Dr. Tait, late Physician to Yarmouth Hospital, " examined a considerable number of the journals of naval Surgeons, who had served in the West Indies, as well as the books of the hospitals there, and traced a second attack of the Ardent Yellow Fever in a very considerable number of persons, particularly on board his Majesty's ships the Circe, Blonde, Thunderer, Pompée, Lily, and Vimieira; and, that no doubt might exist as to the certainty of these attacks, they made a list of the names of the persons attacked, together with the periods of the attack; and to prove that the fever with which the parties were first attacked, was *really* the Ardent Yellow or Bulam Fever, they included in that list the names of some individuals who were attacked about the same periods, and who died with the fatal symptom of black vomit: thereby affording a fair inference that, although the symptoms of many of those who had suffered a second attack might have been in the first instance of a mild nature, yet the disease was the same with that generally denominated Ardent Yellow or Bulam Fever." By the before-mentioned list, which accompanies that answer, it appears that, among the men belonging to the Circe, there occurred between the years 1805, and 1808 inclusively, *fifty-one* cases of *second* attacks; and of these seven had each a *third* attack, and there were, besides, three deaths with black vomiting. In the Blonde thirty-seven cases of *second* attacks occurred between 1807 and 1810 inclusively; of these five had a *third* attack, and six

deaths were accompanied with black vomiting. In the Thunderer, eleven cases of second attacks occurred, and in two of these a third attack, and there were five other cases of death with black vomiting. In the Pompée five individuals had second attacks, of whom two died with black vomiting. In the Amelia, there occurred four instances of second attacks; in the Lily two: and in the Vimieira fifteen, and in one of the last a third attack: one of the second attacks terminated with black vomiting.

Probably these proofs and instances of *second,* and even *third,* attacks of the Ardent, or Bulam Fever, will satisfy all, whose minds are unbiassed, that Dr. Pym's supposed peculiarity has no other foundation in truth, than the effect which has for a length of time been generally acknowledged to result from a continued residence between the tropics, or from an equivalent diminution of vigour and excitability produced, by having undergone the disease. But there are yet other proofs and instances of the like import; particularly those contained in the official answers given by a majority of the medical officers at Gibraltar, to the questions put to them by Deputy-Inspector Fraser, in the year 1814. Among these, Surgeon Sproule, of the Ordnance Department, had two cases of second attacks; Surgeon Lea, acting with the 26th Regiment, had "under his own observation several instances of second attacks and of relapse," each attack being "decidedly marked." Seven of Surgeon Short's

patients (60th Regiment), during the Epidemic of 1814, had the preceding year been seized with "Yellow Fever in Cadiz," and are mentioned by name in his Report. Surgeon Weld, 67th Regiment, declares, that "several men who had the fever at Cadiz and Carthagena, have had the fever here (in 1814), and four men have relapsed;" he adds, that "both attacks were decidedly marked, and similar in their progress." Mr. Amiel, Acting Surgeon to the Foreign Depôt, had under his own care two cases of second attack (which he names), "besides a few cases he had from good authorities," and "has frequently observed relapses." Assistant-Surgeon Brady, 26th Regiment, declares that "he saw eight or ten men who had had the disease last year (1813), and also had the same symptoms this year, two of whom died." He adds, "last year we received some men, from different corps, that had been in the West Indies, to act as orderlies; those men were said to have had the prevalent fever of that country; they resisted for some time* the contagion, but were at length attacked, and some of them died." Assistant-Surgeon Martindale, 67th Regiment, says that, not having been at Gibraltar during any former Epidemic, "he cannot say how far the men might be liable to a second attack in a succeeding year; this, however, is certain, that several men who had the

* This gentleman was one of *the few who thought* the disease contagious.

fever at Cadiz, Carthagena, Alicant, and Tarragona, have been attacked with the prevailing fever of Gibraltar. As to relapses, four have occurred, two of which proved fatal." Assistant Garrison-Surgeon Williams says, that "he had five cases of the Yellow Fever (in persons) who had it last year; but, as they were foreigners, he is not able to trace them." Surgeon Donnett, of the Naval Hospital, likewise gave answers to Mr. Fraser's queries; but having already stated his opinion and experience in regard to second attacks (No. 23), I need here only refer to them.*

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* To these the testimony of Surgeon Barker, 11th Foot, might with propriety, as I conceive, be added, his answer to the same (9th) query being as follows, viz. "I have not observed any instance of second attack, neither were there any which I could call relapse. Two patients sickened again after *a few days* appearance of *convalescence*; but they evidently still had low fever.

"One or two cases *stated* that *they had the Epidemic last year*; but having no documents of the nature of the fever, I consider it more than probable that they had a different complaint. I say, more than probable, because the fact is *already ascertained* that the disease can be communicated but once."

Mr. Amiel informs us, that *at Gibraltar* the opinion just mentioned was held "as a law in medical science," and Mr. Barker seems unfortunately to have imbibed it; else he would not have rejected the evidence which his patients offered in their own persons. Soldiers have peculiar facilities for knowing what their diseases are, because these are unreservedly stated in the surgeon's public returns, and because, in hospitals, unless blind or delirious, they can always observe wherein their complaints resemble, or differ from, those of others. These "one or two cases" were, therefore, in my opinion, entitled *primâ facie* to credit.

Besides these instances of second attacks by the fever in question, a considerable number have been published by different writers, of which it will be sufficient to notice a few.

Dr. Rush, at p. 36 of his Fifth Volume, states, that during the prevalence of the fever under consideration at Philadelphia in 1797, he attended Dr. Physic, and five other persons who are named by him, and who had been his patients under the same fever in 1793, which Dr. Pym has admitted to have been the Bulam, and whose identity with that of 1797, I have reason to know cannot be questioned, having been at Philadelphia during its prevalence.

Dr. Currie of that city, although a decided advocate for contagion, after mentioning (at p. 15 of his Treatise on the Synochus Icteroïdes, or Ardent Yellow Fever) the assertion which Dr. Lining made more than half a century ago, that those who had "once had the disease, cannot take it a second time," adds, "but we have seen several instances occurring *here* a second time, where the circumstances were so unequivocal, that it could not fairly be ascribed to relapse."

Staff-Surgeon Doughty, at p. 183 of his Observations on the Yellow Fever in Jamaica and at Cadiz, says, "I

am aware that persons exposed to the cause, and who have laboured under the effects of the most aggravated form of Yellow Fever, are not likely to have it a second time; but those who have been exposed to the cause of a minor degree, and laboured under this fever in its milder form, will certainly run great risk of being attacked, if exposed to the source of this disease, in a *more powerful degree of concentration;* and more especially if there has been any *regenerated susceptibility*, from a residence, for a given time, beyond the precincts of the generative cause. The 85th Regiment, which suffered so greatly in Spanish Town in 1805, from an exposure to the Morbific Miasm, in its apparent higher degree of concentration, was not in the least affected in Fort Augusta, the succeeding season; not a man was attacked with Yellow Fever. Here I am convinced their health improved, and a consequent regenerated susceptibility was created. It is only on this consideration that I can account for their suffering *again* so greatly in the barracks of Kingston, in the autumnal season of 1807. Many of the soldiers who had laboured under fever in Spanish Town, and recovered, *were attacked again here,* and fell victims to the disease. Three officers died, and I, it is well known to many friends, *was on the brink of the grave,* although I had laboured under Yellow Fever, in its concentrated form, in the same town seven years preceding." He afterwards relates at pp. 184, 185, the

cases of Lieutenant Dowie, and Mr. Piercy, who, in 1807, died of Yellow Fever, with *black vomit*, &c.; though the *latter* had been severely attacked by that disease in 1802, and the former had been attacked with its violent symptoms, excepting black vomit, but two or three months before his last fatal attack.

Mr. J. B. Sheppard, lately second surgeon of the Naval Hospital at Antigua, in a communication printed in the 47th Number of the Edinburgh Medical and Surgical Journal, says, "either the Bulam and Yellow Fever are not the same disease, and consequently all analogical reasoning derived from the latter falls to the ground; or if, as I believe, they are the same, then what has been called the Bulam Fever may, and does, attack the human frame * *more than once:* but the name is erroneous, because the Yellow Fever is not an imported, or, in other words, a contagious disease."

Dr. Pinckard, at p. 170 of vol. ii. of his Notes on the West Indies, second edition, observes of second at-

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* Dr. Dickson, physician to the Fleet, in a communication printed in the 49th Number of the same Journal, after noticing these Observations of Mr. Sheppard, adds, "I have reason to believe that the writer could have instanced a second attack of Yellow Fever in others, as well as *in his own person.*

tacks of the Yellow Fever, " to the mortification of those who have already suffered it, as well as of others, *we know too well* that it may and does attack the same person not only a second, but many times."

Father Labat, as is mentioned at p. 322 of my Essay, relates, that at Martinique, towards the end of the seventeenth century, he was twice attacked with the Mal de Siam, said to have been imported by the French ship of war, l'Oriflamme, which had then recently arrived from the East Indies, where Dr. Pym asserts, that his Bulam Fever never existed, although, with very little consistency, he recognizes its identity with the Mal de Siam.

Captain Beaver also, whose fever at the Island *of Bulama* is said, but without foundation (by Dr. Chisholm, in his Letter to Dr. Haygarth), to have been direfully contagious, distinctly states, that he had *seven* different attacks of it, of which he gives a particular account, as quoted in the Seventh Appendix to my Essay: and as all the fevers at that Island were of one kind, the Bulam fever of Dr. Pym either was not thence derived, or, if his account of it be true, must since have wonderfully changed its nature.

Upon this question I ought not to withhold the result of my own experience, which is, that second attacks are

not rare: individuals have died under my care with the black vomit, who had been attended by me in the same fever during former years, and who had not even quitted Jamaica in the interval.

It becomes me here to mention, that Deputy-Inspector Fraser, in his Report to the Army Medical Board concerning the cases of second attack, stated by the medical officers at Gibraltar to have occurred there, in the Epidemic of 1814, observes, that as two of the surgeons "had made no distinction between the Epidemic and the common Bilious Remittent, it is by no means surprising to find them noting the Epidemic as a second seizure; for," he adds, "I have seen many instances of people having the Malignant Fever in its regular type, who in the preceding year had laboured under Bilious Remittent; I therefore conclude that, in the few instances of second attack, one *must* have been of the Bilious Remittent." We have here a clear indication of that strong bias which has existed in the minds of all the officers who have been at the head of the medical department in Gibraltar during the last ten or twelve years, and which may be supposed to have had no small influence on the opinions of others, although the truth is *now becoming prevalent*. Mr. Fraser's full persuasion that Dr. Pym's allegation on this point was true, has led him to infer, *as a matter of course*, that wherever two attacks of fever

had occurred in the same individual, "*one* of them *must have been* the Bilious Remittent." As it will rarely happen that both attacks are equally violent, this sagacious choice of ground enables Dr. Pym and his adherents, with a degree of plausibility, to elude the force of almost every fact which it is possible to adduce; because, as I have already shewn, there is no characteristic symptom of the supposed Bulam, and therefore, if ten thousand instances of second attacks be proved, it is always easy to assert that only one of those attacks was by the true Bulam. For as black vomiting is, with some most rare exceptions, a mortal symptom, it can hardly be expected to occur *twice* in any *one* person; and if it should, even this might not satisfy or confute Dr. Pym; for when, notwithstanding he calls this vomiting "the fatal symptom *peculiar* to the Bulam" (p. 4), we find him denying,* upon his own gratuitous assumption, that

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* The case which is here alluded to is Mrs. Vaughan's; and certainly a more indisputable proof of a second attack cannot well be conceived: for Dr. Pym affirms, of his own knowledge, that this lady had the Bulam Fever at Gibraltar in 1804; and Mr. Donnet, Surgeon to the Naval Hospital in that garrison, who attended her, and knew the disease well, declares that she died there on the 13th of October, 1810, with black vomiting, yellowness, and other symptoms, rendering it a decided case of the Epidemic (see Dr. Burnett's Account, p. 477). Dr. Pym further admits "Mr. Donnet's Report" of her symptoms and death to be "true;" yet, even in this instance, not less fatal to all his pretensions than

a case of fever in which it had occurred was the Bulam, although proved to be so by a very competent and respectable witness, we may believe that ingenuity like this, would equally disentangle him from every other dilemma, and open a way for his escape.

But to terminate this discussion, I will shew that Dr. Pym confutes himself; and demonstrate, *from his own admission and assertion*, that the immunity which he ascribes to one attack of the Bulam Fever does *not*, even in *his* own opinion, result from any *peculiarity* belonging to that fever more than to other debilitating diseases, but that it is solely an effect of what is commonly called *seasoning*, from the having been born in, or become assimilated to, an inter-tropical climate. I have already shewn that the effects of the Epidemic at Gibraltar in 1804 (in regard to the soldiers who had previously served in warm climates), which Dr. Pym adduced to prove the peculiarity in question, were nothing but the consequence of this seasoning; and his admission, as stated in

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it proved to the unfortunate sufferer, he seeks, and thinks to evade its effect, by assuming the very point contested, and by employing it to prove itself; and, for this end, ventures upon denying her last fever to have been the Bulam, "because she had had the disease in 1804, and *of course* was not liable to a second attack." See the London Medical Repository, No. 33, p. 203.

the annexed note,* is fully equivalent to this. But we are furnished with a much more decisive one in the 33d Number of the London Medical Repository, p. 207, where, in order to answer and repel an important objection, which had been urged against him by Dr. Burnett, he makes the following assertions in regard to the Bulam Fever, viz. "the peculiarity of its sparing natives of warm climates is equally well established as that of its attacking the human frame but once; and not only destroys the case of Mr. Pringle, Ordnance Store-Keeper at Gibraltar, treated by Mr. Humphreys, and brought forward with such triumph by Dr. Burnett, but is the strongest proof in favour of the advantage derived by his family from secluding themselves from all intercourse with the garrison. Mr. Pringle is said to have adopted the most

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* At p. 160, and in other places, Dr. Pym endeavours to undervalue, and even to ridicule, what has been called "seasoning," by observing that, "in Europe no seasoning or length of residence was a defence against the Bulam Fever;" which it could not be expected in reason to be, where the cold of the winter completely annuls the effect of the heat of summer, especially as even this, except in the most southern parts (of Europe), is more moderate than the ordinary heat of the West Indies. Yet he admits, at p. 128, that during the epidemic prevalence of the Bulam Fever in the West Indian Islands, the inhabitants, "from being *acclimatés*," had the fever generally "in so much more mild a form, as rarely to be characterised as the same disease, and as, in all mild cases of it, would not have been suspected to be so, had it not at the same time prevailed epidemically."

rigid quarantine, but did not escape: but *I assert, that as he is an East Indian,* he was not liable to be attacked by the Bulam.* The *fever, therefore,* with which he was attacked, was *the Common Bilious Fever,* and *not the Bulam.*" Here we find the supposed *peculiarity* of the Bulam stated to be *no peculiarity;* we find it acknowledged to be no other than an effect produced with at least equal certainty, by one's having first drawn his breath in a warm climate, which is, however, entirely a misconception, because the immunity in question does not result from the circumstance of a man's having been born within the tropics, but from his having become habituated to the action or impression of a very warm atmosphere, and having thereby lost the morbid excitability created by residence in a cold climate, as well as the habit of generating a greater portion of animal heat than is suited to the high temperature between the

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* According to Dr. Pym, the having been born in the *East* Indies confers an exemption from the Bulam Fever, notwithstanding he contends, and endeavours to prove, that this fever never existed in that quarter of the globe: he has, in like manner, endeavoured to prove that the same fever did not exist in the *West* Indies "for a considerable time before 1793;" yet he informs us, that the "having been in the *West* Indies *before* 1793, was *no exemption* from its attack." p. 160.

To these, and to similar irreconcileable statements, alleged as arguments, in a discussion of the highest interest to humanity and to science, I request the serious attention of my readers.

tropics.—This, however, is not the whole of Dr. Pym's fallacious proposition. His principal motive for endowing the Bulam Fever with the peculiarity in question (if we are to except the desire to be thought the author of a new and important discovery), seems to have been that of promoting a belief of its possessing a *contagious* quality, like small-pox, measles, and some other diseases, by which mankind are attacked only once. He has, however, upon this occasion, greatly exceeded his bounds, by ascribing to the supposed contagion of the Bulam a degree of *sagacity* and *intelligence* which is not possessed by any contagion, nor even by the wisest of mankind; and by which, according to his account, it was enabled to discover the place of a man's nativity, although he was distant from it many thousands of miles; and, along with this sagacity, he has bestowed on the same contagion a *nationality* of disposition *existing in no other,* i. e. that of respecting and sparing an individual on account of his having been born in a particular country. I will not however enter into the digressions to which this imaginary and incredible property of the Bulam might lead (upon which Dr. Pym seems to have very little reflected), but shall submit to the impartial judgment of my readers that his "assertion" of it, completely invalidates all that he had before alleged, concerning the security supposed to be derived from an *attack* of the *disease.*

It has been already mentioned that the Spanish physicians entertained an opinion similar to that of Dr. Pym respecting the immunity supposed to be enjoyed by those who have recovered from an attack of the Yellow Fever, and that Sir James Fellowes attributed the first observation or discovery of it to them. We find, however, both by his, and their representations, that this immunity is as completely and *as certainly* obtained by *long residence* in a *warm* climate, as by an attack of this fever, and in fact that, according to their own statements, the *whole* amounts to *nothing more* than, either the long and well-known effect which is called seasoning, or that which results from the debility occasioned by many other diseases, as well as by this particular fever. Sir James Fellowes, at p. 198, after having mentioned that "no instance occurred of any person, who had passed through the disorder in 1800, having been attacked in 1804," adds, "the same remark was made of those who had been ill with it in South America, as well as of the natives of that country who had resided there many years, * *without ever having had the fever or any disease*

* Sir James Fellowes has subjoined, in a note to this passage, that "this was *not* the case in 1813," which affords a very decisive proof that "the peculiarity of its sparing the *natives of warm climates*" attributed to the Bulam by Dr. Pym is purely imaginary. It appears that among the natives of such climates who fell by the Epidemic of that year in Cadiz, was M. Mexia, one of the South American Deputies to the Cortes, and "celebrated for his eloquence and great talents." p. 290.

like it.". Here we see that *mere residence* in South America produced this *immunity*, without the co-operation of *this*, or of *any similar disease*. In the next paragraph he says, " the Americans from the United States were less fortunate; for a great many of them were severely attacked, and several died; some of whom are said to have had the fever in their own country." (See also pages 150, 191, note, 230, 240, and 346 of Dr. Arejula's Description, &c.* in which similar occurrences are related.) This is exactly what might be expected according to the principles stated in my Essay: the severity of the winters in most of the United States being very sufficient to restore all the vigour and excitability which could be necessary for an attack of the Ardent or Yellow Fever in Spain, even with those who had already undergone the disease in America; and the second attacks and deaths of such persons, as were mentioned by Sir James Fellowes, are additional proofs of the non-existence of Dr. Pym's alleged peculiarity.

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* It is to be observed that, even according to Dr. Arejula, the immunity here alluded to was not *complete*, but only relative. Thus he mentions, among the facts which the experience of the Epidemic had established as " medical axioms," that " those who *had been* attacked by the disorder in the West Indies or South America, or had resided a long time in that part of the world, either were not at all indisposed, although communicating freely with the sick, or if they had any illness, it was not a dangerous one, or it was so slight as not to confine them to bed." p. 238. See also p. 243.

But not to fatigue my readers with a redundance of proofs of second attacks, I will subjoin only one more, which, however, is derived from an highly respectable source, and extracted from an important communication recently made to the Army Medical Board by Mr. Ferguson, Inspector of Military Hospitals in the Windward Islands; which unquestionably proves that at least *one* individual, who had almost miraculously recovered from an attack of the Ardent Fever with *black vomiting*, was afterwards attacked *again*, and died with the *same* direful symptom.

"Another piece of doctrine (says Mr. Ferguson), has been promulgated from the writings of the authors above alluded to (Drs. Pym and Fellowes): that the Yellow Fever cannot be received by the same subject more than once. Of this we again, who live amongst Yellow Fever, not only know nothing, but we see it contradicted by the daily experience of our lives. We are aware that it is particularly a disease of the robust, the sanguineous, and the rigid in fibre: he who has escaped from one serious attack, is completely disqualified for a second in this climate, until he can find the means of restoring this inflammatory Diathesis, by a course of unnaturally high gross living, to which Englishmen are so prone; we do not deny that the susceptibility, towards a very acute *disease*, may be greatly lessened by the exhaustion of that

principle, through the attack of such a one as Yellow Fever; but all experienced officers, have seen *second* attacks under those circumstances, and have witnessed two, three, and four in the same person, when cut short by timely remedies, and not permitted to run their course; terminating ultimately in *black* vomit, hemorrages, and death." *Here,* in a note subjoined, Mr. Ferguson adds, "It is certain that if having had the West India Yellow Fever, secures an exemption from the Gibraltar Fever, this last gives no security in kind. Capt. Johnson of the Queen's regiment, *now here,* had the Gibraltar Fever in 1804, and he has just now recovered *with difficulty,* from a very alarming attack of the prevailing Epidemic." He farther observes, that in the small garrison of Barbadoes, "a man of the Queen's regiment died lately, of *aggravated Yellow Fever,* after having since his arrival here suffered a *well-marked* attack of the same. Another of the 25th regiment also died in the same way a short time ago, who had been treated by Dr. Jackson, for the Epidemic Fever, that prevailed in St. Kitt's in 1812, where his case was reckoned a remarkable one, on account of its severity; and various others of a similar kind have occurred in that corps. The *black* vomit being ordinarily a *mortal* symptom, cannot, of course, be often seen *twice* in the same subject; and I am well aware that the subtle disputant will here have a *subterfuge,* by denying that the first attack could have been Yellow Fever, the true Bulam: but again we have

proofs in the small island of Barbadoes, where the first patient that Dr. Caddell, a physician of the greatest experience, ever treated, had distinct black vomit (it made the strongest impression upon him), she recovered miraculously, and *died* some years afterwards of the *same disease*, and with the same symptom."

The effect of *frost* in producing a *sudden extinction* of the Yellow Fever, was noticed in my Essay, as proving that the disease must proceed from a cause existing in the *external open atmosphere*, or within or near the surface of the earth, and not within the chambers of the sick, which are artificially warmed, upon the occurrence of frost; and consequently that this fever is not the effect of personal contagion, which being generated by those who labour under the disease, cannot within their beds or bed-chambers be ever exposed to the action of frost. But Dr. Pym feeling the force of this proof, has attempted to evade or rather to convert it to his own use, by alleging as one of the *peculiarities* of the Bulam Fever, that "it *differs from all other diseases* in having its contagious powers increased by heat* and destroyed by cold, or even by a free circula-

* If this disease were really propagated by contagion, and if, as is here asserted, its contagious powers were increased by *heat*, it would become most prevalent, when the summer's heat is greatest. But this does not happen.—On the contrary it is notorious that at

tion of moderately cool air." An allegation for which he does not produce the smallest reason or authority; and which in his sense of it, is at variance with all the facts relating to this subject. And indeed, this effect of frost, in arresting the progress of the Yellow Fever, can admit of only one satisfactory explanation, that of its proceeding from the *same local* and *external* cause which produces the Remitting and Intermitting Fevers; which cease in the same manner, from the action of frost. That this should hinder the production in, and extrication from the earth, of *marsh*, or fever-exciting miasmata, or should cause a precipitation or re-absorption of them, when already extricated and suspended in the atmosphere, will be readily conceived and believed; and indeed, it is notorious that no other cause of disease is half so susceptible of being aggravated in *power* and *virulence* by the action of heat, and weakened or destroyed by cold, as these miasmata, which are absolutely harmless, at all seasons in the colder regions of the north, and are rendered inert by the winter, in more temperate climates. In truth, the effect of frost in stopping the progress of Yellow Fever, is exactly similar to its ope-

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Gibraltar, Cadiz and other parts of Spain, as well as at New York, Philadelphia, &c. the fever in question does not occur as an Epidemic until the autumn, when the greatest heat is passed; and for this obvious reason, that the previous existence of a high temperature forsome weeks at least, is necessary to prepare the miasmata, and give them the necessary force and maturity.

ration in putting an end to Remitting and Intermitting Fevers; and therefore, it affords the strongest ground to presume that they all proceed from one and the same cause; for it *is not* credible that frost would produce exactly the same effect in regard to each, if their causes were different. Dr. Currie of Philadelphia has endeavoured to obviate this conclusion, in his letter to Dr. Hosack, published in the American Medical and Philosophical Register, vol. I. p. 181 & seq. where, after admitting that the Yellow Fever, and the Intermittents and Bilious or Remittents do *agree*, in being "destroyed or disarmed of their noxious power by the frost," he adds, "this, however, only proves that the *contagious* principle, which is the cause of the Yellow Fever, requires the same portion of caloric to preserve its volatility, and keep it suspended in the atmosphere, as the miasmata of marshes, or the exhalations of putrefying substances." This however is but a laboured and unsatisfactory explanation.—Matters so very dissimilar as marsh miasmata, and personal contagion, are not likely to be acted upon exactly in the same manner by any one cause, nor to require precisely the same portion of caloric to preserve their volatility; and moreover the circumstances in which they *naturally exist*, are so very different, that the temperature applied to one, in the external atmosphere or in the uncovered earth, cannot be similar to that of the other in the beds, or bed-chambers of the sick. If Yellow Fever

were produced by personal contagion, those who approached the beds of the sick, where it is generated and *accumulated*, and where there is *no frost*, would unquestionably be liable to take the disease, without the smallest protection from the *external frost*. But it is found by experience, that those who nurse or visit patients labouring under this fever, are never attacked by it, *subsequently* to the occurrence of frost. And this fact is easily explained by referring the disease to miasmata, existing in the earth, or extricated from it, and suspended in the open air; but it must be quite inexplicable if we assume personal contagion to be the cause of the disease; for as this is constantly generated in and emitted from the bodies of the sick, it cannot in their beds and bed-chambers be acted upon by frost.

The extinction of the plague, by the high temperature which prevails between the tropics, has by some been thought analogous to the cessation of Yellow Fever in consequence of frost; but in my judgment these effects cannot be assimilated with or compared to each other. The temperature which destroys the contagion of the plague, naturally prevails within as well as without the apartments of the sick; whilst cold, which stops the progress of Yellow Fever, is excluded from their apartments, and can have no action upon any supposable cause of the disease which does not *naturally* exist *without doors*.

There are two other peculiarities of the Bulam Fever, which, although not included with the preceding in Dr. Pym's general descriptions of it at pages 5 and 6, and between pages 228 and 234, have been annexed to it in the progress of his work; they relate to Relapses, and to the "Sequelæ" of the disease, and a very brief notice of them will suffice.

In regard to the former, he says, at p. 225, "I cannot call to my recollection one relapse in the Bulam Fever." But in opposition to this my readers have already seen that the following Medical Officers declare in their official Reports to have witnessed relapses, viz. Drs. Gray, Dickson, and Denmark, and Mr. Tobin of the Navy, and Messrs. Lea, Weld, Amiel, and Martindale, of the garrison of Gibraltar. In addition to these testimonies I am able to quote those of Dr. M'Arthur who in his Report (No. 7,) says, "A relapse in the Bulam is by no means uncommon. I experienced it myself, and patients have relapsed under my care, who died with black vomit:" of Surgeon Sproule, who "had two cases of relapse;" of Surgeon Short, acting with the 60th regiment, who observed "one case of relapse," of Assistant Surgeon Humphreys, Royal Artillery, and Hospital Assistant Thomson, who likewise witnessed each one case of relapse, and of Hospital Assistant Sutherland, who "attended one or two persons with fever, who had just been

ten days or a fortnight recovered from a previous attack." I will here only add that Sir James Fellowes declares, at page 63, (apparently on the authority of Dr. Arejula), that "relapses were very frequent and fatal."

The last of Dr. Pym's peculiarities, relative to the sequelæ of the Bulam, requires notice, not from any intrinsic value which it possesses, but because he insists much upon it, and this might lead some persons, who have not seen the disease, to suppose that his statements in that respect were correct, if they were allowed to pass without observation. In distinguishing the Bulam from the "Bilious Remitting Fever," he represents the latter (at p. 61), as being "almost always combined with affection of the *liver*, patients after recovery from the Remitting Fever being very generally subject to obstruction or disease of *that* viscus." This, however, is very contrary to my experience, and seems to me to have no other foundation than the popular, and as I conceive erroneous, idea (noticed at p. 21 of my Essay, and also at p. 2 of this work), that in the fevers of warm climates the secretion of bile is increased, and that the liver must consequently be morbidly affected. It does certainly appear that the fevers which are caused by miasmata are, cæteris paribus, naturally disposed to become more protracted in proportion to their mildness, and that, according to the length of time which they are suffered to *continue*, will be the pro-

bability of their producing obstructions or other morbid derangements of the abdominal viscera. These, however, in almost all instances appear to be slow in their growth, if the patient was clear of visceral obstructions previously to his seizure. But where miasmata have sufficient concentration or virulence to produce a Yellow Fever, they generally act upon the human body almost immediately, and the acute attack and rapid course and termination of such a fever leave no time for the production of the peculiar and sluggish derangements of the viscera, just mentioned, especially in persons of vigorous habits, not liable to chronical affections, who alone are the ordinary subjects of Yellow Fever. According to my experience in Europe and the *West* Indies (for of the diseases of the *East* Indies I have no personal knowledge,) dyspeptic affections, and diarrhæa or dysentery are the *more common* sequel of repeated attacks of *remittent* fevers; and these with diseases of the *spleen* are frequent consequences of repeated and protracted *intermittents:* dropsies, likewise, are sometimes a consequence of either of these species of fever; but compared with the above, I have found chronic affections of the liver to be rare. If Dr. Pym means, by what he says at p. 264 in opposition to Dr. Burnett, that those who recover from the Bulam never suffer any morbid consequences, he is at direct variance with the declarations of Drs. Arejula and Riseuno in this respect, the former of whom states at (p. 216,) that ascites or general

dropsy, and sometimes, but less frequently, hectic fever, and pulmonary consumption *supervened as effects of the disease;* and the latter, that "in *many* instances the disease terminates either in ascites or some other species of dropsy." See p. 234 of Dr. Burnett's Observations.

I fear that many of my readers will think that I have committed a waste of facts and authorities to disprove allegations which were unworthy of even half the notice here bestowed upon them; but the confidence with which they have been advanced, the importance that has been attached to them, their plausibility, and the strong impression which they appear to have made on the minds of some persons high in political, or in medical rank, have rendered me anxious to dispel the errors to which they were conducive, and spare no pains to annihilate a chimera pregnant with mischief.

CHAP. III.

IN the fourth part of my Essay (p. 291 to 496), I have adduced, as concisely as possible, a great, and (as I had even apprehended), in some degree, superfluous mass of evidence, decisively proving the fever in question to be void of contagion. That this evidence was deemed incontrovertible by Sir James Fellowes and Dr. Pym, I have a right to conclude, because they have evaded nearly the whole of it. The latter, indeed, endeavours to persuade his readers that he has done otherwise; and his pretensions in this respect impose on me the duty of noticing the arguments and means with which he has aimed to overthrow my observations, and the facts by which they are supported. The reader will, I trust, feel that if these had been left unnoticed (as they would have been, if weightier considerations than personal ones had not governed me), Dr. Pym would think he had a pretext for holding them up as unanswerable, and others might thence be led to suppose that they were so; and, under that impression, to bestow a degree of credit on his statements, of which, upon even a slight examination, they will be found unworthy. I therefore entreat the

reader's patience, and trust that he will be induced to accompany me through the brief examination of them, which I propose to make in the first instance, and without which, the cause of truth in this important inquiry would materially suffer.

Dr. Pym, after imputing to me a contradiction which has no existence, but in an unfair representation, says, (p. 164) "I shall proceed to examine Dr. Bancroft's authorities in support of this disease being non-contagious. Among the first I may reckon Dr. Nooth."—It is true that, in my account of the Epidemic at Gibraltar in 1804, I represented Dr. Nooth as having believed that it originated from a cause similar to that by which miasmatic fevers are produced; and I did this because I had been well informed that he had ascribed it to *local* causes, and declared his conviction that it was not contagious. But it now appears that I was mistaken in supposing (although the supposition was obvious and natural) that Dr. Nooth considered what have been commonly, though improperly, called Marsh Miasmata, as the principal of those *local* causes. Notwithstanding this mistake, however, I made none in regard to the object for which his name was introduced, although Dr. Pym has endeavoured to lead his readers to an opposite conclusion, by referring them to an official letter (at his page 37), addressed by Dr. Nooth to the Surgeon-Gene-

ral, dated 10th of October, 1804, in which, after describing the "excessive fatigue" he had undergone, particularly "by being obliged to attend at General Barnett's head-quarters," he says, "I very soon contracted the fever in that focus of contagion." Here, as is manifested by the whole tenour of Dr. Nooth's correspondence concerning the Epidemic, he has used the term *contagion*, not in the strict sense to which I, as well as many others, have lately confined it, i. e. as signifying a matter generated in a living diseased body, which is capable of reproducing in other bodies a disease specifically similar, but in the sense now often allotted to the term *infection*, as meaning a morbific matter not produced by the living body; and in the same way he has used the term infection to signify what we mean by contagion. Thus in the *same* letter he adds these words, "the disease by no means seems to be *infectious*, but the whole atmosphere on the Rock is *pestiferous*; and in another letter to the Surgeon-General, written nearly four months afterwards (on the 2d of February, 1805), alluding to the clamours excited by the advocates for contagion, he says, "for my own part, I am well convinced that the Epidemic Fever that lately raged amongst us, was the consequence of the extreme heat, drought, and calms, that prevailed from the middle of April to the end of October; and as to the *infectious* nature of the disease,

it is by no means so clear a circumstance as the public are apt to imagine: I have, I must confess, still my doubts upon the subject, and cannot be convinced, by what I have seen, that the disease is *personally infectious.*" Yet, clear as the import of these words must appear, Dr. Pym permits himself (at pages 43 and and 180) completely to pervert it into "an acknowledgment of change of opinion;" and availing himself of the loose, and perhaps unguarded, expression about having "contracted his fever in a focus of contagion" (used by Dr. Nooth at a time when yet so ill that he was "but just able to hold his pen"), he cites it as conclusive evidence of the writer's having admitted the disorder to be personally contagious. That Dr. Pym was quite sensible of the nature of the expedient here resorted to, and that he even suspected his readers would be equally sensible of it also, is obvious, from his confessing "that it would be reckoned *unfair* in a criminal cause;" although "he trusts that it will not be reckoned so in a *medical* one." On the delicate compliment thus conveyed to the understanding of his readers, I need not remark; but, having such an intimation of the sort of candour and good faith with which he has entered into the discussion of questions of the highest medical importance to mankind, they may perhaps after this, hesitate at surrendering their belief on controverted points into his hands. I will only

add, that I have good authority for asserting that Dr. Nooth has always retained, and does *still retain*,* *as at first*, his belief that the Epidemic under consideration was void of contagion.

Two other authorities of mine, Drs. Rush and Pascalis, are disposed of by Dr. Pym in a way about as fair as the preceding. He quotes passages from these authors (see his pages 179, 206, 208), which had been written at an early period, when they confess that they had but an insufficient acquaintance with facts connected with this question; and although both have since thought very differently, yet he adduces, as fit evidence to lay before his readers, their crude and discarded opinions, in preference to those which were afterwards delivered by them as the result of greater knowledge and more just reflection, and thereby flatters himself with having annulled their testimonies. Dr. Jackson he has also treated in the same manner, at pp. 145, 147.

Dr. Lempriere is likewise mentioned by Dr. Pym

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* Dr. Dickson, in his late Report concerning the Bulam Fever, makes the following communication to the Transport Board, viz. "I have lately had an opportunity of knowing that Dr. Nooth retains his original opinion of the disease" (at Gibraltar), "and of the existence of sufficient *local* causes, to account for its appearance, and particularly the previous state of the weather in 1804."

(p. 165) as one of my authorities; and indeed I did, at p. 349 and 350 of my Essay, introduce some quotations from his observations on the diseases of the army in Jamaica, respecting the Yellow Fever which had prevailed with more than ordinary violence at that island in 1793, 1794, and 1795, and which for that reason was by many believed to have been imported from Grenada. In the passages there quoted, Dr. Lempriere delivers a different opinion, declaring that *it did not act or spread like a contagious disease*, and he positively asserts that it had *no contagious quality;* ascribing its production to the excessive rains in the month of May, and the subsequent extraordinary heat in June, July, and August. *All this, however, Dr. Pym evades;* but he selects a passage, which I had noticed, although without either approbation or disapprobation, respecting the suspected existence of a sort of *hybrid* fever, produced by the combined operation of typhous contagion, and of marsh miasmata, and of which Dr. Lempriere was inclined to doubt "whether it be not of an infectious tendency;" and this passage he connects with some others in Dr. Lempriere's work, so as, by referring to an opinion long since discarded by Dr. Jackson, to find a pretence for asserting that this Hybrid Fever was no other than the Bulam. And thus he conceives himself to have gained over this witness.

Dr. Pym, at p. 173, says, "Dr. Bancroft's next

authority for non-contagion is M. Gilbert," Physician-in-Chief of the French Army under General Le Clerc; and, after quoting what I stated of this writer's ascribing the fever at St. Domingo to a heated atmosphere, loaded with marsh effluvia, he adds, " M. Gilbert certainly does say so; but he only mentions this as the *general opinion*, and is decidedly of a contrary opinion himself! and if Dr. Bancroft had not been much more laconic than usual, or if he had been anxious to give M. Gilbert's own opinion, he would have followed up his quotation from page 94, respecting the non-contagion of this disease, by giving his own words, which are—'*La fièvre jaune n'est pas contagieuse: cette opinion est celle de la généralité des praticiens.*'——' Cependant on ne peut se dissimuler qu'une maladie aussi grave, et d'un caractère putride et gangréneux, ne puisse se porter par communication de l'air respiré, ou par le contact des effets imprégnés de ces miasmes, sur les hommes qui, par état, ou par dévouement, s'exposent à tous les instans du jour à l'action des causes qui la font naître, et qui l'entretiennent. Tel est le sort des officiers de Santé, des employés dans les Hôpitaux,' &c."

That there is no foundation whatever for this very serious charge will be manifest, 1st. because I had taken care, in a long note subjoined to the same page, 343, to state *most fully and clearly* the purport of all that I am

charged by Dr. Pym with having *omitted*, the first part of the note being in the following words, viz. " 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;* but these causes (miasmata) must be sufficient *alone*, and the emanations from the sick must therefore be superfluous," &c.

2d. Because, although the meaning of the passage quoted by Dr. Pym be not clearly expressed, there is nothing that I can perceive to warrant the assertion that M. Gilbert did not deliver *his own* as well as the *general* opinion, when he declared the Yellow Fever not to be contagious. Nor does that quotation contain any thing repugnant to this declaration, so far as relates to the *original* nature of the fever: it only indicates that those who entertained it had not divested themselves of an erroneous notion, still held by many persons even in this country (as will shortly be noticed), that diseases not originally or naturally contagious might acquire a contagious quality by crowding, putridity, &c. In fact, the passages which have been quoted in my Essay, or by Dr. Pym, from the

work of M. Gilbert, are, strictly speaking, not his sentiments *singly*, but (what I considered to be of much greater value), those of a general meeting of "all the Army Medical Officers of the First Class residing at the Cape (François), and the neighbouring Posts, united with the Practitioners of the Town and the Country around," who were summoned by the General-in-Chief, as M. Gilbert states, p. 63, "to assemble, and consult together, communicating reciprocally the results of their experience, in order to fix upon some general plan of treatment," &c. This meeting, it appears, discussed various propositions concerning the symptoms, causes, &c. of the fever in question, in the order given at p. 64, and having come to a decision upon them, M. Gilbert, as he states, "drew up the report of their sentiments in consequence," (which report he gives from p. 65 to 102) observing at the same time that "the various opinions there expressed, when balanced, differed *but very slightly* from each other, and rather in regard to the sick individual than to *the character of the disorder*;" a statement which surely cannot authorize even the suspicion that M. Gilbert had himself dissented from any one of the opinions about the causes of the fever, as delivered in the General Report which he composed. By referring moreover to the preceding parts of his work (which are altogether his own), especially at pages 11, 26, and 51, it will be found that he repeatedly mentions swampy grounds as abounding in St. Domingo, and occasioning

the worst fevers; and we can therefore have no difficulty in believing that he concurred entirely in the declaration made by the officers assembled as above, viz. that "the Yellow Fever is only the highest form of Bilious Remittents ('il y a tout lieu de croire que la fièvre jaune n'est autre chose que le maximum des fièvres rémittentes bilieuses')," and also in "the opinion of the generality of practitioners," that it was not contagious, remittents being now acknowledged, and even by Dr. Pym, to be devoid of contagion.

At page 174, Dr. Pym mentions as my "*next authority*," Dr. Hillary: and after reprinting my quotation from his work at pages 331, 332, he adds, "this is one of Dr. Bancroft's strongest proofs of non-contagion; but he has cut Dr. Hillary *short* in the same way that he did M. Gilbert; for if he had lengthened his extract, by including the sentence immediately *preceding it*, he would have done more justice to the question: for Dr. Hillary, his highest authority, decides against him most positively." He then adds the following quotation from Dr. Hillary's volume: "This fever *very rarely or never* is infectious, or *contagious to others;* not *even to those who attend the sick*, except a *chance* time when in its most *putrid* or *malignant* state, at the latter end of the disease, or soon after the *death of the patient*, when *the season is very hot*, and this fever is accompanied with *symptoms* of *some other malignant fever* which is then *epidemical*, or *con-*

tagious, as happened once at Antigua, and once or twice in this island; and the same may probably have happened in some other places."

In this quotation all that I can discover is, that Dr. Hillary had *heard* of a fever as having formerly prevailed epidemically in one or two West Indian islands, such as that called *Kendal's Fever* at Barbadoes, which was by some erroneously supposed to have been highly malignant and contagious, because many persons were attacked and a considerable number died of it; and that, being well acquainted with the doctrines then in vogue, connecting putridity in living as well as in dead bodies with contagion, and being about to declare *unconditionally* that the Yellow Fever possessed no contagious quality, a sentiment of *caution* led him to premise the *vague exceptions* just mentioned, lest he should be charged with absolutely denying the possibility of its acquiring a contagious property by the aid of *putrefaction,* especially when accompanied with the symptoms of *some other malignant fever then epidemical or contagious.* But as all this proceeded from a respect for the prevalent notions of the time, which had no *solid foundation;* and as I did not mean to state his opinion of what, according to those notions, might be merely *possible,* but to inform my readers of the *fair result of his personal experience and observation,* I did not extend and encumber my work by introducing the visionary and useless matter which Dr. Pym blames me for omitting; but chose

rather to content myself with this important declaration, which contains the strongest testimony that Dr. Hillary or any man could give *from his own knowledge* in regard to the non-existence of a contagious quality in the Ardent Fever, 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 house when they had the fever upon them, before they came in. From whence we may conclude that *it has nothing of a contagious or pestilential nature in it,* and that *it is a very different fever in all respects, as it will more fully appear hereafter.*"

To set aside the pretence advanced by Dr. Pym that the Yellow Fever described by Dr. Hillary was not the Bulam, I need only refer to the very accurate description which he has given of this disease with all its worst symptoms, including that of black vomiting, " the fatal symptom peculiar to the Bulam Fever."

My next authority is thus introduced at p. 176, " Dr. Bancroft quotes Dr. John Hunter's opinion against the contagion of this disease, although from his own words he decidedly treats of the fever in its remitting form, and not in its concentrated continued form, as it has been described by Dr. Moseley, Dr. Jackson, and others." This

is one of the many attempts made by Dr. Pym to evade the testimony which is unfavourable to his views, by his distinction between the ordinary Yellow Fever of the West Indies, and that called by him Bulam, although I have shown by the best testimonies that the Epidemics in Spain, which he admits to be the same with the latter, did frequently remit. Dr. Hunter, as Physician to the Army in Jamaica, had the care of the military hospitals there from the beginning of 1781 to May 1783: and the sources from which he derived his experience "must be allowed (as he says) to have been of the most ample nature" (p. 82,); and as it appears that "in less than four years (to the date last-mentioned) there died in the island 3500 soldiers," (p. 58,) of whom a great proportion were destroyed by fevers, it is probable that few physicians have had greater opportunities of observing the Ardent Fever in its violent forms. He states expressly, (pages 64, 72, 134,) that he uses the term of Remittent Fever as synonymous with that of Yellow Fever, and of the Vomito Prieto of the Spaniards: he particularly notices the black vomit as a frequent symptom (64); and his general description of the course of this fever is not, in my opinion, surpassed for accuracy, science, and utility, by any with which I am acquainted. I therefore rest assured that no impartial and well-informed person will be disposed to call in question the validity of Dr. Hunter's testimony in regard to the non-contagious quality of the fever under consideration.

Finally, Dr. Gillespie, Physician to the Navy, whom I had cited, but without the least reference to contagion, is unjustly animadverted upon among my authorities on this point, and a passage from his publication is quoted by Dr. Pym (p. 178), wherein mention is made of a fever which was supposed to have spread by contagion on board of an armed sloop, but which, as there is good reason to believe, originated from a very different source; I mean the foul state of the ship's hold, which has in a multitude of other vessels produced similar effects, and created the appearance and the belief of contagion, where none existed, as will be particularly noticed in the next chapter. But I am now able to adduce Dr. Gillespie's *most matured* and *decided* testimony against contagion in the Bulam, and shall do so in another part of this chapter, where it will appear with more propriety.

Here Dr. Pym closes the account of what he terms my authorities; and he manifestly intends to have it believed that he has noticed and refuted *all* those which I had adduced; when in fact he has completely shunned even the mentioning of nine-tenths of them. The few whom he notices were obviously selected only because they had said or admitted something capable of being distorted contrary to the real and sincere meaning of each; and in effecting this distortion he exults, as "having by cross-questioning my witnesses, brought out *the truth*," and "convicted me upon

my own evidence :" although in regard to the great body of those who are more properly my witnesses, he is so far from having cross-examined them, that he has not even looked them in the face; and my readers, I firmly believe, will be convinced that he has not been able to invalidate or weaken any *one* testimony or opinion which I had alleged to prove the fever in question to be void of contagion.

From the *authorities*, Dr. Pym proceeds (at p. 181), to " make a few remarks upon the *arguments*, which Dr. Bancroft uses, with the view of getting rid of the importation of the disease into Philadelphia, Cadiz, and Gibraltar; and of *imposing* upon the understanding and dazzling the senses of his readers, particularly those who had not opportunities of gaining information, or perusing original histories of the disease, as it appeared in those places." He then notices my account of the situation of Philadelphia, and adds that, " if the Epidemic derived its origin from heat and moisture, it must appear rather singular that it did not occur oftener than three times in the course of a century; and it must appear still more strange that, when the disease made its appearance in 1793, the Physicians as well as the inhabitants, without inquiry or consideration, should trace its origin to the Sans Culotte, and the two vessels from Hispaniola, as they did in 1707 to the Arethusa from the Havannah, and that too without

any disagreement as to the particular vessels in which, or the island from which, it had been imported." All this, however, is very far from being correct. Those persons at Philadelphia who supposed the fever to be of *foreign* origin concluded, of course, that it had been imported, and they consequently sought out the vessels on which the importation could be charged most plausibly: and a French privateer with a prize ship from Europe, and two small vessels said to have arrived from some port in Hispaniola, were fixed upon by a number of persons as the importers. Thus they had recourse to *four* vessels as having done that for which only *one was required;* but as there was no reason to charge one with the michief more than another (nor in fact, to charge any one with it), all were comprehended in the charge; although without any proof or probability that the Bulam Fever, which is supposed to have first manifested itself in the spring of 1793 at Grenada, had been thence introduced into any place from which either of these four vessels sailed, so early as they must have done to reach Philadelphia and *produce an epidemic in that city* in the month of July, 1793. There was, moreover, no such unanimity as Dr. Pym represents on this subject; and even Dr. Rush, although he did at that time adopt the opinion of the fever's being contagious ("being misled by Dr. Lining and several West India writers," as he says) was so little satisfied with the accounts of its pretended importation, (which were after-

wards refuted by Dr. Caldwell of Philadelphia) that he ascribed the disease to a cargo of *putrefying coffee*, as I have formerly mentioned: and at this time, as I am persuaded, none of those to whose minds truth is accessible give any credit to those accounts. In regard to the College of Physicians in that city, who are represented as having "ascertained the source of the infection," it is well known that, if a majority of that body *in point of numbers* believed in contagion, there was a majority in reputation who adopted the opposite opinion, and, separating themselves, formed the Academy of Medicine there. But I shall very shortly prove that the accounts of the pretended importations of Yellow Fever are all destitute of foundation, so far at least as they are made to depend on contagion.

I will not follow Dr. Pym in his irregular progress four or five times across the Atlantic; but, being in the United States, will proceed directly to Charleston, where we find him (at p. 187), quoting part of a letter from Dr. Lining to Dr. Whytt in the Edinburgh Medical and Physical Essays, vol. 2. p. 373: he omits, however, as not suiting his purpose, the most important part of that letter, in which Dr. Lining states that "if any persons from the country received the disease in town, and sickened on their return home, *the infection spread no farther, not even so much as to one in the same house:*" which is the

more remarkable, because at p. 416 of my Essay, (to which, instead of p. 470, he *ought* to have referred, Dr. Lining's account being *there* particularly mentioned) I had noticed *the omission of this very passage* by another advocate for contagion, (who had filled two of his pages with *other* quotations from Dr. Lining), as by no means a fair proceeding. As Dr. Pym could find no support from any other medical man *living* or *dead* in South Carolina, they having for a considerable number of years been unanimous in rejecting all belief of contagion in the Ardent Fever, (though since 1792 it has appeared very often epidemically at Charleston) he retires from this town, without making the least allusion to any one of the decisive proofs and authorities which I had there found and produced.

From Charleston I had proceeded northward through all the United States as far as Boston, every where collecting and examining facts; but all these, as they were most adverse to his opinions, he has passed over in silence, with the exception of three particular matters, upon which he looks back from Gibraltar; and which, says he, "Dr. Bancroft considers as his most decisive evidence in favour of non-contagion." p. 193. The first of these relates to the sale which, as I had stated at p. 739, (extracted from p. 92 of Dr. Valentin's Traité de la Fièvre jaune) was made of all the bedding, blankets, clothes, &c. used by the patients in the hospital at Philadelphia, which had

been exclusively appropriated to the reception of patients ill of the Yellow Fever, during its prevalence there in 1798, and which were bought by the agents of the French government, and employed for their sick when they had not been even aired, and this without any communication of the disease from them. This fact Dr. Pym endeavours to explain by saying that, as "the disease had ceased at the approach of winter, before the bedding in question was sold, there can surely be nothing very singular in this contagion or disease not attacking Frenchmen, after having ceased its ravages upon the Americans, or, in fine, after its existence was at an end." Dr. Pym, however, should have recollected that he has himself adduced the opinion of Mr. Vance (at p. 64), that the contagion of the Epidemic of 1810 at Carthagena had "remained dormant over the winter," to revive and produce that which he observed there during the next summer; since which Sir James Fellowes has ascribed to a similar dormancy and revivication the Epidemic which recurred with such violence at Malaga in 1804, and for the production of which he could not, even after personal inquiries, find any other cause: and from all that I have been able to learn, the bedding in question was not used during the winter, but sent partly to the West Indies, and partly distributed in the French fleet under Admiral Vanstable, which sailed from Virginia in the following spring, and arrived at Brest some time in June, when there could be no *cold* to im-

pede the operation of the supposed contagion. A similar occurrence has been related by Dr. Burnett on the most authentic information at p. 273 of his work. After mentioning the cessation of the Epidemic of 1804 at Carthagena in January, 1805, "the last patient having been received into the Royal Hospital on the 23d of that month," he adds, "on the fifth of February following a French frigate, having on board the wounded of the Arrow sloop, which she had captured a few days before, after a gallant resistance, arrived in the harbour; the wounded were immediately landed, and placed in the same beds and bedding, in which several patients, who had died of the prevailing fever, had lain, without the beds or bedding having been either washed or aired; yet not one of these men was attacked with the disease in consequence." This important fact is opposed by Dr. Pym (at p. 261), with merely a repetition of the "same argument" as in the preceding case. We know, however, certainly, upon the highly respectable authority of Dr. Stewart of Grenada, which even Dr. Chisholm must admit, that in that very island the bedding and clothing of those who in 1793 took the Bulam Fever in the town of St. George, and afterwards sickened and even died in the country did not communicate fever* to any person whatsoever; and

* Dr. Stewart, who conceived the fever in question to be contagious, says, with great candour, in his letter to Dr. Hosack, "but I must observe that I met with no instance of the disease being

this was also the fact in regard to the same fever at Dominica, as Dr. Clark has attested: and as Dr. Pym assures us (p. 8), that "the contagious powers of this disease are increased by heat," the contagion ought in these islands at least to have manifested itself. It has indeed been stated by Dr. Stewart, that, "in the *fatal* cases, the bedding and clothing were *commonly* destroyed; but as this was never done till after the patient's death, sufficient time must have intervened, even in these cases, for a previous communication of the disease, if contagion had existed. A like testimony of the non-communication of the disease by the clothes, &c. of those who were ill of the fever, acknowledged by Sir Gilbert Blane, Sir James Fellowes, &c. to have been the Bulam, during its prevalence in Leghorn in 1804, is given by Dr. Palloni in the second volume of the Edinburgh Medical and Surgical Journal: and similar testimonies may be found in every part of the United States of America in which the disorder in question has prevailed, and also in Spain, as will soon appear from the declarations of Dr. Arejula, &c.

Dr. Pym, aware, doubtless, of the weakness of his ex-

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communicated to others, either visitors or attendants," by those who were seized with this fever, "after their return to the country." See his letter to Dr. Hosack, at p. 188, vol. III. of the American Medical and Philosophical Register.

planation in regard to the bedding, &c. of the Philadelphia Hospital, expatiates on the inhumanity of thus selling "such contaminated *rags*, however *mild* the disease might have been;" and then, without saying how he discovered that the articles sold were *rags*, or that the disease *in that hospital* had been *mild*, he recurs to a gratuitous "*supposition*," (p. 197) "that in the *heat* of argument against contagion, Mr. Deveze, the Agent, had mentioned the circumstance to Dr. Valentin as *possible*, if put to the test," and this he chooses to adopt rather than believe that the transaction was real. Of such reasoning I need not take farther notice.

The second of my "evidences" or arguments cited by Dr. Pym is stated at pages 423, 424, and 425, of my Essay; it respects the universal exemption of the *physicians* at New York, during the Epidemic of 1805, by avoiding the spots infested with miasmata, and also the non-communication of the disease, either by the dissections of the bodies of persons who had died of the Yellow Fever, in which these physicians were frequently employed, or by the many experiments made in that city and at Philadelphia with the matter of black vomit, applied repeatedly in the way of inoculation, as well as taken into the human stomach. Concerning these facts, he gives an imperfect quotation, and then conceives that he has explained and controverted the whole, by *sup-*

posing that all the physicians and others who had thus exposed themselves with impunity, must previously have had the disease, adding, " if it had been known to these gentlemen that, having had it once, they were not liable to it a second time, they might have saved themselves the trouble of avoiding infested spots," &c. This, however, is a species of *knowledge* which never has been acquired, especially in the United States of America, where the winter's cold precludes the effect even of seasoning, and where experience has shewn, in multiplied instances, that individuals have suffered second and third attacks of the fever.

The last of my " evidences," as selected by Dr. Pym, is that which results from the well-known fact " of the Ardent Fever's 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 attacked by the disease after being removed beyond the *reach of marsh miasmata;* though, in many of these, the fever appeared in its worst forms, and proved mortal" (see p. 413 of my Essay). Against *all the proofs* which I had alleged *of this fact*, Dr. Pym vainly opposes, and *can only oppose*, the *pretended* importations of the Bulam " at Philadelphia, Cadiz, and Gibraltar," none of which ever had any real existence. This will be demonstrated in the proper places, as far

as it is possible to extend *negative* evidence; and much farther than the weak, equivocal, and often absurd and incredible stories, alleged as *affirmative* proof on these points, can require or deserve.

I have now examined all that in any way merited notice of what Dr. Pym has advanced against my authorities and arguments, with the exception of some circumstances relative to Cadiz and Gibraltar, which are reserved for future consideration; and I cannot but believe that my readers will have been convinced of the fallacy of those principles upon which he has endeavoured to explain, or rather to evade, my inferences, and of the abortiveness of his endeavours to invalidate, in a single instance, either my testimonies or my reasonings. There remains besides a great mass of evidence, of which he has studiously avoided even the smallest notice; and this must of course be considered not only as subsisting in full strength, but as having been deemed by him unquestionable and invulnerable: for otherwise, with his dispositions, and the latitude of every kind in which he has indulged, it may be presumed that it would not have been left without some hostile attempt. To this evidence, therefore, I refer my readers with confidence; and more especially to the very accurate and respectable one of Dr. James Clark, at pages 332, 333, 334, and 760, 761, of my Essay; that of Mr. Young, Inspector-General of

Hospitals, and of all the Superior Medical Officers of the Army under Sir Ralph Abercromby in the Windward Islands, p. 334, 335; those of M. M.* Desportes and Valentin at St. Domingo, p. 338—341; that of Dr. Hector Mac Lean, with the opinions of Drs. Jackson, Scott, Wright, and Gordon, and nearly, if not all, the other Medical Officers of the British Army in St.

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* Dr. Pym, at p. 110, says, that M. Desportes, "although he had *doubts* as to the importation of the Yellow Fever, was aware of its not being a *constant resident* in St. Domingo." This his representation, however, is incorrect, *as usual*, M. Desportes having had no doubt, that I can discover, on the subject. He was fully convinced, from the observations of fourteen years at Cape François and Fort Dauphin, that the prevalence or absence of the Yellow Fever at those places depended upon the *changes* of season in regard to heat and moisture. He certainly was aware that it was not a *constant resident*, at least, as an Epidemic, because he mentions five out of the fourteen years as having been distinguished by such *a mild* or *moderate temperature*, that this fever only appeared *sporadically*. Dr. Pym, in many parts of his work, attaches importance to the circumstance of the Yellow Fever's not being a *constant* resident in the West Indies, &c. and considers this as a proof of its contagion and importation, although it *merely* indicates that such a concourse of high temperature and of local causes, as is necessary to aggravate the common Endemic to an Epidemic violence, does not happen *every year*. With his doctrines it is incumbent on him to point out some spot where this fever is a *constant resident*, and whence it is occasionally exported to other countries; this, however, it will be utterly impossible for him to do. Certainly there is none such on the western coast of Africa, where no contagious fever ever existed, unless carried thither in a few instances; and in these it soon ceased.

Domingo, p. 341, 342; that of Dr. Hume, p. 346, 347; those of Dr. Walker and Dr. Grant of Jamaica, p. 350, 351; that of Dr. Ramsay, and of all the Medical Practitioners of the State of South Carolina, declared unanimously at a General Meeting in Charleston, p. 355—359; that of Dr. De Rosset of Wilmington, in North Carolina, p. 359; the opinions of Drs. Valentin, Taylor, Hansford, Selden, and Whitehead, in Virginia, p. 360, 362; that of Dr. Davidge at Baltimore, p. 363, 366; that of Dr. Vaughan, in the State of Delaware, p. 367—369; the opinions of many physicians at Philadelphia, between pages 372 and 386; and at New York, p. 387—399; and those of Dr. Coit, of New London, Dr. Wheaton, of Providence, and Drs. Warren and Brown, of Boston, p. 401—406. I request also the attention of my readers to the facts partly stated, and partly recapitulated between pages 406 and 430; and, finally, to the very important official Message from the President of the United States on this subject to the Two Houses of Congress, p. 430, containing such an uncontradicted and incontrovertible statement of facts, as ought, in every unprejudiced mind, to remove every suspicion of the existence of contagion in the Yellow Fever, at least, in that part of the world.

But, although I deem the mass of evidence to which I have just referred, as more than sufficient to overthrow

Dr. Pym's superstructure, especially as the foundation of it has been removed in the first chapter of the present publication, I shall proceed to extract and adduce, from an abundant store, such a multitude of *additional* facts and authorities as must, I think, satisfy every mind which is not so invincibly biassed, as to be completely inaccessible to the truth, and incapable of any correction or change of opinion. That such minds exist is, I fear, a lamentable truth.

Dr. Chisholm had asserted, that the Ardent or Yellow Fever, as it occurred in the West Indies, had been destitute of any contagious property, until the supposed importation of a *new* fever, the Bulam of Dr. Pym, at Grenada, by the ship Hankey, from the coast of Africa, in the year 1793: and the latter, although he has found it expedient to admit that a fever similar to this Bulam had formerly occurred in the West Indies, contends that it had been extinct for a considerable number of years, previously to its revival by the Hankey, and has taken pains to collect and produce evidence to prove this from Mr. Weir, late Director-General of the Army Medical Department, and others. This fable has, I conceive, been sufficiently refuted in the Seventh Appendix to my Essay; but as it has been made a main support of Dr. Pym's doctrines, it is proper here to mention, that a fever exactly similar to that which has since been called

Bulam, did prevail to a considerable extent at New York (as is admitted by the partisans of contagion there) in the year 1791, nearly *two* years *before* the Hankey arrived at Grenada, and that it caused the deaths of General Malcolm and between thirty and forty other persons, as is related in a letter from the Rev. Dr. John M'Knight to Dr. Hosack, and published in a work undertaken avowedly to uphold the cause of the advocates for the existence of contagion in the Yellow Fever, I mean the New York Medical and Philosophical Register, at p. 293 of vol. iii. As usual, it was at that time said that this fever had been imported from the West Indies. A farther account of "the Yellow Fever with which the city of New York was visited in 1791," is given by Dr. Hosack in his work on the laws of contagion, as extracted from what he terms an "excellent dissertation on that disease," by Dr. Jonas Addoms, which is as follows, viz. "About the middle of August, 1791, a contagious fever appeared in the city of New York, which first discovered itself near Peck-Slip, a part of the city thickly inhabited, its houses generally small, and badly ventilated; many of the inhabitants were in indigent circumstances, which is a frequent cause of the want of cleanliness. Here it raged a considerable time; it then began to spread, as some attendants on the sick became infected,* who lived

* The attendants on the sick, by attending them in the place where the fever began and prevailed, and where they had of

in other neighbourhoods. By this time it was carried to other families, and most generally could be traced to this source. It likewise proved more particularly fatal near *the place where it first appeared*, than in any other part." He adds, that the weather becoming cool about the middle of October, " the disease greatly abated, and in a short time disappeared." Dr. Hosack, to obviate all doubts about the nature of this fever, says, " Dr. Addoms, the author of that dissertation, since that time resided many years in St. Croix, and being associated with the late Dr. Gordon of that island, had ample opportunities of seeing the Yellow Fever in all its forms. During his last visit to this city, not long before his death, he informed me that the disease which he had seen in New York in 1791, was precisely the same which he afterwards saw in St. Croix, and which frequently pre-

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course been likewise exposed to the action of the local miasmata, which had produced the first cases of fever, were attacked in their turns, as might be expected, when they removed to their own homes; and in this way the disease is said to have been traced to its original source: and as superficial observers generally confound the operation of local miasms with the effects of personal contagion, the fever was supposed to have been contagious, although the whole account of it perfectly agrees with what commonly happens in regard to miasmatic fevers, especially the circumstances of *its remaining longest* and *proving most fatal* in the spot *where it first appeared*, which does not happen with fevers produced by contagion.

vailed during his residence there, more especially among Europeans newly arrived within the tropics."

In the following year, 1792, as Dr. Ramsay and other writers of unquestionable veracity have attested, the Yellow Fever appeared at Charleston, in the exact form in which it prevailed there epidemically in several subsequent years, and then attacked a considerable number of persons with its usual mortality (see p. 355 of my Essay). And as this occurrence, as well as that of the preceding year at New York, were *anterior* to the arrival of the Hankey at Grenada, Drs. Chisholm and Pym must be driven to the necessity either of abandoning their pretensions in regard to the *importation* of the fever in question by the Hankey, or of, at least, admitting that a fever exactly similar to that called Bulam may, and did, *originate* from *local causes* at New York and Charleston long before this ship had left the coast of Africa. For as to any importation of the fever in either of these years from the West Indies, Dr. Chisholm and Dr. Pym have both denied the possibility, by denying the existence of any such fever there for, at least, several years previous to 1793; and there seems to be as little reason for deriving it from any other part of the world on those occasions. If, therefore, it be admitted that in the instances of New York and Charleston just mentioned, a fever like the Bulam was produced merely from *local* causes, the inference is ob-

vious, that it may be produced by *similar causes* in *other* suitable places; and consequently, that its supposed importation and propagation by contagion are unnecessary, and also improbable. That such a fever has been thus produced with its worst symptoms (including the black vomiting), in situations where no person did, or could, with the smallest appearance of reason, suspect the existence of contagion, is a fact, which I will now proceed to establish.

Mr. Johnson, in his respectable work on the influence of tropical climates, has given us the history of a most violent and mortal fever, attended with black vomitings, &c. which appeared at the little island of *Edam*,* near

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* Dr. Pym has asked, at p. 158, "if either heat or moisture can be supposed capable of producing it?" (i. e. the Bulam Fever) "how is its non-existence in the East Indies to be accounted for, where we have the marsh fever in its worst form, under *Jungle*, and other names, without any suspicion of contagion, and where no disease resembling the Epidemic Fever of the West Indies, Cadiz, or Gibraltar, has been mentioned by any author?" To this question, which, however, scarcely deserves notice, the accounts now to be presented of the fevers at Edam and Johanna will afford an explicit answer; but if he should reject these on the plea that, by "*East Indies*," he only meant the *Continent* of India, and not islands in the vicinity of Java or Madagascar, the following extract, also taken from Mr. Johnson's work (p. 39), in regard to "*the Endemic of Bengal*," must, I conceive, satisfy his inquiry, viz. "Several cases" ('of the *Marsh*

Batavia, which seems to have been uninhabited until it was unfortunately selected for the establishment of an hospital, during the blockade of the latter place; and, from its having been so selected, we may presume that, at least, it did not contain any apparent marsh, though the soil emitted such noxious exhalations, that every individual who remained a single night on the island, was attacked with this fever, and in such circumstances that no person did, or could, suppose that it originated from, or was in a single instance propagated by, contagion.

The following account of this fever is extracted from page 166, & seq. of Mr. Johnson's work, and was written, as he tells us, " by a very intelligent surgeon of the expedition" sent in the year 1800 to blockade Batavia, who is said to have noted the facts as related on the spot. The mortality is stated to have occurred among a part of the crews of the squadron, and of the soldiers of the 12th Regiment. Of the latter, sixty soldiers *in health* were at different times landed to do duty at Edam Hospital, and the other buildings on the island between the 1st of October and the 12th of November, and of

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Remittent Fever') " occurred under *my own inspection*, where there was *a yellowish suffusion on the skin, as in the Endemic of the West*" (Indies), " with *vomiting of matter, bearing a considerable similarity to the grounds of coffee.*"

these "thirty-one died."—"Of the remaining twenty-nine, who were embarked on breaking up the blockade (the 12th of November), twenty-two died at sea: the other seven were sent to the Malacca Hospital, where all or nearly all shared the same fate."—"*All* the soldiers getting ill on Edam *sixteen marines* were landed from the Centurion to do *night* duty."—"The whole of these were seized with the fever, and *thirteen* died; two recovered, and one was sent to the *Malacca Hospital.*"—"Of those (*i. e.* the sick) sent to the Hospital at Edam, very few recovered."—The writer adds, "From our first arrival at Batavia in August, until our return to Malacca in January following, we only buried *one man of fever*, who had not slept on shore at *Edam Cuypers* or *Onrust* islands; whereas almost every person who slept a single night at *Edam died.—No ill-effects* were experienced by going on shore in the *day-time, or among the sick* in the Hospital. I myself regularly visited the sick at the Hospital of Edam every day, with perfect impunity, till one night that I staid rather late, attending an unfortunate Surgeon of the *Dædalus*; in consequence of which I was *three* days afterwards seized with the fever.—He continues:

"With respect to the question, whether or not this fever was contagious? *I am decidedly of opinion that it was not so.* For, if all the nurses and medical attendants at Edam died, it must be remembered that they were equally ex-

posed to the cause of fever, whatever it is, as the soldiers and seamen who did duty at the barracks, and other buildings, or who were sent to the hospital for other complaints; all or nearly all of whom shared the same fate. Moreover, what I conceive decides the question is this, that, although on our raising the blockade of Batavia, great numbers of sick, in every stage of the fever, were brought on board from the hospital at *Edam*, yet not a single nurse or medical attendant of any description ever suffered the slightest attack of the fever; nor did any circumstance transpire, that could in the *least* favour the idea of contagion, notwithstanding the great *accumulation* of sick on both decks, rendered it a matter of impossibility to separate them completely from those who were well, nor at all times to prevent a considerable generation of effluvia." The symptoms of the fever just mentioned, as described at p. 170, 172 of Mr. Johnson's work were a sudden accession of "giddiness and cold chills, sense of debility and vomiting, with pain over the orbits of the eyes, and in the epigastric region. The patient frequently falls down and is insensible during the paroxysm; his body covered with cold clammy sweats, except at the pit of the stomach, which always feels hot to the palm of the hand; the pulse is small and quick. On recovering a little, this train of symptoms is succeeded by flushings of heat; increased pain over the *orbits* and in the sinciput; pain and sense of internal heat about the stomach and pre-

cordia; oppressed breathing, &c." "The eyes now become as it were protruded and the countenance flushed. Retching and at length vomiting of discoloured bilious matter comes on, &c." "The length of this paroxysm varied from six to eighteen hours, and was generally succeeded by cold rigors; very often low delirium preparatory to the *next*:" in which "all the symptoms become gradually, often rapidly, aggravated; particularly *the head-ache*, pain and tension in the epigastric region, and vomiting; some patients on shore were carried off in eighteen, twenty-four, or forty hours."—"A great proportion changed in a few days to a *bright* yellow; some to a leaden colour."—"Vomiting of *black* bilious stuff resembling *the grounds of coffee*, frequently commenced early, and continued a most distressing symptom."—"The pupil of the eye was often dilated, and would not contract on exposure to light.—In others there was a great intolerance of light: both indicated danger. Low delirium was a pretty constant attendant of this fever from first to last: sometimes, though rarely, there was a raging high delirium. Mr. Carter's was a case of the latter, which he had in a very terrible degree, with *red*, inflamed and protruded eyes."—"The brain appeared the organ chiefly affected at first: the stomach and liver in succession." These facts are of high importance, because they prove most decidedly that the *worst* form of the Ardent Yellow Fever, with *black vomiting* was in nu-

merous instances produced solely by exhalations arising from the *earth*, or soil of the island of Edam; and that not being the product of a contagious power, it did not manifest any such power in a single instance.

To corroborate the preceding statement, I shall insert an extract from the Report of Mr. Niell on the Bulam Fever (No. 22), respecting the production of a fever similar to that which has been just described with black vomiting, &c. in another small island in the Indian Ocean, where, as at the island of Edam, it was impossible to suspect contagion, or any other cause of fever than exhalations from the earth, viz. " In the year 1801, when Surgeon of a sloop of war going out on a secret service to the Red-Sea, after doubling the Cape of Good Hope in the best possible health, we arrived early in the month of April at Johanna, situated between Madagascar and the coast of Africa; an island of great fertility, producing tropical fruits, vegetables, rice, &c. in wild luxuriancy. On our arrival at this delightful spot, as our stay was to be but short, every thing was instantly prepared for wooding, watering, and procuring fresh beef, vegetables, and fruit for our use; and for the accomplishment of these various duties different parties were appointed, but none, except the watering party, composed of twelve individuals, slept, or remained on shore, during the night, while we remained at this island (seven days). Soon after sailing, symptoms

of fever of an unusual kind appeared, which in a few days attacked the whole of the watering party in an aggravated form, excepting one who had the command: he had served in the West Indies before, and escaped with a very trifling attack. The general symptoms were, an oppressed pulse, pungent heat on the surface, bloated countenance, a dull, heavy, inflamed eye, violent head-ache, pain at the epigastric region, and an invincible irritability of the stomach; the vomiting in all cases distressing, of a bilious nature and a yellow greenish aspect, which towards the fatal termination of the disease assumed a dark brown olive or *chocolate colour*. In none of the bad cases were *remissions* well marked, and in most of those who died, a yellow suffusion of the skin of a lemon hue, was conspicuous only a short time previous to death; in all, however, after death, this appearance was common. The discharges by stool manifested a similar variety, but they were so highly corrosive and acrid as to excoriate the anus and nates, and to excite the greatest dread in the patient on the approach of the evacuation."

"It may be sufficient to state, that, out of the twelve men thus violently attacked, *six* died: those who recovered had not the dark-coloured vomiting; and their convalescence was extremely tardy. Many of the men who had occasion to be on shore during the whole of the day were attacked with fever, but in a milder form; and

in them it always observed *distinct remissions*, and in a few cases intermissions were witnessed. In this instance, had the ship's crew landed and slept on shore, the disease would most probably have affected the whole, which would have improperly given rise to (a belief of) the existence of contagion, and that we had received 'a highly contagious disease' at this island, *as the Hankey*, it was said, *did at Bulam:* but here we see it clearly defined and a limit circumscribed on its sphere, beyond which it could not pass; we see it confined to those most exposed and *at night;* those doing duty on shore during *the day only* we perceive affected in a trifling degree, and *none* suffered of those who were *but little* exposed to the influence of this island by working: the officers therefore experienced a complete immunity from this fever, except one who had it slightly."

"In the latter part of the same year, and in the same ship, I had occasion to visit the island of Madagascar, and remained there six weeks, touching at different ports, harbours, and bays, wooding, watering, and procuring refreshments, without any sickness being the result. This, however, I ascribe to the precautions that were adopted, from our late experience at Johanna, in not allowing the different parties on those duties to be on shore too early in the morning, or to a late hour at night."

"In the year 1806, at the same season precisely, I had occasion once more to revisit Madagascar in a seventy-four gun ship, for the purpose of obtaining supplies of fresh beef, vegetables, fruit, wood and water. In conducting these duties it was necessary to employ many of the men; and of all the parties thus employed none remained on shore during the night except a guard of marines, sent in the evening on purpose for the protection of the water-casks, &c.; and it was so arranged that no man had occasion to be out of the ship more than one night. Notwithstanding these precautions, a fever of a malignant nature appeared among the marines, and of twenty-four attacked in a violent degree, six fell victims to the disease. The symptoms here were not exactly similar to what we witnessed at Johanna; death in several cases was more sudden and unexpected, preceded by a violent burning sensation at the epigastrium, which was only a precursor of death by a few hours, and in one case by a few minutes. In these cases neither was a yellow suffusion of the skin constant, nor the eye so highly inflamed, nor the countenance so much flushed as in the fever of Johanna; but a sallow, dingy, disagreeable aspect of the countenance prevailed, which Sir Gilbert Blane correctly describes in his Treatise on the Diseases of Seamen, p. 409. In some of these cases I was inclined to think the individuals in a state of intoxication, from the very great degree of vertigo and staggering present; but a short

time served to convince me of my error. This variety of appearance in the two diseases I ascribe to peculiarity of constitution only, and not to any difference of climate; those who were affected at Johanna were young and recently arrived from their native climate; whereas the people subjected to the Madagascar fever had been some length of time in India, and had but lately arrived from a long cruize of four months, the greater part of which we had been on salt provisions, and symptoms of scurvy were appearing among us for some time before."

The Report of Dr. M'Arthur to the Transport Board (No. 7), contains two examples of a similar nature, which are not less decisive than the foregoing, viz. "In 1804, His Majesty's ship Amelia, lately from England, cruizing to windward of Barbadoes for the protection of the trade, anchored at the Devil's Islands, off the coast of Surinam, in order to procure water: the islands are low, swampy, and uninhabited. In a few days some of the officers and a number of the ship's company were simultaneously seized with fever. She arrived in Carlisle Bay (Barbadoes) on the 16th of October, having during the preceding three days lost the Surgeon and ten men; and her Captain, Lord Proby, died on the day of her arrival. This ship had been so considerable a time at sea, and so cut off from every source of contagion, that not only the men but the officers, attributed the fever, erroneously no

doubt, to some poisonous property in the water, and sent me a bottle of it to analyze.—One hundred and thirteen of the worst cases were selected, and removed to the hospital; but scarcely an officer or man escaped the fever in one form or other."*

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* It appears by the extracts made from the Amelia's Medical Journal, by Drs. Harness, Gray, and Tait, and transmitted to the Privy Council, that Mr. Reader, then Assistant Surgeon to that ship, had noted some particular occurrences not mentioned by Dr. M'Arthur, viz. that this ship had arrived in the West Indies near the end of July, 1804, and continued healthy until the second of October following. That on her arriving at the mouth of the river of Surinam, boats were sent to *Paramaribo*, to obtain refreshments. That the Berbice schooner, lying guardship there, received orders to *convoy* them, with their stores to the Amelia. "This vessel (says Mr. Reader), had been sickly previous to our arrival, and had lost many men from fever. *Every precaution was adopted to prevent any communication with her people.* On the 8th (of October) one of our people, in pulling down the river in the launch, expired at the oar: on the 9th I. Wilson was attacked with fever of a very malignant nature. On the 10th the Surgeon, First-Lieutenant, Master, and several men were also attacked with this malignant disease. On the 13th the First-Lieutenant, Surgeon, and Master died; and Lord Proby, the Captain, became ill and died on the 16th: many of the ship's company died of this fever on the passage (to Barbadoes) most of them *having the fatal symptom of black vomit.*"—As the Assistant-Surgeon was probably a young man, and not fully sensible of the morbific power of the marsh effluvia, which abounds on the swampy coast of Guinea, he might naturally be inclined to suspect that the fever in question was in some unknown way derived from the Berbice schooner, although her fevers (originating also from the marshes near which she was stationed, or from her having in like manner had her people employed on similar services at the same islands) could not have been contagious. That the fever which thus occurred on board the Amelia was not produced by contagion must be certain, because,

"The single instance of the Amelia, amongst many others which I could adduce as *disproving the contagious nature of the Bulam or Yellow Fever*, is, I trust, conclusive. I shall however produce another instance from a respectable, though not a professional, authority, which, from this circumstance alone, may with some minds be entitled to more credit."

"About the month of October, 1794, Admiral John Harvey, then commanding His Majesty's brig L'Actif, at anchor in Port Antonio, on the north-east side of Jamaica, being at the time perfectly healthy and without sick on

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first, there is every reason to believe that no communication took place with the Berbice schooner, and there were no persons beside with whom the frigate's people could have communicated, the islands being "uninhabited;" and secondly, the fever of the Amelia's people did not manifest any contagious quality, even when one hundred and thirteen of the worst cases had been selected and sent to the Hospital at Barbadoes, of which Dr. M'Arthur was then Physician.

Since the preceding note was sent to the press, I have been favoured with a letter from Dr. Dickson, containing an extract from one which he had just received from Dr. M'Arthur, on this subject, and in these words:—"With respect to the *Amelia*, I assure you that neither Mr. Reader, nor any of the surviving officers of that ship, ever expressed to me the slightest suspicion of the fever in question having been introduced by *contagion*. They uniformly, however erroneously, attributed the fever to the water at the Devil's island, and as I have mentioned elsewhere, sent me a bottle full of the water to be analysed. It has always been convenient for medical men, when they were at a loss to account for the appearance of a disease, to call in to their assistance the agency of contagion."

board, impressed ten men from a Liverpool vessel direct from England; the whole of them were taken ill with fever, such as prevailed at that time at St. Domingo, *without any communication with the shore*, of which six or seven died *with the black vomit*. The brig continued afterwards healthy as usual, the disease not having been communicated to any other person on board." Dr. M'Arthur adds, that the brig foundered subsequently on her passage to England, and the officers lost their papers and effects; otherwise he supposes that fuller information might have been obtained from the Surgeon's journal.

A similar instance is mentioned in the communication from the Transport Board to the Privy Council, *i. e.* in Mr. Secretary M'Leay's letter to Lord Viscount Chetwynd, viz. "Mr. G. Swan, Surgeon of the Thracian, relates seven cases of fever, *with symptoms similar to the Bulam*, which occurred *at sea*, between the 30th of November and the 15th of December, 1814. A boat having been sent up a river in the island of Cuba for the purpose of reconnoitring an enemy's vessel; the surrounding country was marshy and uncleared, and those only who quitted the boat were affected. The disease did not spread through the ship's company, and it is evident, from the above statement, that it was of *local* origin."

Although the black vomit is not particularly noticed in this extract as a symptom of the fever to which it relates,

there can be no doubt on this point, because Drs. Harness, Gray, and Tait, by whom the Journals of the Naval Surgeons were examined, were perfectly acquainted with the work of Dr. Pym, and the symptoms which he states as being peculiar to the Bulam, and it must have been by them that the symptoms in the seven cases of the fever on board the Thracian were deemed to be "similar to the Bulam."

In addition to these important facts, I will adduce others not less so, to the same effect from the highly interesting Report of Dr. Dickson (No. 5), viz. "On the 4th January, 1808, the Circe frigate went to English Harbour (Antigua), to refit. The place was then healthy; yet in *five days* from exposure, fever began to appear, and between the 9th of January and the 2d of February, one hundred and forty men were sent to the Hospital, and twenty-two had died, on the *fourth* and *fifth*, but *none later than the seventh day*, or *without black vomiting*. This ship had arrived in the country in April, 1805: only two cases of Sporadic Yellow Fever had occurred before October, when after being five days only in English Harbour, the disease made its appearance; but in consequence of putting to sea, though forty men were added to the sick-list, it only proved fatal to five, who were the first attacked. The ship again remained healthy until September, 1807, when, on going to Antigua, thirty-four cases of fever were added to the list; the disease disappeared on getting to the northward.

Again, on entering English Harbour in January, 1808, *'no man requiring medicine,' the sickness and mortality* I have just related *took place*, though the vessel had been (nearly) *three years in the country*. No *stronger proof* of the Yellow Fever's depending on *local causes*, and arising at *any* season of the year from undue exposure, can be given. The Blonde also, after being a week there, towards the end of March and in April, 1808, had one hundred and sixty men taken ill; twenty-two out of sixty-two died at the hospital, some on the third day with hæmorrhages from the mouth, nose, &c.; and nine died out of ninety-six slighter cases treated at the Capstan-house, and four afterwards at Barbadoes."—"After the Blonde, the Julia, L'Eclair, Heureux, &c. suffered in proportion, while *refitting* at English Harbour, the consequences of which duty, and the causes, I deplored in my letter to the Board of the 29th May, 1808, and again, in an enclosure of that, which was dated on the 14th of June."—"While one hundred and twenty-two men died at Antigua, between the 1st of April, and the 30th of September, the difference of health in an open Bay, like Barbadoes, is well contrasted by observing that the Belleisle, Ramillies, and York ships of the line, afterwards joined by the Captain and Intrepid, remained at that anchorage without any accession to the usual list of diseases, from April to the end of July."

Most of the preceding cases of Ardent Fever at An-

tigua appear to have been produced, during that which is commonly deemed the *healthy* season, by the dangerous duties of heaving down and refitting, by which seamen are more than ordinarily exposed not only to existing local causes, but in an especial manner to the air of ship's holds; which in vessels that have been long within the Tropics is often so highly vitiated as to produce fevers similar in their nature to those of marsh effluvia, although sometimes, if possible, more fatal. Of this fact other proofs will be found in the following chapter.

Having incontrovertibly established by the preceding extracts, that exhalations from the soil have produced a fever exactly resembling that which has been called Bulam in its worst form, but which was devoid of contagion, I will now adduce other unquestionable evidence to prove the production of the Remittent, and of the Ardent, Yellow or Bulam Fever at the same times, in the same situations, and from the same *local* cause, *i. e.* febrific exhalations from the earth.

The following instance to this effect is extracted from the Report of Dr. M'Arthur (No. 7), viz. "In April, 1809, about eight hundred prisoners of war, taken at the *Saints*, arrived at Barbadoes. Five hundred of these men were conscripts, and very recently from France. Some of the prisoners were put on board the Highland Lass, prison-ship; others were confined in the town-jail; and

the remainder, about five hundred, were placed for temporary security in an old barrack situate on the margin of the marsh-land, near St. Ann's, which had for a long time been deserted on account of the unhealthiness of the situation. The army and the navy were at this time perfectly healthy, no case of Yellow Fever having appeared for months before. Towards the end of April many of the prisoners in this barrack were attacked with a *Remittent* Fever attended with bilious symptoms; and soon afterwards several cases of *real Yellow Fever* occurred, of which several *died,* with the *black vomit,* esteemed by Mr. Pym as a characteristic symptom of the Bulam Fever. At this time the prisoners in the jail, and on board the prison-ship, whom I visited daily, continued perfectly healthy. Both the *Remittent,* and the *Yellow Fever* here appeared to arise from the *same* cause; and I feel assured, had the prisoners continued in the same place of confinement until a later period of the season, that a dreadful mortality would have been the consequence. Fortunately for them, they were soon removed into transports, in order to be conveyed to England, and with their removal the fever ceased."

A similar example is likewise furnished by Dr. Dickson in his Report (No. 5), viz. "At Barbadoes and Antigua, I had generally seen the disease of an ardent *continued* form, and did not fully understand why authors talked of a Bilious *Remittent* Yellow Fever, until after the

capture of the French and Danish Islands. But the anomalies of fever, the shades and changes which it assumes according to the intensity of the exciting causes (which *there* were *purely* and *wholly local*) the state of predisposition, or the spot of residence, could no where be more strongly pourtrayed than in the destructive Epidemic of *Mariegalante* in the autumn of 1808, from the most concentrated marsh miasmata; when the different types of fever were converted into each other, of the *worst* and *most aggravated species* I have ever witnessed. Some were affected with the *highly concentrated* Yellow Fever in the *continued* form; others with *Comatose Remittents* or *Intermittents*, the exacerbations of which were so violent as to carry off a patient in two or three paroxysms; while others sunk into a low protracted character of fever, resembling Typhus." He says farther concerning this Epidemic, that, "after *heavy* rains succeeded by *intense* heat, and assisted by irregularities, the garrison, consisting of marines, became sickly in July, and by the first of August had lost one hundred and seventy-six men; and before the end of September the loss amounted to *two hundred and thirty-four* men, out of three hundred and fifty originally landed upon that island, as detailed in his communication to the Board on the first of August, and more particularly in that of the 31st of October, 1808."

As the Report of Mr. Mortimer (No. 14), contains further particulars on this interesting subject, the hospital

at Marie Galante having been under his charge, I will here add an extract from it, viz. "Our troops, composed for the greater part of marines, debarked in excellent health in July, 1808. The town of Grand-bourg became head-quarters, and barracks the most airy and commodious were selected; whatever was deemed necessary to the preservation of the health and discipline of the garrison was immediately carried into effect by the Governor. Nevertheless but few weeks elapsed before *fever* appeared in full malignity: to be attacked, and to fall its victim, were terms nearly synonymous. Other barracks in a *more elevated* situation were chosen and occupied, and at the particular request of the Commander-in-Chief I took the medical charge." The progress of the disease "was in many instances so rapid, that different men bearing arms in the morning have, in a very few hours, been buried in the same grave. In intenseness and malignity, this fever has not been exceeded. The town of Grand-bourg has extensive marshes on three of its sides. With the wet season commenced the action of miasma on the troops: for the first fortnight no men enjoyed better health; during that time much rain fell, and great heat succeeded:—need we more fruitful sources of disease? The settled inhabitants generally enjoyed good health, with the exception of *intermittents*, as did several people recently from Guadaloupe."

It remains to be shewn that the Yellow Fever which occurred in the three last instances was not contagious, and the proofs which I shall adduce for this purpose (in the order of the quotations), will also shew the result of the long and extensive experience, in regard to this fever in the West Indies, of the very respectable gentlemen from whose Reports they are quoted.

The testimony of Dr. M'Arthur on this subject has already been partly given; to that it will be proper to make the following addition likewise from his Report (No. 7). After citing the description of the fever in question, contained in his letter to the Transport Board (which details minutely its symptoms, including the black vomit, together with the appearances after death, as examined in about one hundred bodies), and concluding from a comparison of these with Dr. Pym's account of the Bulam fever, that "no one can doubt that the disease which he denominates the Bulam, did prevail at Barbadoes from 1804 to 1809, more or less generally on board His Majesty's ships Amelia, Northumberland, Atlas, Nimrod, Trinidad, Dart, Blonde, Acasta, Camilla, Cerberus, Thais, Ulysses, Belleisle, Ramillies, Captain, Recruit, Pert, Vimieira, and Pompée; also on board the Sally, Simpson, Majestic, Edward, and Allison transports, &c. during which period the unseasoned troops

suffered equally with the navy by a similar fever, which, in 1804, was attended by a plague-like fatality in the family of General Myers, the commander of the Forces;" he adds, "no place could be better adapted to propagate and disseminate contagion than the building which we occupied as an hospital in Bridge Town until May, 1807. It was situated about a quarter of a mile from the wharf where the patients were landed, and on the lee-side of the town, confined by miserable huts, huddled one upon another. The patients and their bedding were carried through the town to this place by such labourers as we could hastily employ for the occasion. They were obliged to rest and take shelter from the sun in any house that would receive them; and, to the credit of the poor white and coloured inhabitants, they never were forced from their doors. The nurses, washerwomen, and sempstresses, were black or mulatto women, who daily visited their families in our neighbourhood; and, as there were no accommodations for any description of officers in the hospital, lodgings were procured for them in various parts of the town, and they were received without hesitation, labouring under *the most malignant* form of the disease, *even in articulo mortis.* Yet, with this free communication with the inhabitants in every part of the town, the fever did not spread generally, nor partially, neither in its aggravated, nor its milder form. If it be contagious, in what way can we account for the native

inhabitants of Bridge Town escaping *every degree of it* during a period of five or six years?"

Dr. Dickson has delivered his opinion upon the nature of the same fever in his Report (No. 5), as follows, viz. "I cannot well conceive the possibility of a disease being contagious without its being indubitably traced to such a source during so long a period in the West Indies; but, on the contrary, I have seen nothing to induce me to alter, in any degree, the opinion I expressed in 1808, of the non-contagious nature of the Yellow Fever, in the 4th volume of the Edinburgh Medical and Surgical Journal: and, as I there observed, the frequency of a contrary opinion at first, and the uniformity with which it has been rejected after longer experience in the West Indies, is one of the strongest proofs that can be given of its fallacy.—The whole history which I have kept of the health of the fleet, and the various weekly returns which I was in the habit of receiving, abound with instances of the disease attacking individuals, or a certain number of men, in a ship without going further, and of its becoming prevalent only from undue exposure. I have already at considerable length endeavoured to explain how this endemic may, and does, often prevail epidemically, without any reference to contagion; and, so far as I recollect, the idea of the latter has been almost uniformly discountenanced by the army and navy practi-

tioners of experience, whom I have met with in the West Indies. I am therefore decidedly of opinion, that the Yellow Fever of the West Indies is not *contagious.*"— He then alludes to the notion (which I shall examine in another part of this chapter), " that the original nature of a disease may be altered by crowding, filth," &c. which he says, that " he does not mean to deny;" but adds, " all I contend for is, that the legitimate Yellow Fever, as I have seen it, is not contagious."

Dr. Dickson's Report contains likewise the opinion of Dr. Rutherford, formerly of the Naval Hospital at Antigua, upon the same subject, which is expressed in the following terms, viz. " From an extract of a letter in my possession, written by Dr. Rutherford in October, 1793, he seems to have considered the fever which prevailed at English harbour as most malignant and infectious; but, after farther experience, in a letter dated the 26th of February, 1795, alluding to a report circulated of a contagious fever raging in the hospital, he says, he thinks it his bounden duty to represent that such reports were totally groundless, and must proceed from a gross misconception in the reporters of the nature of that complaint, which at that time prevailed in the hospital. For a fever of the description which that is of (namely, the Causus, or Yellow Fever of warm climates), is allowed by every author and practitioner of eminence conversant

with it, to have no relation whatever to those fevers called infectious; nor can there be an instance adduced where it has been communicated from one person to another. Indeed, the disease in question is endemic in warm climates, and epidemic in particular seasons and constitutions. But what strongly characterizes it (as distinct) from contagious diseases is, that it does not indiscriminately attack people of all ages, sexes, and countries, but is only the disease of youth and manhood, seldom attacking people advanced in life, or the female part of the creation, and as seldom any native of the country where it prevails; nor even natives of cold, who have resided any time in tropical climates. Dr. Rutherford then proceeds to state 'cases in point,' and proofs which, in his opinion, cannot be controverted." Dr. Dickson adds, that "he has no reason to think that, in these extracts, Dr. Rutherford was writing of *different* diseases, but that the opinion which he *first* expressed of *its nature* was *relinquished*, when he found in each subsequent season a disease *equally malignant* and *fatal, without its being communicated*, as he asserts, *in a single instance.*"

The opinion of Mr. Mortimer, who states that he served nine years in the West Indies, will be shewn by the following extracts from his Report (No. 14). "Having held the charge of the several (Naval) Hospitals at

Mariegalante, Antigua, Martinique, and Barbadoes, during periods when fever was epidemic at those islands, many opportunities have been afforded me of observing it in its various modifications, and in several instances of detecting its origin. But, from whatever causes arising, I have seen during the years 1808, 1809, 1810, and part of 1811, the same train of symptoms, the same suddenness of invasion, and rapidity of progress to recovery or death, common to all. I have never, during these periods, had occasion to think the disease was communicable from one individual to another, although it has prevailed generally on shipboard, and in some of the garrisons."—"In the many hundred cases of fever placed under my care at Barbadoes as well as at the other hospitals at different periods confided to me, I do not know of an instance of its communication to another patient under treatment at the same time for a different malady; and yet it may be presumed, that if any such instances had occurred in so long a period as *seven* years of *hospital practice*, three of which were marked by the prevalence and fatality of this disease (Yellow Fever), I could hardly have failed to notice them."—"I can particularize *many* instances occurring during my *nine years'* service in the West Indies, of men sent from the flag-ship to expedite the equipment for sea of vessels in which fever has prevailed" (probably from the foul state of the hold), who, having been *there* attacked, were received on board

their own (flag) ship without, *in any instance*, communicating it to others." Here I may observe, as *an incontrovertible truth*, that a disease, which possesses no contagious power, never can have been produced by contagion.

Having been led to cite opinions in regard to contagion from three of the Reports to the Transport Board, I will now state those which are delivered in the remainder of the Reports on the same subject.

1. Dr. Weir is of opinion, that the fever under consideration is " decidedly of an inflammatory and contagious nature."

2. Dr. Blair thus states his opinion, viz. " I believe no one doubts the Bulam Fever to have been highly infectious when first imported into Grenada in 1793, as previous to that time the West Indies had been for many years as healthy as most other countries,"

3. Dr. Gillespie, to whose opinion allusion has already been made, expresses himself as follows, viz. " I am of opinion that this disease is spontaneously generated in innumerable instances, without any reason to suppose that it had been communicated by contagion, and that in hot climates and seasons it becomes epidemic." Of such

generation he declares " that he has seen many instances in ships of the squadron serving in the Leeward Islands between the years 1778 and 1782, and in the Naval Hospital in St. Lucia," and that " out of between *six* and *seven thousand* soldiers who perished in St. Lucia during that period, a large proportion died, as he believes, of the Yellow Fever." He adds, " that he was stationed at Fort Royal, Martinique, with the Medical Superintendence of Seamen and Prisoners of War between the years 1795 and 1802, where he had, at least, *ten thousand patients under his care during that period*, a proportion of whom were affected with this disease. " I can venture (continues he) to assert, as my firm belief, that there was scarcely an instance of a ship of war, during upwards of seven years that I was on that station, in which this disease was not *spontaneously generated, without reason, to suspect contagion:* a reference to the journals of the surgeons of the respective ships, and to the books of Martinique Hospital, will, I doubt not, confirm this assertion. The same assertion may also be extended to a great number of transport-vessels employed on that station during the period specified." He adds, " I have no doubt that where men *much predisposed* to this complaint, in an *epidemic* state of the atmosphere, and strongly acted upon by terror and despondence of mind, are *crowded* together, the sick and healthy, in unventilated and *foul* ships, hospitals, barracks, or dwelling-

houses, this fever may, and has become, infectious.*—All febrile complaints, when depending on an epidemic state of the air, even a simple catarrh, or common cold, may be looked on as infectious in a certain degree to persons predisposed to receive the disease, and this I think is exactly the predicament in which Yellow Fever should be placed as to its infectious nature."—" I attended the dissection of a great many patients who fell a sacrifice to this disease; and my assistants, as well as the attendants, regarded this melancholy, yet necessary, mode of investigating the complaint without terror, and *without danger*. I have never had the Yellow Fever myself, and I have as little apprehension of ever catching it *by infection*, as I have of catching a Remittent or Intermittent Fever, or even a common catarrh, although I look upon any one of the occurrences as possible."

4. Dr. Gray, after mentioning various services in which he had been engaged, says, " during this long period of service, I cannot recal to my recollection a single instance of the fever being communicated from one person to another, either amongst the medical attendants, or nurses; there were no other precautions used except the

* As this opinion is held by other medical gentlemen besides Dr. Gillespie, I shall presently state some observations respecting it.

common and necessary ones of ventilation and cleanliness." He concludes as follows, viz. "I am of opinion that the Yellow Fever of the West Indies, the Endemic Fevers of Bengal, and of Batavia in the East, with those on the Coast of Africa, and the Bilious Remittent of the Mediterranean, and southern parts of Europe and America, spring from the same source, marsh miasmata, being in a more or less concentrated state, and modified by local circumstances of climate, &c."

5. The opinion of Dr. Dickson has already been quoted.

6. Dr. Burnett has so fully delivered his sentiments concerning the Bulam Fever in his publication, that it is scarcely necessary to state here even so much as that he considers this disease to be not contagious.

7. The opinion of Dr. M'Arthur is already given.

8. Dr. Denmark believes that the disease is "not ab origine contagious, but, in its progress, capable of becoming so."

9. Dr. Gardiner says, "I cannot relinquish the idea I have ever entertained, that the fever of 1813 (at Gibraltar) was engendered by causes existing within the walls

of the garrison, and not introduced by persons from without." The principal of these internal causes is, an increase of population, from "multitudes of labouring poor," which "far exceeded all former precedent," and the lodging-houses for them "crowded beyond all conception;" these, with "the heat of the season gradually increasing, and the total absence of rain for many months, may possibly be considered agents powerful enough to generate typhus fever, without directing our attention elsewhere."

"The sphere of contagion, I believe, is generally considered very limited, and I think the fever of 1813, at Gibraltar, tends to confirm this opinion; for, at the same time the inhabitants and military in the centre of this place were hourly falling its victims, the soldiers encamped at the south, the artificers confined in the dock-yard, and the thousands on the neutral ground, *nearly all* escaped harmless; and of those who perished in any of these situations (if any did), the infection had been imbibed previous to removal from its source."

He afterwards questions "if it is probable that this fever would have been restrained in its effects to any particular spot" (as he relates it to have been), "had it been *brought* into the garrison? but if it is admitted to have been engendered by *local* causes, then it is not

supposing too much, perhaps, to account in this manner for its limited extension.

10. Dr. Veitch, having mentioned his voyage to the West Indies in 1794, as Surgeon to His Majesty's ship Regulus, says, "the master of the ship was attacked with the black vomit at Port Royal, Jamaica, and he died on board; but no officer of the gun-room or attendant was attacked, although he received the most assiduous attention from all. Lieutenant Wilkinson was at a period considerably subsequent attacked with the same fever, and was sent in a convalescent state to the hospital at the Mole; but no extension of the fever to others in the gun-room followed—a circumstance inconsistent with the occurrence of a *contagious* fever *in so small a space.*" —"The Regulus returned to England in 1796, and after refitting at Plymouth, was again ordered to the West Indies, on which station she remained until 1799, during which interval the Yellow Fever repeatedly appeared on board among the ship's company, as well as officers; but it never exhibited the character of a contagious disease."

"In 1800, I was Surgeon to the Saturn, and had the honour of serving under the flag of Admiral Totty, who appointed me Acting Physician to the Fleet under his command; and in that ship insulated cases of fever oc-

curred, but still bearing no analogy to a contagious fever."

"From 1808 to 1813, I was Surgeon to the Royal Naval Hospital at Antigua, which gave me ample opportunities of seeing the character and cause of this fever, as few ships, when it became necessary for them to remain any length of time in harbour to refit, entirely escaped the influence of this disease: and *the result of my experience* and *observation* in *that capacity* leads me decidedly to state that the fever which destroys so rapidly the young and vigorous Europeans, and on which the name of Bulam Fever has been very improperly imposed, is *not contagious* at Antigua, nor in any part of the West Indies: and I now proceed to offer the grounds of this opinion. I have seen this disease in many situtions, and at different islands; and as far as my observation extends, there is no fever more uniform in its character and tendency than the Yellow Fever, when impressed on the constitution of the young and vigorous European of the sanguine temperament on his first arrival in the West Indies. The Hospital at Antigua was on many occasions found too small for the number of patients it was necessary to admit, and therefore by no means well adapted for classification; so that *all the wards* of the hospital were *often exclusively occupied* with cases of *Yellow Fever*, while the galleries

were appropriated to those afflicted with ulcers, wounds and chronic complaints; in fact there was often *a necessity* for their sleeping *in contiguous cradles*; yet *in no instance* was fever communicated from such intercourse to those labouring under ulcers, wounds, and chronic dysentery, or to those subjected to operations. The apartment for the reception of officers laboured under all these objections in a still more striking degree." In proof of this he describes them, in such manner as to show that every thing was favourable to the propagation of contagion, had it existed, and then adds, "yet here the same immunity prevailed. When these cabins were not all occupied by sick officers, my assistants took possession of them without dread or inconvenience; and during the whole time I did duty at that hospital, only two of these gentlemen were assailed with fever, Mr. Pool and Mr. Pike; the latter died; the former *recovered,* but some months afterwards he *died* of *that disease.*"

Dr. Veitch adds, "the numberless instances of insulated cases of Yellow Fever which occurred in ships on the Jamaica station, and on the Leeward Island station, among the men and officers, effectually refute the opinion that the disease is of a contagious nature; and it was this circumstance, so often repeated in many ships, which at an early period attracted my notice and contributed greatly to form my opinion of its cause."

11. Dr. Vance expresses himself on this subject in the following terms, viz. "Respecting the contagious nature of this disease I have to state that no instance of the disease having been extended by contagion occurred within my own observation in the West Indies, nor in the different situations in which I have served in the Mediterranean, where the disease was of common occurrence; nor have I heard in my common intercourse with professional men or others that it was considered a contagious disease. The best proof of the prevalence of that opinion in the West Indies may be found in the willingness displayed by the attendants of the sick in ships and on shore, to afford their services in the worst cases of the disease without any apprehension or injury. If a contrary opinion prevailed, the numerous instances of the disease occasioning the death of the finest individuals in a few days would deter even the nearest relations from performing the requisite offices for the unfortunate sufferers. It may therefore be inferred, that a subject which so deeply involves the safety of society, would not remain in doubt after the experience of many ages."

Dr. Vance subjoins the opinion of his successor in the Theseus on this point, who died of fever at Jamaica himself; wherein this officer says, "In a very short time I was completely convinced of being right in my conjectures as to the non-contagiousness of the Yellow Fever."

12. Mr. Gregory's Report does not touch on contagion.

13. Dr. M'Namara delivers his sentiments thus; "as far as relates to the disease being infectious, I am most positively and decidedly of opinion that its cause can in every instance be traced to adventitious circumstances without referring it to contagion."

14. The opinion of Mr. Mortimer has been recently stated.

15. Dr. Kein is silent in regard to contagion.

16. Dr. Crichton considers the Yellow Fever which he has seen, at the Naval Hospital, Antigua, as "not being contagious."

17. Dr. Magrath has delivered no opinion on the question of contagion.

18. Mr. Forbes says, in his Report dated "Barbadoes, 21st January, 1816," "During a service of upwards of three years in this country, I have certainly seen some very strong instances of (what I conceive to be) the non-contagious nature of Yellow Fever; and never saw one

wherein the data in favour of non-contagion, did not, in my opinion, infinitely preponderate over those in favour of contagion."

19. Mr. M'Leod's opinion is as follows, viz. " As to its contagious quality, I cannot speak positively; but on the whole, I should be inclined to think it was not so, for the following reasons:—Contagious fevers, such as the Plague, &c. are observed to respect no age or sex; but the Yellow Fever almost invariably seizes those *whose constitutions account for the seizure,* namely young, robust, plethoric men, who are at the same time more particularly subject to all the before-mentioned exciting causes (fatigue in the sun, irregularities, &c.) whilst old men, children, and women (with the exception perhaps of the Spanish peasantry, where the youmg women do laborious work and whose habits approach to the masculine), are seldom its victims. In the next place, I have never observed those immediately about the sick more its objects than any others."

20. Mr. E. H. Brien gives his opinion in the following words, viz. " I considered the Yellow Fever of the West Indies, as occurring in an individual now and then, to be incapable of communicating itself to those contiguous; but when several were labouring under the disease at the same time, I believed it to be highly contagious."

21. Mr. Tobin delivers his sentiments thus; "it was the opinion of some well-informed medical men, whom I met in the West Indies, that the Yellow Fever was not contagious; however, I perfectly agree with Mr. Pym as to its being so: and this opinion is not formed from any preconceived theory, but from actual observation; the whole of the nurses who attended the sick of the Arab were seized with it, and it neither spared sex, age, temperament, or colour, a black woman having died of it who left England with us." *

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* Dr. Dickson has given, in his Report (No. 5,) a general history of the state as to health of the squadron in the Windward Island station during the seven or eight years he there held the office of Physician to the Fleet, in order to prove (contrary to Dr. Pym's assertions at pages 28 and 113, "that *for some years past* the Bulam Fever has seldom, if ever, appeared in several of the West India islands," and "that for centuries past the disease has been *only an occasional visitor* in the West Indies,") that it had existed in each of those years in various ships and islands: and he has in that history stated the following particulars concerning the sickness of the Arab's people alluded to by Mr. Tobin, viz. "In December, 1806, and January, February, and the subsequent months of 1807, several deaths occurred from Yellow Fever in different ships; but there was no general sickness except in the Lily at Trinidad (where the troops were reported to be also unhealthy), the Dart, and the Cygnet at Antigua. In May this fatal fever became *general in the Arab at sea* (though that ship had been eighteen months on the coast of Africa), in consequence of *much exposure* in rafting spars from a store-ship to the dock-yard at Barbadoes; and between the 17th of May and 21st of June, thirty-one men died on board." Drs. Harness, Gray and Tait have also extracted the following account from the Arab's Medical Journal, which is con-

22. Mr. Niell says, " I have endeavoured to prove (by the statements before quoted concerning Johanna and

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tained in the Report from the Transport Board to the Privy-Council, viz.

" Mr. Tobin, Surgeon of the Arab in the year 1807, attributes the introduction of the fever which prevailed on board that ship to the constant exposure of the people to the sun whilst at Barbadoes, being constantly employed in rafting timber from a store-ship to the shore, up to their necks in water, or else working *in a burning sun at the dock-yard*, at both which places *they never failed to get drunk*. Those employed on the above service were first seized, and the greatest mortality prevailed amongst them."

These statements must be admitted to account very sufficiently for the production of the fever on board the Arab, without any reference to contagion, and I may be permitted to observe that, if so " general" and so " fatal" a disease had been caused by contagion, it could not have been difficult to trace it to that source ; but no such source is mentioned, or seems to have been suspected, by Mr. Tobin ; nor is there any reason to suppose the existence of any source at that season (i. e. " May," which is almost uniformly the most healthy month of the year in the West Indies) by which the Arab could have been infected. Of Mr. Tobin's sincerity and impartiality in subsequently considering the fever in question to be contagious I am fully persuaded ; and it was very natural that he should do so, having recently come to the West Indies, and " being ordered home immediately afterwards." Greater experience however would, I am also persuaded, have operated on his mind as on the minds of almost all other persons, and have led him to view the disease as we find that the more experienced, and equally impartial, Physician to the Fleet then viewed it, i. e. as proceeding from improper exposure to *local* causes, joined to excessive irregularities. A passage in one of the letters with which Dr. Dickson has favoured me, dated " Bath, 12th August, 1815," is not inapplicable to this subject. " When I first went out to the West Indies, as Surgeon of a frigate, I was disposed to believe in the doctrine of

Madagascar, and others) that the disease commonly called Yellow, now Bulam Fever, is not a contagious or infectious disease in the Tropics, and that it existed long previous to the year 1793;" and having afterwards related various occasions on which he had observed this fever, particularly at the fall of Guadaloupe in 1810, when "a great number of men were attacked with it, (he adds) in this and any former sickness I have ever been a witness to, on board ship in a tropical climate, I have never observed any of the medical attendants or nurses to be subject to the prevailing disease."

23. Mr. Donnet, to use the words in Mr. Secretary M'Leay's letter, "is of opinion that the disease is not contagious under the precautions of ventilation and cleanliness;" but he thinks that when "attended with the alarming symptoms of black vomiting, petechiæ, and hæmorrhages," it "assumes in that form the property of propagating itself. But that property, he adds, is counteracted by ventilation, &c. and it becomes inert by re-

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contagion, and had I left it, after witnessing some of the instances of general mortality that soon after happened, I should probably have *continued in that belief;* but I was early led to doubt of its correctness, by finding that the Yellow Fever at distant periods proved fatal to two, and afterwards, on going to Antigua, to five men of those first attacked, without, in either of the three instances, extending any farther."

moval not far distant from the place in which the disorder originated; for instance, at Gibraltar, individuals labouring under marked symptoms of that disease, and conveyed to the neutral-ground, Windmill-hill, Europa, or on board of ships in the Bay, will in any of those places go through the disease without communicating the disorder to any of their attendants or neighbours. This fact, I believe, was well ascertained during the autumnal fevers of 1813 and 1814."

24. The opinion of Mr. Whitmarsh is represented in the same terms with the preceding in the above letter, and appears in truth to be exactly similar. "Transport a person (says he), affected with the fever to a spot where free ventilation can be experienced, and it will not be capable of reproducing itself in a second person, who has not been exposed to, what I conceive to be, the exciting cause or causes of the disease, i. e. a vitiated atmosphere, produced by exhalations from vegetable and other matters in a putrescent state."

In addition to the preceding reports, the Gentlemen mentioned in Mr. Secretary M'Leay's letter thought it might be useful to ascertain and state the opinions of the Surgeons of several of His Majesty's ships employed in the West Indies relative to the origin, or contagious nature, of the disease; and were therefore induced to make

the following extracts from the Remarks contained in their Journals, as coming from men perfectly impartial, and who had the best opportunities of making observations, and of forming a judgment on the points in question, viz.

"Mr. Hinds, Surgeon of the Arab, in the year 1801, attributes the introduction of the fever which appeared on board that ship to a draft of men, who were brought from the *Tromp,* prison-ship, and who had been taken out of a prison-ship at Guadaloupe, where a very malignant fever at that time raged.

"Mr. Hamilton, Surgeon of the Vimieira, in the West Indies, in July and September, 1810, had eight cases of Yellow Fever which occurred on board the ship at sea. One died with black vomit. The disease did not spread through the ship's company, although no precautions were taken against contagion. He relates one case of second attack.

"Mr. Lithgow, Surgeon of the Blonde, in the West Indies, had four hundred and forty-three cases of fever between the 21st of September, 1807, and the 16th of July, 1810, out of whom many were re-attacked a second or third time during that period; thirty-five died on board, and one hundred and fifty-three were sent to the Hospital.

He attributes the disease to the influence of climate, and to the fatigue and exposure in refitting the ship at different periods.

" Mr. Sheppard, Surgeon of the Circe, remarks that he had many cases of Ardent Yellow Fever on board, which he attributes to the influence of climate, the local situation of English Harbour, Antigua, and to the occasional fatigue and exertion of refitting the ship.*

" Mr. M'Namara, Surgeon of the Brazen sloop in the West Indies, had eleven cases of Continued Ardent Fever, on board, from the 15th of November 1811 to the 14th

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* This gentleman has since published his sentiments " on Yellow Fever" in the 47th number of the Edinburgh Medical and Surgical Journal ; in which, after mentioning that he had served " more than eight years in the West Indies," and was " Second Surgeon of the Naval Hospital at Antigua at a period when the Yellow Fever prevailed in an unusual degree in several of His Majesty's ships *under repair* in English Harbour," he says, " the daily familiar examples in the West Indies of the promiscuous introduction *amongst the healthy*, of men labouring under the Yellow Fever in its various stages without the communication of the same, *although no means of prevention are practised*, present an accumulation of facts in favour of *local origin*, which, divested of hypothetical research, or oral reports, rest on the obvious truth, that, if *contagion* formed any part of the character of the tropical endemic (the Yellow Fever) the effect would be manifest, in those instances of *unrestrained intercourse*, by *the due production of the disease;* and *the truth of such contagious propagation* would, from the *multitude* of the examples, have speedily received *unequivocal confirmation*."

of November, 1812; four terminated fatally with the black vomit. To the influence of the weather at this season of the year, he attributes the disease. It did not spread through the ship's company.

"Mr. Duins, Surgeon of the Liberty brig, in the years 1810 and 1811, in the West Indies, had seventeen men attacked with the endemic of the country; which he attributes solely to the intense heat, heavy rains, and calms which they experienced, in a four weeks passage from Cape François to Antigua; the symptoms were similar to those of the Bulam Fever.

"Mr. Rae, Surgeon of the Leyden, remarks, that he did not consider the fever which occurred at Carthagena, in the years 1811 and 1812, as infectious, although it was denominated Yellow Fever.

"Mr. Boyd, Surgeon of the Repulse, remarks, when at Mahon in August, 1811, that an Inflammatory Bilious Fever made its appearance amongst the people, and nearly similar to that which prevailed in the ship last season. He attributes the disease to the great increase of temperature, that prevailed during the ship's continuance in harbour; and to the people being much exposed to solar influence, and fatigue; as an instance of its non-contagious nature, he observes, that not one of the men employed as

nurses were attacked with the disease, although constantly about the persons and clothes of the sick. Forty-two men were attacked with the disease; four of whom, with a marine officer, died, having the fatal symptom of black vomit.

"Mr. Linton, Surgeon of the Lily, in 1807 and 1808, in the West Indies, attributes the cause of Continued Ardent Fever to plethora, constitutions unassimilated to tropical heats, intemperance, exposure to the heat of the sun, to the humid night air, fatigue, &c.; he has had no reason whatever to consider the disease as contagious.

"Mr. Reekie, Surgeon of the Pompée, in the West Indies, had many cases of fever, from the 29th of October to the 11th of November, 1808, which he attributes to the communication which the ship had with a prize taken in her passage out;* the crew of the prize having been pre-

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* Dr. Dickson, in a letter, dated Clifton (Bristol), July 8th, 1817, has kindly favoured me with extracts from the official reports made to him, from time to time, respecting the diseases which occurred on board the *Pompée* between the 22d of October, 1808 (when she arrived at Barbadoes, from England), and the month of May, 1810; together with the results of his own observations, at the several visits which he made to that ship; but unfortunately, I had gone for a few weeks to the Continent, one day previous to the date of his letter, after having sent my *Sequel* to the *press*: and it has there-

viously unhealthy, and having lost several of her people from this disease. The fever made its appearance first

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fore been out of my power to derive that advantage which I should otherwise have gladly done from this communication. I may, however, shortly state from it, respecting the fever which appeared in that ship, subsequently to her capture of the French vessel, that Dr. M'Arthur, then Physician to the Naval Hospital at Barbadoes, to which more than one hundred of the *worst* cases of this fever were sent from the Pompée, has delivered the following opinion concerning it to Dr. Dickson, viz. " I have no doubt that from the *filthy* condition of that vessel (the prize), confined air, and putrid effluvia did assist in giving a predisposition, to the crew of the Pompée, to fever, when the exciting causes were applied ; but I cannot think that the sickness of the French was communicated in any other way (that is by infection), to the English." The opinion of this judicious and able Physician, must carry with it the greater weight, when we consider, that if the fever in question had been produced by contagion, it must have *retained* and *manifested* that quality, in the many cases of it, which were under his immediate care ; and if it had done so, his opinion would have been different. Having stated Dr. M'Arthur's opinion, Dr. Dickson adds, " my own opinion corresponds with his : the ship arrived at a pressing period of service, *crowded* with men highly disposed to febrile action, and making great exertions to get ready for sea : the want of hatchways on the quarter-deck, the stowage of the booms, the quantity of stores, &c. were all unfavourable to ventilation ; and in watering and dock-yard duty, and the opportunities of committing irregularities, they were much exposed to the various causes of fever. The great and sudden influx of this disease and its declension in so short a period, even though one hundred and fifty *new* men (soldiers) were embarked, are circumstances, which accord better with the belief of its being the result of those powerful, but temporary exciting causes, than of contagion. That the first and greatest sufferers should have been those in the prize, is

amongst those men of the Pompée that manned the prize. Thirteen out of the total number, seventeen, who died on board, and at the hospital, were of that party. *

"He remarks, "such has been the unhealthy state of the ship, during the four months this Journal comprises, that no sooner had we got rid of the first fever than the ship's company were overrun with ulcers." And again, "as those began to leave us, the fever made its appearance in a more malignant form than ever, but fortunately by great attention to ventilation, dryness, &c. its progress was completely stopped."

"The first fever in October and November, was no doubt (he says), the Endemic of the country, or in other words, the Yellow Fever."

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not extraordinary, since they were not only exposed to the before-mentioned causes of disease, but were moreover employed in clearing an extremely *foul* and offensive hold. In fine, in climate, season, and the relative circumstances of the ship, there were evident causes to account for the fever."

* Mr. Mortimer gives the following particulars in his Report concerning the sickness on board the Pompée, which he cites as "another proof of the effect of miasmata in the crews of vessels," viz. "Having captured to windward of Barbadoes, a French brig (Le Palineure), on her passage homewards from Martinique, whose men were *suffering from the worst kind of fever*, and where the *effluvia* from *the hold* were *intolerable*, *almost every person on board*

" He attributes the second fever which prevailed in February to infection from the sailors of the Amphitrite frigate, who were brought on board in a state of sickness from Fort Royal Hospital.*

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was attacked. On the removal of the prisoners to the Pompée, the malady prevailed very generally there ; nor can it be wondered at, when we consider the very little attention paid to cleanliness by Frenchmen, and consequently the quantity of filth brought on board with their baggage. It is moreover worthy of remark, that disease *continued to rage* in the prize *until her hold* was completely cleared, and exposed to the purifying effects of ventilation."

* Of the fever which occurred on board the Pompée at and after the siege of Martinique in 1809, when large parties of men were actively employed on shore and in the boats, &c. Mr. Reekie, her Surgeon, in his Report to Dr. Dickson says, " From the sudden and great prostration of strength, not only at the end, but at the beginning of the disease (which cannot be accounted for by the febrile action) its rapid progress to a fatal termination, and the great disorder of the whole nervous system, I have little hesitation in pronouncing this fever to be Typhus Gravior ; and I have little hesitation in giving it as my opinion, that it has been introduced into the ship by contagion. It has not been occasioned by fatigue on shore."—" It was on board, that this fever first made its appearance," &c. He adds, " I have no doubt whatever that it has been introduced by the sailors who belonged to the *Amphitrite, French frigate.*"—" As a fever attended with great mortality, spread through the ship's company of this frigate, I have every reason to suppose it was one of the same description as that communicated to us."—On this subject, however, Dr. Dickson seems to be undecided.—He says, in a letter to me, of the 28th of June, 1817, that this fever " was of a low, insidious, and modified form, different from the usual Yellow Fever, which certainly gave colour to its being connected with the Amphitrite's crew."—" If it really

"Mr. Linton, Surgeon of the same ship, in his Journal of 1809 and 1810 remarks, that many of the Continued Fevers which occurred on board were of a nature pecu-

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had any contagious property, I should incline to consider it as a modification of Typhus; but if so, it was evidently very weak and soon died away. This leads me to the possibility of Typhus existing between the tropics."—"A tropical climate, I am decidedly of opinion, is most unfavourable to the existence of Typhus contagion; that it grows weaker and weaker, and soon becomes extinct, but that it can and sometimes does exist in ships *strongly imbued* with it, I am inclined to believe, and particularly in French ships, from various reports in which I find Typhus mentioned."—This statement of Dr. Dickson, for whose opinions I entertain a very high respect, founded as they generally are, upon just reasoning and an extensive acquaintance with facts, accords entirely with the result of my own experience as delivered between pages 503 and 510 of my Essay. I have no doubt that the contagion of Typhus does subsist in vessels in which it may have been prevalent for some time after their arrival between the tropics; but I believe that it always becomes more feeble and inefficient by the action of a high temperature, and that, unless favoured by an unusual co-operation of circumstances, it does not, many weeks, retain in that temperature the power of propagating itself: however I cannot agree with Sir Gilbert Blane who (in the sixth volume of Medico-Chirurgical Transactions) after admitting that "when typhous poison exists in a slight degree, a warm climate *dissipates* it," adds, "but, when in a concentrated state, it is *exasperated* by the heat of the atmosphere, and by paludal exhalations, which universally exist in the vicinity of West India harbours." That this same cause (heat) should ever act with such obvious incongruity, in direct opposition to the modus operandi of every other physical agent, is more than I can believe, without strong proof, of which none is adduced by Sir Gilbert Blane.

liarly malignant, which he attributes to the ship being crowded with troops, prisoners of war, and British seamen liberated from jails, hospitals, &c., and also to great irregularities and unavoidable circumstances, abolishing the interior œconomy of the ship.

"Mr. Purdie, Surgeon of the same ship, Pompée, in the year 1810, remarks, that at the time he joined the ship, she was suffering from a fever of a very malignant form, and that during the siege, and after the capitulation of Guadaloupe, the crew were much employed in different services, in a very hot and confined part of the island. For five weeks the ship was at anchor in Basseterre, close to the shore, in the closest and most sultry anchorage perhaps in the whole Leeward Islands. He observes that, from sultry heat, a want of due circulation of pure air, hard duty, and more hard drinking, while the Pompée was suffering from fever at this place, the other ships lying at Englishman's-head, and enjoying a fine breeze at the distance of three miles from the shore, had scarce sickness enough to muster a sick-list.

"Mr. Purdie, when Surgeon of the Argo, in 1808 and 1809, had many cases of fever on board, from the 3d to the 22d of November while at Sierra Leone: the symptoms were similar to the Ardent Yellow Fever. He attributes the disease to marsh miasmata.

"Mr. Campbell, Surgeon of the Circe, in 1800 and 1801, observes, that he found the Yellow Fever always infectious; and as a proof, he says, that eight Surgeons in the course of the last seven months either died, or were invalided in the West India station, from fever, whilst no Captain died, or was invalided during that period. On the 5th of October the crew sailed from Port Royal, Jamaica, with orders to water at Bluefields. The ship at this time was perfectly healthy. In consequence of the great fatigue, and exposure of the watering party,* during

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* The immense losses which have been sustained by the navy in the West Indies by the business of watering alone, have been recorded by authors, and particularly by Sir Gilbert Blane at page 92 of his work on the Diseases of Seamen, and the late Dr. John Hunter, pages 17 and 108 of his Observations on the Diseases of the Army in Jamaica. At Rockfort in Kingston-harbour, the usual watering-place for the navy, Dr. Hunter states, that examples occurred "where out of sixty or seventy men sent on that duty, not one has escaped a fever." To the late Admiral Lord Hugh Seymour, however, who died Commander-in-chief, the navy on the Jamaica station are indebted for the abolition of that service, by the substitution of small vessels, called tanks, which are navigated by negroes, and have their holds divided into compartments, in which water is received in bulk from the reservoirs at Rockfort, and carried alongside the men-of-war, by whom the water is pumped on board. By the regular use of these tanks the Jamaica station, from being generally the most sickly and fatal of all the stations for the British navy, has been rendered one of the most healthy. The late Admiral Rowley, Commander-in-Chief at Jamaica, favoured me in 1811 with some documents to establish this happy change, which, to my regret, have been mislaid; but I have the best authorities

the night and day, to the hazy exhalations which float along the woody beach of Bluefields, he attributes the cause of a most maliguant fever, which made its appearance on board the Circe at that period. Although this disease might be attributed, in his opinion, to the above circumstances, yet he considered the disease to have become afterwards contagious: as a proof of which, of six women who attended the sick, one died of this fever, two narrowly escaped, and the other three complained of sickness and nausea, and latterly refused to attend the patients. Three old men were then fixed on, one of whom died, the other two complained and took medicines, but continued at their duty. Mr. Holme, the Assistant Surgeon, was attacked with the disease, while bleeding a patient ill of this fever, and died. Mr. Campbell had a second attack of this fever from the same cause. Twenty-five persons were attacked by this disease, ten died with the symptoms of black vomit."

Accounts from three other naval surgeons, viz. Mr. Swan, Mr. Tobin, and Mr. Reader, are also given in

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for knowing the correctness of the statement. That the fevers which occurred among the Circe's people ultimately acquired a contagious property, as Mr. Campbell has supposed, will presently be shown to be contrary as well to reason as to experience.

Mr. Secretary M'Leay's letter, which, however, it is not necessary to repeat here, as they have already been stated.

Having stated the opinions delivered in the Reports transmitted to the Privy Council, it may be proper to give a summary of them; and I will therefore mention that, of the *twenty-four* gentlemen from whom those Reports were obtained, *three* (Mr. Gregory, No. 12, Dr. Kein, No. 15, and Dr. Magrath, No. 17) have omitted the statement of any opinion on the subject of contagion, as connected with the fever in question; *three* others (Dr. Weir, No. 1, Dr. Blair, No. 2, and Mr. Tobin, No. 21) have expressed their opinions that *it is contagious; one* of them (Mr. Brien, No. 20) declares his belief that, in individual or solitary cases, it is "incapable of communicating itself to those who are contiguous," but "that, when several were labouring under the disease at the same time, he believes it to be highly contagious;" and *another* gentleman (Dr. Gardiner, No. 9) appears to think, that *local* causes contributed at least as much to the production of the fever at Gibraltar in 1813, as contagion. Of the remaining *sixteen*, the majority have *absolutely* and *positively denied* the existence of any contagious property in this fever; and the rest have declared their belief that it is not

naturally or *properly* a contagious disease, although several of them are inclined to believe that it may (as they suppose to happen with most other diseases) acquire a contagious property by *crowding*, *filth*, &c. This latter *opinion*, whether true or false, is *foreign to the question*, as relating to the natural and original properties of this fever. That it is an *erroneous* opinion I have endeavoured to prove by facts and arguments in my Essay, and I shall soon revert to the subject. Most of the sixteen gentlemen, who declare that the fever under consideration is *not contagious*, have alledged *decisive facts* to support their declarations, some of which I have already quoted; and I shall hereafter have occasion to notice some of the others. I should think it my duty equally to notice the facts of the two gentlemen, who have declared in favour of the supposed contagion of the Ardent Fever (as I have done in regard to the occurrences related by Mr. Tobin), had I discovered that any facts had been stated by either of them to support his belief of its possessing that quality. The only remark which I shall offer on this subject is, that we have here *sixteen* opinions against *three*, or at most, *five*, if the two opinions to which I allude (of Dr. Gardiner and Mr. Brien), conditioned or qualified as they are, can fairly be reckoned as on the side of contagion. What other or farther preponderance the sixteen opinions ought to acquire from the facts adduced in their support, and

the experience of those by whom they are stated, I leave to the judgment of my readers.

Of the *eleven* gentlemen from whose journals extracts have also been adduced, it will be seen that only *three*, viz. Mr. Hinds, Mr. Reckie, and Mr. Campbell, have related facts which seem to indicate the operation of contagion, in their present obviously defective form, but which admit of easy explanation without any aid of contagion, more especially those which occurred on board the Arab and the Pompée, as will be shewn by my next chapter: and the remaining *eight* express their disbelief of contagion, ascribing the fevers which they witnessed to the ordinary causes.

Having determined in *this* chapter to adduce, in preference to all others, the *unpublished* documents and proofs with which I had been favoured against the existence of contagion in the Ardent Fever, I have abstained from noticing several valuable facts lately produced and attested by Mr. Doughty, Surgeon to the Forces, in his Observations on the Yellow Fever in Jamaica and at Cadiz, and others published in the "Journal des Officiers de Santé à St. Domingue," in 1802 and 1803, and several others which were not mentioned in my Essay, but which greatly confirm all my conclusions on this subject. And as I believe that the testimonies and proofs which I

have stated here, with those in my Essay, are abundantly sufficient for my purpose, and as many others will incidentally be presented in treating of the occurrence of the fever at Cadiz, Gibraltar, and other parts of Spain, I shall, in this chapter, content myself with a few interesting facts and statements, taken from the excellent and important communication already mentioned from Dr. Fergusson, Inspector of Hospitals in the Windward Islands to Sir James M'Grigor, Director-General of the Army Medical Department.

"In regard to the contagion of Yellow Fever (says Dr. Fergusson), all those the best qualified to judge, that is to say, those who have spent their lives and devoted their services in the countries between the tropics, where it is so often present, are of the same opinion. I do not pretend to say, that amidst the heterogeneous mixture, of which our profession is composed, I have not heard some profess, and even subscribe to, a different belief; but I can declare that I never knew of a single instance of any one, provided he had experience of the disease, acting as if he believed in its being contagious, or taking any of the precautions which the instinct of fear, or the smallest degree of common prudence, would in that case have dictated."

"I presume it will not be denied, that those who

served during the war in St. Domingo, where so many British troops perished, must have had some experience of Yellow Fever. I remained there till the last, and saw the work of destruction completed. At first, every new comer, whether medical or otherwise, had his fears; or, I should rather say, had the firmest belief in contagion; but with none did that prejudice remain beyond the year. It vanished infallibly as soon as he saw, and had experience of, the disease; and I can declare that, during the latter years of our sojourning there, with hundreds of cases daily before our eyes, mixed in every conceivable way, with the surgical, the convalescent, and the healthy, I have never heard the idea started, nor do I recollect a single precaution, advice, or observation, that acknowledged the existence of contagion, ever being directed to the Medical Staff of the Army from any quarter."

——— ——— ——— "It is *now* (i. e. *October*, 1816) several years since the highest grade of the Remittent, or Yellow, Fever has prevailed *epidemically* in the West Indies, though sporadic cases have been constantly occurring; but *this* season (1816) it has visited, in a remarkable manner, some of the unhealthiest of the towns, such as *Pointe au Pitre*, and Basseterre in Guadaloupe; St. John's, Antigua; and Bridge Town, Barbadoes. In all, it has been confined for the most part to the towns, and, except at Bridge Town, to the unseasoned Europeans. *There* it extended to seasoned sojourners, even to Creoles

born, from the interior of the country, who, in the time of the insurrection, were obliged to resort to the town on military duty. The French strangers from Europe suffered more invariably and severely in Guadaloupe, than I had ever known any importation of similar numbers do in the British Colonies. In no one instance was an attack of *true Yellow Fever* seen amongst the British troops *there;* but if any one doubted of their suffering from the same exciting causes of fever, though modified from the circumstances of seasoning, he might, in the course of half an hour's ride any morning, *have satisfied himself of the identity of the three diseases* in the person of the *stranger* expiring in his lodgings with *black vomit* and hæmorrhages; the seasoned soldier at the hospital close by, probably doing the same, under the regular type of Remittent Fever, with a retentive stomach, and *comatose* brain; or the same description of subject, or planters of the country, undergoing the regular paroxysm of ague, in the *cooler* quarters of Voltier, or Beau Soleil Barracks, on the *higher* grounds immediately above the town."

"At Barbadoes the distinction and *identity* has been still more nicely marked. *There* a perfectly *raw*, newly-arrived regiment (the Queen's) came out from England in May, and were quartered at St. Anne's Barracks, on a comparatively elevated *table* of well-ventilated rocky land, at a time that numerous civilian strangers were arriving at the

hot unwholesome town," (Bridgetown) " within the distance of a mile. These last have been cut off in a great proportion by the very worst symptoms of Yellow Fever; but though the Queen's have lost * many men, from *severe well-marked Remittent* Fevers, only *eight* † of these, amongst the equally unseasoned military, arrived at the *highest* grade of Yellow Fever, as characterised by *black* vomitings, and hæmorrhages. Their fatal fevers have often approached very near to it, the remittent form *losing type, as it always does when aggravated,* so as to become ardent and continued, with constant vomitings; but except in the eight cases just mentioned, none of the others showed the diagnostic symptoms of the *very highest grade, which almost invariably were present* upon those that died in the town.‡ Here, within a very short compass, we see a *minute* modification of *cause,* operating

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* " The communication between the barracks and the town has been unrestrained by any regulation."

† " Several more cases of *black* vomit occurred after this paper was written."

‡ " The 25th and 60th Regiments have, in like manner, had numerous cases that died with a yellow, or fuscous livid skin, after frequent vomitings that were sometimes ropy and brownish, but never *black.* The colour of the matter vomited is not essential to constitute the diagnosis of this form of the fever; but the increase of the fluid ejected, beyond what has been taken in, more particularly if *glairy,* certainly is, and decidedly marks the gastric affection."

(or producing) as *minute* a difference in the *form* of the fever ; a *precise* one however, from which the inference is unquestionable, that the troops and their neighbours the civilians, were not on the *same niche,* but certainly within the same *pale* of morbid influence.* How long they are to preserve that station of health is doubtful; for there are marshes in abundance to leeward of St. Anne's, which in close sultry seasons like the present, have produced amongst the newly-arrived military, the very *worst* degrees of Ardent Tropical Fever : and here, as marking the nicer shades of mal-arious influence, and the *total absence of contagion,* it may not be uninstructive to remark, that, though the Queen's inhabited a large barrack-room *in common* with part of the well-seasoned 25th regiment, and the 60th regiment, they alone experienced any difference of health, between those that inhabited the *ground* floors and the *upper*-story † that no in-

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* " The difference of heat between the barracks and the streets of the town, as marked by the thermometer, has been seldom less than four, and often as much as six, degrees during all the hot sultry days of the present season."

† It appears, by an official report on this subject by Assistant-Surgeon Ralph, 2d Foot, that in the month of August the attacks of fever on the ground-floor were as one in twenty, and on the upper floor as one in thirty ; but that in September those on the ground-floor were as one in fourteen, and those on the upper as one in twenty-four. A similar occurrence in the *same* island is related at page 229 of my Essay. Facts of this nature afford

fection spread from their cases of *black vomit* amongst themselves, or to the comrades of the other regiments in the same barrack; that they have lost no hospital servants from fever, and have had scarcely any sick,* that all the white servants in the hospitals at Barbadoes, though engaged in attending many of the *worst cases of Yellow Fever that ever were seen*, as *landed from the navy*, have been healthier beyond all comparison, and suffered less from fever in proportion to numbers, than any other class of military in garrison; and that no case of fever has occurred in the person of any medical officer, excepting one, who, for five weeks *previously*, had been stationed at a healthy military post in the country, remote from the theatre of fevers, and from all hospitals.†

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very strong proofs that the prevailing fevers were caused by miasmata from the earth, those who were nearest to their source being the most severely affected by them, which would not have happened if the fever had been caused by contagion, but more probably the reverse, because the heated effluvia from the bodies of those below would naturally have ascended upwards, when they might have operated upon the men on the upper floor.

* "They have employed too, their full share of hospital-servants; their sick victualled in the General Hospital, being arranged there regimentally, and attended by their own medical officers and servants, under the direction of the Physician."

† "This remarkable exemption of the hospital-servants certainly goes to prove, what has so often been observed here, that when the body is charged with the elements of fever (whatever these may be), its immediate exciting causes are, exposure to the

In another part of this communication Dr. Fergusson, after proposing certain considerations tending to elucidate the rise and progress of Miasmatic Fevers adds, " Thus, to our imperfect knowledge, the *Epidemic* visitation, *this season*, of Yellow Fever at Guadaloupe, Barbadoes and Antigua, while it spared the equally unhealthy countries of Trinidad, Tobago and St. Lucia, and when imported into St. Vincent, could not remain there, may appear less capricious and unaccountable. It would have done the same to the writers on the fevers of Spain, if they had not dismissed all their doubts, and set inquiry at rest by attributing the whole to contagion; but had they inquired, instead of peremptorily deciding, they might have found that, during the epidemic visitations of these fevers, not only do towns and districts escape altogether, as we see

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heat of the sun, more especially when fatigued with exercise, getting wet, and drunkenness, to all which the hospital orderlies were less exposed than the troops in barracks."—By an official return which accompanied this communication it appears, that during the time in which *every fifth man* of the whole strength of the garrison had been attacked with fever, only *one in twenty-eight* of the whole number of orderlies employed in the hospitals (amounting to a hundred and ninety-eight) had been so attacked, and that only two deaths from fever occurred among these orderlies, giving a proportion of one death in ninety-nine. Dr. Menzies, Deputy-Inspector of Hospitals, by whom this return is made, has also stated as follows: " It is ascertained that of the orderlies discharged by their own desire, or from misconduct, none came back as patients. One of those returned as having died from fever, was a very drunken character."

at present in the West Indies, but that *different parts* of the same town are differently affected; and so limited often is their influence, that *one story of a house*, and one section of a ship, will be strongly affected by it, while other parts of the same tenements remain healthy; and then their wonder at what has been called the desultory progress of this fever, during the short course of an European autumn, need not have been so great, as to have appeared inexplicable through any source but that to which they referred it. At Barbadoes, our hospitals of late have been in a regular course of *importation of Yellow Fever from the navy*, but *not even inoculation** has been able to produce the disease upon any member of the Hospital Corps; by whom, I may truly say, that the sick have been received with open arms: for the anti-social doctrines of ideal contagion are not preached among us here, to the prejudice of duty and humanity. An example of the efficacy of *quarantine* laws, *where no contagion* exists, may here be amusing. At *Martinique* they established a *strict quarantine particularly directed against Guadaloupe*, and they have been consumed with Yellow Fevers;

* "Hospital-Assistant Moon, on opening the body of a patient that had died of very aggravated Yellow Fever, *wounded* his finger; the wound produced high inflammation, similar to what occurs in the dissecting-rooms in England, running up the course of the lymphatics to the glands of the axilla, with *symptomatic* fever, but no more."

but at Dominica, Tobago, St. Vincent's, &c. where they established *none*, they have not had, as far as I have learned, a single case; although at the last-mentioned island, both the Tigris and the Childers ships of war, imported distinct well-marked instances of the disease from *Pointe* au Pitre, on the evacuation of Guadaloupe. The first of the above ships continued healthy for nearly three months afterwards; the second has been the subject of a particular report."

Believing that the unrefuted, and irrefragable evidence formerly adduced in my Essay, with the very decisive facts and authorities which have been added in this chapter, will be more than sufficient to convince all whose minds are not invincibly prepossessed on this subject, and that, in regard to these, no additional proofs, however numerous, would prevail over prejudices so unalterable, I shall here *desist* from any further production of evidence against the supposed *contagion* of the Ardent Yellow Fever *generally*, though much remains to be adduced against the *particular* allegations of Dr. Pym and Sir James Fellowes, concerning the occurrences, causes and effects of this fever at Gibraltar, and in different parts of Spain.

But before I proceed any farther, I earnestly request my readers attentively to reflect upon the facts stated in this chapter; and especially upon the readiness with

which numerous medical men, respectable by their characters, their conduct, and their professional ranks, have come forward to make confessions which are generally felt as in some degree humiliating, by acknowledging that they had, when they first arrived in the regions of Yellow Fever, entertained opinions, deeply fixed in their minds by the ordinary course of medical education, which, however, after more extensive observation and better means of information, they had found reason to abandon as erroneous. and been forced to adopt conclusions directly the reverse, in regard to the alledged contagious nature of the Yellow Fever. This is stated to have been done by Dr. M'Lean, Dr. Fergusson, and their colleagues on the Hospital Staff at St. Domingo; it was done also by myself and almost all on the Hospital Staff in the Windward Islands, (see the letter of Mr. Young, Inspector-General, on this subject at page 335 of my Essay), it was done by Dr. Dickson, and, as he declares, generally by others in the circle of his acquaintance, and, beside many others, it will soon appear to have been done by Dr. Erly at Sierra Leone, on the very coast whence Dr. Pym and Dr. Chisholm pretend to derive their Bulam Fever. In all these cases, the change of opinion has been made spontaneously and disinterestedly, by the silent and gradual, but certain operation of truth; and without any desire to gain credit by a supposed preservation of many lives from a danger, which had no existence, and without any of

those views to promotion and reward, which may have produced some of the exertions and erroneous statements lately made, in regard to the fever under consideration. I moreover intreat my readers to consider and declare, whether in their judgment it be credible, or possible that the Ardent Fever should really possess the contagious power which Dr. Pym, and Sir James Fellowes have ascribed to it, and yet have not only manifested no such power, in many thousands of instances, but on the contrary have given in a multitude of cases the most decisive evidence of its being absolutely destitute of any such power, even when those morbid ejections, which are deemed to be the most mortal symptom, and the surest evidence of the disease in its highest violence, are taken into the system by inoculation, and into the stomach by being swallowed, or when their effluvia are received into the lungs by inspiration in close and heated rooms. Certainly, if these ejections and effluvia had possessed any contagious power, it must have manifested itself in such trials ;—and if that power does not exist in them, let the Contagionists, on whom the *onus probandi* necessarily falls, point out in what part or substance of the human body it does reside; and let them collect this matter and see if they can in a single instance produce the Yellow Fever by it. So many persons confidently believe that this contagion has no real existence, that I am persuaded many will be found ready to undergo any reasonable experiment or trial that may be proposed,

even with the most *concentrated Essence of the Bulam.* But if these Gentlemen can not in a single instance produce the Yellow Fever, and thus demonstrate the existence of its supposed contagious property, as those of the small-pox, of the measles, &c. are daily made evident, if all their proofs of contagion in the Yellow Fever depend upon misrepresentation, or are of such an equivocal nature that the facts or effects may be better explained by referring them to local miasmata, they should abandon their pretensions and abstain from farther controversy.

The immense mass of evidence *against* the notion of contagion in Yellow Fever, and the great *deficiency of it on the other side,* where, if it had existed, the proof would naturally have been *most obvious and abundant,* must, I think, soon completely destroy that notion. It has certainly weakened it in the minds of some of those who were, until lately, its most firm believers and supporters. Among these I may cite Dr. Hosack of New York, who had thoroughly imbibed the fables of Dr. Chisholm, and established a periodical work principally for their support. That he has yet avowedly and completely discarded those notions is more than I can pretend to assert; but I can say, that he has published such a modification of his creed in regard to contagion as, in my judgment, is equivalent to the renunciation of it; for, in his " Observations on the Laws governing the Com-

munication of Contagious Diseases," he states his belief that they are, "in general, communicable or contagious *only through the medium of an impure atmosphere;* the air being rendered thus *impure* by the *decomposition of animal and vegetable substances*, as in low *marshy* countries, or by concentrated effluvia, as in camps, jails, hospitals, or on ship-board." The diseases which he mentions as requiring this impurity of the atmosphere to enable them to manifest their supposed contagious power, are the "plague, dysentery, typhus fever in its various forms of jail, ship, hospital, or * lake fever, and *the Yellow Fever.*" The atmospheric impurity which he deems necessary for the propagation of the Yellow Fever is,† if I rightly understand Dr. Hosack, that which is

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* If Dr. Hosack had ever become personally acquainted with the plague, the typhus fever of the northern parts of Europe, and the dysentery of armies, he would not, I am persuaded, have included any two of these in the same class, nor would he have classed either of them with the Yellow Fever. The "Lake Fever" is purely and decidedly a marsh fever, occurring near the low grounds adjoining the numerous lakes within the United States, or those which form their boundaries.

† That I have not misrepresented the sense of Dr. Hosack will, I think, be manifested by the following extract, in regard to the Yellow Fever, also taken from his publication, viz. "The history of every visitation of this disease in the United States establishes this truth, that it is, or is *not*, contagious, depending on the qualities of the air to which it may be communicated. It

first mentioned by him as resulting from "the decomposition of animal and vegetable substances, as in low marshy countries." But as it has been proved, in thousands of instances, that this species of atmospheric impurity is of itself fully competent to produce the Yellow Fever, without the co-operation of any contagious principle, the supposition of the presence and agency of any such principle is useless, improbable, unphilosophical, and inadmissible. Upon the new and *untenable* position thus taken by Dr. Hosack, he admits and asserts many of the facts which have been employed to prove the Yellow Fever to be *devoid of contagion,* such as the impossibility of communicating the disease in the pure air of the country, where persons had sickened and died after imbibing the poison or miasmata in New York or Philadelphia, and their attendants had continued in health; and he says, "if it were necessary, I might go on to cite every return of the Yellow Fever with which the United States have been visited, to shew that the *progress of the*

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has not only *regularly* made its *first* appearance in our *sea-port towns*, and in *those* places *where the air is most impure* at *that season* of the year, and in *those seasons when such impurities acquire their greatest virulence;* in those houses which are most crowded with inhabitants, and where there is least attention paid to cleanliness: but whenever the same disease has been thence conveyed to other parts of the same city or town, or into the country, it either was propagated or extinguished, *according to the local circumstances of the place to which it was conveyed.*"

pestilential poison has ever been commensurate with the impurities of the atmosphere, and that, when sufficiently diluted with pure air, it ceases to propagate itself." But after the multiplied proofs which I have given of the production of the most aggravated forms of Yellow Fever in uninhabited or uncultivated situations, where no one did or could suspect the agency or existence of any pestilential poison or contagion, the assumption of this last *superabundant* cause, without any proof or appearance of it, must be deemed altogether unauthorized, and highly objectionable: and those who admit that this supposed contagion cannot act without the aid of such a state of the atmosphere, as does *of itself* notoriously and unquestionably produce the Yellow Fever, do, in effect, acknowledge that it has no existence.

I have already mentioned in this chapter that several of the gentlemen, whose Reports were transmitted to the Privy Council, and who denied the existence of contagion as a property originally or naturally belonging to the Yellow Fever, were, nevertheless, disposed to believe that it might acquire that property in certain circumstances and situations, particularly those in which sick persons are crowded together in close or ill-ventilated rooms or apartments, and where there is a manifest neglect of cleanliness, followed by an accumulation of filth, &c. That the air under such circumstances may be

vitiated and rendered unwholesome, so as to aggravate the diseases of those who are already sick, and to predispose those in health to sickness, when by their occupations they are compelled to breathe it, I am ready to admit. I am ready also to admit, and I believe it to be a certain fact, (as will be seen in my next chapter) that the putrefaction or decomposition of vegetable, and perhaps animal, matters in close unventilated places, such as the hold of a ship, will generate miasmata, which, in their effects at least, resemble the exhalations from marshes, and which, being commonly by confinement more accumulated and concentrated, may produce effects even more deleterious than those of any open marsh. But I perceive no sufficient reason for admitting that the fevers which are so produced are ever specifically contagious, or capable of reproducing a similar fever in other persons, who have not breathed the same or a similar atmosphere with that which originally produced the disease. In my Essay, between pages 102 and 156, the erroneous opinions which have very generally prevailed on this subject have been noticed, and, I conceive, sufficiently refuted by facts and arguments, which I earnestly recommend to the consideration of my readers, from a belief that, if duly weighed, they will scarcely fail to produce conviction in the minds even of those who have been, in a considerable degree, prejudiced on this subject, which is indeed the case of the greater number even of well edu-

cated and well informed medical men.* It is my decided opinion, and I have there stated facts and reasons to

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* Mr. Johnson, in a note at p. 163 of his work on the Influence of Tropical Climates, after mentioning a fever on board the Russel ship of war in the East Indies, adds, "when I say that the fever was generated in the Russel from filth, and became afterwards *contagious* from crowding, and dirtiness of the sick, I only state the opinion of the medical gentlemen who surveyed the ship and crew at Madras. But I do not hereby attempt to controvert the doctrine which Dr. Bancroft has taken such pains to establish of late; namely, that under no possible circumstance, can accumulations of filth, or the crowding of men, in sickness or in health, generate a contagious fever. I have read with great impartiality (perhaps with some bias in favour of the author, from the great congeniality of our sentiments on most other subjects) his arguments and elucidations, but I cannot say that conviction has followed. It is true that, by great ingenuity and elaborate research, he has succeeded in throwing an air of improbability, or even of ridicule, on certain medical histories and opinions connected with the above, which still pass undoubted, and are fully believed by nine-tenths, or rather, the whole of the profession. But let it be remembered, and deplored, that proofs of holy writ have too often shared the same fate! On considering the above doctrine in all its bearings, I cannot see a single useful purpose which its undisputed establishment could answer; but I know that it is calculated to produce numerous evils. Dr. Bancroft seems a little apprehensive of this himself, for he observes (p. 102), "I have no desire to weaken any of the prejudices which tend to promote cleanliness, &c.;" and again, "I flatter myself we shall all find within ourselves sufficient motives to remove, or avoid filthiness, even when convinced that it does not produce contagious fever." Alas! does Dr. Bancroft forget what the poet says of a still greater evil than dirtiness?—

> 'Vice is a monster of such horrid mien,
> That, to be hated, needs but to be seen;
> But seen too oft,' &c.

support it, that *specific* contagions are each and severally *creations* of *divine wisdom and power*, as *distinctly*

"Let us beware, then, of imitating the infidel, who, by subverting what he is pleased to term prejudices,

> 'Robs us of that which not enriches him,
> And makes us poor indeed.'"

The application of the latter part of Mr. Johnson's note I do not understand, nor the connexion which it had in his mind, with the generation of contagion by filth and crowding. Does he mean to inculcate, that though contagious fever cannot be generated by the means before-mentioned, the "undisputed establishment of this *truth*" (connected, as it certainly is, with others of high importance to medical science, and the health of mankind) would not only prove useless, but a source of "*numerous evils*," by making us culpably inattentive to cleanliness, and that nothing can hinder us from becoming so, but a groundless and fallacious *dread of contagious fever;* and, that by annihilating this dread, I shall rob him and others of something, without which, they must be "poor indeed," and that I ought therefore, in writing on an important medical subject, to have suppressed the truth, and inculcated the belief of falsehood? Does he believe that there are no sufficient motives to remove filth and nastiness from us, without the apprehension of contagious fever? Have I, in allaying that apprehension, pretended that no other morbid effect could result from filth, and the foul atmosphere produced by it, particularly in the holds of ships, &c.? or did I not, in my last Appendix, ascribe to this cause the *worst* of the fevers to which that Appendix had a relation? Does Mr. Johnson conceive that Divine Wisdom has made the belief of error and falsehood necessary to the promotion of virtue, and the preservation of good manners and good morals, as well as of health? And on the other hand, does he think that no harm is produced by inculcating erroneous doctrines respecting the origin and cause of the most dangerous of fevers, those which, during several years, have

and *designedly* exerted for *their* production, as it was to create the several species of animals and vegetables around us;—that *each* has preserved its *specific* character and properties unchanged by any commixture with other morbid or morbific matters in the many thousands of human bodies through which it has, in the course of generations, been transmitted and continued to us;—that, as the conservation and propagation of these contagions indispensibly require the *co-operation* of the *living* body through which they are successively transmitted, no means which *human* wisdom can devise, no combination of dead animal and vegetable, or of mineral substances, whether produced fortuitously, or by the most judicious and elaborate process and contrivance of human art, will ever be able to generate any one of these specific contagions any more than it will be able to produce a horse, an eagle, or an orange-tree. Does any man believe that he can, by any art and means, generate the contagion of small-pox, or of measles, or of any other disease known

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almost desolated the southern parts of Spain, and thereby misleading its government in regard to the proper means of removing their cause, and hindering their recurrence? Does he consider as *trivial* the other *deplorable mischiefs* produced in Spain by *false* doctrines on the subject of contagion, which are described at pages 492-495 of my Essay?—With these mischiefs Mr. Johnson is certainly not chargeable; but their having occurred is *one among many proofs of the serious and extensive importance of medical truths.*

to result from a specific contagion? and if not, how can he persuade himself that such generation can be effected by *fortuitous* mixtures and accumulations of animal and vegetable matters, however impure? An unwholesome atmosphere, in a particular unventilated or close situation, may be produced by various means, and that atmosphere may become the cause of fever, or of another disease; but that disease will never possess the power of reproducing itself, or a similar disease, in another person, unassisted by an atmosphere similar to that by which it was originally produced. And from the *ingenerable*, *peculiar*, *inimitable*, and *unalterable*, nature of each specific contagion, we may conclude that no disease, to which a specifically contagious property does not *originally* and *naturally* belong, will ever acquire that property by the means under consideration; and, least of all, those fevers which are produced by exhalations from the earth, and which, *without any such property*, are found to prevail so *extensively* and *fatally* in all warm climates, that if they could acquire a contagious property besides, all intertropical countries, at least, must soon be either deserted or depopulated.

Even the late Dr. George Fordyce, who, until near the close of his life, had taught and asserted that *all fevers* possessed a contagious property, was at last brought, in his Fourth Dissertation on Fever (the last

published before his death), to relinquish this opinion in regard to the semitertian of hot climates, by which term he designated the Yellow Fever, and to admit, " 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 inhabitants of those towns," in which the hospitals were placed. If the Yellow Fever ever could acquire a contagious property, it doubtless would do so in hospitals appropriated exclusively to the reception of persons labouring under that disease; but experience has invariably proved that such hospitals in Philadelphia and New York never afforded the smallest reason to suspect that any such property had existed, or was acquired in them. But a fact, which is *unambiguous* and *decisive* on this point beyond all others within my knowledge, is, that which I have lately adduced (p. 186) upon the official testimonies of Mr. Inspector Fergusson and Dr. Menzies, as having occurred in the military hospitals at Barbadoes so recently as in the last autumn (1816), when a hundred and ninety-eight orderly men, who were constantly occupied in ministering, in every possible way, to " the worst cases of Yellow Fever," and were inhaling, day and night, an atmosphere impregnated with their breaths, and the emanations from their bodies, were nevertheless so remarkably exempted from fevers, as compared with other soldiers who were quartered at a distance from the sick and the hospitals, that

while every sixth man of the latter was seized with the prevailing fever, only one in twenty-nine of the orderlies suffered by the disease, and but two of those attacked with fever died—this, too, during an *epidemical* visitation.

In regard to the generation of contagion by the accumulation of persons who are *not* sick (although the facts related between pages 118 and 137 of my Essay cannot require any addition to impress conviction on this subject), I will cite the experience of Dr. Dickson during the *seven* years in which he had the superintendence of the British Fleet in the Windward Islands, who states in his Report to the Transport Board, that "the prisoners of war, though *often much crowded* in the *prison-ships*, were generally healthy; not as Mr. Pym supposes from being out of the way of contagion, but partly from their being generally of a more reduced and *assimilated* habit, and particularly from their being unexposed to the exciting causes, to avoid which, is to avoid the endemic fever."

But whether it be thought possible or impossible for a disease, not naturally contagious, to become so by filth or crowding, the character of the Yellow Fever, as a non-contagious disease, will not be affected by either of these conclusions: for, as Dr. Dickson has observed in one of the letters with which I was favoured by him, "if such

additional applications and means be required to give the fever this power of propagating itself, such power is adventitious, and the want of these auxiliaries, these fermentative and assimilating processes, is a proof that the disease is not of itself infectious. If it become such only by such auxiliaries, and ceases to be so when they are removed, surely in its simple and legitimate form it has nothing inherently contagious in its nature." But, considering how repugnant it would be to the well-known simplicity, uniformity, and constancy, of the operations of nature, it does not to me seem credible, that a property so extraordinary, so obviously the result of a *special purpose* and *provision* of our great Creator, should be *occasionally acquired*, and afterwards *dismissed*, by any disease, not originally endowed with it; especially as the evils which might be thus produced are incalculable.

CHAP. IV.

HAVING reason to believe that the Yellow Fever has been much more frequently produced than is generally imagined by miasmata resulting from the decomposition of vegetable and other matters in the holds of ships, and in other confined or unventilated situations, and that this cause of fever has often created the appearance and belief of contagion where none existed, I have thought it might be useful to make it the subject of a separate chapter.

At pages 104 and 105 of my Essay, when contesting the possibility of generating or producing a *contagious* fever by the noxious vapours emitted from "those fortuitous and ever-varying collections of unclean or putrefying matters, commonly denominated filth," I have admitted and declared that they may, as in the form and "*instance of marsh* effluvia, produce diseases, including *fever;*" though "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." And at pages 795 and 796, I have dis-

tinctly ascribed the violent cases of Yellow Fever produced in the seamen sent from the Hussar frigate on board the French ship *Raison*, to the noxious or foul air resulting from the decomposition of such matters, and have referred to a considerable number of instances and proofs of the production of similar fevers, by the exhalations from foul ballast, and decomposing vegetable and animal matters, retained in the holds of different vessels, &c. But notwithstanding these plain intimations of my belief on this subject, it has been supposed, and reported that, in denying the production of *contagious* fever by the causes or decompositions just mentioned, I had also intended to deny the power of these causes to produce *fever of any kind*—than which nothing can be more adverse to my opinion. For example, Mr. Mortimer, lately Principal Naval Medical Officer at Barbadoes, in his Report to the Transport Board (noticed in my last chapter), says, "we are of opinion, *in contradiction to that stated to be Dr. Bancroft's*, that vapours emitted from the holds of ships, having in them commixtures of various animal and vegetable substances in a state of putrefaction, are quite sufficient for the production of the *worst* kind of Bulam Fever;" * that to such

* In another part of his Report, Mr. Mortimer gives the following explanation in regard to his error respecting my doctrine on this subject. He says, "I have not had the advantage of perusing

effluvia may justly be attributed much of the depopulation occurring among sea-faring Europeans in inter-tropical climates. We are borne out in this opinion from the numerous examples happening in our own practice, a few of which have been related, and others made known to us by men of indefatigable research, and of unquestioned veracity."

A similar erroneous supposition seems to have induced Dr. Burnett to say at p. 476 of his work, in regard to a fever on board of some transport vessels, "in this opinion I differ both with Dr. Bancroft, and the convenient admission of Mr. Pym."

As many of the articles forming the lading of vessels, and most of those provided for food, are susceptible of putrefaction or decomposition, and portions of them are continually breaking, falling down, and collecting among the ballast, and in various parts of the hold, it is to be expected that, whenever these substances shall there find the requisite degree of moisture (which, being

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Dr. Bancroft's publication: he is stated by Mr. Pym to deny the power of offensive vapours emitted by matters in a state of decomposition to produce fever." This certainly is a very incorrect statement; for I have constantly deemed and represented such vapours to be the most general and most productive of all the causes of fever.

moderate, may frequently be supplied by leakage, as well from water-casks as from the ship's sides, for even the best conditioned vessels are seldom completely secured from leaking), they will naturally undergo that peculiar fermentative or decomposing process by which febrific miasmata are produced. In ordinary circumstances, and in mild temperatures, these may be of little consequence; but if similar matters be allowed to accumulate, and if a sufficient warmth be at the same time afforded, as happens in intertropical climates, it is not surprising that, in situations so confined as the holds of ships, especially if the ballast be itself suitable for the purpose, the miasmata engendered under the latter circumstances should acquire a very noxious degree of concentration. It is indeed well known to all who are acquainted with nautical affairs, that the mass of filth produced and collected in vessels in the manner just mentioned, and particularly between the limbers, after *long* or repeated voyages, is, upon examination, commonly found to be large in quantity, and highly offensive by the vapours emitted from it.

Sir Gilbert Blane (see Medical and Chirurgical Transactions, vol. vi. p. 507) observes, that "one of the most prolific sources of foul air and bad smells in ships, has been the putrescent matters absorbed and retained by the gravel, sand, and other *earthy* substances heretofore used for ballast:" and he afterwards mentions (p. 510) those

cavities under the floor of the hold, which used to be receptacles of filth, and of all manner of vermin dead and alive, proving perpetual reservoirs, and resources of foul air and offensive exhalations; and afterwards, having noticed "the unwholesomeness belonging to bilge-water," he adds, that the *well* of a ship has "become so contaminated, as in innumerable instances to produce instantaneous and irremediable suffocation." And in the same communication he mentions, as one of the causes of the sickness which occurred in Lord Rodney's fleet at Jamaica in 1782, "the clearing and fitting of the French *prizes* then in an extremely filthy state." In this way he supposes they caught the Yellow Fever, which I also believe, but without believing as he seems to do, that it possessed any contagious quality. When these facts are duly considered, the production of the worst forms of miasmatic fever from this cause will not be thought surprising or improbable. Accordingly, Dr. Dickson in his beforementioned Report to the Transport Board, says, "the power of *impure*, but strictly *local*, effluvia in producing Yellow Fever on board ships also, as well as on shore, is unquestionable: I have seen it repeatedly arise from a foul state of the hold, and notwithstanding every attention, *continue* till this has been rectified, and then, and not till then, did the fever cease; as was instanced in the Blonde, Gloire, Star, Wanderer, and many others, and particularly in the *Dart*, in April, 1807, from the offen-

sive matter at the bottom of the water-tanks. So many people were taken ill, after going on board this vessel, lying guard-ship at Barbadoes, that it was difficult to account for it, except on the principle of contagion, until the *peculiar construction of the ship*, viz. her being divided into compartments below, so as to prevent the circulation of air, and the stowage of water in *bulk*, were adverted to: and, on examining the large tanks or cisterns, their bottoms were found covered with an offensive slimy mud, or deposition. On the 17th of May, I find it noted in my Registry, that the bulk-heads had been knocked down, and the cisterns cleaned out; and on the 24th, that "*for the last week no fresh attacks had occurred on board the Dart.*" In the Thetis, in 1809, the Report states, that the fever did not appear until the *hold* was broken up, when about a dozen of men so employed were taken sick, and that four out of five carpenters, who lifted the limber-boards, *died*. In his account of the fever which prevailed on board the Nyaden, the surgeon observes, "in clearing the after-*hold*, which was very offensive, several men immediately took the fever, some of whom died." This effect is the more evident when contrasted with the healthiness of some vessels *close* to them, which were either accustomed to the climate, or differently employed."

In my preceding chapter I have, on Dr. Dickson's

authority, stated a remarkable instance of the production of Yellow Fever, accompanied, in twenty-two cases, with *black vomit* and consequent *death*, on board the *Circe* frigate, principally from the duties of *clearing the hold*,* *and heaving down;* by which so many of the ship's company were soon after attacked with this fever, that a hundred and forty-six men were sent to the Hospital at Antigua. In another place, Dr. Dickson observes, that impure effluvia will be most apt to be generated in a *new-built ship*" (probably from the same cause which renders water so highly offensive in *new casks*), " espe-

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* In one of the letters with which I was favoured by Dr. Dickson, he says, " I could cite numerous instances of fever occurring on clearing the holds of ships, particularly in the *hot* months; indeed, this has been so general, that it ought never to be performed in the West Indies, when, by sending the ship out of the country, it is possible to avoid it; and I have the gratification of thinking that my earnest representations against heaving down ships in that climate, have occasioned a considerable diminution of this mortality." And in confirmation of this opinion, I beg leave to add, that many facts and considerations induce me to believe that no ship of war ought to be sent on service between the tropics, without a previous and thorough clearance of her hold; and that none ought to remain in such a station more than eighteen months or two years, without being ordered back to a *temperate* climate, *there* to undergo a similar clearance, it being, as I am convinced, generally true, that whenever the Ardent Fever has become prevalent in ships which do not communicate with the shore, and are not within the reach of miasmata proceeding from it, the cause has existed in, and been generated by, the foul state of their *holds*.

cially if built of *green wood*, or where the *shingle* ballast has not been restored for a length of time; or had not originally been carefully selected." And he observes that if such exhalations as were produced in the Dart, "be admitted to occur occasionally in a man-of-war, where cleanliness is proverbial, it is easy to perceive that, by the agency of heat and moisture, they may, under particular circumstances, in a transport, or a merchant-ship, become so abundant and concentrated, that the hold, with a very figurative expression might be denominated a *ship-marsh*."

In addition to these instances, I will adduce one stated by Mr. Johnson, at p. 164 of his volume, and which he seems to have erroneously considered as being at variance with my doctrines.—He says, "in the month of February, 1806, Yellow Fever of a very severe description, broke out in His Majesty's schooner *Trinidad*, at Barbadoes, which was evidently occasioned by the stench arising from dirt, filth and stagnant water in her hold; for as soon as the latter was washed and cleaned, and scuttles cut for better ventilation, the Fever disappeared. Dr. M'Arthur, Physician to His Majesty's Naval Hospital at Deal, can certify the truth of this statement."*

* The seven cases of Yellow Fever which occurred at Marseilles in the persons of the first and second officers and of five sea-

The Contagionists at New York and Philadelphia have on several occasions ascribed the production of Yellow Fever in the United States, to vessels said to have arrived from the West Indies *generally*, without naming any particular port, and much oftener without ascertaining the prevalence or existence of Ardent Yellow Fever, at the port whence it is supposed to have been imported. It has happened too, that the vessels said to have imported this contagion, have arrived with their crews *in full health*; but on coming to their moorings, or rather to the wharfs at which they unloaded, and breaking bulk, they have emitted very offensive vapours or exhalations, and persons living in the adjoining houses, or working in spots contiguous to these vessels, and especially those going on board, have not only complained of these nauseating or disagreeable effluvia,

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men belonging to the American ship Columbia, from Providence in Rhode Island, of which a full and detailed account is given by Dr. Segaud, one of the Physicians by whom they were attended, in a letter to Professor Fouquet, dated Marseilles, 21st Brumaire, an. xi., appear to have been produced from the *cause* under our consideration.—Of these seven persons, six died with *black* vomiting, on the seventh day.—Being dissected, their stomachs were found inflamed, and they all contained black or coffee-coloured matter. The two officers sickened and died at their lodgings on *shore*, but no communication of disease took place in any of the Physicians who met in consultation upon their cases—nor in those who visited the seamen on board the ship—nor in those persons who performed or was present at the dissections,—nor in any of those who attended or visited the sick officers on shore.—For this Letter see page 395 of Professor Berthé's volume.

but have been very soon attacked with the Yellow Fever in the severest and most fatal forms. After reading with attention all the affidavits and other documents published by Drs. Hosack and Francis, in the second volume of the American Medical and Philosophical Register, between pages 278 and 331, respecting the production of Yellow Fever at the *Wallabout*, a small village in the neighbourhood of New York; also the account (vol. i. p. 253—283) of a similar production of this fever at Brooklyn, another village opposite to and only separated from *New York*, by the Hudson's River; together with the account of another production of it, at Perth Amboy, in New Jersey, (as related vol. iii. p. 97—103 of the same work) I am fully convinced that *every individual* of those who were attacked at these several places, derived his or her *fever* from the noxious vapours emitted from the holds of the several vessels (or some of them) which were supposed to have received or imbibed contagion while *floating* in the atmosphere of a West-Indian harbour; where it is notorious that neither the air of a chamber containing persons dying of the worst form of Yellow Fever, nor even that of the wards of an hospital filled with such persons, is capable of producing the disease.* And I am fully con-

* To account for the non-communication of the Yellow Fever by persons who sicken and die of that disease at places in the country, Dr. Currie of Philadelphia (see his Letter to Dr. Hosack in the American Medical and Philosophical Register, vol. i.

vinced by a multitude of facts, that whenever the occurrence of this fever has in any one place, been connected with the arrival of a vessel from any other place, it has *depended exclusively upon the emission and operation of noxious exhalations or miasmata produced in the holds of such vessels by the means and causes lately described:* and this observation I would particularly apply to the supposed importation of the Yellow Fever at Martinico in the year 1686, by the *Oriflamme* and other ships from *Siam*.

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page 195, & seq.) says, " the contagion of Yellow Fever like that of Typhus or Jail-Fever, requires to be *accumulated* or *concentrated* in the atmosphere, in confined unventilated situations, in order to render it capable of producing disorder in those exposed to it."—But what reason is there to believe that this supposed contagion might not accumulate in the bed-chambers of those who have died of this disease in the *country*, as well as of those who died of it in New York and Philadelphia, or that it did not so accumulate in the *Yellow Fever Hospitals*, *at a little distance from the cities*, in which there was however, no *appearance* or *effect* of contagion? And if this accumulation and concentration be, as the contagionists alledge, absolutely necessary, how can they pretend that a vessel by *merely* lying in the harbour of an island in the West Indies, where perhaps a few cases of Yellow Fever have existed on shore, *at a considerable distance*, could have received into her *hold*, such a portion of contagion, *diffused in the common atmosphere of that harbour*, as shall suffice, when the vessel afterwards reaches the United States, or any other distant Port, to produce an epidemic fever in the town or city where she unloads? Does the atmosphere of any harbour in the West Indies, ever contain such a portion of concentrated contagion as is by them said to be necessary for the production of Yellow Fever?

I must add that in many of the before mentioned instances the crews of the suspected vessels were healthy; and there certainly was no communication of the fever by any of the persons who took it from the miasmata emitted by the vessels, at the *Wallabout*, or at *Brooklyn*, or at *Perth Amboy*; though several of these persons sickened, and died at New York, some of them in *crowded lodging-houses*. This was particularly the case with those who imbibed the noxious miasmata at *Brooklyn* in 1809 (see the New York Medical Repository, vol. xiii. p. 199), where the sickness produced was more *extensive* than at either the Wallabout in 1804, or Perth Amboy in 1811; thirty persons having died of the fever derived from that source, most of them with *black vomiting*, &c.; and yet it is stated, in the best authenticated accounts, that all who were attacked, either *lived* or *worked* within a circle of two hundred yards *semidiameter* from the *Concordia*, the vessel accused of this morbific importation; indeed most of them had been on board, and probably all *very near to her*. Drs. Hosack and Francis endeavour to account for the non-communication of this fever (on the supposition of its being contagious) by any of those to whom it proved fatal in New York, by saying, "It was because, in *Brooklyn*, the atmosphere was affected with that *peculiar kind of impurity*, arising from its being *assimilated to the air in the hold of a foul ship*; while in *New York*, there was no such ship, no *Concordia*, *poison-*

ing us night and day by *pumping out her foul* and *infected bilge-water*."—See the New York Medical and Philosophical Register, vol. i. p. 279. But surely all this may be explained in a much more *simple*, *obvious* and *probable* way, by ascribing these effects *exclusively* to the noxious miasmata emitted from the Concordia, than by supposing that they only co-operated, in giving *effect* to *contagion*, of the existence of which there is neither proof, nor probability. These miasmata from the holds of ships have in many other instances been found capable of producing the most aggravated Yellow Fever, without the aid of contagion; but the fevers produced by them were not contagious; and concluding, as there is every reason to do, that this was true of the fever which occurred at at *Brooklyn*, the Wallabout, and Perth Amboy, we have an obvious reason, why no fever was communicated by those who died at New York, after imbibing the morbific miasms at Brooklyn, and the other places just mentioned.

That the fevers mentioned by Mr. Johnson to have occurred on board the Russel, the Sceptre, and the Centurion, in the East Indies, originated from, and were continued by, a cause similar to that under consideration, appears to me highly probable from several of the facts stated by him.—Indeed, he says, in regard to the Russel, that he has "good reason to believe that it was *self-gene-*

rated, in consequence of great negligence and filth in various departments;" but that "it soon *acquired* the power of propagating itself," &c.—I have already stated the reasons, which convince me that such an *acquisition must always be impossible.**

I shall conclude this chapter by an account of the remarkable events which have lately occurred on board the *Regalia* transport, and by some observations that were obviously suggested in considering these events. My information on this subject is derived from a report made to the commander of the Forces in the West Indies, on the state of the Regalia transport, dated the 26th of September, 1815, by Dr. Fergusson, Inspector of Army Hospitals, from answers made by the same gentleman to certain questions proposed to him on this subject by Sir James M'Grigor, Director-General of the Army Medical

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* In regard to the fever which lately prevailed on board the Chichester store-ship, Staff-Surgeon Doughty says, p. 212, 213 of his volume, that the whole of the persons on board had been exposed to the *endemic cause*, before she sailed from Port Royal (Jamaica), where the "Yellow Fever was prevailing in a virulent and destructive degree to my knowlege." I have no reason to dispute this statement.—But should it ever be made probable that the fever in question did not originate, as Mr. Doughty supposes, I should be disposed to look for its cause in the state of her hold, which, after a *considerable stay in the West Indies*, might well contain miasmata, similar to those which have so often occasioned fever in foul ships between the Tropics.

Department, and from a *Sequel* to those Answers; together with certain Official Documents, contained in an Appendix to that Sequel; and moreover from a letter written to the Commissioners for the Transport Service, &c., by Mr. Mortimer, Principal Naval Medical Officer at Barbadoes, dated October 14, 1815, and also from this gentleman's Report to the said Commissioners, respecting Dr. Pym's publication: of all which I am permitted to avail myself.

The Regalia transport left Guernsey in November, 1814, and, after a stay of some weeks at Plymouth with soldiers on board, sailed first to Cork, and thence to the Coast of Africa, where she arrived on the 15th of February, 1815, "full of troops in perfect health." She was employed from this period until the latter end of June in removing troops and stores from one post to another: at which time she replenished her hold with water, &c., and with a supply of *green*-wood for fuel, which was "*cut down* and *brought on board the same day*." There were embarked about the same time, in the Regalia and three other transports, seven hundred and ninety three black recruits, described as being "mostly young and uncivilized Africans;" the exact number sent on board the Regalia does not appear, but "many of them were embarked *sick* from the Hospital, with ulcers, fluxes, &c." These men remained on board about three weeks, when the vessels

quitted the river Sierra Leone, on the 18th July, and sailed for Barbadoes. A Medical Officer had been appointed to accompany them, but was hindered by sickness. The medical supplies sent on board for their use appear to have been scanty, "the want of medical stores having been assigned as a reason for sending sick from the hospitals to the ships." Nor was the diet for them suitable, being suddenly changed "from one wholly vegetable to the ordinary ship's rations, which were served to them during the voyage." It seems that the transports arrived at Barbadoes a little before the 24th of August, and in the passage a considerable mortality is represented to have occurred, as well among the negroes as among the crew. The causes just mentioned in regard to the *former* proved very injurious to their healths, fourteen of them having died on board the Regalia, and thirty-eight on board the other transports; and "one hundred and fourteen sick, labouring principally under dysentery, dropsy, ulcers, and cachectic leuco-phlegmasia," having been sent from the vessels collectively to the hospitals at Barbadoes on their arrival. It is further stated, that in the two ensuing months, from the 24th of August to the 24th of October, there were admitted "in the whole two hundred and eighty-eight of those men, of whom seventy died, viz. fifty-three of fluxes, five of dropsy, nine of pulmonic complaints, one of ulcers, and two of fever," the great majority of whom appear to have been from among those brought by the Regalia, it being stated that from them

"one hundred and eleven fluxes were received into hospital," and that "fifty-three" of these cases proved fatal, "independently of those who died on the passage." —"From the black recruits that the Regalia imported," says Dr. Fergusson, "a most putrid malignant dysentery was introduced into and filled our hospitals; but the disease did not in a single instance * infect any medical attendant or servant employed about the sick."

The sufferings of the whites on board of the same vessel were, however, of a different, and even of a more fatal (and, in a medical point of view, more interesting nature). The crew had remained healthy until the 13th of July, five days before they sailed from Sierra Leone: but a boy was then seized with fever, who died about the third day;

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* "They were received (says Dr. Fergusson, in another part of his Report), into our hospitals without any extraordinary precaution, fear, or scruple; for, though the medical schools of Europe have rung for centuries with assertions of the contagion of dysentery (about which, as their Professors seldom served in armies, or lived amongst organized bodies of men, they could know very little) every Regimental Surgeon who has served a campaign, or Surgeon of a militia regiment in an autumnal encampment on any of the downs of England, knows well that, however practicable it may be, through an undue accumulation of sick and neglect of cleanliness and ventilation, to propagate a *typhoid* contagion under the leading forms and features of dysentery, ulcer, pneumonia, &c.; dysentery itself, under all ordinary circumstances of accommodation, is no more an infectious disease than hæmorrhoe or catarrh." These sentiments accord with mine, as published in my Essay, p. 515.

and two died of fever the day after sailing. "The men continued to fall ill one after another on the passage, until all except the cook and one boy had suffered attacks, and five out of twenty-one died before her arrival at Barbadoes."—"A military officer and his lady also, who had been some time on the coast and had embarked in perfect health with the recruits, were taken ill with the same fever, and both fell victims."—"The Captain's wife died after making the harbour (of Barbadoes, where the ship remained four days to take in water), and the Captain, Palmer, immediately sickened and died on the passage to the Saintes." The ship remained two days at these islands, and then proceeded to English Harbour, Antigua, and "underwent fumigation, without the least effect in arresting future attacks, or their fatality;" and after a detention of three days she returned to Barbodoes. During this short voyage a mate, who had been shipped at Barbadoes from another of the African transports reported to be healthy, sickened and died; a boy shipped at the same island from the Lord Eldon, (then a perfectly healthy transport) fell ill and was sent to the hospital; and also an apprentice to the ship. On the Regalia's returning to Barbadoes, "a former mate (then of necessity Captain) fell ill, and anxious for his own life, quitted the ship" to go on shore, and recovered; "a new mate, shipped from a healthy Newfoundland vessel, was, on the ninth day of his being on board, sent to the hospital with

fever," and "another man* taken ill on the same day, but not sent to the hospital till twenty-four hours had elapsed, died there."

In consequence of so many losses Mr. Mortimer states that he made known "to the Agent for Transports his opinion of the Regalia's condition, and of the necessity of taking every thing out of her hold, and exposing it to the certain effects of concentrated heat from stoves, the hatchways being closed;" and also that "to the authority and weight of representation from Dr. Fergusson, Inspector of Army Hospitals, may be attributed the prompt clearance of the vessel, and, he fully believes, the consequent safety of many lives."

"About the time however when the crew commenced cleaning the hold, the Regalia shipped three fresh men, one of whom was seized with the fever and was sent to the hospital, and another died at Guadaloupe of the Yellow Fever on her passage to England." The ship's cook also, upwards of fifty years of age, who had been healthy

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* This was the case reported by Deputy-Inspector Forbes, in Appendix, No. 3, of James Champion admitted into the Hospital at Barbadoes, from the *Regalia* transport, with a fever, which appears to have *remitted*.—Before his death, he had "*constant vomiting: the fluid exhibited a coffee colour.*"

up to the period of cleaning the hold, was then seized with the fever, and died at Barbadoes.

"All who died were affected with vomitings and bleedings from the mouth, nose and other places."

"From the Documents produced as an Appendix," says Dr. Fergusson, in his Official Report, "it will be seen that the Regalia, transport, on her arrival at Barbadoes, was not put * under any restraint or quarantine, but communicated freely with the sea-ports "of Barbadoes, the Saintes, Antigua, and Guadaloupe; landing the severely ill or dying subjects of that disease amongst the inhabitants, and at the hospitals of Barbadoes and Antigua, without communicating any infection at any of these places." Finally, "after having undergone a purification, she sailed from Guadaloupe crowded to a very great degree with French prisoners and their families from the *jails* under

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* This is stated to have happened "through an oversight, while the army was absent at Guadaloupe."

In his *Report* Mr. Mortimer states that when the Regalia arrived at Barbadoes, it was believed she had imported a highly pestilential disease, and that "if quarantine laws had been in force, she would assuredly have been interdicted all communication with the shore; and consequently more lives would have been lost, for want of such preventive and curative measures as, when at length happily adopted, were found to be successful."

the most *dangerous circumstances to health*, with a case of * *Yellow Fever* actually *dying on board* the day before she left Basseterre Roads, but without communicating any such fever to the unfortunate passengers, † or leaving any fever behind her in Guadaloupe, or importing any at the ports she ultimately reached."

"As the question (continues Dr. Fergusson), will naturally next arise, how such a fever as that which destroyed so many of the crew of the Regalia, and attacked almost every one that came on board of her, to supply the the place of those that had perished, could spread so unerringly, and prove so destructive, without being infectious, I shall enter into it at present. The quantity of green wood laid in at Sierra Leone on board the Regalia for fuel must have been very considerable; for, after she had

* A Report of this case was made by Acting Staff Surgeon Ayton, by which it appears that the subject of it, a stout young man, had black vomiting and died.

† "The first Yellow Fevers that were heard of at Guadaloupe (says Dr. Fergusson) occurred at least five months afterwards, amongst the *civilian strangers* newly arrived from Old France; and these, while the British retained possession of the island, never extended to the seasoned inhabitants and troops in garrison; with the exception of some *white* officers of the black corps that garrisoned Pointe au Pitre, who, at the very time of the evacuation of that place (not so much as three months ago) were seized with the Yellow Fever, and died of it in St. Vincent, without communicating the disease to any person whatever."

been several weeks in the West Indies, there were still as many tons of it left, as, in the master's opinion, would serve for a voyage to Europe.* The *ballast too* had never been changed or shifted from the time she left England, nor *for any discoverable time before.* It was what is called *shingle* ballast, small stones with a considerable mixture of mud and other impurities; and when I examined it on board the Regalia, it had been much

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* Mr. Mortimer appears to have coincided in opinion with Dr. Fergusson in regard to the causes of the fever on board the Regalia. "Experience (says he), had brought before me on several occasions the fatal influence of *foul-air* on the health of a ship's company—it had taught me to believe that (for the greater part) the devastating causes of fever are local; and as so many vessels were lying at anchor along-side, with their crews perfectly free from disease, and fever had not carried off its victims from any of the other transports employed on the same service, I could not but suspect the wood taken on board the Regalia to have had a baneful influence:—it was stowed chock-forwards, the spare cables abaft, and abaft these, small-gravel-ballast, looking extremely dirty." He adds that from the manner in which this ship had been employed, "from November to August, it could not be unreasonable to suppose that, amidst this mass, much animal and vegetable matters had been commixed."

Dr. Fergusson likewise declares, "that no Yellow Fever existed *at that time* at Bridgetown (Barbadoes), amongst the ships in the bay, or the troops in garrison; the *first* case of Yellow Fever that was heard of in the town occurring about the beginning of the succeeding year, in the person of a youth, a stranger from Europe, and belonging to one of the mercantile houses; and that for more than twelve months after the arrival of the Regalia, none but *insulated sporadic* cases of the disease were known in Barbadoes."

fouled by leakage from the water-casks. The ship in respect to leakage was far from being a dry ship, and from that circumstance, might, with better ballast (of iron or large stones) have proved a very healthy one; but the absorption of the sea-water amongst foul-ballast and green-wood could scarcely fail to prove unwholesome.* In other respects the Regalia, in all her apartments of cabin, steerage, and betwixt decks, was uncommonly lofty and well aired; and so far from being crowded, she had about double the tonnage for the complement of negroes she brought over that is commonly allowed for troops. She was excellently found in every species of provisions, and stores; and her discipline and cleanliness were unobjectionable. In short, there was nothing in her, nor about

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* Mr. Mortimer in his Report, instances the *Nayaden* frigate, which, in consequence of her having taken on board a quantity of wood in a *green* state, at Dominica, lost one third of her crew by a fever, which was only stopped by her complete clearance and fumigation."—This last operation, if useful, probably was so only by drying the hold.—Water in contact with *green*-wood in an intertropical temperature, would naturally undergo decomposition, and afford hydrogen-gas; and the wood itself, acted upon by oxygen derived from the water, would yield carbonic-acid-gas; and these vapours together might form hydro-carburetted-gas, which seems to be a constituent part of the exhalation from marshes. But it may perhaps be doubted, whether a quantity of green-wood alone, with the application of water, would suffice to render Miasmatic Fever prevalent in a ship, without the co-operation of foul-ballast, or of other decomposeable matter.—For otherwise we should probably have had more frequent instances of such effects.

her, that could either generate, or permit the retention, if introduced, of the matter of Typhus Fever. The cause of disease was therefore, I am clearly of opinion, to be ascribed to the green-wood laid in at Sierra Leone, operating, along with the foul ballast, to furnish, when impregnated with the gases arising from putrid sea-water, morbific miasmata, similar to those that on land arise from marshes, when exposed to the influence of the higher degrees of atmospheric heat. Why this morbific power operated differently on the blacks and on the whites may be explained from the fact, that the African is very rarely amenable to those influences that affect white men with Intermittent, Remittent, or Yellow Fever. If they operated at all therefore on them, they must have produced some other disease; but I see no reason to attribute the dysentery of the blacks, from which so many perished, to other causes than those that have been proven to exist, viz. the sending numbers from the hospitals at Sierra Leone to the ships ill with that disease, the want of proper remedies and of medical attendance during the voyage, and the highly improper change of diet from one wholly vegetable to the ordinary ship's rations."

After the *preceding* pages had been printed I was favoured by Dr. Fergusson, who has recently returned to England, with two Reports made by him to the Army Medical Board, partly on the subject of the Childers

sloop of war, which vessel affords an additional instance and proof of the production of numerous and fatal cases of Yellow Fever by the foul condition of a ship's hold, and I therefore subjoin extracts from them:

The first of these Reports, dated Barbadoes, 8th Sept. 1816, contains the following details, viz.

"On the 26th ult. the *Childers* arrived late in the evening in Carlisle Bay, and next morning I accidentally heard that a Lieutenant from the ship had been on shore reporting her sickly condition, and seeking for medical assistance. I immediately went on board and found the Captain, five medical officers, one midshipman, two petty officers, and seventeen seamen ill of fever. Four midshipmen, two pursers, and fifteen of the people had died of the disease during her ten days passage from Trinidad." ——"Dr. Wray arrived on board before I left the ship, who took charge of the sick, and all necessary relief and refreshments were as soon as possible afforded them, preparatory to their being removed in the cool of the evening to our general hospital on shore."——"The sick medical officers were in a state little capable of giving clear accounts of the origin and progress of the disease; but from what could be collected from them, we understand that the invasion of the fever was sudden and violent; that severe vomitings came on earlier than commonly oc-

curs in the true Yellow Fever; but that these ceased as the disease advanced towards its termination either in death or recovery. The truth of this was speedily exemplified in the cases of four men taken ill on board the brig on the night after the sick had been brought on shore. They had all been at work, had eaten their allowance during the day, and gone to bed in health, from which they awoke with burning fever, followed by delirious head-ache and vomiting and excruciating pains in the back and limbs. They were brought to our hospital, where all have done well. The day following passed without any admissions or attacks of fever. On the 29th five were similarly taken ill:—the next day three; and no day passed with a smaller number of fresh cases, until the night of the 4th instant, when the ship, after having reported four fresh cases, that could not be landed on account of the severity of the weather, was blown out of the bay, with only fifteen effective hands on board, and has not since been heard of. It is distressing to relate, that the Surgeon of the Brazen sloop of war, who happened to be at Barbadoes in search of his ship, and who very handsomely volunteered his services on board the *Childers*, took the fever on the fourth day after being on board and speedily fell a victim. The Surgeon of the Scamander, too, who had frequently been on board of her, fearlessly and actively doing his duty both afloat and on shore, was seized with the fever on the 2d instant, and is now lying in

a very hopeless state in our hospital, along with an Admiralty Clerk sent to act as purser, who is nearly in the same condition. Both these cases were marked ones of Ardent Tropical Fever, and exhibited none of the symptoms of the Typhoid type."

"It is most gratifying to report that no sickness of any kind has been communicated to any medical attendant or other person of whatever description employed about the sick, although the devotion of the Medical Officers to their duty on this interesting occasion has been unbounded."

The Second is Dr. Fergusson's Quarterly Report to the Army Medical Board from the 25th of September to the 24th of December, 1816, which states as follows, viz.

"At the close of the *last* Report we left the Childers sloop of war under circumstances of peculiar uncertainty and distress. She however arrived safe at Antigua, where the purifications intended for her at Barbadoes, were promptly ordered at the dock-yard (English Harbour), when the hold of the ship was found to be in so extraordinary a state of filth, fermentation, and impurity, as most amply to account, in the opinion of the Medical Staff of the Navy, for the rise, progress, and continuance of the

fever, that had prevailed on board her. Since her purifications she has continued perfectly healthy, though she has taken many fresh hands on board. The Scamander frigate, which had brought from Trinidad such marked and fatal cases of the Yellow Fever in the persons of four midshipmen, remained during the greater part of the quarter, with the exception of some trips to sea, in the anchorage of Carlisle Bay, well out seaward, without shewing a single fresh case; but the flag-ship (the Antelope), the Tigris frigate, and the Brazen sloop, that had all taken up for weeks the unwholesome anchorage of English Harbour and St. John's, Antigua, were severely visited, particularly the Antelope and Brazen, with the worst Yellow Fevers; and here it is worthy of remark, that these ships were affected precisely in proportion as they had lain a longer or shorter time in those unhealthy places."—"The Brazen might have come in only for a *third* degree of suffering, if she had not subsequently gone into St. John's, and continued there for several weeks, by which means the whole of her crew were taken down with Yellow Fever, and a considerable number perished. The Fevers in all these ships were found to be more manageable and more easily conducted to a favourable termination in our hospitals on shore, than those that occurred amongst the troops; but some others that were brought from a ship detained by the Scamander, that lay in Car-

lisle Bay, *close to the beach*, proved deadly and malignant in the highest degree."

In addition to these reports, Dr. Fergusson on the 15th of August, 1817, favoured me with a letter, in which he says, " I had two documents respecting the *foul* state of the *hold* of the Childers, which *together* made a very strong case indeed; but I can at this moment lay my hand only on one of them (which is inclosed); the other, I fear, must be in a trunk sent to Scotland from Barbadoes with my books. In my narrative respecting that ship, left with you yesterday, I alluded to its covering a Report from Dr. Wray, Physician to the Forces, wherein he particularly mentions the hold to have been so foul and offensive, that he found it impossible to *descend into it.* The same thing occurred to myself, and was mentioned in different Reports and letters to the Director-General. —At the time when I attempted to go down into the hold, the seamen declared that on different occasions, when they were sent there, the *candles would not burn.*"

The document above alluded to is an extract from a letter to the Transport Board, written by Mr. Neale, Surgeon of the Antelope, flag-ship, dated the 30th of September, 1816, which was given to Dr. Fergusson by Dr. Crichton, then principal Medical Officer of the Navy on that station, and is as follows, viz.

"In a former part of this letter I took the liberty of hinting that this fever, from its type, was or might be the result of paludal effluvium, and the following circumstances led to the adoption of such a conclusion. The holds of all vessels, particularly ships of war, contain large quantities of vegetable matter, in casks often insufficiently strong to prevent their contents from escaping; such as pease, oatmeal, cocoa, flour, sugar, and wood for fuel, and when united with a due proportion of humidity, so as to produce a given change by putrefaction, a gas is evolved, under a certain degree of atmospheric temperature, highly detrimental to animal life, as is daily evinced in low swampy or marshy situations, and in all uncleared intertropical countries, where much brush-wood prevails, at a certain season when vegetable nature suffers a temporary decay. As the hold of this vessel had not been cleared for several years, I believe at least three, the above conjecture became probable, and it was corroborated most fully by clearing it (the hold) at English Harbour, as a combination of the above vegetable substances with the bark of green-wood and chips in a certain putrefactive union, with moisture, must have occupied a considerable portion of the hold, for its removal required two large, *square lumps*, from the dock-yard; and those individuals who hitherto resisted the influence of this miasm, now suffered in clearing away a mass of highly putrid and of-

fensive matter.* Even some of the government slaves, who have ever been exposed on similar duties, employed here, experienced the noxious effects of this effluvium, and narrowly escaped, by being sent to the hospital."

"If this was the cause of disease, which I have no reason to doubt, it ought to operate as a caution, in future, to all vessels going on foreign stations, that their holds should be rendered pure and wholesome before leaving England: it would be ultimately the saving of many lives to the country."

For other facts and observations on this subject, Dr. Fergusson has referred me to a publication in the last Number (51) of the Edinburgh Medical and Surgical Journal, written by Mr. Birnie, Assistant-Surgeon of His Majesty's ship *Antelope*, of which the following are extracts, viz.

"Immediately after our arrival in this country (Barbadoes) about the beginning of March last, when the inhabitants of Bridgetown were perfectly healthy, and no

* "I had this Report (says Mr. Neale), from Lieut. Astley, who had the command of the brig at the time, and Dr. Crichton, Surgeon of the Naval Hospital, who repeatedly visited that vessel officially during the period of expurgation."

cases of fever on shore, (at least I am certain that no one belonging to the ship had been near, or indeed had heard of, any sick person on shore) a fever characterized by all the symptoms which Mr. Pym has attributed to the Bulam Fever, made its appearance on board the Antelope, and since that period, *a hundred and ten* cases have occurred in her, of which thirty-one only have died; of these thirty-one, nine either lived entirely in the fore and after cock-pits, or messed, and consequently passed the greater part of their time, there. None who had black vomit recovered; and of the thirty-one, seven only had black vomit, and of these seven, six were of the nine mentioned above as living almost entirely below, where the atmosphere in this country is thick and heavy, and produces a peculiar hot sensation on descending from above into it. The temperature is always about the same; is often below what it is on deck, and from the continual burning of candles, the crowding together of several people, the *debris* of pantries and mess-rooms, not always exceedingly clean, together with the want of circulation of air, may have caused so great a proportional number of those who were obliged to mess or live below, to die, and have black vomit."

"In August a disease broke out on board the *Childers* brig, while anchored in the Gulph of Paria, off Port of Spain, Trinidad, where the squadron had gone to pass

the hurricane-months. As her Surgeon and Assistant were both attacked, I was sent to assist Mr. Brown of the Scamander, in taking care of the sick. At that time twenty-eight out of ninety men were labouring under a disease, which made its appearance by the same circumscribed pain of the forehead, affection of the eyes, variable pulse, hot, dry, pungent skin, and succeeding irritability of stomach, which ushered in the disease on board the Antelope. In the first ten days about thirty persons died; three women and two infants fell victims to it—and this was called *bilious remittent*. But the only difference which appeared to me to exist between the disease on board the Antelope (which was called *Bulam Fever*), and that on board the Childers, was, that in the former, black vomit appeared in seven cases out of a hundred and ten, while on board the Childers I observed it in one only: (it was, indeed, said to have existed in the cases of two of the women, but as I was seized with the disease myself before they died, I cannot assert that they had it on my own authority) and that the disease on board the Childers was attended with a much greater mortality."——

"The lower deck of the Antelope, where the people mess, is always well aired, except in a heavy sea, which does not often occur in the West Indies, and is always kept in an exceedingly clean state, which accounts for the disease not running through the ship's company, and for its comparative manageableness; *but, in the cock-*

pit, every person was attacked except two seasoned hands; about one half died, while, in the other parts of the ship, not more than a fourteenth."——"The lower deck of the Childers, where the people mess, was dirty in the extreme. *On lifting the hatches of the fore or after-holds,* a horrid suffocating stench issued from them; it was confined, lumbered with lockers, and the heat increased by the fire-place being on it. It was in a nasty filthy condition, and I do not recollect that it was attempted to ventilate it by wind-sails. This state of the vessel, with the want of accommodation and attendance, and the sudden fall of the medical officers, accounts for the mortality on board of her, and the disease attacking all her crew: only three escaped an attack, and about one half died. Here we observe a remarkable coincidence between the extent and mortality of the disease in *our cock-pit,* and in the *Childers.*"

"The Tigris and Scamander frigates have also been visited by this dreadful scourge, but have not lost more than five or six persons each, which exemption they owe to their high state of discipline, their cleanliness, and attention to ventilation; while the Brazen, under circumstances somewhat similar to those of the Childers, has lost twenty-four out of eighty men. I do not mean to say that she was dirty, or in bad discipline, but her lower deck, where the people live, is narrow, crowded, and *has*

neither ports nor scuttles, with two decks above it. The different degrees of mortality exhibited in the different ships of this little squadron, according to their different degrees of description, ventilation, and cleanliness, I think sufficiently prove that all the various appearances of these diseases arise from the same origin, and are effects of the same cause, modified, as we have seen, by various circumstances."

"On board the Antelope no precaution was taken to prevent contagion; yet, out of three hundred and twenty men, having the freest intercourse, not more than fourteen or fifteen at a time were attacked. The surgeon, two assistants, and surgery man, were always among the sick, and the three last slept continually in the midst of them, and none of them had the slightest attack of fever for many months afterwards, and only the two assistant-surgeons after being sent, one into the Childers, and the other into the Brazen. The officers frequently came to see the sick; their mess-mates were allowed to visit, wash, and dress them; yet no appearance of the fever having ever been communicated by contagion occurred. On his being attacked, I lent a pillow to one of my mess-mates, who died eight days afterwards, having had *black* vomit; yet I slept on it for four months without any other precaution than that of changing its case, and without the least attack of fever during all that period."

Dr. Fergusson concludes his observations on the subject of Yellow Fever, and of the occurrences which relate to the Regalia, with the following important remarks, viz. "I am aware how much I have been favoured by circumstances, and what a different interpretation the facts I have collected would have borne, had the present epidemic that now afflicts the islands, broken out in the ordinary course of seasons *a year earlier*, at the time the Regalia was here; my task would then have been a much more difficult one, for these (facts), instead of assisting me to elicit the truth in the manner I have done, would in that case have been turned to the confirmation of error, and the perpetuation of the delusions, in regard to *imported contagions*." The justice of this remark renders it highly interesting; for although, as I have reason to believe, the errors which were propagated by Dr. Chisholm's publications in regard to the *Hankey* are now nearly, if not quite, dissipated by the facts and arguments contained in the seventh Appendix to my Essay, it is highly probable that such a coincidence of events as that just mentioned by Dr. Fergusson would have encouraged many to consider the occurrences on board the Regalia as a second importation of Bulam Fever from Africa, notwithstanding the decisive evidence which we possess, that, at least on the western coast of that continent, no *contagious* fever exists, and that, in the few instances in which typhous contagion has been carried thither (as

among the blacks sent some years since to Sierra Leone from Nova Scotia, pages 129 and 275 of my Essay), the heat of the climate has soon extinguished the contagion.

I shall here introduce a few short extracts, applicable to this subject, from a Report made by Dr. Erly, Deputy-Inspector of Hospitals, addressed to Sir James M'Grigor, Director-General, &c. and dated Sierra Leone, the 20th of September, 1816, respecting the fever of that part of Africa which prevailed as an *epidemic* at the settlement just mentioned during the last autumn, 1816. Of the prominent symptoms of this fever Dr. Erly says, " there is, in general, an intense pain in some part of the head, and this frequently, but not in all instances, in the direction of the frontal sinuses, or orbits; the heat of the body is, in all cases, above the natural standard, and frequently excessive; general pains prevail in the trunk and extremities"—" the stomach is *affected* in almost every instance, and this affection is variously modified from a slight degree of nausea, to severe and distressing vomiting: indeed, *pressure* on any part of the epigastric region invariably produces an uneasy sensation, and sometimes severe pain; the countenance, from the commencement of the disease, exhibits indications of anxiety and distress; the eye is dejected, and loses its brilliancy; the skin on the second or third day (and this occurs in almost every instance) loses its transparency, and becomes dis-

coloured by a dirty yellowish hue, not easy to describe, and of different gradations, from the slightest to the deepest shades; the bowels are in general torpid; the tongue at the commencement is whitish, and in the advanced stage dry, and covered with a white incrustation—but it is seldom or never, under favourable circumstances, loaded with *black* coloured sordes, and not frequently even with a brown fur; the presence of black sordes on the tongue is an extremely unfavourable symptom, and it in general proceeds from a crowded and condensed state of the sick."—" Some degree of delirium most frequently accompanies the exacerbations, but it is for the most part of a mild character, a sort of wandering or confusion in the train of ideas:"—" and, to complete the melancholy detail of symptoms, every ray of hope vanishes from the timid and dejected patient; his mind becomes gloomy and depressed; and this unfortunate state occurs in most instances, even at the very invasion of the disease."—" The fever being thus established by its primary and introductory links, viz. rigors, heat, and sweats, runs its unabated course for the space of twenty-four or thirty hours, and then suffers some slight degree of *remission;*" which gives fallacious hope as usual—" for the patient, a few hours after the deceitful pause, is again attacked with redoubled violence, and all the symptoms become aggravated; the vomiting is probably urgent and incessant; the patient is bathed in clammy sweats, which

bring no relief; his anxiety and distress are increased; the pains in his head, body, and extremities, are intense; his pulse is rapid and small; and now, at this early period, debility in many instances, with all its accompanying symptoms, sufficiently manifests itself."—"The exacerbation having thus continued its course for a certain period, probably equal to the former, undergoes *a second time* some degree of *remission*, but not so perfect, nor of so long duration, as the former; our hopes are again revived for some short time, when a third exacerbation takes place, which is followed by either the *death* of the patient, or a still slighter (and scarce perceptible) degree of remission than the former."—"The disease in general thus completes its course."

Dr. Erly considers marsh miasmata as the cause of the disease; the surrounding country, remaining "uncultivated in the wild luxuriance of nature, clothed with lofty trees and impenetrable *jungle* of perennial verdure," and "the average range of the thermometer throughout the year being about 82°; the rains, which pour down in torrents, continuing more than six months of the year," and, with the tornada season, their duration being "nearly eight months:"—"indeed," says Dr. Erly, "the degree of insalubrity incident to the different settlements on the coast seems to bear some proportion to the quantity of rain that falls there, and to the duration of the rainy

season." We see by this description that the *endemical* fever of the coast of Africa (like that which occurred at the island of Bulama), even when it prevails with epidemic violence, always *remits*,* as I have repeatedly asserted in opposition to Dr. Chisholm and others; and it can hardly be necessary to add, that it is void of contagion, and therefore incapable of being communicated to, or propagated in, any other country by importation. On this subject Dr. Erly says, "I have ever held it as the safest rule of conduct, and it was *early impressed on my mind*, to consider *all fevers* as either less or more *contagious*; and of this I never had the least doubt, previous to my arrival in this country: but now, from what I have hitherto been able to observe here, I can safely declare (and which is *in direct opposition* to the *previous bias of my mind*) *that the fever of this country is not contagious*; but at the same time I beg it may not be inferred from this, that I mean to assert its non-contagious nature under the influence of every circumstance, for on the contrary, I *believe* it will become highly contagious when the sick are much crowded, and that sufficient attention is not paid to cleanliness and ventilation; a strong instance of this occurred, as I have been informed,

* This is one of many facts which demonstrate the great impropriety of giving the name of Bulam to a fever supposed, by Dr. Pym, to have no remission.

last year on board a transport." This transport was undoubtedly the *Regalia,* and some contagionist had doubtless concluded, and *reported,* that the disease in that ship, from its violence and the number of persons attacked by it, was contagious; and this report, co-operating with the early impression which Dr. Erly acknowledges to have been made on his mind, in regard to the supposed contagion of all fevers, as well as the generation of it by crowding, uncleanness, &c. has led him to adopt the common error, with which the sentence last quoted from his Report is concluded, not only without the support of any fact, but *in opposition to all his experience;* for he concludes this subject in the very next page with these words, viz. " In fine, I have not hitherto *been able to trace a single case of this fever* communicated to any individual, by either the secretions, effluvia, or contact of another labouring under its influence;" which *is all the evidence that can be desired* upon this subject.

As the fever exciting exhalations, commonly, but improperly, called marsh, or paludal miasmata, are so closely connected with the subject of this and the remaining parts of my sequel, it may be proper here to observe that these miasmata are known to us only by their *effects*; which however have been manifested so unequivocally and so fatally in the East and West Indies, and in Africa, as well as in the warmer parts of the conti-

nents of America and of Europe (particularly in Andalusia, on the shores of the Mediterranean, of the Archipelago, of the Black Sea, of the Sea of Azof, &c.), that scepticism itself cannot question the existence and operation of this widely extended and most powerful cause of disease. The many varieties in the types and degrees of violence of the fever resulting from this cause (the *malaria* of the Italians), although doubtless occasioned, in some degree, by the various conditions and constitutions of the sick, must, I think, be, in part at least, imputed to differences in the degrees of concentration, in which these miasmata are imbibed by the body, and probably to some variations in their composition or *nature*, occasioned by a difference in the *decomposeable* matters from which they are derived, or in the *temperature* which influences their production and *maturation*. A supposition of these differences can alone enable us to understand, why a long residence on the banks of the Ganges affords but little security against the miasmata of Batavia, of Ceylon, of Senegal, or of St. Domingo; why, as Baron Humboldt asserts, persons brought up at La Vera Cruz, and who are not *there* in danger of the Yellow or Miasmatic Fever, are liable to be attacked by it when they go to the Havana, or to Jamaica; why the natives of the Havana are, in their turns, in the like danger at Vera Cruz, and Jamaica; and why, as *is well known*, the inhabitants of the *interior marshy situations* of South

Carolina have been so frequently attacked by the Yellow Fever after sleeping even a single night at Charleston between the months of June and November, while the seasoned inhabitants of that city, who might remain there in safety, are commonly attacked with the milder remittents and intermittents of the *interior* country, upon removing thither during the summer and autumn. How these miasmata vary in their nature or their composition, we know not; and can only conjecture, by taking into our consideration the several matters existing in those places, from which these miasmata appear, by their effects, to have been extricated, and also the *gaseous fluids*, which similar matters are known to produce by the decompositions to which they are naturally disposed in the temperature prevalent at those places.—Vegetables, by decomposition, afford carbon, hydrogen, and oxygen; animals afford the same, with the addition of nitrogen and a surplus of hydrogen, in proportions suited to the formation of ammonia. But the effect of this nitrogen, or of the ammonia thus produced upon the carburetted hydrogen gas, which results from *vegetable* decompositions *alone*, has not been yet ascertained; nor do we know how the exhalations, produced by the joint decompositions of both animal and vegetable matters, differ from those of decomposing *vegetables only*. Berthollet, in the second volume of the Mémoires de la Société *d'Arcueuil*, contends that hydrogen, carbon, and

oxygen, may combine in unlimited proportions, and form various combustible and insalubrious gases, which he would call oxy-carburetted hydrogen: but Dalton and Henry think that oxygen is not an essential element of these compounds, and that hydrogen and carbon, forming alone carburetted hydrogen, or hydro-carburetted gas, are capable of uniting in but *few* proportions. This gas may be copiously extricated, by stirring the mud at the bottom of stagnant water; and we may at least conjecture that, with some modifications, marsh miasmata principally consist of it; the air collected immediately *over*, and *close upon, the surface of marshes*, having been found to contain hydrogen and carbonic acid gas in large proportions. But chemistry possesses so few means of separating the immediate principles of bodies without a *destruction*, or instant *recombination*, of some of them, that we may probably never be able to detect, or ascertain, the constituent parts, and much less the varieties, of marsh miasmata, or to discover that substance in which their febrific power resides.

All human beings (and, as I conceive, human beings only) are disposed and liable to imbibe these fever-exciting miasmata, in the places in which they are exhaled, and probably in equal portions, cæteris paribus; although the contrary might be supposed, when it is seen that, while some individuals are attacked with miasmatic

fevers, others who have been similarly exposed, continue to enjoy good health for a considerable time, or until that vigorous state of the body, which has enabled them to withstand the morbific influence of these effluvia, suffers at least a *temporary* diminution, by exposure to rain, or to cold, especially at night, or by exhaustion from fatigue, or intemperance, or by accidental injuries, such as the fracture or dislocation of a limb, or the occurrence of diarrhœa, cholera morbus, hæmorrhage, &c. or by abortion or parturition; in which cases (especially in the West Indies) intermitting or remitting fevers often ensue, which, but for this diminution, might have been obviated by those exertions of the living power, that sometimes overcome, and ultimately expel, from the system the miasmata so imbibed, when they are not too powerful to be resisted (see pages 241 and 242 of my Essay). Probably every *fertile* spot within or near the tropics, when supplied with sufficient moisture by rain or otherwise, does invariably emit these miasmata, which are as invariably imbibed, especially during the night, by those who come and remain for some time within their reach, and in greater quantities or proportions by those who sleep or reside nearest to the surface of the earth, as is stated at p. 185, and also at p. 229 and 230 of my Essay. A gradual and habitual exposure to the impressions of these miasmata seems either to diminish their morbific influence on the body, or to enable it, in a considerable degree,

to resist their impressions, so as to obviate the violent forms of fever; but, in the West Indies, even those persons who have acquired this habit, often find it expedient to remove for a time beyond the reach of those exhalations, by making a *sea* voyage, or a visit to some colder climate, in order that the powers of life may have time to expel and free the body from this cause of disease, which can rarely be done in situations where farther imbibitions of it are frequently taking place.

I have, between pages 237 and 244 of my Essay, noticed the very different, and often very distant, spaces of time at which the miasmata in question *manifest* their morbific influence in the human body, and this difference enables us to account for the variations which occur in regard to the periods at which the supposed Bulam Fever attacks those who have been exposed to its cause, which cannot otherwise be satisfactorily explained. Dr. Pym says (p. 24), in regard to the epidemic at Gibraltar in 1804, that "the *fourth* day was generally the time the contagion seemed to require to shew itself; and this was ascertained most positively in innumerable instances." It is, however, well known that strangers who landed there during that epidemic, were, in several instances, attacked on the *second* and on the *third* days after their landing. And Mr. Amiel, in answering the questions of Deputy-Inspector Fraser, says, "from the circumstance

of people having been taken ill on the *first* day of their landing here (i. e. at Gibraltar), and from the information I had, that people who fled from this garrison in 1804, were taken ill on the coast of Portugal many days after they had left this place, I am induced to believe that the disease may be put into action at any time, from the first moment of exposure to a period of fifteen or twenty days." And Dr. Riseuno, a respectable physician at Carthagena, in the written account which he gave to Dr. Burnett of the supposed contagious or Bulam Fever in that city (as stated by the latter at p. 240), says, that "whoever exposes himself to the contagion of Yellow Fever, and afterwards withdraws himself from all contact with the contagion, the disease will, if he be infected, make its appearance within the space of *twenty* days exactly." But if contagion were really the cause of the fever under consideration, and if the facts connected with its supposed application or communication had been carefully observed and accurately reported, or in other words, if contagion had been actually received, so as to become the cause of disease at the times supposed, there could have been no such diversity in the reports concerning its effects, there being no disagreement among physicians in regard to the number of days which elapse after the contagion of small-pox has been received into the lungs, or the time which intervenes after its application by inoculation, before the commencement of the eruptive fever.

If, however, we admit the fever in question to have resulted from the action of miasmata, there will be no difficulty in explaining the cause of its manifesting itself at very different periods after exposure to them, this being consonant with the usual effects of miasmata, varying as they do in their degrees of virulence or concentration. At Gibraltar, where there is commonly much less moisture than at Carthagena, the miasmata, when maturated, may be expected to act speedily and powerfully; but at Carthagena, which (besides its *mast-ponds*) is almost surrounded by marshes, it may be presumed that they would have been more diluted or less concentrated; and accordingly we find that, although marsh fevers prevail most extensively every autumn in that city and its vicinity, they do not often assume the *continued* and *violent* form to which Dr. Pym has given the name of Bulam, and therefore the effects of the effluvia from *these* marshes may well require a greater number of days to manifest themselves, than is required by the more virulent miasmata at Gibraltar, so as to have warranted Dr. Riseuno in assigning a longer interval between the application of the cause, and the actual production of fever. But if the fever in question had been produced at Gibraltar and Carthagena by a contagious power, that power would not have required exactly twenty days to manifest its effects at the latter of these places, and only four days at the former.

CHAP. V.

HAVING in my preceding chapters proved, as I think, beyond the possibility of doubt, in minds unprejudiced, that the fever improperly called Bulam, has no existence as a disease (sui generis); that it is no more than a violent form of the remittent of hot climates; and that its supposed *peculiarities* have no foundation in *fact;* and all this being equally true of the *Andalusian* fever, to which Sir James Fellowes has applied the fallacious name of *pestilential,* I might leave the *baseless* superstructures, raised on these chimæras, to fall spontaneously. But as the truth on this important subject cannot be too *speedily* or too plainly demonstrated, I will here proceed to ascertain the principal facts connected with the recent occurrences of this fever as an epidemic at *Cadiz,* and other parts of Spain, as well as at *Gibraltar.*

Between pages 439 and 473 of my Essay, I have given an account of the occurrence of the Yellow Fever at Cadiz, and other parts of Andalusia, as well as at Malaga and Carthagena, in different years, preceding the year 1805,

to which account I confidently refer my readers; having, even with the aid of Sir James Fellowes and Dr. Pym, discovered no error in it, excepting one which regards certain *localities* at Cadiz, where I am said to have improperly described the ground in the eastern part of the town, as being *low* and *damp*, and the marshes around the bay, as being *nearer* than they really are. That I may have erred in these particulars is very possible, as I was never on shore at Cadiz, although I accompanied Sir Ralph Abercrombie and the armament destined to attack that city, in the year 1800. My description, therefore, could only be taken from the accounts which were given by others, and which were probably not sufficiently minute, accurate, and distinct, to enable me to avoid the mistakes in question. These mistakes, however, it will be proved, are perfectly *harmless* in regard to any inference which I drew from them, although my opponents have endeavoured, and with little reason, as will soon appear, to produce a contrary belief.

In treating of the Ardent or Yellow Fever, as it has occurred at Gibraltar, Cadiz, and other southern parts of Spain, I ascribed its production to the action of those vapours, or exhalations, which result from the decomposition of vegetable, or vegetable and animal matters, in a temperature of not less than 80° of Fahrenheit's thermometer, and which are commonly called marsh or pa-

ludal miasmata; an appellation which, in compliance with custom, I had occasionally adopted, though I well knew, and had repeatedly declared, that such exhalations or vapours are often emitted from soils and situations which had no resemblance to a *marsh*. Sir James Fellowes, however, and Dr. Pym, have both found it *convenient* to *suppose*, and have endeavoured to make others *believe*, that, in alledging marsh miasmata as the cause of this fever, I had exclusively contemplated those vapours, or exhalations, which arise *solely* from a distinct ostensible and formal *marsh;* and that, to convict me of error, and establish their own doctrine of contagion as the cause of Yellow Fever, it would be abundantly sufficient to state that there is *no marsh*, either in Gibraltar or Cadiz, and to add, that *intermitting* fevers do not often occur at either of these places. As these authors have professed that a principal motive for their writing was that of *refuting* my Essay, it was unquestionably incumbent on them to *read* it carefully; and, in doing so, they might have found that, in order to *preclude* the possibility of any such misconstruction as they have been pleased to adopt, I did, at p. 91, in a note, make the following declaration, viz. "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 inti-*

mate that such miasmata, &c. are emitted solely from 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."—In the same page, I had previously mentioned these miasmata as *existing* "*in a variety of unsuspected places*."—Afterwards, at p. 195, I stated "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 therein*."—And I have farther observed, at p. 202, that, for reasons mentioned there, and in the preceding pages, "*any piece of ground*, in *a hot climate*," which was composed of ingredients suitable in other respects, "would, if supplied with only a *moderate* quantity of water, be soon in a fit condition for emitting *very concentrated miasmata*; and that 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*."

Again, at page 225, I quoted Baglivi, who has men-

tioned the miasmata, or mal-aria at Rome, as acting only in particular spots or parts of the city; and asserts, as matter of wonder, that the *healthy* were separated from the *unhealthy* spots only by very short spaces; and I mentioned from information which I had obtained in that city in 1802, that the fevers in question, " some times prevailed among the inhabitants on one side of a particular street, whilst those on the opposite side entirely escaped their attacks; and that this was said to have often happened in a certain portion of the *Corso*." I moreover quoted a Report of the Board of Health of the city of New York, dated January 20, 1806, in which it is asserted, that " various houses, in different parts of that city, have on *the recurrence of every malignant* (or Yellow) Fever, proved to be the *principal seats of the disease*, and the graves of their inhabitants;" (a fact incompatible with a belief that contagion had caused or propagated the disease) and in a note to page 226, I quoted similar facts from *Lancisi* and *Ramazzini*. I also at page 231 cited instances, showing that the tops of hills, several hundred feet above the level of the sea, were liable to fevers occasioned by these miasmata; and other instances of similar import at pages 202, 226, 227, &c. So many remarks and examples to the same effect, might certainly be deemed sufficient to protect me from the perversion of my meaning, which has been employed for the purpose before-

mentioned. That these gentlemen could themselves have been deceived in regard to my meaning is scarcely credible; not only because it had been so fully and distinctly explained by me, as, in my judgment, to preclude all room for a mistake, but because they appear to have purposely resorted to an expedient suited to confirm the misrepresentation of which I complain; I mean that of endeavouring to persuade their readers, that not only agues or intermittents, but remittents also, are rare occurrences both at Cadiz and Gibraltar,* and thus inducing a belief

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* Dr. Pym, at page 134 of his volume, after mentioning *Cadiz* and *Gibraltar* says, "Marsh Miasmata and their consequences, Remitting and Intermitting Fevers, are totally unknown to either;" an assertion, which, in regard to Remittents at least, will, I believe, surprise all who may read the remaining part of this Sequel, and particularly the pages respecting Gibraltar. Perhaps Dr. Pym may imagine that, as the term *bilious* is commonly applied to the remitting fevers of warm climates, this application may authorize him to suppose that they are not produced by marsh, or fever-exciting miasmata;—an error to which Sir James Fellowes appears to incline, as he manifestly endeavours as much as possible to avoid calling the *Endemial Remittent* of Andalusia by its proper name; e. g. at page 253, after saying that "towards the equinox, those complaints which belonged to the *declining seasons*, then became aggravated and many of them terminated *fatally*," he observes that afterwards "the greatest part, changing their character altogether, were converted into chronic affections.—Thus (adds he), it was commonly observed that the *Bilious* Fever, strictly so called, put on the form of *Intermittents*, and assumed the various indistinct and anomalous signs, of which, in these climates, they are so susceptible." Every one acquainted with the subject will however know that the fever, here purposely called *Bilious*, must have

that fever-exciting miasmata do not exist at these places. It is true, indeed, that the *intermitting* form of fever is not frequent at either of them, because the miasmata produced in a high temperature, and without any superfluity of moisture, have a natural tendency to excite Remittent or Continued Fevers, rather than Intermittents. But the occurrence of Remittents, which (as Sir James Fellowes admits at page 402, and as will hereafter be abundantly proved) prevail almost every summer and autumn in Gibraltar and the southern parts of Spain, proves the existence of these miasmata, as decisively as it could be proved by an equal prevalence of agues* or intermittents.

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been the Endemic Autumnal Remittent, described by a multitude of authors as prevalent in the southern parts of Spain, which, at the approach of winter commonly assumes the intermittent type; and this Sir James Fellowes has indeed ultimately acknowledged at p. 402, where he mentions, "the Bilious *Remittent*" as being "*common* to that country (Andalusia) during the *autumnal* months."

* As a most authentic proof, however, of the prevalence of "intermittent fever" in Spain, I may cite the "Sketch of the Medical History of the British Armies in the Peninsula," given by Sir James M'Grigor, and printed in the sixth volume of the Medico-Chirurgical Transactions, who, among the important facts recorded therein, mentions that "twenty-two thousand nine hundred and fourteen cases of this disease altogether were admitted into the regimental hospitals," and adds, p. 415, "so common is ague in many parts of Spain and Portugal, that the inhabitants do not term it a disease."

I am able to adduce the decisive evidence of Dr. Fergusson to

It is, however, a matter of indifference, in regard to the truth and stability of my conclusions respecting the cause

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the same point, who has obligingly put into my hands a paper containing pathological observations, written by himself in Spain, while officiating as Inspector-General of Hospitals to the Portuguese Army, from which the following are extracts, viz.

"That miasmata, or the exciting causes of Intermittent or Remittent Fevers, whatever they may be, *abound* universally in the Peninsula, is certain, from the effects daily experienced amongst every class of men, natives as well as strangers, during the hot season of the year. Even when the aquatic weeds and uncovered marsh give no warning of danger, all are subject to these diseases, and the inhabitant of the most *arid soil* is often as liable to be attacked as the tenant of a swamp." To the preceding statement, he adds the following note, viz.

"*Coria*, whence I now write, at this moment very healthy, is afflicted during the autumnal months to so great a degree with endemic fevers, that scarcely one in a hundred of the inhabitants escapes an attack. With them, as with all the rest of the world, when immemorial experience has shewn that they cannot avoid a calamity, it goes for nothing. They call it the calentura, and contemplate it with the same indifference as a Turk does the *plague*. When it comes, they take refrigerants and bark, let the annual proportion of victims pass to the grave as a matter of course (which, considering the number who are affected, is wonderfully small, and amongst newly-arrived Englishmen would be ten times as great), and patiently await its extinction by the approach of winter. If questioned upon it, they seem amused by our curiosity, and answer that it is only a *calentura!* Yet Coria, if surveyed by the most scientific medical observer unexperienced in these climates, might be pitched upon as a station of refuge against endemic diseases, for it stands on *a dry country, upon a rocky eminence*, within a very short distance from the River *Alagon*, one of the purest and most limpid streams in *Spain*, running over

of the Yellow Fever, whether or not I have clearly understood and accurately described the local sources of those miasmatic exhalations, which I consider as the cause of the Yellow Fever at Cadiz, and I may add, at Gibraltar, &c.; since the undeniable *prevalence at all these places*, during the summer and autumn, of *Remittent Fevers*, the *acknowledged offspring* of *such exhalations, indisputably proves* and *demonstrates their presence* and *influence*, however they may be produced, or from whatever source they may be derived.*

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sand and gravel, through an arid plain, where there is not an *aquatic weed*, nor a speck or a line of marsh, within miles of the town." "The Alagon having, however, no confining banks *at this point* constantly during winter floods, overflows and stagnates upon the neighbouring sands; and it is from the waters thus left, and also from those of brooks amongst the hills near the town, as clear and as little marshy as the Alagon, but which, during the winter floods, likewise overflow their sandy banks, that the miasmata are produced, which generate the autumnal endemic fever."

* The following is extracted from Travels in the South of Spain, by Wm. Jacob, Esq. M.P. F. R. S. in 1809 and 1810. "The rocks on which Cadiz is built, are of a very singular structure: the basis of their composition is probably *pechstein;* and in this, by its *glutinous* matter, the shells, pebbles, quartz, sand and marble, have so intermixed, and hardened in the course of years, as to form a compact kind of stone. This glutinous matter is at first of a greyish black, but in process of time, when mixed and combined with other substances, changes into a very light yellowish colour; it possesses so much tenacity that pieces of brick, tiles, chalk, shells and other rubbish thrown into the sea, become incorporated with it, and in time so firmly united as to appear of solid stone." Perhaps this peculiar founda-

That I may bring the *main* question in dispute to a *fair* and *short* decision, I will begin with the fever which prevailed epidemically at Cadiz in the year 1800; where, according to those who consider it as a fever sui generis, it had not appeared during a space of thirty-six years. The question then to be decided, is, whether this epidemic was merely an unusual aggravation and extension of the common remittent, which occurs so constantly and generally in Andalusia, and other parts of Spain, *from local causes,* or whether it was a different disease, imported from another country, and propagated in Spain, by a *contagious* quality.—Those who maintain the latter alternative of course admit that, if the disease had formerly occurred in Spain, it had been extinct for more than thirty years; and that consequently, a *new* and *recent* importation must have taken place, to produce the epidemic under consideration. It is therefore incumbent on them to *prove,* that there is *somewhere on our globe* a spot on which this disease had existed not long before the time of its supposed importation, and where it was *found*

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tion of Cadiz may, by its disposition to absorb and retain moisture, operate like *clay* in promoting the formation of fever-exciting miasmata. Sir J. Fellowes states, page 21, that Dr. Gonzales had declared that "his experience did not accord with the boasted salubrity which the ancients attributed to Cadiz."—"That *every* species of disease had appeared there which is peculiar to the season," &c.

to possess a contagious power; without which, its *importation* and *propagation* in Spain must have been impossible. That they have either proved this, or that there is in fact any such place on earth, I most confidently deny; so far at least, as the best of human evidence can authorize a denial. It need not be stated that the importation of the Andalusian Epidemic of 1800, was by the Contagionists and by the Government of Spain, charged upon a fine new vessel, built at Baltimore, and called the Dolphin, which had been hired at the Havana by Don Pablo Valiente, Intendant of Cuba, and Member of the Supreme Council of South America, for his own conveyance, with that of five servants and fifteen other passengers, to Cadiz.—That she sailed from the Havana on the 27th of May; stopped about the first of June at Charleston, South Carolina, to procure American papers; sailed thence on the 11th of June, and arrived in the Bay of Cadiz on the 6th of July. Now, if this vessel imported the Yellow Fever into Spain, she must have imported it either from the Havana or from Charleston; this latter being the only place with which she had any communication, after her departure from the former. But, to render such an importation *possible*, it is absolutely necessary that the disease should have existed in one of these places before the Dolphin left it, and that it should have possessed a contagious quality *there;* for, without that quality, it could neither be importable nor communicable. And it

is a fact well ascertained, that the Yellow Fever had not appeared, either at the Havana or at Charleston, during several months previously to the departure of the Dolphin; and that, when it did, or does, appear at these and other places in America, it is not found to have any contagious quality. The fact of its not having appeared at Charleston before the 20th of June, in the year 1800, was, in consequence of the prosecution which Don Pablo Valiente was suffering in Spain, *certified unanimously* at an extraordinary meeting of the Medical Society of the State of South Carolina, on the 5th of April, 1801; *twenty-two respectable practitioners being present;* as was stated at page 456 of my Essay. And these gentlemen also declared, on the ground of *facts* specified in their certificate, that the disease in question, had never been propagated by contagion, according to their knowledge and belief: a declaration which accords entirely with the opinion expressed in the same society at their meeting on the first day of the present century, as is stated in Dr. Ramsay's Review of the Improvements, &c. in medicine, in the eighteenth century, which was *then read* to that society, as mentioned at page 262 and page 352 & seq. of my Essay.—Concerning the Havana I have stated at page 455 of the same work (and my statement remains uncontradicted) that it was *juridically* proved before the Royal Audienza of Seville, previously to the complete acquittal of Don Pablo Valiente, who had been accused of having

imported the contagion of Yellow Fever into Spain, *that this disease had not appeared at that port* (the Havana) in the year 1800, until sometime after the Dolphin had quitted it. And, in regard to the fever's not possessing any contagious quality *there*, or even at *La Vera Cruz*, where its prevalence is more *frequent* and *violent* than in any other of the Spanish American colonies, the fact is asserted as being in conformity with the general belief and experience of the inhabitants, by very respectable authorities, particularly by Baron Humboldt in the second volume of his Political View of New Spain:* and this is moreover acknow-

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* Baron Humboldt says, "between the tropics, e. g. at Vera Cruz, the *Vomito* (prieto) is universally allowed to be not contagious." He thinks it has occurred sporadically at Vera Cruz, &c., whenever persons brought suddenly from a cold climate, have been exposed in the torrid zone to air loaded with miasmata: and that in the sixteenth and seventeenth centuries the mortality there was probably much less than afterwards, because the tropical part of America was only visited by the Portuguese and Spaniards, whose constitutions were less affected by heat, than those of people from more northern countries; and because the early colonists in the West Indies were not collected in such populous towns as they were afterwards. He observes that, in the most sickly season, the *shortest stay* in Vera Cruz is sufficient to excite the disease in *strangers;* and that the inhabitants of the city of Mexico, going to Europe, generally remain at *Xalapa* till the moment of sailing; set out in the middle of the night, and are carried through Vera Cruz in a litter, to the boat waiting for them at the Mole; yet these often die of Yellow Fever in a few days; and in like manner, Europeans, who on their arrival find litters prepared to carry them

ledged to be true, even by Dr. Arejula; the most respectable, according to Sir James Fellowes, of all the Spanish Authorities.

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up to *Perote*, do not always escape. Here we perceive that the disease is produced merely by passing through the *atmosphere* of the low district extending from the foot of the hilly country to Vera Cruz in the *night*, when the inhabitants are in their beds; and as this is done without entering any house, or stopping at any place, *personal* contagion, if it existed, could not reach such passengers. He adds that, during the last fifty years, the Yellow Fever has not appeared on the coast of the Pacific Ocean, except at *Panama;* and *there*, as at *Callao*, the commencement of an epidemic is often marked by the arrival of some ships from *Chili;* not that they imported the disease from a country where it *never existed;* but because its inhabitants, coming from the *healthiest* country in the world, experience the same fatal effects of a sultry atmosphere loaded with putrid emanations as the inhabitants of the North. Dr. *Arejula* also, at page 149, agrees with Baron Humboldt, in asserting that the Yellow Fever, or black vomit never occurs in the more inland or second line of villages or towns from the *sea-coast;* ("en las segundas poblaciones, o'en las de tierra adentro mas immediatas á las del mar"), and that, even on the sea-coast, it does not spread to any place north of the river Antigua, six leagues from Vera Cruz: that the black vomit has not been known at Xalapa, one day's journey from Vera Cruz, at which place patients having the disease, with hæmorrhages, &c. are continually arriving: and that it has not been possible to communicate the disease at Mexico by any of the numberless people who have gone or go daily to that city from Vera Cruz. So that we find, in Spanish America, as well as in the United States, &c. that the Yellow Fever cannot be communicated to any person, who has not *actually inspired the miasmata of those very towns or places in which it originates*,—and consequently that contagion has no existence.

The following statement is translated from page 147 of Dr. Arejula's Work, viz. " But it is fit to speak with correctness and truth on a subject so important to mankind: the Spaniards have given the name of black vomit to a putrid fever (" *Calentura Putrida*") in South America, *devoid of contagion,* (no contagiosa) more or less malignant; peculiar to Europeans recently arrived in that part of the world; or to Indians (Mexicans) *descending* (from the higher lands) *to the sea-ports, in which* it prevails; which, when very severe, is often attended with *black vomit;* a symptom at most times *fatal;* and this vomiting being so formidable, physicians have with reason employed the term to designate the disorder, and at the same time to point out its violence. It is an undoubted fact that the Endemic Fever of the summer and autumn, which occurs in many of our *American* ports, called the black vomit (*Vomito Prieto*) is accompanied by many, or almost *all* the symptoms, which we observe in our Yellow Fever (of Andalusia) and is *nearly related* to it."—He then professes to distrust some observations which he had made on this disease, when a young man, adding, what I may *now say* of " the black vomit, is upon the authority of persons of candour and superior knowledge; and on the information they have given me, of the treatment of this disease, by that distinguished Army Physician Don Miguel M. Ximenes, and of the practice success-

fully employed by Dr. D. Florencio Perez Comoto, in Vera Cruz." In the next page, after repeating his assertion, that the black vomit *is not contagious*, he adds, " but there is no doubt of our Yellow Fever (Fiebre Amarilla) being so, in the *French* islands to such a degree, as citizen Millet, who lived in them a long time, has assured me, that when * colonists found any one of their family, either father or son, &c. labouring under this fever, they put by him two jars of water, and *abandoned him;* whereas the *American* Spaniards *do not fly from such sick persons, but assist them without the least fear of contagion; because the experience of ages* (" la sucesion de siglos") *has taught them that the disease is not catching. Never have our vessels from Spanish America brought us the seeds of it* (la semilla de aquel) *even when they have sailed with it;* while on the contrary, the Yellow Fever has run from one to another distant part of the two worlds, like the small-pox," &c. Here it must be admitted that Dr. Arejula, notwithstanding the good-sense and accuracy for which he has been extolled by Sir James Fellowes, manifests no small confusion and inconsistency of ideas. He asserts, and was doubtless fully convinced from an abun-

* This is very contrary to what I have observed or experienced from French colonists in several of the West India islands.

dance of *concurring testimonies*, that the Yellow Fever or black vomit had, by the long and uniform experience of the inhabitants of the Spanish West India possessions, been found to be destitute of any contagious property; but he had heard reports of the alledged propagation of the disease by contagion from the French, and other West Indian colonies, to the United States of America, and by not distinguishing between the effects of miasmata and of personal contagion, he had, like the Spanish physicians in general, adopted the belief of contagion in the Yellow Fever of Spain; and, from its having, as he supposed, this property, he was led to imagine that the former and latter were not *exactly similar*, though he admits that they were *kindred* diseases (parecidas) and so much alike in their symptoms, that he knew of none, by which they could be distinguished from each other; and he expresses (at page 152) his hope, that "some able medical officers of the navy, who have attentively examined the *Yellow Fever*, may be able to compare it with the *black vomit*, in the voyages they frequently make to Spanish America, where it prevails epidemically, and to inform us *certainly*, and *in a scientific manner*, if these fevers be *one and the same sort of disease*, or if there be any signs by which they differ and may be known one from the other." Now, if, as is proper, we lay aside the information given by citizen Millet (which, besides being questionable, is

foreign to the subject) of the inhumanity practised in some of the French islands from a notion that the Yellow Fever was contagious, and also the reports which Dr. Arejula had probably become acquainted with, of the supposed importations of the Yellow Fever into Philadelphia and New York, and fix our attention upon his assertion, and his declared belief, that the black vomit had been found by the experience of ages, in the Spanish American possessions, to be *void of contagion*, and *incapable* of *being brought to Spain*, we must be sensible, that he could not, without *manifest absurdity*, believe that the Fever which prevailed epidemically in Andalusia in the year 1800, was produced by an importation of contagion from the Havana, where it is *non-contagious*. And, in fact, we have no reason to conclude, from his publication, that he did believe the groundless reports spread respecting the Dolphin; though, as she had been charged with this importation, he thought it proper to give such an account, as he has done, of the circumstances relating to it. In his answer to the first question proposed to him by the Board of Health in Denmark, "Whence was the poison or infection of the Yellow Fever reproduced?" He says, "I cannot answer this problem with physical certainty; I have stated in the chapter in which I endeavour to prove this disorder to be contagious, that I conceived it had been introduced into Cadiz in 1800, by one or more sea-faring

people, and propagated successively from one to another, until it prevailed epidemically in the town, *through the processions:*" and after mentioning particular places, to which he supposed it to have been conveyed from Cadiz, he thus proceeds; "although the proofs all indicate that the Yellow Fever came from *some* foreign part, I do not absolutely deny that it may have arisen sporadically in some person (as it must have done in the first person who had it) and communicated itself from him to others:" and he concludes this paragraph, page 447, by observing that he has ascertained that the Yellow Fever has *re-appeared* in years which were very dry, and productive of bad harvests, "de malas cosechas."—He had said nearly the same thing, at page 250, viz. "I have no physical certainty for denying that the Yellow Fever may not have originated *here* (in Cadiz); for the first man who had this fever, *did not catch it;* this, however, is neither regular nor probable, and I believe that it may have spread from one, or more of the many vessels which arrived here, whether from the United States, (*which is probable**) or from Africa, which I do not

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* Here we find Dr. Arejula disclaiming *in effect* all belief that the Yellow Fever had been imported to Cadiz either from Africa, or the Spanish American colonies (*including the Havana*) or from any other part of the world, excepting the United States; and in regard to *these*, the evidence produced concerning Charleston was

think presumable; or from *our American possessions*, or other parts of the world, *which is very unlikely*; (que no es lo regular) what is certain is, that we suffered from the Yellow Fever *here*, although *I repeat my persuasion that it cannot be ascertained whence it came.*" Again, at page 256, he says, "I do not venture to determine with certainty, whether the epidemic was received here by infection, or *originated* here; though *analogy* leads me to believe it to have had a foreign origin." We see, by these quotations from his volume, that Dr. Arejula had no foundation for believing that the epidemic of 1800 had been produced at Cadiz by contagion, excepting his *prejudices* and his *analogies*,—that, being led by these to think it not likely that the epidemic had *originated there* from domestic *local* causes, he was of course disposed to suspect an imported contagion to have produced it; but that he was utterly unable to fix upon any *particular vessel* as its *vehicle*, or *any place* as able and *likely to have furnished it*, excepting *generally* the very extensive coast of the United States; from which it could not by any possibi-

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very sufficient to prove that no such importation could have taken place from that port, and much less could it have happened from any of the states *farther north*, where the fever, when it does occur, makes its *first* appearance, at a *later* part of the summer or in the autumn. The inference then from Dr. Arejula's admissions or statements, and from indisputable facts, *must be* that *there was* in *reality no importation* of the Yellow Fever at Cadiz, from any place whatever.

lity have been conveyed to Cadiz, so early as would have been necessary to produce the epidemic in question, supposing, what is contrary to the strongest evidence, that the Yellow Fever really possessed a contagious property.

After reporting Dr. Arejula's account of the Dolphin, Sir James Fellowes adds, that he can no-where find in the Professor's work that he had "deemed it proper to reject the stories concerning the Dolphin." If a *formal* declaration of such rejection was sought for, I believe it could not be found; but, that he did in effect reject them, is, I think, evident from the extracts quoted by me from his work, and from his answer quoted by Sir James Fellowes, p. 432, to the question "who introduced the disorder into Cadiz?" viz. "*I believe it to be wholly impossible to explain this* clearly," an answer which he certainly would not have made, if he had believed those stories, charging, as they did, the importation *exclusively upon the Dolphin.* Sir James Fellowes adds, p. 435, that Dr. Arejula told him, "he *believed* the circumstances mentioned concerning the ship" (Dolphin).—What *circumstances* are here meant, I know not. He might have believed most of those mentioned by himself, without believing that a contagion, producing Yellow Fever, had been imported by the Dolphin; and I certainly hope, for the credit of Dr. Arejula, that he did not ever mean to tell Sir James Fellowes that he

really believed such contagion to have been imported *in her*, because his doing so would have indicated a degree of inconsistency very little reconcileable with sanity of judgment, for the following reasons, viz. In several parts of his work, and particularly in a note to p. 15, Dr. Arejula says, that he differs from *Cullen* in regard to the conveyance of contagion by *fomites;* and adds, that he "has not known any town into which the Yellow Fever had ever been introduced by *goods* or *effects;* but had discovered *in all places*, that it had been brought by some individual or person to the infected town." And again, at p. 316, he states it to be his opinion, concerning the conveyance of contagion by goods or effects ("en las ropas,") that it was, if not impossible, *difficult*, and *most unlikely*. "I speak," adds he, "of our epidemic; and this is strengthened by recollecting that, in those places in which the origin of the epidemic could be ascertained, it was always some *person*, who had not previously undergone the disease, *who brought* it thither, the fever always first shewing itself in him." Now, as no one *person*, either of the passengers, or crew, on board the Dolphin, was *ill* at the time of her arrival, or was afterwards attacked by the Yellow Fever, it follows of necessity, that, if there had been a communication of contagion from that vessel, it must have been made, not *personally*, but by goods or effects, which Dr. Arejula has deemed incapable of conveying it; and, therefore,

having this conviction, he could not have made a proper use of his reason, if he believed that any contagion had been communicated from the Dolphin, so as to produce the epidemic of 1800 at Cadiz. In regard to the colloquial information which Sir James Fellowes states at p. 436, that he took pains to procure from the *Spanish* captain of the Dolphin, it is not entitled to credit, when it is opposed by the evidence delivered on oath at Seville by Don Jose Caro, Chief Physician to the Hospital at the Havana, as well as by his personal declarations to Dr. Arejula, and more especially on a medical subject. According to that gentleman's testimony, the three persons who died on board the Dolphin were attacked with specified disorders different from the Yellow Fever; and the last of their deaths had occurred ten days before the vessel arrived in the Bay of Cadiz, where, as Dr. Arejula relates, she, and every person who came in her, were detained for ten days more under the strictest quarantine (guards being placed on board, and armed boats around her at some little distance, to cut off all communication), at the end of which time the Dolphin was released, and obtained pratique, it being found that every person in her had continued in good health. Had a contagious disease actually existed on board of this brig, its communication to the shore under such circumstances must have been impossible, at least if there be any use in quarantine regulations and precautions. But with evidence so

decisively proving that no such contagion was on board the Dolphin, *or even in existence*, no one can seriously believe that this vessel brought and communicated the cause of the Epidemic in question, and it must therefore be useless to prolong this discussion. Indeed Sir James Fellowes himself, although he manifestly wishes to have this story of the Dolphin believed by others, has thought it prudent to declare at p. 438, that "it is not his purpose to offer any decided opinion as to the origin of the epidemic at Cadiz in 1800," &c.

On this subject Dr. Pym has not observed equal caution;* but his observations do not, in my judgment, require or deserve any particular notice.

The contagionists having completely failed in their efforts upon *this*, as they will be found to have done on every subsequent, occasion, to point out any particular *place whence*, or any particular *vehicle by which*, an importation of contagion, capable of producing the Yellow Fever, could with the smallest probability be supposed to have been made; and the multiplied proofs which I

* At p. 6, of his volume, he says of his Bulam Fever, that, in "the year 1800, it again shewed itself at Cadiz, *was traced to importation*," *(which is manifestly untrue)* "and spread thence to Seville, Xeres, Malaga, and other places."

have given from the most respectable authorities, that the Yellow Fever is not a contagious or an importable disease, being so irresistibly conclusive, it would, I think, be a waste of the time of my readers, as well as of mine, if I were to enter upon a serious and minute refutation of the *idle stories*,* and the frivolous objections, which

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* Among these stories, Sir James Fellowes has seriously adopted and related the following, at p. 481 of his volume, viz. Don Diego Alvarez de la Fuente, one of the inhabitants of Malaga, having been appointed to superintend the three districts of the Barrio de Perchel, and, "on the 15th of November, 1803, having ascertained that a quantity of *infected* cotton goods was concealed in a certain house in this Barrio, he gave information to the Governor and to the Junta of Health, who afforded him the necessary authority. He proceeded to the house, in the *stable* of which a *box* was found *buried*, containing several parcels of cotton, and cotton goods, plain and coloured muslins, carpets, &c. *The removing of these goods occasioned the deaths of the Custom-House officer, Don José Malat*, of his *three assistants*, and *of two galley-slaves*, who were employed to *dig them out;* so that all the persons who assisted the Alcalde, *including the family that lived in the house, fell victims* to *the activity of the putrid miasms.*"— And we are farther told, that this statement appeared in the Madrid Gazette of the 6th of December, 1803. That the persons employed in digging up this box might have died (as nearly twenty thousand persons did) of the Yellow Fever in Malaga, in that summer and that autumn, is very credible; but, that the digging up of a box with goods smuggled and concealed, in *order to be sold*, and which no person could have any motive to *infect*, and which all the infection contained in ten thousand bodies, either suffering, or dead, from Yellow Fever, *would not have been able to infect*, so as to produce the deaths here mentioned, is indeed much more than I can believe; and, considering that this hap-

my opponents have brought forward, either against my doctrines, or in support of their own. According to most of their allegations and statements, the supposed Yellow Fever, to produce the effects ascribed to it, must sometimes have possessed greater activity, and have extended itself further and with greater velocity, than any contagion which ever has been known; and yet, on other occasions, they are forced to acknowledge that it is either wholly inactive, or at most but feeble and precarious in its operation, unless assisted by other powerful causes. Thus Sir James Fellowes, after putting the credulity of his

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pened in the middle of November, it certainly is at variance with every thing that is *intelligible* in the following account, given at his 402d page of the Yellow Fever in Spain, viz. its "contagious property seemed to depend on a certain *temperature*, which is necessary to the existence of the disorder, and a *combination of circumstances*, connected with *individual predisposition and climate*, which, although *difficult to define*, may be *comprehended* by those who have *resided* in the country, and who have *studied* the *character*, *habits*, and *mode of life of the inhabitants*." That long residence and study in *Spain* are requisite to discover that contagion *there* regulates its operations by a knowledge of, and regard for, the characters, habits, and modes of life of the inhabitants, is an observation which I was not prepared to expect.

Of nearly equal value and credibility are the statements given by Sir James Fellowes, respecting Bombardier Fenton's wife, at p. 118; that respecting Mack, the tailor of De Roll's regiment, at p. 126; that of a girl, the daughter of Belando, at p. 121; that of Mr. Alexander at p. 145; and that of Pratt, and the captain of his privateer, at p. 455; with others which I pass over.

readers to very severe trials by his accounts of the tremendous power of the alledged contagion of "the pestilential fever of Andalusia," comes at last to a conclusion which entirely changes its *terrific* form, and leaves it but little more than an equivocal, unintelligible existence. At p. 403 he makes this *conclusive* declaration, viz. "As far as my experience goes, I should be induced *to believe*, that *human contagion* having acquired a concentrated violence from a combination of *peculiar* circumstances, joined to the *epidemic tendency of the bilious remittent of the country, gave rise to the pestilential disorder in Spain.*" If this author had any distinct conception of his *own meaning* in regard to the *composition* of this *strange, incomprehensible chimera*, the alledged cause of the epidemic in Spain, we may justly *wonder*, and *regret*, that he was not able to express it more *intelligibly* to others.* What species of *human* contagion is to be here understood? what were the "*peculiar* circumstances" which gave it this "*concentrated virulence?*" and why, after having (no matter how) acquired this *vi-*

* I request that I may not be understood to insinuate that Sir James Fellowes is not sufficiently capable of forming *distinct* ideas of objects that have *any real existence*, and of communicating those ideas to others, as I am well convinced *of the contrary;* and I merely consider his failing to do this in regard to the supposed contagion of Yellow Fever, to be at once the effect, as well as a proof, of its *non-entity*.

rulence, was it unable *to act*, *unless* " joined to the *epidemic tendency of the bilious* remittent of the country?" which might *alone* suffice to produce and account for the disease; this having *generally*, according to his own description, and to the acknowledged fact, been attended with *remissions*, and having, in reality, been nothing but an aggravation of that form of fever, produced by causes which will be soon noticed, and which were abundantly sufficient for that purpose without the presence and co-operation of any supposed contagion. Here it will be seen that Sir James Fellowes has, in a great degree, adopted the *ideas* of Dr. Hosack, of which I have already taken sufficient notice at p. 193. He had indeed in a former part of his work, quoted one of Dr. Lind's undigested, and afterward discarded, notions respecting the Yellow Fever, expressed in the following words, viz. " the fever may be communicated by contagion, but this contagion *is very slight*, unless co-operating with a *bad air;* and in a *ship* is often greatly checked, if not *wholly destroyed*, by going out into the open sea."—" This," says Sir James Fellowes, " is *consonant to the opinion which I have formed on the subject*, and accords with the observations which were afterwards made in the different epidemics of Spain, &c." But it certainly does *not accord* with *his* accounts of the *wonderful velocity* and *force* with which the supposed contagion is alledged to have extended itself from Cadiz to Seville, and other

parts of Spain. That there was sufficient "*bad air*," mal-aria, or miasmata, in Cadiz, to produce an epidemic in 1764 as well as in 1800, is a fact capable of abundant proof; and it was by removing from situations where it prevailed, that ships had become healthy, as mentioned by Dr. Lind, and not in consequence of the extinction of any contagion by ventilation. But I must here return to the 403d page of Sir James Fellowes, where, immediately after the words recently quoted, he adds the following: "It is *possible* that persons coming from *Vera* Cruz, or the *Havana*, and carrying with them the *seeds of disease*, admitted to be endemic in those places, might, during their passage in a *crowded* ship, undergo such a change of constitution as to produce the disorder, with the *additional property* of *generating it in others highly predisposed*." Here we have another *seeming explanation*, which does not *convey* (if it proceeds from) any *distinct ideas* of the things to be explained. What are we to understand by "*the seeds of disease?*" Are these the miasmata which do of themselves produce the Yellow Fever at Vera Cruz and the Havana, and which, when persons sail from these places, before they have had time to create fever, often produce it *at sea?* If these be what is meant by "the seeds of disease," I must deny that they ever have, by *crowding in a ship, undergone such a change*, as to produce the disease, with the addition of a contagious property; and it is moreover to

be observed, that the most reasonable of those who still retain the *old* (and probably, soon to be exploded) *prejudice* in regard to the supposed generation or acquisition of contagion by crowding, do generally admit that the property said to be so generated or acquired, is but *temporary and transient*, ceasing almost as soon as the cause or crowding is removed. But *where* is this crowding supposed to have taken place? In the *Dolphin*, a fine brig, pierced for eighteen guns, chartered by a person of rank and fortune for his own conveyance, with that of his servants, and such gentlemen as he was willing to admit on board? Is it credible that *he* would permit such crowding and filth, as to produce contagion? Or could such crowding take place in a vessel of her dimensions, when the whole number of persons on board, including seamen, did not much exceed thirty? And in regard to other ships, of which no one has been pointed out, I may observe generally that crowding is a rare occurrence in vessels coming *from* the American hemisphere; because most of the persons who go to that part of the world, either die, or settle themselves there, and the number who return is usually too small to occasion crowding. But if Sir James had looked to the regular course of events for information on this point, instead of seeking mere plausibilities, he must have found a redundancy and uniformity of facts, that are decidedly hostile to the supposition he has adopted, in the numerous trans-

ports filled with troops, or with prisoners, and sometimes with invalid and sick soldiers, which have been sent to Europe from the West Indies in the course of our late protracted warfare, without a single instance in which a contagious Yellow Fever has been either generated on board, or communicated at the ports of their arrival. The case of the Regalia in returning to England, recently mentioned at p. 224, is one of a multitude which might be cited; and equally harmless did the Calypso prove to be on her passage homewards (see p. 736 of my Essay), although charged by Dr. Chisholm with the original generation of the malignant pestilential fever at Bulama.

It is however possible, although it seems unlikely, after the proofs which he must have witnessed of the total absence of contagion in the Yellow Fever in the West Indies, and after the admission of this fact by Dr. Arejula,* and so many others, that Sir James Fellowes may have meant positive "human contagion" by his "seeds of disease;" but if so, the change which he supposes to be produced by *crowding*, must be superfluous, as well as

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* This admission of Dr. Arejula (which is most adverse to the doctrine of Sir James Fellowes) has been already stated at p. 265; but Sir James, although he has filled many pages with other quotations from him, has carefully avoided all notice of *this* passage.

that *high predisposition* which he states to be necessary, and which, if necessary, affords an additional reason for disbelieving the reality of any such contagious property.

That the Spanish physicians, amidst the consternation produced by the unusual extension and increased violence, as well as mortality, of the fever which prevailed epidemically in 1800 through a great part of Andalusia, should have yielded to popular prejudice, and to the *convenience* of accounting for these events by a supposed importation of contagion, need not, perhaps, excite our wonder; but that any enlightened, unprejudiced person should do so at *this* time, when we know that all attempts to prove the importation, or even the existence, of any contagion, as connected with Yellow Fever, have entirely failed; and that, notwithstanding the difficulty of producing negative evidence, a great and irresistible mass of adverse proofs have been stated, is, or in my judgment would be, most strange—especially considering the many local and atmospherical causes which had manifestly co-operated to *aggravate* and give *an epidemical extension to the usual bilious remittent of that season.*

"Towards the latter end of the year 1799," says Sir James Fellowes, p. 33 "the weather was remarkably severe, and it continued so during the months of January, February, March, April, and May, of the year 1800, with

equal irregularity. Excessive *cold heavy rains* and violent winds alternately succeeded each other, so that there was scarcely any appearance of spring; the heat of summer set in from the beginning of June, and by the month of August the mercury in Fahrenheit's thermometer rose to near 90°, according to Gonzales's meteorological observations; and the prevalence of the east wind, or *Levante seco y abrasador*, as it is called, tended to increase the distress which the *intense* heat of the weather generally occasioned."* Now here was a concurrence of causes which must have obviously operated to give to the bilious remittent the *utmost violence* which miasmatic fevers are susceptible of, according to invariable experience in all countries where they prevail. The heavy rains and extraordinary cold of the spring months were exactly suited to create in the *human body* that unusual accumulation of excitability and inflammatory diathesis, which would afterwards render it highly susceptible of the most dangerous excitement, and at the same time to afford to the earth an abundance of moisture, which, being afterwards acted upon by the *scorching dry east wind*, and a temperature of nearly 90° of Fahrenheit, would produce and *maturate* a copious supply of the most viru-

* For a more particular account of the morbid effects of this "*burning dry east* wind," or Levanter, see p. 12 of Sir James Fellowes's volume.

lent miasmata; and these, aided by the sudden transition from a very cold spring to a temperature exceeding that of the West Indian islands, must of necessity exasperate the fevers, when produced, in the highest degree. But as a continuance of this high temperature would be necessary during several weeks to produce and give full maturity to the fever-exciting miasms, and for their effects on the body to be made manifest, we find it stated in a Supplement to the Madrid Gazette, published on *this subject* exclusively by the authority of the government, that *bilious remittent fevers began* to appear at Cadiz about the 8th of August, in the Barrio de Santa Maria, the most dirty, crowded, and ill-ventilated quarter of the city; and these having some days afterwards attracted the particular notice of the magistrates, the physicians were convened, but differed in their opinions concerning the cause and nature of the fever, as a considerable part of them, to use the language of Sir James Fellowes, "considered it as a *simple epidemic of the season,* rejected all idea of contagion, and spoke only of the effects of *heat,* of the *dry* state of the atmosphere, of the exhalations from the sea, of the drains, of the low tides," &c. p. 35. *Nothing therefore

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* Sir James Fellowes says (in a note at p. 36), that, as I had omitted a note annexed by Dr. Arejula to his Observations on this subject, he will insert it "from the *original* work, *given to him by the Professor himself.*" The note states, that "the two physicians, Don Francisco Far, and Don Pedro Navas (who had most

was decided at this, or at the subsequent meeting; and the disease, though at first mild, seems to have continued its progress, and increased in its violence, as miasmatic fevers are commonly found to do in similar circumstances; and as this might be expected to do, while the very high temperature, with a dry, and almost *stagnant atmosphere, continued.* "By the middle of September," says Sir James Fellowes (at p. 45), "the deaths amounted daily to two hundred: at this period the air, from its *stagnant* state, became so vitiated, that its noxious qualities affected even animals; canary-birds died with the blood issuing from their bills, and in all the neighbouring towns, which were afterwards affected, no sparrow ever appeared during the epidemic.*

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vehemently maintained that the prevailing disorder was not contagious, affirming that they had not lost a patient), were the first who died of the fever in question, within four or five days of each other." Now I am at a loss to comprehend why I should have inserted this note, or why Sir James Fellowes has done so. He surely cannot have done it in order to publish the gift of a book from Dr. Arejula; and I should suppose he cannot imagine that the deaths of these physicians would be admitted as proof that the fever was contagious; for whether it proceeded from miasmata or contagion, physicians, who were exposed to its cause like other persons, might be expected like them to sicken, and sometimes to die.

* Here Sir James Fellowes quotes as follows from Dr. Arejula: "Dogs were affected by the epidemic more than any other animals; next the cats and horses, poultry and canary-birds; and these last died voiding blood from the mouth: and the dogs and

If this description of the morbific qualities of the atmosphere at Cadiz, and other places, in which the *epidemic* prevailed in 1800, be *not greatly exaggerated,* there certainly can be no need to look for any other cause to account for its production; and that this cause did really exist in a very unusual degree, may be inferred, not only from its effects in Andalusia, but in many places along the coast of the Mediterranean, and on the opposite coast of Barbary, at Tangiers and Tetuan: indeed Professor Berthe was so sensible of this fact, that at page 366 he does not scruple to admit, that, if no contagion had been introduced at Cadiz, (as he had been *misled* to believe) in 1800, the causes of disease there, and in other parts of Andalusia, were such that a violent *epidemic,* or Miasmatic 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

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cats also had this symptom, but were more subject to the black vomit, and dark evacuations."—Sir James Fellowes adds, "all the physicians in Cadiz and Malaga who have written on this disorder, and with whom I have conversed, have confirmed to me these facts."——Here I would seriously ask, whether Sir James Fellowes has really believed that the atmosphere of Andalusia could have been so much and so extensively infected by an *imported* contagion, as to produce these effects; and, if not, why he is so anxious to inculcate a belief that the epidemic had such contagion for its cause?

coast,* which he admits to have been void of contagion; although by his own description it could have been different from the epidemic at Cadiz only in *degree* of violence. But Sir James Fellowes endeavours to divert the attention of his readers from the consideration of *local atmospherical* causes to a belief in the presence and operation of

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* At page 429 of his volume Sir J. Fellowes has stated objections to what I had in my Essay, alledged of the extension of Miasmatic fevers along the coast of the Mediterranean, but he has done it with a *confusion* of dates. I had asserted at p. 454, that this extension in the year 1800 reached as far as Genoa, where 150 persons died daily for some time of the *Yellow* Fever, properly so called, and as that city had been sometime closely *besieged by land, and blockaded by sea*, I inferred that it could not have been produced by an *imported* contagion. A similar fever occurred at Leghorn in 1804, and Sir James Fellowes is pleased to suppose that it must have been introduced to the *latter* city by a vessel from Malaga; adding, "but it did not appear at *Genoa*, as far as I know." Whether he means that it did not appear at Genoa in 1800, or in 1804 is to me doubtful; but however that may be, I cannot admit that the truth of such a fact is disputable, because he did not know it. He adds, "how the same epidemical constitution of the atmosphere should have extended along the Mediterranean and affect Leghorn only, and no other intermediate place, I do not understand, &c." My observation on this subject related only to the year 1800, in which *intermediate* places were affected; and particularly *Cette*, which being near Montpellier, the abode of M. Berthe, he was able to speak of the disease with certainty; and he describes the fever of that year, in that city "as a true epidemic, which frequently assumed a malignant character, and terminated fatally in a few days."—See his pages 363 and 364.

imported contagion, by introducing stories fit only to mislead the most credulous; and which are incompatible with facts the best ascertained, and even quoted by Sir James Fellowes himself, from his great authority, Dr. Arejula.—Such is the popular, extravagant report circulated (and given by Sir James, at page 40 of his work) without any sort of evidence, that Don Joseph Villialta, Acting Member of the Board of Health, had been *bribed* to give "*pratique* to the Dolphin, sooner than the time required," (an allegation contrary to the well-known *fact* stated by Dr. Arejula, and *quoted by Sir James*, of her having been kept in *strict quarantine*, until the full expiration of twenty days, from the last death which took place on board) and that he also "*connived* at a communication with persons from the shore, and particularly with smugglers, and others living in the Sopranis and Boquete;" though it is difficult to conceive either the possibility of, or the motive for, any such communication, amidst all the precautions taken to hinder it; and infinitely more difficult to believe, that, if it had taken place, any contagion would have been conveyed to the shore, from a vessel in which, according to the strongest evidence, it could not have been contained. As the climax of this report, we are told, that Don Joseph Villialta, who lived near "the Boquete, where the disease broke out" (and consequently was there greatly exposed to the same miasmata, which had caused

it in others) " was *reported* to have caught the disease, and it was added that, *struck with remorse* at *the dreadful effects which he foresaw* were likely to result from his misconduct, and feeling deeply the reproaches which were *heaped* upon him by his acquaintance, *he gave himself up to despondency*, and died in a few days, of the prevailing fever, about the 20th" (probably August). We are thus expected to believe that this officer's acquaintances, convinced that he had introduced a most dreadful *contagion* at Cadiz, and *that he was actually sick* of the *fever produced by it, did,* without any regard for their own personal safety, approach his sick bed, in order to *heap reproaches upon him ;* and that " these reproaches were *felt so deeply*, that *he gave himself up to despondency, and died in a few days* of the prevailing fever." But this appears to have been *merely* a report of which no one knew the *origin.*

I trust it will not be expected that I shall particularly notice, much less refute, such *improbable* reports. And, in regard to others, excepting statements, in which there is good reason to believe that essential facts have been either omitted, or misrepresented, I have found no one which does not admit of a more obvious, natural, and probable explanation by recurring to the action of miasmata, than to that of contagion, as the cause of the epidemic : and therefore, after again referring my readers to the account of this Epidemic, as given in my Essay, I shall content

myself with answering such of the particular objections urged by Sir James Fellowes as seem to deserve some notice. And first, in regard to the procession at Cadiz, mentioned by me at pages 451 and 452, which was said to have been the principal cause of spreading the disease, I persist in thinking that the numerous attacks by the fever, which are stated to have ensued *almost immediately,** were occasioned, not by any communication of contagion (which, had it existed, would have required more time to produce the disease) but by an exposure to the burning rays of the sun, during *seven* hours, and the fatigue, terror, and other mental impressions, which were endured by an immense concourse of people, walking principally in the *lowest* parts of the town, where fever-exciting miasmata were the most prevalent. Those among this multitude, who had previously imbibed a sufficient dose of these miasmata, would by this exposure and fatigue, have brought on a more *speedy attack* of the fever; and others would be apt to imbibe them, during such exertions in streets and places where they chiefly

* M. Berthe, at p. 69, states on the authority of a principal magistrate at Cadiz, viz. the "Procurador Mayor, Don Miguel de Irribaren," that on the *day after* this great procession, the number of sick was increased by between five and six thousand *new attacks;* an effect *so sudden*, that it could not have resulted from the dissemination, *during that procession*, of any species of contagion yet known.

abounded:* and although Sir James Fellowes refers to what *we* saw of processions in Portugal, I do not find reason to believe that persons *so ill* as those must have been, who were labouring under the Yellow Fever, were likely to have joined in the processions at Cadiz, and to have caused an extension of the disease by the communication of its supposed contagion to others.

In a note to page 426 of my Essay, I had corrected an erroneous statement made by Professor Berthe, who, misled by the contagionists in Spain, had multiplied the *three* deaths on board the Dolphin, so as to make them *nine*. This at least was a proof, *among many others*, that I did

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* Gibbon, after noticing the hostile attacks which had rendered the Campagna of Rome a dreary wilderness, in the seventh century, adds, vol. iv. 543, that "in a season of excessive rains, the Tiber swelled above its banks, and rushed with irresistible violence into the valleys of the *Seven Hills*. A pestilential disease rose from the stagnation of the deluge, and so rapid was the contagion, that fourscore persons expired in an hour, *in the midst of a solemn procession, which implored the mercy of Heaven.*" And for this he refers to Gregory of Tours, lib. x. c. i.—We have here an instance of a violent miasmatic fever, which was called *pestilential* with as *little reason*, as that of Andalusia has been so called by Sir J. Fellowes; and an instance of a procession attended with peculiar mortality, and as unavailing as that of Cadiz. I had in my Essay mentioned marsh miasmata as contributing to the evils resulting from the latter of these processions, and Sir J. Fellowes, conformably with the misconstruction of which I lately complained, makes this *answer* at p. 41, "I have already shewn in the description of Cadiz that these *marsh* miasmata do not exist there."

not copy, or adopt the statements of that author, without proper discrimination; but Sir James Fellowes, from the want of a better opportunity to depreciate my work, or rather my authorities, has seized on this, although it could only have suited a purpose directly the *reverse,* and said at page 426, "This is one of the numerous instances of the inaccuracy of the French report, from which Dr. B. has *principally* collected his information, on the subject of the fever in Andalusia." And in the next page, after mentioning Dr. Pascalis, as another of my authorities, he proceeds to say, "I made particular inquiries amongst the Spanish physicians, how far these different accounts (of Berthe and Pascalis) were to be credited, and the report they gave was this; that with regard to the French commission, not one of its members ever saw the disorder which the Précis Historique professed to particularize; for those gentlemen *remained quietly at Madrid, until the fever had subsided.* They then undertook the journey, and during their progress through Andalusia, they were *indefatigable* in collecting from individuals, all the *remarkable stories* and *popular accounts of the fever,* which formed the substance of their report. M. Berthe, chief of the Commission, remained at *Malaga* twenty-four hours.* It must however be acknowledged that several

* I have not been able to discover that Professor Berthe, or either of his colleagues ever visited Malaga; and it would appear that, on

of the facts stated in that work, were confirmed to me by the Spanish authorities." Here I am to observe, that the ignorance displayed by these Spanish physicians, on whose information Sir James Fellowes relies to establish the supposed demerits of the Members of the French Commission, is *less astonishing than his own;* as manifested by his adopting and publishing that information.—Had he *read,* as he professes to have done, M. Berthe's Précis Historique, he would have found that the French physicians were so far from *waiting at Madrid* until the Andalusian Epidemic had subsided, that they did not even *go to that capital,* in their way to Cadiz, and that they did not depart from Montpellier until the 25th of November (which was only three days after their appointment) and took their course by *Barcelona, Valentia,* Cordova, &c. directly to *Seville,* and thence to Cadiz.—That they did not, and could not, with all their endeavours, reach any place where the fever had prevailed, until after it had

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leaving Cadiz, they proceeded to Xerez, and thence by Utrera, to Carmona, " premier point d'ou nous étions partis, il y avait environ deux mois, lorsque nous pénétrames dans l'intérieur du cordon," Précis Hist. page 45, and that they returned into France by the route by which they came. As the fever at Malaga in 1800 was somewhat less violent than at Cadiz, Seville, &c. it was considered by the Members of the French Commission to have been the ordinary autumnal remittent, although with more than usual malignity. (See page 337, Précis Hist.) Dr. Arejula however (page 154 of his work) and other Spanish physicians have considered it as the Epidemic Yellow Fever.

ceased, is a fact which was stated as matter of regret by themselves, and therefore the information of their not having seen the disease was at least superfluous. They were selected for this mission by the University of Montpellier, in consequence of an application made by the Spanish Government, for the opinion of that University respecting the epidemic; and as they were sent and *especially protected* by the French Government, whose Ambassador (Lucien Bonaparte) was then all-powerful in Spain, and were particularly recommended to the Spanish authorities, in all the places to which they went, and were, (as Sir James Fellowes observes), indefatigable in their endeavours to collect information, there can be no doubt of their having collected the *best which the country afforded;* at least the persons *named* as being their informants,* were in situations the most respectable, and the best suited to afford all the knowledge which had been acquired on this subject. These informants were indeed generally prepossessed with a belief that the epidemic had been produced by an imported contagion; and in consequence of this belief, groundless as it was, they had listened to "the remarkable stories and popular ac-

* M. Berthe at page 39 makes his acknowledgments to fifteen of the most eminent physicians *at Cadiz* by name (including Dr. Arejula) for the readiness and candour with which they had communicated every fact within their knowledge, and at page 33, to several of the magistrates, physicians, &c. at Seville.

counts of the fever," and reported them to the French physicians, who, (as I had occasion to mention with regret, in my Essay) had permitted themselves to be *misled* by these stories, and accounts, so far as afterwards to publish them, as evidence of the contagious nature of the epidemic. But in this respect, their error does not appear to have been at all greater than that of Sir James Fellowes, whose volume certainly contains *stories and popular accounts* far more extravagant and incredible than any published by M. Berthe, and without an equal degree of candour in stating facts unfavourable to the doctrine of contagion, whenever his informants, the Contagionists, had made him acquainted with them, which indeed appears to have been seldom.

Sir J. Fellowes, after these reflections upon M. Berthe, proceeds to Dr. Pascalis—" The respectable physicians (says he), in Cadiz and Malaga could give me very little information of Dr. Pascalis; they recollected a person of that name, who came into Spain from Pennsylvania, with a set of queries, to which it was not always *easy* to give immediate answers; and as he had been personally known to but *few of them*, they could give *no* account of him. It was believed that, dissatisfied with his reception, and the *imperfect* information he obtained, the Doctor returned to America in disgust, and afterwards wrote an

account of the fever which he had not *seen*;* from which Dr. Bancroft has quoted several circumstances, that are positively contradicted by Dr. Arejula, and all other respectable Spanish physicians." These uncandid reflections seem to have been made, only because Dr. Pascalis, who took the trouble of going to Spain *to ascertain the truth* respecting the Yellow Fever of that country, has stated some facts repugnant to the doctrine of the importation of contagion, which I have quoted; and these seem to have afforded all the knowledge which either Sir James Fellowes or the Spanish physicians possessed of the Doctor's account of the fever. But having *seen* my quotations, I think it was a duty incumbent on Sir James to have specified those facts, which, as he says, have been contradicted by Dr. Arejula, &c., for at present I am unable to discover them. As Dr. Pascalis did not adopt the Spanish prejudices in regard to contagion, and as these physicians probably were unwilling to communicate facts adverse to those prejudices, it is possible that he may have been disgusted; yet I do not recollect any expression of disgust in his publication.

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* Though Dr. Pascalis might not have seen the Yellow Fever in Spain, he must have seen many cases of it in the United States of America, and as he had once been inclined to think it contagious, he must have seen it with eyes not indisposed towards the discovery of truth.

In regard to my quotations from either Professor Berthe or Dr. Pascalis, I believe they have been made with sufficient discrimination; and my readers will probably be of this opinion, so far at least as concerns the former, when they recollect that Sir James Fellowes, from his inability to adduce a single instance in which I had permitted myself to be *misled,* was under the necessity of employing one in which I had corrected a mistake, in order to discredit this one of my authorities: in doing which it *ought not* to have escaped his notice, as it appears completely to have done, that Dr. Pym, to support his assertions *on the side* of contagion, has filled twenty-four of his pages (from 77 to 101), entirely with quotations from M. Berthe's volume, and had, at page 15, stated his reasons for believing that M. Berthe and his colleagues "were men of honour and integrity, who considered truth as sacred," and for "placing *implicit confidence* in the detail of facts and observations" which that volume contains. For the rest, I must confess that I did not expect to have been censured by Sir James Fellowes respecting my quotations or authorities; having been led to believe that the number and importance of my facts, and the respectable sources from which they were derived, (many of them being but *little known* and of *difficult access*) had obtained for me no inconsiderable portion of approbation.—Certainly *he* might have added to the number of my *Spanish* authorities when he left England, to join the British forces

at Cadiz: having been in that city some years before, he had procured the latest publications on the subject of the Yellow Fever there; and from the terms on which we had lived, I did imagine that he would have willingly favoured me with a perusal of them; but being unable to obtain this favour, at the visit which he made to me immediately before his departure, I *earnestly* requested that, as he was going to the place where these publications might be repurchased in every bookseller's shop, he would leave them with me, and in return accept their full value or cost. *That he refused this is a fact which he must remember;* and if he will also remember and state any reasonable motive for his refusal, except an unwillingness that I should have the chance of discovering *any fact unfavourable to the doctrine of contagion,* which the Spanish physicians might have published, he will enable me to remove from my mind some impressions which I have been unwilling to entertain.

As another proof of my solicitude to procure all possible information on this subject, I think Sir James Fellowes must have known, that by the kindness of Mr. Canning, when Secretary of State for the Foreign Department, a series of questions concerning the epidemic of Andalusia, which had been proposed by me, were sent to the British Ambassador in Spain, with a request that answers to them, from persons the best informed, might be

procured and returned. Perhaps he may also know, how it happened that this request proved as fruitless as my application to himself.

One thing which I have quoted as a *fact* from Dr. Pascalis, and which Sir J. Fellowes and the contagionists, for obvious reasons are disposed to deny, is that the epidemic of 1800, began at *Seville* a little *sooner* than it did at *Cadiz*, whence they pretended that it was communicated to the former of these cities—(See page 454 and also the *note* at p. 459 of my Essay). By the statement prepared and published by order of the Spanish Government, already noticed, it appears that *fevers* which were deemed to be the usual *remittents* of the season began to appear in the *low* and unclean Barrio de Sta. Maria at Cadiz, about the 8th of August.* At what precise time these had

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* Dr. Pascalis says, "*public records* make it appear that the sickness broke out on the 2d of August in Cadiz, the supposed infected Port, and on the 23d of July in the suburb of Triana at Seville."—See New York Medical Repository, 2d Hexade, vol. iii. page 389. Here Dr. Pascalis fixes the commencement of the disorder at Cadiz somewhat earlier than the statement published by the Spanish government, which indicates at least that his intentions in regard to the priority of its appearance at Seville were fair and upright. He doubtless considered the first Remittent Fevers as similar to those which constituted the epidemic; and Dr. Arejula, at page 245, mentions these as having appeared about "the end of July or beginning of August, 1800, in the south eastern part of the town, known by the name of Barrio de Santa

so far spread and increased in violence as to be generally thought to differ from the fevers ordinarily occurring there at that season, cannot, I presume, be now ascertained; and it seems probable that the disagreement among the physicians concerning the nature of these fevers, was only terminated by the reports so confidently, though unjustly, propagated of the supposed importation of contagion by the Dolphin.

In regard to Seville I can find no Spanish authority

Maria, and extending to the streets of Sopranis and Boquete."—He adds, however, that "he had not been able to learn in which house the first sickness occurred; for the physicians neither considered the disorder as dangerous, nor dreamt that it could be caught; but nevertheless the increased numbers of the sick in these quarters, *about the middle of August*, excited the attention of the people, and also of the magistracy." Afterwards, at page 155, he says, "the Government began to discover that the sickness (in the Barrio de Santa Maria) exceeded greatly the proportion common to the season, in other years; and thereupon summoned a competent number of physicians, who declared that the fevers prevailing were of short duration, and required but little attention, and that they were only to be considered as simple synochal fevers, void of contagion." He adds, that even "at a *third* meeting of the faculty, summoned by the Cabildo, there were many physicians who declared that they had not had a single unfortunate case.—Others said that they had cured two or three hundred persons of the prevailing sickness, without losing more than one or two, and that the fever did not deserve serious consideration". The day of the month on which this meeting took place is not mentioned, but from circumstances stated by Dr. Arejula it must have been some time *after the sixteenth of August*.

excepting the "*public records*" mentioned by Dr. Pascalis, for fixing the precise day on which the epidemic really began. Whether this want of information is to be ascribed to the *gradual progress* of the disease, from the milder cases of fever to the more violent, as at Cadiz, or whether the physicians abstained from mentioning dates, because they would not accord with the belief which it was wished to inculcate, that the disease had been introduced from Cadiz, I know not. M. Berthe, who wrote from the information given to him at Seville, says at pages 59 and 60, that the epidemic first declared itself in the suburb of Triana, on the south-west side of the city, and only separated from it by the river Guadalquivir; and the first persons attacked by it belonged to the family named "*los Lebrones*," among whom it proved fatal to several persons; and probably this *mortality* was the reason, why the epidemic was said to have commenced in that family; the *milder preceding* attacks being overlooked as the ordinary remittents of the season. M. Berthe says that after being thus introduced at Triana, the disease was there confined to a small space for more than fifteen days; and afterwards spread through the whole suburb, occasioning great mortality. He adds, at page 64, that the suburb of Los Humeros, directly opposite to Triana, and merely separated from it by the river, was the quarter to which the disease first extended itself, while Triana was *ravaged* by it. These two suburbs, *adjoining the river*

on each side, were *low* * and filthy; and inhabited chiefly by the lowest classes of the people. How great the in-

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* It is remarkable that both at Seville and Cadiz as well as at Philadelphia and New York, the Yellow Fever invariably begins and prevails in *low moist* situations near the water. Dr. Hosack, now a moderate, though once a warm contagionist, says (see Edinburgh Medical and Surgical Journal, page 360 & seq. 1815) of the Yellow Fever at Philadelphia in 1793, "*it is conceded on all sides* that it made its first appearance in *Water*-street, and that all the cases of this fever were for two or three weeks evidently *traced* to that particular spot:" that *there*, "the air was at that time in a very offensive condition, from a quantity of damaged coffee, which was exposed upon the dock, under circumstances favourable to its putrefaction and exhalation." He adds that, "in general the more remote the streets were from *Water-street*, the less the calamity they experienced." This happened in subsequent years, particularly in 1803, see page 380—385 of my Essay. In regard to the fever at *New York* in 1795, Dr. Hosack says, it "appeared first on the *east-side* of the city, but spread thence from Dover-street to *Peck's-slip* (where it prevailed almost exclusively in 1791) always confining itself to *low* situations, though remote from each other:"—and he refers to Dr. Bailey, the health officer, as having stated "that the number of those taken sick in *low* situations, compared with those residing in more *elevated* parts of the city, may be computed as *twenty to one*"—Surely contagion never acts in this way. In regard to the Yellow Fever of 1798, Dr. Hosack says, "it for the most part followed the course of the vitiated atmosphere."——"Indeed (adds he) the extension of this disease, as has been already frequently observed, was so circumscribed within the limits of this impure air, that it became very generally believed that in that season, whatever may have been the case in former years, the disease exclusively arose from domestic sources." And M. Berthe for himself and his colleagues says, that the epidemic of Andalusia in 1800 "was always most destructive in *low and humid* situations." "I might (he adds) cite on this subject the ravages committed by the disease in several villages on the banks

terval was, from the time at which the *Lebrones* were first attacked, and that in which the disease first appeared on the opposite side of the river, I cannot ascertain.—But it must have been more than fifteen days, according to the statement of M. Berthe; and it was not, as far as I can understand his meaning, until the disease had shewn itself in the suburb of Los Humeros, that the edict of Don Antonio Fernandez Soler, dated the 3d of September (of which a copy is given by M. Berthe) was issued, forbidding the inhabitants of Triana and other quarters of the city, to remove from their dwellings, without having first obtained permission from the magistrates.

These facts, it must be owned, do not enable us to ascertain the exact day on which the epidemic may with propriety be said to have commenced at Seville; but, in conjunction with those stated in the subjoined note * they

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of the Guadalquivir and compare them with the different results which it had in other villages at a small distance, but farther from the river and on *rising ground*. We have procured the most accurate accounts in this respect, which it is useless to mention in detail as they all resemble each other." See his volume page 161. Can this be deemed contagion?

* By Dr. Arejula's Second General Table, which contains a statement of the number of persons who died by the epidemic at Seville in 1800, on each of the one hundred days to which his statement is confined, it appears, that in the *first* of this century of days, viz. the 23d of August, ten persons died; and on each of

make it highly probable that Dr. Pascalis did not err, in connecting it with the 23d of July. Certainly however,

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the two succeeding days twelve,—and fourteen the next (26); the same number on the 31st; and in the first five days of September the whole number was sixty-two; amounting on the average to about twelve each day. On the *last* of the one hundred days, which was the 30th of November, the deaths amounted to nineteen. It is evident therefore, that this Table does not comprehend either the first or the last deaths produced by the epidemic; and from their having amounted to nearly as many on the 23d of August as they did on the five first days of September, it may be presumed, that the progress of the disease from the first attacks, would have been as slow *before* as it was after the 23d of August; and considering that the attacks always precede the deaths by several days, and that probably here, as well as at Cadiz, mild Remittents prevailed for several weeks without any deaths, I think there is good reason to conclude, that the first appearance of the disease at the suburb of Triana (where the whole number of attacks amounted to nine thousand nine hundred and twenty-two) might very well have been as early as the day mentioned by Dr. Pascalis, viz. the 23d of July. The whole population of Seville in 1800 is stated in this Table to have amounted to 80,568, and the deaths during the one hundred days before mentioned to have been 14,685; and of these the greater part took place in the month of October, as has almost invariably happened in all the cities of Spain, as well as at Gibraltar; because the full force of the summer's heat seems to be necessary to give to the miasmata their utmost maturity and efficiency. This would not happen if the disease were produced by contagion. By Dr. Arejula's second Table, it appears that the deaths in September of this year at Seville amounted to two thousand one hundred and six,—in October to nine thousand two hundred and thirty-six,—and in November to one thousand two hundred and twenty-three. That the epidemic of 1800 should have begun sooner at Seville than at Cadiz is rendered very probable from the well-known circumstance (which is attested by Dr.

the violent and *mortal* cases must have occurred at Seville as early as the middle of August, if not sooner, and this was at least as soon as any appear to have occurred at Cadiz; and when we consider the feeble and slow progress of the disease at *Cadiz*, where, as M. Berthe asserts, it was often unable to make its way from one side of a street or square to another, and that at Seville more than fifteen days intervened before it was communicated from Triana to the opposite suburb of Los Humeros, though the intercourse from one to the other, by a *bridge* as well as by *boats*, was probably as frequent as it is between London and Southwark, it seems absolutely incredible that the epidemic at Seville should have been derived from Cadiz.

Sir J. Fellowes, at p. 177 of his volume, quotes my observations respecting the village of Alcala de Los Panaderos, and also my note respecting himself; and adds, "the facts above referred to by Dr. Bancroft are *true;* but his *manner* of stating them, and the *conclusions* he draws are

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Arejula, p. 243) that "the heats of summer are without any comparison greater in the more inland parts of Andalusia than in Cadiz," and they might therefore, in such a season, be naturally expected to have matured the miasmata sooner (and even to a more virulent degree, as appears to have been really the case) at the former, than at the latter of these cities, the proportion of sickness as well as of deaths having been considerably greater at Seville than at Cadiz.

by no means correct.—On my return to England I mentioned to Dr. Bancroft several *curious* circumstances, tending to shew that the disorder which had prevailed at Cadiz, Malaga, and Gibraltar, was contagious; but he had either forgotten them, or perhaps they might have appeared to prove *too much* against the position which he had laid down, and endeavoured to maintain, that the disorder in question proceeded from marsh miasmata, and was in no instance infectious. He has rather preferred mentioning my visit to Churriana, and my supposition about the burning of aromatic herbs." This insinuation against me is both unwarranted and injurious. I certainly have forgotten the "*curious* circumstances" said to have been mentioned to me by Sir J. Fellowes; but I well remember that he was never communicative to me on this subject, except upon two or three occasions, when by stating particular facts to him, he was induced to throw off some of his reserve. But whatever these *curious* circumstances may have been, I am confident they were not suppressed or omitted by me, "*because* they appeared to prove *too much* against the position which I had laid down;" *for nothing which he ever mentioned to me* had any such appearance, either to my eyes or my understanding; nor has any or every thing which he has since published weakened in the smallest degree my conviction of the truth of that position; and as he never could have had reason to think that any fact communicated by him to me

had produced the *least* alteration in my belief of the *local* origin of the fever in question, it seems to me that Sir James Fellowes might easily have found a *better* and more *probable* motive for my not publishing his supposed "*curious circumstances.*" How my *manner* of stating the facts to which he alludes, or my *conclusions* from them, are incorrect, he has not explained.

I ought here to observe, that among other places, along the Mediterranean coast, at which the Bilious Remittent Fever prevailed with unusual violence in the year 1800, *Malaga* suffered considerably; and Dr. Arejula (p. 154), appears to have considered it as the Epidemic Yellow Fever, or Vomito Negro, as does Sir James Fellowes; and it is probable that M. Berthe, and his associates would have done so, had not M. *Paroisse,* Surgeon to the French embassy, who was at Malaga, and treated some cases of the fever in *that* year, assured them that it was *not contagious;* and in short, that it was "*a Bilious Remittent Fever,* but *accompanied with malignity.*"

In the following year, 1801, the Yellow Fever occurred and prevailed to a considerable extent at *Medina Sidonia, an inland town,* thirty miles from the sea. How it originated, the contagionists have never been able to explain, consistently with their notions. Of this Fever, Dr. Burnett has, at page 490, published a description, trans-

lated from the Spanish of Dr. Tadeo La Fuente, by which, as well as by the account also given by Dr. Arejula, page 257, it appears to have been similar to that which had prevailed epidemically at Cadiz and other places in the preceding year: it was in some degree *less* aggravated, although the black vomiting occurred in several of the mortal cases.

A similar fever, but less violent in degree, and much less extensive in its effects, appeared also in the year 1801, both at Cadiz and Seville, and for some time excited considerable alarm, as mentioned at page 467 of my Essay. But the weather being neither so hot nor so dry as in the preceding year, it soon declined, and was then deemed a *Tabardillo* or remittent fever, attended with *Petechiæ* or purple spots. There was no pretence of imported contagion this year; but if the *local* causes had operated as strongly at Cadiz and Seville as they did at Medina Sidonia, there can be no doubt but one or more vessels would have been accused of this criminality.

In the year 1803 the Yellow Fever again prevailed at Malaga much more extensively, and with much greater violence than it had done in the year 1800. And as the contagionists, in conformity with their notions, were compelled to ascribe it to a foreign cause, it became necessary to discover, and point out, some *place* whence, and some

vehicle by which, it could have been introduced. They however, in attempting this, were not favoured by the events of that year, as no vessel had arrived, upon which the importation could be charged with even the smallest probability. And certainly among all the efforts which have been made at different times, to assign probable ways and means, in order to account for the supposed importation of contagion, at places where the Yellow Fever has prevailed, none ever exceeded in weakness those which relate to the pretended importation of the epidemic, or its *cause* at Malaga in 1803. Not being able to find any *one* vessel which could with any probability be *fixed* upon for this purpose, Dr. Arejula has had recourse to *four*; as men sometimes employ many *bad* arguments to supply the want of *one* that is good. The first of these vessels (Le Jeune Nicolas) a Dutch store-ship, had come from Smyrna, whence the plague has been brought sometimes, but the Yellow Fever *never*.* Two of the other vessels were French brigs, the *Dessaix* and the *Union*, which had

* Notwithstanding this fact, Dr. Arejula mentions a report of a smuggler said to have gone on board the Dutch ship from Smyrna, and brought away samples of cotton and tobacco, and to have afterwards sickened and died.—It is admitted, however, that the disorder of which he died, whatever it may have been, did not spread to any other person; and that an interval of thirty-five or thirty-six days occurred, before "the sickness broke out in another family, which is regarded as the source of the epidemic."—Sir J. Fellowes, page 164.

sailed from Marseilles for St. Domingo, each having on board upwards of one hundred men, "chiefly deserters, prisoners, or convicts" (says Sir J. Fellowes); "almost the whole of them (he adds), had been *forced* on board, and were taken from the forts, St. John and St. Nicholas, in which the *jail*-fever at that time prevailed:" and a considerable number of them had died of the disease on board the two brigs. Had the jail-fever prevailed at Malaga after the arrival of these vessels, they might reasonably have been suspected of having imported the contagion of it: but the Yellow fever *being a very different disease,* and Marseilles being a place at which it has never prevailed, except as a *miasmatic* fever from *local* causes; the error of considering these brigs, as the importers of the Yellow Fever at *Malaga,* is manifest, unless indeed a greater error was in contemplation; I mean that of supposing that the *contagion of typhus or jail-fever had changed its nature,* and become the *cause or contagion of Yellow Fever.*—Great as the latter mistake would be, Sir James Fellowes appears to have fallen into it: for, after giving an account (page 172), drawn up by M. Delestra, a French practitioner at Malaga, who was principally charged with the care of the sick belonging to the two French brigs from Marseilles, and who positively states that the disease with which they were attacked was the *jail, hospital* or *ship-fever,* of Pringle, Huxham, Monro, &c, and that these ships as well as Le Jeune Nicolas, were

condemned to the strictest quarantine, and *guarded night and day*, " par une Felouque armée," &c. without the least communication with the town, until the 7th or 8th of September, Sir James adds, " These are the facts stated with respect to the vessels in question, and the contagious nature of the disorder which appeared on board them. From the inadequate precautions which were taken by the Spaniards to cleanse and purify their ships, it is not *unreasonable* to conclude, that infection *may* have been communicated from them to the town, through the medium of boatmen, smugglers, and others."

Sir James Fellowes does not want common sense; but he must either have laid it aside on this occasion, or he must have *purposely* written in this *vague* manner, in order that he might *appear* to say something without being responsible for any thing. Did he mean to connect his observation about " the inadequate precautions that were taken by the *Spaniards* to cleanse and purify *their* ships," with the *French* brigs from Marseilles, to which the facts *immediately preceding* this observation related, and which were kept in *strict quarantine* until *six weeks after* the Yellow Fever *had begun* to shew itself at Malaga? If he did, as it has not been even pretended that the Yellow Fever had occurred in any person belonging to *the latter*, it will, I conceive, be difficult for him to assign just

grounds for such a connexion; and if he did not, and wished to make himself *intelligible*, why did he occupy so many pages of his volume with transactions which he must have deemed foreign to the importation of Yellow Fever into Malaga: and in the *very same paragraph* (which related to the French brigs), proceed *immediately* to his observation respecting the supposed want of due precautions in the Spaniards to cleanse their ships, without having either *previously* or *subsequently* mentioned a single Spanish ship accused of having had on board any person *ill of Yellow Fever*, or any *fomites* capable of producing it at Malaga?* Though Sir James Fellowes has thought proper to waste a number of pages upon the *four* vessels before-mentioned, he (as well as Dr. Arejula) appears to despair of making it even probable that any case or *cause* of the Yellow Fever had existed on board either of them; and he therefore pro-

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* The *last* of the four vessels in question was the Providence, from Monte Video, whence no contagious fever was ever believed to have been imported. Why the Providence was mentioned on this occasion, I am unable to conceive, unless it was from a convenience of mentioning four vessels rather than three. She had arrived on the 9th of June, and there being no reason to apprehend that she was in any way infected, she had been admitted to "pratique" as soon as this fact was ascertained, which was almost three months before the epidemic commenced; and it is not pretended that there was ground even to suspect that any contagion had been derived from that vessel.

ceeds to state from the latter, " that the disorder which broke out in Malaga in 1803, was believed positively to have commenced in the house of an inhabitant in the district of *Perchel,* on the west side of the *Guadalmedina,* of the name of Christopher Verduras." He (adds Sir James) " was a caulker by trade, but a noted smuggler, and a person who was likely from his character to do any thing for gain. It was generally believed in Malaga that Verduras, about the latter end of August, had brought from on board one* of the vessels in the bay, and secretly conveyed to his house, a person who was labouring under a disease, of which he soon after died; and it was afterwards discovered that the body was *privately buried* in the neighbouring church of St. Peter in *the middle of the night.*"† This popular, and probably groundless tale,

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* Sir James Fellowes adds in a note, " some affirmed it to be the Dutch vessel, others an American ship." It may be presumed that such conflicting assertions were made without either knowledge or truth. The Dutch vessel from Smyrna, as has been already observed, could not be reasonably suspected of having the Yellow Fever on board; nor are we told that any sickness whatever had occurred in her: and in regard to a *nameless American* ship, while no one is mentioned or known to have been at Malaga at that time, and much less to have had the Yellow Fever, or its contagion on board, I will only say, that if vague allegations, incapable of being disproved, were admitted, there is no falsehood which might not be maintained.

† Could such a furtive *midnight* funeral have taken place in a church in Malaga, without being obstructed by, or known to either watchmen or police-officers?

might indeed be true, without its being true that the person said to have died had been ill of *Yellow Fever;* and even if he had died of it, the thousands of proofs that it possesses no contagious quality, would preclude any such inference as is here drawn by Sir James Fellowes and Dr. Arejula, I mean, that of his having caused the epidemic at Malaga in 1803. But there are contagionists who imagine that they have proved every thing which they desire, by mentioning a *smuggler*, and *an unknown nameless vessel.* The presence of Yellow Fever in the unknown vessel is immediately *presumed,* and its introduction by the *smuggler* follows as an indisputable consequence. In regard to the present subject, after having been told that Christopher Verduras had secretly conveyed to his house a sick person "*about the latter end of August,*" it is stated by Sir James Fellowes, at p. 165, that "on the 26th of August, Michael Verduras, the son of Christopher, was taken ill in the same house," &c. Now, by the words "the *latter end of August,*" I should conclude that the sick man could scarcely have been landed so soon as the 26th, when Michael Verduras is said to have been attacked; and some days certainly would be necessary for the supposed contagion to produce disease. Dr. Riseuno, a Spanish physician, asserts positively (as I have already mentioned), that it requires exactly twenty days.

Local causes abundantly sufficient to produce the Yellow Fever at Malaga, were described by me at p. 468 and 469, partly on the information of Sir James Fellowes, and more especially in the low suburb called the Barrio de Perchel, in or near which Michael Verduras was attacked, and where the disease, from the operation of these *local* causes, soon appeared in others.—I say this, because the contagionists have utterly failed in their endeavours to make it in the slightest degree probable that any other could have existed. It would therefore be as irrelevant in me to notice particularly the order in which these attacks occurred in different individuals, as it was in Sir James Fellowes to employ on this subject six of his pages in details, which, when not completely insignificant, are altogether incredible. It must have been much easier to invent and propagate such stories, *after* the persons to whom they relate *were dead*, than it is for a reasonable unbiassed mind to give any credit to them.

It being unnecessary here to make any addition to the account given in my Essay of the progress and termination of the epidemic at Malaga in 1803,* I proceed to

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* Dr. Pym says, p. 6, that the Bulam fever "appeared again at Cadiz in 1803."—"But that year, many of the neighbouring villages took precautions against it, and, by establishing quarantine regulations, saved themselves from its ravages." Here beneficial effects are doubtless ascribed to quarantine regulations which they

that of 1804, in which, according to the computation of Dr. Arejula, forty-five thousand eight hundred and eighty-nine persons died of the Yellow Fever in the southern cities of Spain; and yet the contagionists, with all their inventive powers, and the readiness with which some of them have indulged themselves in unwarranted assertions, have not ventured even to pretend that any contagion was, during that year, imported into Spain.* Deterred by the

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did not produce. That the *local* causes of the disease were less virulent in Andalusia in 1803 than in 1800, is manifest, from their effects having been less efficacious in producing the disease, even in Cadiz; and this, without quarantine regulations, would have saved the neighbouring villages, as it did Cadiz, from the severer forms of the disease: no attempt is made which I can discover, to prove that any contagion was this year imported into Andalusia.

* I ought here to mention, that Dr. Burnett, at p. 238, gives the translation of a letter written to himself by Dr. Riseuno, a physician at Carthagena, in which it is asserted, that in the year 1804, the Yellow Fever "was communicated by a Spanish man of war which left Alicant, while the fever raged in that city, whence, during the same year, it was carried to Cadiz and Malaga." Dr. Burnett adds, "I could get no precise account how this took place." Dr. Pym, on the contrary, asserts that the medical gentlemen at Carthagena said the fever had been brought to that city in that year, from either Malaga or Cadiz, by *smugglers*, who, as their proceedings are commonly but little known, have frequently been chosen by the contagionists to convey the Yellow Fever to the places in which it appears, because it is always impossible to disprove charges relating to them, unless they are contrived very clumsily. But, in regard to Carthagena, there

failure of their laboured endeavours to account for the alledged importation of this disease into Malaga in 1803, they have discreetly avoided the subject, and abstained from offering any opinion concerning the origin of the disease in 1804; I can, at least, discover none in the work of Dr. Arejula, (who, being then sent to Malaga by his government, was able to make inquiries on the spot), nor in that of any other Spanish writer which has fallen under my observation. But Sir James Fellowes was not equally discouraged; for, although he did not visit Malaga until 1805, he does not hesitate to assert (p. 177), that the supposed contagion had "remained *dormant* during the winter months, *never having been extinguished;*" and having in this manner procured, or accounted for the presence of, a sufficient stock, he infers that the contagion was in that year conveyed from Malaga, either directly or indirectly, to a considerable number of cities and towns in the kingdoms of Grenada, Valencia, Cordova, Seville, and Murcia, as well as to *Leghorn** and

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are but few places where the causes of marsh miasmata are more *obvious* and abundant, or where their morbific power is more constantly displayed;—and, in regard to Alicant, we are not told how the supposed contagion entered that city in 1804, unless it be understood to have come from Malaga.

* By the description of the Yellow Fever at Leghorn in 1804, given by Professor Gaetano Palloni, in his "Osservazioni Mediche," &c. it appears to have been characterized by the following

Gibraltar. But although some fabulous stories are occasionally mentioned to favour the belief of this propagation of contagion, it must not be supposed that the slightest proof was, or could be, adduced in its support. Indeed, the contagionists in general do not regard *proofs* as necessary: for, believing that the disorder can only be produced by one cause, they are confident that this cause *must* have *been conveyed* to every place in which the disease appears, even when no modes of doing it are discoverable. Thus Sir James Fellowes appears to believe that his assertion, that the contagion of the preceding year had not been extinguished during the winter months, is proved by the fact of the first cases of Yellow Fever in 1804

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symptoms, viz. "In the beginning, a propensity to vomit; pains in the limbs; a most intense head-ach, more especially in the forehead and temples; a sensation more or less painful in the pit of the stomach and region of the liver; a colour like that of jaundice, and, towards the end, a *vomiting of matter like coffee-grounds;* bleedings from the throat and nostrils; hiccuping and convulsions." Nothing like proof or probability of any importation of contagion to produce this fever, has been exhibited; but Sir James Fellowes, after having conveniently found a stock of contagion preserved, as he says, at Malaga during the preceding winter, sends a sufficient portion of it to Leghorn by some *unknown* vessel, with just as much reality as it was conveyed from Malaga to Cadiz, and many other towns in Spain—and yet, unfortunately, the fever supposed to have been *produced* at *Leghorn by this contagion*, was manifestly *destitute of any contagious quality* in that city, as is demonstrated by the most respectable accounts given of it *there*.—See page 117.

having occurred not in the Barrio de *Perchel*, but in the street *Pozos dulces;* but why it should have been *preserved* there, and extinguished in all other parts of Malaga, he has not told us, nor am I able to conceive.

By the statements in Dr. Arejula's work it appears, that two persons died in the street of Pozos dulces on the 29th and 30th of June of the Yellow Fever, and that, before the 23d of July, fifteen other persons had died in the same street, in eight different houses, but little distant from each other; and that, at this time, only *one* other person had died of that disease at Malaga, and this was in the street of Los Marmoles, directly across the river.—So that here the supposed contagion, which is said to have spread with almost the rapidity of *lightning* from Cadiz to Seville in 1800, remained, as commonly happens with miasmatic fevers when they occur in large towns, *confined to a very small space for nearly a month.* In the beginning of August, however, the fever appeared in many parts of the town nearly at one time, and the weather being very hot, most of the sick died. But, upon a *sudden diminution* of the temperature of the atmosphere, the disease was so far mitigated, that on the 14th of August, the physicians (as Dr. Pascalis mentions) generally subscribed a certificate "that no epidemic then existed in that city, and that the existing disease was only a tabardillo," (or remitting 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 died."——The weather, however, soon became again *intensely hot*, and the disease increased so rapidly in violence, that the deaths on the 21st of August amounted to a hundred and twenty-eight in that single day.*

Mr. Jacob mentions in his Travels into Spain, concerning the epidemic of this year at Malaga, " that in the convent of the Capuchins, the friars of which were employed in attending the sick, the only persons who died were one clerical and two lay brothers, although their number in the convent amounted to between ninety and one

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* Sir James Fellowes, at p. 99, confirms this account by saying, that " though the fever continued to gain ground in Malaga during the month of July, yet many of the Spanish physicians, notwithstanding their dear-bought experience the year before, *decidedly* maintained for some time that the disease was not the same as that of the preceding year—that it was the *Tabardillo* (Spotted Fever) of the country."—And again, at p. 196, he says of the epidemic of 1804 in Spain, " the state of the weather, and the temperature of the atmosphere, had a sensible influence; for, upon a hot and dry day, or during the Levant winds, a greater number of individuals were taken ill, and those already under the fever became worse. The disease broke out during the *hot* season, it was *propagated by heat*, and subsided by cold and moist weather, as in 1800:"—all which *confessedly* happens in marsh or miasmatic fevers, and in no other.

hundred." "I wished (says he), to ascertain the cause of this fact, but as it was regarded as a miracle, all inquiries upon the subject proved fruitless." On looking, however, at Dr. Arejula's map, I find an obvious cause for the immunity experienced by these Capuchins, for their convent appears to be situated upon *elevated ground* at the very acclivity of the hills, on the north side of the town; and this being their place of abode, and more especially that in which they commonly passed the *night* (the time in which miasmata seem to prove most pernicious), their occasional services among the sick were attended with much less danger, which would not have been the case, if the disease had been personally contagious.—Sir John Carr, in his Travels in Spain, mentions, on the authority of Mr. Laird, who had resided forty-eight years in Malaga, that the bakers who daily brought bread to that city from the villages of Alhaurin, Churriana, &c. and returned *in the evening*, entirely escaped the epidemic; while those who passed the *night* there, were attacked by it, and commonly died. This is exactly what happens in many other places where the Yellow Fever has prevailed, and particularly at Charleston, in South Carolina, where those who visit and remain in the town only during the day, commonly escape unhurt, while the sleeping a single night in the town is followed by an attack of the fever when they return to the country, where, however, although it often proves fatal, it is never com-

municated to others. No contagion within my knowledge has ever acted in this way; but it is eminently characteristic of the action of fever-exciting miasmata.

The epidemic of 1804 began at Carthagena about the 5th of September, and prevailed to a dreadful extent, and with great mortality, occasioning the deaths of more than one third of all the inhabitants; and it did not entirely cease until the month of January. Dr. Burnett, at page 273, has stated a decisive proof that this fever was not contagious, as may be seen at page 116 of this Sequel, to which he adds, that "a blacksmith who worked in Carthagena, but whose residence was at some distance without the walls, contracted the disease, and died in the same bed with his wife; yet neither herself nor their children were infected."

In the same year the epidemic did not manifest itself at Cadiz until the 28th of August, according to the general abstract copied from the work of Dr. Arejula, and published by Sir James Fellowes, at pages 478 and 479; it was less violent than in the preceding year, but it occasioned two thousand eight hundred and ninety-two deaths, of which *all, excepting two hundred, were males;* females, as in other places, having been, for the reasons already stated, attacked in much smaller proportions, and with much less violence—a peculiarity which does not

accompany any known contagion. As I do not find any fact respecting the occurrence of this epidemic in the other Spanish cities or towns in this year, which requires particular notice, in addition to the accounts given between pages 440 and 473 of my Essay, I shall only refer my readers to that account; and, after a short abstract of Dr. Arejula's chapter on contagion, shall proceed to the consideration of the epidemic as it appeared in this year at Gibraltar, which, by reason of some *local* peculiarities, may be most conveniently examined in a separate chapter.

As Dr. Arejula is stated, particularly by Sir James Fellowes (at p. 171), to be the highest and most respectable of all the Spanish authorities in support of the alledged contagion of Yellow Fever, I have thought it proper here to introduce an abstract of his sixth chapter, which is *exclusively* devoted to that subject, in order that my readers may see how weak and inconclusive all his facts and arguments really are, and how insufficient to give even the appearance of probability to that which he endeavours to prove. He begins by observing (p. 238), that in the production of the epidemic fever of 1800, the concurrence of *three* causes was essentially requisite, viz. "a predisposition or internal cause; an occasional or external cause, i. e. contagion; and a con-cause, i. e. a high temperature; so that this fever never occurred when

any one of these was wanting:" and he lays down the six following data, "which (he says) ought to be held as medical axioms;" and which he makes the foundation of all his arguments, viz.

1st. "Those who had passed the first seven months of the year 1800 at Cadiz, and afterwards continued in the city, were seized with the epidemic, unless they had resided a long time in the West Indies, or South America."

2d. "Those who had been attacked by the disorder in those regions, or had resided a long time in them, either had no illness whatever, although communicating freely with the sick, or if they had any, they were in no danger, the fever in them being so slight that they were not confined to their beds."

3d. "All those who had not removed from Cadiz in the first seven months of the year, but afterwards left the city early, and retired to live insulated in the country, escaped the disorder."

4th. Those who lived out of Cadiz until the epidemic commenced, and came to that city during its prevalence, were seized by the fever in the same manner as if they had suffered the great heat of the summer, and breathed

the Levant winds which reigned *there* during the hot season."

5th. "Every one who, having quitted Cadiz, took refuge in a town in which the disorder afterwards appeared, ran equal risk with those who did not abandon their homes."

6th. "All those who had left Cadiz, and, without having had the epidemic, returned to it; or who came into that city from any other place, when the rains had ceased, and the weather had already become cold, escaped the fever without exception."

He then proceeds "to prove that the predisposition, the occasional cause, and the con-cause, must co-operate in order to create the disease."—"To understand this, says he, it is necessary to admit the existence in the individual subject, of a *peculiar* matter susceptible of being altered by the contagion that produces the disease, which is put into motion when the occasional cause touches it, and the temperature of the atmosphere is at thirteen degrees or more of Reaumur's thermometer (62° or more of Fahrenheit) which temperature I consider as the con-cause." Accordingly, "those who remained in Cadiz, without having lived a long time in America, or had the

fever there, underwent the epidemic, because the predisposition existed in them, and the occasional cause or contagion, and the con-cause or high temprature, acted immediately upon them; while those who had had the disease in the West Indies, or had lived there, and passed the hot season (verano) in Cadiz, even near the sick, were not attacked by the disease, because although the contagious cause and the con-cause were present, they wanted the predisposition."—" And those who left the town and removed to the country, where they indisputably sustained a greater degree of heat than those who remained in Cadiz, and carried with them the predisposition, nevertheless escaped the disease, because the occasional cause or contagion was not present." He gives two illustrations to the same effect from the action of variolous contagion, and from the fermentation excited in a mass of syrup (" almibar") by the addition of a ferment in a sufficient temperature, which illustrations I need not quote: and concludes his chapter with a statement of the following facts, which he considers as decisive proofs that the Yellow Fever is contagious, viz.

" By his wise regulations, the Officer commanding the regiment of dragoons of Maria Louisa preserved his men so effectually, that no one of them was attacked by the epidemic, although they were encamped close to the Isla de Leon, in the Venta de la Catalana, and other quarters in

the district of Puerto Real, posts which were all situated between the sickly towns. In Estepona, and Medina Sidonia, the epidemic did not establish itself, although there were ultimately in the latter of these towns some persons ill of the fever; an effect which was with certainty attributed to the carriers or others, who came from sickly places, and used to steal into the town.—("Entraban furtivamente en el pueblo.") The epidemic did not reach Paterna: but about four-and-twenty men belonging to it, who trafficked in sickly places, were taken ill, and the greater part of them died in a lazaret. Veger and Conil, which are situated close to spots where the epidemic prevailed, cut off the communication with them, and preserved themselves. Of eight men who went out of Algeziras, and communicated with persons ill of the epidemic, seven died, as I was assured, and the eighth had also the fever; but this town suffered nothing, because they knew how to guard themselves. A detachment which came from Ceuta, and remained two or three days in Cadiz, were every one seized with the disorder, and the officer alone survived, as they told me, the rest having died of it."—Such are the proofs, and *the only proofs*, which Dr. Arejula adduces, in order to establish the presence and operation of contagion in the epidemic of 1800; and, as he relies with the greatest confidence upon them, it will not be doubted by those who have read his work, that he has in this chapter *put forth all his strength* to support this

important part of his doctrine; and yet every intelligent unbiassed reader will not fail to perceive, that all that Dr. Arejula has here advanced, either of proof or argument, may be at least as easily and satisfactorily explained by a reference to *local* miasmata, as by supposing the presence of contagion; and probably it would have been susceptible of no other than the former of these explanations, if the facts and circumstances connected with each event had been as minutely stated as they ought to have been, in order to render the truth unquestionable.

Each of Dr. Arejula's axioms is strictly applicable to the ordinary effects of local causes; and his examples may all be readily explained in this way, while some of them are incompatible with the operation of any known contagion. Do small-pox, and measles, &c. require that the individual on whom they are to operate *should not have lived in the West Indies or Spanish America,* and that he should be placed in a temperature of 60° of Fahrenheit, or more?—Whenever particular towns or places escape the epidemic, Dr. Arejula (like Sir James Fellowes and Dr. Pym) ascribes the escape to the precautions employed against it, without any proof that such precautions ever were beneficial against this disease.—And wherever it occurs, he supposes that there was a want of these precautions, or that they were frustrated by a clandestine introduction of contagion. But if such introductions were

practicable, where they are said to have taken place, could not the same description of persons, who are supposed to have carried contagion into Medina Sidonia, and who abound in Spain, steal into other towns with equal facility, in spite of all quarantine orders? Can any one acquainted with the dispositions, habits and duties of soldiers, especially of dragoons, give faith to this account of the regiment of Maria Louisa's being so accurately and completely immured or insulated, that no individual belonging to it could approach a sick person during five or six months? And is it credible, if the Yellow Fever had really been contagious, that among the many thousands of persons who fell victims to it, and the still greater number of thousands who were attacked by it, Dr. Arejula would not have been able from his high official situation, which procured him the best means of information, to collect proofs of this fact, less equivocal than those on which he has found it necessary to rely?

CHAP. VI.

IN the short description which I gave of Gibraltar,* at page 473—475 of my Essay I expressed my regret that, in regard to its *local peculiarities*, I was compelled to rely on observations which I had made at times when, not foreseeing their connexion with the present subject, my examinations and inquiries were not so minute and particular as they would otherwise have been. And I am now willing to believe that, respecting some few circumstances *of little or no importance to my reasonings*, my account was deficient or incorrect, particularly in regard to the height of the rock, which, when some printed copies of my volume were sent to me at Jamaica, I with no little surprise found to be stated as more than two thousand feet high; † though it is in fact only between

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* Gibbon observes that the corrupt and familiar appellation of Gibraltar (Gebel al Tarik) signifies the mountain of *Tarik*. *He* was the Saracen chief who first invaded Spain, and landing at the Pillar or Point of Europa, encamped on the ground which has since been taken, fortified, and occupied by Great Britain. See Gibbon's History, vol. v. 368.

† When I left England for Jamaica, in January, 1811, a small part only of my volume had been printed, and from the haste in

fourteen hundred and fifteen hundred feet above the sea, at its greatest elevation: but this error does not, so far as I can discover, affect my conclusions respecting the production and existence of fever-exciting miasmata at Gibraltar, and I therefore deem it quite unnecessary to occupy the time of my readers by any particular attention to this subject. They will indeed naturally expect, from the expedients before employed by Sir James Fellowes and Dr. Pym, to misrepresent my meaning, and evade the fact concerning the existence of these miasmata at Cadiz, that similar expedients will have been adopted by them, in regard to Gibraltar; and accordingly the former at page 77 says, "during the *heavy rains*, the superfluous water, which falls in *torrents* from the mountains, is carried off through the common outlets into the sea; this is a fact known to every one who has resided in Gibraltar: the idea therefore of any portion of water descending from the

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which some parts of it were written, the M.S. was imperfect in several places. That *I* could not intentionally have made this mistake, if it was made by me (which I doubt), will be evident from the following extract of a letter which I had the honour of addressing in the spring of 1805 to the Marquis of Camden, then Secretary of State for War and Colonies, concerning the fever just ended at Gibraltar, viz. "This Rock is nearly three miles long, and about 1400 feet high: its summit is a sharp ridge about a mile and a half in length, running from north to south: it ends almost perpendicularly on the north and east sides, but slopes gradually to the south and west," &c.

mountains, and running into the town, so as to occasion miasmata, &c. is as visionary as that suggested by an extraordinary writer, who supposes that the water of the wells there, being corrupted, engendered diseases amongst those who drank of it." Now when Sir J. Fellowes wrote this sentence, to discredit my opinion of the production of *miasmata* at Gibraltar, he must, I think, have known that *heavy rains* do not usually fall there in that season of the year, in which the miasmata are supposed to be matured, and that this observation was misapplied.*

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* Even Dr. Pym has *asserted* at page 139 of his volume, that "it is a rare occurrence to have a shower of rain at Gibraltar, between the 1st of June and the 1st of October." And as it is especially and almost exclusively in these four summer months that the miasmata in question are prepared and extricated, so as to produce their morbid effects on the human body, I am utterly unable to conceive any commendable motive which could have suggested to Sir J. Fellowes the employment of this uncandid observation. I will here notice his quotation at pages 447 and 448, of what I had mentioned, of my shewing him Dr. Donald Monro's account of the occurrence of Bilious Fevers at Gibraltar, and his observation, that from my "*paragraph*," it would appear that his opinion had coincided with mine on the nature of the disorder of 1804; *with this remark*, that it would have been more candid in me "to have added, that although he had admitted the truth of Dr. Monro's remark, he had expressly stated to Dr. Bancroft that the fever of 1804 was materially different, in its symptoms and character, from the simply bilious disorders which prevail annually at Gibraltar, as mentioned by Dr. Monro." Now having carefully read over the paragraph in question (at pages 478 and 479 of my Essay), I am utterly unable to discover any reasonable cause or motive for this

In like manner Dr. Pym asserts at page 32, that such an idea as that of the fever at Gibraltar having been produced from marsh mismata, could not originate in the mind of any person, who had ever visited that garrison, but for the purpose of establishing a favourite hypothesis; for in the list of diseases prevailing there, we have not one *ague* in the simplest form to support it.* And to coun-

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remark by Sir James Fellowes; and I am confident that no man possessing and exercising common sense, would ever think that I had insinuated, or given room for any one to imagine, that Sir J. Fellowes agreed in opinion with me on the subject of the epidemic at Gibraltar. I well knew that he had determined to publish his opposite opinion, and that *he* thought this a sufficient reason for *my* not doing so. But although I was willing to make use of his *testimony* in regard to *facts*, I never was solicitous to have it believed that his *opinion* was in unison with mine on that subject. He had only reached Gibraltar when the epidemic had nearly, if not quite, terminated; at least the very few remaining cases had, as usual, lost all the aggravated symptoms which are thought to distinguish it from the common Bilious Remittent; and therefore I did not think his opinion of any importance; it certainly had no influence on mine.

* At page 476 of my Essay I had anticipated Dr. Pym's observation, and stated that "Agues do not often occur in the town of Gibraltar;" but I did not consider this circumstance as indicating that marsh miasmata did not exist there; for I well knew that this form of miasmatic fever was seldom produced, except by miasmata *prepared* and *emitted* from the earth in *temperate* climates; or in situations, which although in warm countries, by their great *elevation* are cool; or in which there is a *redundance* of moisture: and that fever-exciting miasmata, when extricated from soils which are both hot and dry, provided the moisture is *barely sufficient for their*

tenance this fallacy (which is a repetition of that noticed in regard to Cadiz) he introduces a return of the sick on

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production and extrication, do commonly produce fevers, which, although remitting, are capable in young and robust constitutions, of being aggravated into *dangerous continued fevers*, by an atmosphere unusually heated; and especially when it is moreover nearly stagnant, as happened at Gibraltar in 1804. This is particularly the case at Antigua and Barbadoes. Dr. Dickson, Physician to the Fleet and Inspector of Naval Hospitals in the West Indies, in a letter to me dated Bath, August 12, 1815, mentions the Endemic Fever "in *hot* and *dry* situations like Barbadoes," as being generally of a *continued* and *more ardent form*, while (he adds) "*I have distinctly observed* the disposition to remit, and to assume a more *protracted* type, with a strong tendency to *relapse*, and to *visceral disease*, in places more under the influence of *marshy* effluvia; such as Martinique, Guadaloupe, and Marie-galante." "At the latter in the autumn of 1809, I had much opportunity of observing the effects of *locality*, in modifying and *diversifying* the *type*, as exemplified in the intermittent, remittent and *continued* characters; and in converting these, by change of place, *into each other*, and into the severest and worst forms of each, which I have ever witnessed." Dr. Ferguson, Inspector of Hospitals, also observes in the Sequel to his Report, &c. (already noticed) that the "*pestiferous quality*" of marsh miasmata, "is very certainly generated from the *scarcity* of water, *where it has previously abounded*, provided that scarcity be short of *actual dryness;* that a *high* temperature of atmospherical heat is indispensible towards its production; and that in proportion to the *intensity* of the temperature, is the *intensity of power in the miasmata produced;* varying in its effects on the human frame, from the ordinary *ague* of Europe, and of the *mountains of the West Indies*, to the highest *grade of aggravated Remittent* or *Yellow Fever;*"— He adds, "In the attempts that have been made to establish the point of *Yellow* Fever's being a *new* disease, and belonging altogether *to a different family* from that of the intermittent and re-

the 20th of August 1804, a *little* before the miasmata had begun to manifest their efficiency in that year; and be-

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mittent class, much stress has been laid on its *seldom occurring at the same time with ague*, and *its rarely breaking off in convalesence, into that type*. But as has been said before, *ague is not a common production*, however obvious and abundant its sources may be, in *hot low lands*, on or near the level of the sea, where Yellow Fever only is found. It is very rare, for instance, to hear of an ague originating in the Leeward sea-port town of Basseterre, Guadaloupe, either among the troops there, or the inhabitants: but in *barracks, on the cool marshy hills above the town*, at an elevation of less than 1000 feet, it *was a very common disease*, both amongst the officers and the soldiers; while their comrades of the same corps in the barracks of the town, suffered from the more *concentrated* forms of the *Remittent* Fever *alone*. The same may be said of nearly the whole of the West India towns. They are all so marshy, that in colder latitudes, they could not possibly escape being infested with agues; but these very seldom originate, and are nearly unknown amongst them. In this way it is common to hear the inhabitants of Barbadoes boast, that an ague cannot be found in their island; although they have various marshes, particularly near Bridgetown, and *during sickly seasons come in for their share of Yellow Fever*: the reason is plain; there are very few ridges in Barbadoes, of sufficient elevation to belong to *the region of ague*: even supposing their sides to be marshy, which they *never are* (being composed of the *driest calcareous* strata). The marshes are all in the lowest levels of land; and when their morbific miasmata act upon the human body, they produce the *greater or the less concentrated forms of Remittent Fever*, according as their powers are regulated by the temperature and climate of the season, or as the subject is presented under more or less favourable circumstances of seasoning, excitement," &c.

I could readily adduce other similar testimonies. But the foregoing are so decisive in their tendency, and they proceed from

cause no *agues* are mentioned with the few cases of fever in that return, claims to have established the point in question. After having noticed between pages 475 and 480 of my Essay, the means by which miasmata might be produced at Gibraltar, and the frequent prevalence in that garrison of *Bilious Remittent Fevers*, I added that "with such evidence of the morbid effects of marsh miasmata" (so called in conformity with common usage, though often not derived from a marsh) "there can be no doubt *that however produced, they often exist in that place during the summer and autumn*; and as the marsh fevers of Cadiz, Malaga and other towns, but little removed from Gibraltar, had by the *excessive heat* and drought of that summer, been converted into a violent Epidemical Yellow Fever, can it (said I), be surprising that this should have also happened at the latter of these places?"—I have now before me statements made by *very respectable authorities*, sufficient to fill many pages, *and all attesting* the almost invariable prevalence of what are called Bilious *Remittent* Fevers, at Gibraltar during the summer and autumn:—but as this fact is not only *notorious*, but has been *fre-*

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gentlemen who are so respectable by their stations and characters, and so highly qualified by their extensive knowledge and experience in regard to the subject under consideration, that I shall merely refer my readers to pages 205—212 of my Essay for additional evidence to corroborate that which is here given, should any more be desired.

quently admitted by my opponents, I shall content myself with occasionally introducing a sufficient number of these statements, where they may be wanted for other purposes, and after reminding my readers that these * fevers in-

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* Numerous facts in my possession (some of which have been observed by myself, and the remainder have been stated or recorded by highly respectable authorities), attest that, in the southern parts of Europe, and in warm climates generally, the inhabitants of the towns and villages which, like Gibraltar, are situated immediately at the *foot* of a lofty hill, are remarkably subject to autumnal Remittent Fevers: but as the situation and local circumstances of the garrison just mentioned have by some persons been deemed so unsuitable to the production of fever-exciting miasmata, as to render it very improbable that they should have caused the epidemic under consideration, it may not be improper here to introduce, in addition to other proofs contained in different parts of this sequel, an extract from Dr. Fergusson's Inspection Reports to the Army Medical Board, respecting particular places in the West Indies, similarly situated, in which this cause of fever operates most powerfully, although, according to all circumstances and appearances, they would be thought even less likely than Gibraltar, to produce it, viz.

After describing the "low leeward situation" of Basseterre in Guadaloupe, at the very base of the magnificent Souffriere, (which rises behind it to the height of five thousand three hundred French feet) as being productive of the worst form of Yellow Fever," Dr. Fergusson adds, "at the southern extremity of the town, where the land is *highest* and *driest*, stands Fort Matilda, on a slight rocky eminence, which, to the astonishment of every one, has always been an extremely unhealthy quarter. It is untenable by *white* troops; they are sure to perish there; but the blacks have rarely any sick."—Dr. Fergusson afterwards says, "The leeward coast of Guadaloupe from Pointe la Haye to the point of Basseterre, of more than thirty miles in extent, presents a line of

disputably prove the existence of the miasmata, of which they are confessedly the effect, I shall proceed to an exa-

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volcanic mountains, varying from three to five thousand feet in height, which run from north to south, close to the shore, and utterly exclude the breeze. Sometimes these (mountains) terminate by precipices into the sea, but oftener in sandy plains of small extent, which, being uniformly banked in by means of sand and stones thrown up by the sea, are without exception extremely pestiferous, although in general very dry *superficially*, and on a transient view as little to be suspected of unwholesomeness as any surface I ever saw. The small circumscribed valley of Bouillante, which may be called the expanded mouth of a ravine, is one of the deepest, and from the nature of the high grounds which surround it, of the most inclosed, and therefore the most unhealthy, of them all. Even the coloured people, natives of the country, speak of it with dread, and it would be madness to think of building a barrack there for men to lodge and sleep in, although there could be no danger whatever in their visiting it during the day, at any hour after eight o'clock, for the purpose of taking the *natural* vapour baths, if proper sheds and accommodations were erected." Of these baths, "highly efficacious in the cure of rheumatism, palsy, dysentery, &c., far beyond any thing that art and labour, aided by every command of fuel and money, could procure," Dr. Fergusson gives an interesting account; but as it is foreign to my present subject, I pass it over to introduce the following statement, with which that gentleman has favoured me, in addition to the facts contained in his Reports, viz.

"At the capture of Guadaloupe about the beginning of August, 1815, then a remarkably healthy season, one division of the British troops debarked at Bailliffe, about four miles below Basseterre (the capital) on the leeward side of the country. This division was principally engaged with the enemy, and all the wounded were collected at the village of Bailliffe, under the direction of Staff-Surgeon Kerr, who had served all his professional life in the West Indies, and had for a course of years resided in the most unwhole-

mination of the principal facts concerning the fever of 1804 at Gibraltar.

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some town of Castries at St. Lucia, without ever having suffered from fever in any shape. On the 2d of August, after the capture, I went to see the wounded, and make arrangements for their removal, when I found Mr. Kerr surrounded by some coloured people, who were adjuring him not to remain at Bailliffe, and asserting that it was death to Europeans ever to sleep there. This was the second deputation of the kind which had waited on him, some among them from a considerable distance, for this purpose; and we were both much amused at their apparent earnestness, which we imputed to their anxiety to get rid of us. As the village was situated close to the sea, on an open gravelly beach, the hills above it remarkably ferruginous, with a hard, dry surface, and their numerous water-courses, though in many places worn deep, uniformly rocky at bottom, and by no means impure in any respect, it appeared to us to be a very delightful and healthy spot; and the wounded were therefore permitted to remain there two days longer; after which they were brought to the general hospital at Basseterre: and as the service was then over, Mr. Kerr prepared to return to his station at St. Lucia. On calling to take leave of me, he complained of head-ache so violent that it appeared to stupify him; and I endeavoured to dissuade him from embarking, but in vain. He embarked accordingly, and landed in a day or two afterwards at St. Lucia, only to die of an aggravated Remittent Fever, under which he had laboured during the whole passage.

"The wounded that were brought from Bailliffe, suffered considerably in the general hospital of Basseterre from the same disease; and two out of the three Assistant Surgeons, who had been there with Mr. Kerr, also suffered so much shortly after from Intermittent Fever, that they were obliged to return to Europe for recovery.

"The situation of Basseterre itself is so similar in many respects to that of Bailliffe, that it may account for the illness of the last-

Of the *local* circumstances which appear to have co-operated in producing this fever, some account has been

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mentioned; but the fate of Mr. Kerr made a strong impression upon me; and as soon as I could find leisure, I made an inspection of the Leeward Coast, in company with Monsieur L'Herminier, a distinguished naturalist of the town of Basseterre, who confirmed all that the coloured people had told Mr. Kerr and myself; and, in answer to my enquiries how such a surface could possibly give out pestiferous emanations, he made me observe the height and proximity of the continuous ridge of mountains, that intervened on a line of about thirty miles between the coast and the windward side of the country, to the utter exclusion of the breeze, and consequent deficiency of ventilation, to which he attributed the noxious influence that was there so invariably experienced by Europeans. Subsequent observations, and examinations of the Creole coloured settlers, during two other inspections of the same coast, amply confirmed all this, proving beyond a doubt that the mal-arious influence, even where the surface appeared unobjectionable, was uniformly the most fatal, when the ventilation was most defective."

If M. L'Herminier meant that the fevers in question were caused by the want of ventilation *simply*, he is doubtless in error:—they can only have been caused by mismata exhaled from the soil; which however will necessarily be most accumulated, and therefore most powerful, cæteris paribus, where the atmosphere is least ventilated.

There are obvious causes for the greater insalubrity (in regard to miasmatic fevers) of a place which lies at the base of a mountain, than of another which stands on a level plain. The latter may be considered as acquiring its portion of decomposeable matters only in the ordinary and slow course of vegetation: whereas the former, besides its natural portion, is constantly receiving accessions of these from the grounds above: and the same happens respecting moisture, which mountains, by attracting and detaining clouds, and also by more copious dews at night, are able to obtain in sea-

given in my Essay, and *others* will be mentioned in the quotations which I shall occasionally introduce, from statements made by gentlemen much better acquainted than I am with these localities; at present therefore it will be sufficient for me to suggest, as *obvious* and *prominent causes* of the epidemic in question, the accumulation of decomposable matters within the town, and the *long prevalence of a dry and scorching east wind,* which produced a very high atmospheric temperature, without any salutary ventilation of the place, as it was completely obstructed in its course by the high mountain behind the town, *in* and *over* which the air was for many weeks nearly stagnant. A similar dry and scorching east wind, blowing with too little force to change and purify the atmosphere, has invariably preceded and accompanied every recurrence of the Yellow Fever at Cadiz and other cities of Spain. And its effects, in the year 1804, were very

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sons, in which open plains may be parched up. In the dry summer of 1800 I ascended several times the summit of the rock of Gibraltar by different paths, and I well remember to have observed numerous small rills of water; which, when arrived at the base, might there have contributed to the production of miasmata. The mischief, if any, arising from these sources might be obviated by constructing aqueducts to receive and convey the water from them to one or more reservoirs; and a great additional benefit might thereby be conferred on the inhabitants, who, in seasons of drought, suffer much from the want of water, as I witnessed in 1800.

extensive and remarkable.* But the contagionists have as much as possible disregarded this, and all other *atmospherical* and *local* causes, and assert, that the epidemic was exclusively produced by the introduction of a specific contagion, from one of the neighbouring Spanish cities, and particularly Cadiz. Of the stories *contradicting and refuting each other* on this subject, I gave some account, at pages 480 and 481 of my Essay. But Sir James Fellowes, and Dr. Pym, having no other resource, *continue* to *repeat* and *rely* upon these tales with so much confidence, that a more particular examination of them has become necessary.

The individual principally charged with the introduction of the contagion into Gibraltar, was, according to

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* Mr. Amiel, acting Surgeon to the Foreign Depôt, in answering Mr. Fraser's questions says, "The *Epidemic* did not appear in Gibraltar before the year 1804, because the miasmata arising from those noxious substances which had been suffered to accumulate, had not yet obtained that degree of maturity which was required for the production of such a general and malignant disease; but a milder form of it has been *long* known here, under the denomination of Bilious Remittent, as is averred by the medical men acquainted with this place. This fever became frequent in 1803, and in 1804, when it assumed, about the latter end of August, a very *aggravated form*, the character of Bilious *Remittent* being however yet *perfectly distinct*, as was declared at a medical consultation held to report on the nature and progress of the disease."

Sir J. Fellowes, named Santos. He is stated by Dr. Pym (page 20), on the authority of Mr. Kenning, then Assistant Surgeon of the Royal Artillery, to have landed at Gibraltar, on the 28th of August, from Cadiz; and to have been attacked with the fever on the 29th, after having previously lodged at Cadiz "*in a house where some persons died* of the *then prevailing* fever:" and this account Dr. Pym assures his readers, "is to be *depended upon.*" —Sir James Fellowes, however, at page 114, gives an account of the examination of *Santos*, in which the latter asserts that he had left Cadiz on the 23d or 24th of August, and arrived at Gibraltar on the 25th; and that *two* days after, he was taken ill of the fever, &c.—"He said that *he heard of no sickness prevailing at Cadiz when he left it*; but confessed he was in a room with one man, who was lying in bed, ill of a fever, *as he believed;* that the house was a tavern, where he had gone in accidentally to get something to drink; and that he did not know who the person was, that was lying in the bed sick." It is added that Santos, previous to his examination, "had been assured that he had nothing to fear from stating the truth." Afterwards, at page 454, Sir James Fellowes inserts the copy of an affidavit, from the Secretary's office in Gibralter, sworn the 4th of March, 1804, by Mr. Pratt, master cooper in the naval victualling-yard at Gibraltar, in which the deponent states, that "He arrived at Cadiz the 3d or 4th of August. He did not understand that the

town was unusually unhealthy. He lodged at the tavern Del Sol, in the street Hondillo, where he remained about *fifteen* or *sixteen* days, *when* he was taken ill; having continued so for *eight* days, he had symptoms of *black* or *bloody vomiting;* at this he was much *alarmed,* and fearing lest he should be sent to the public hospital, he *removed* to another quarter of the town, and ultimately recovered. He had however a very *yellow look,* which prevented the master of a vessel from taking him on board, and bringing him to Gibraltar, lest it might be the means of putting the vessel in quarantine. Mr. Pratt further deposes that he and the Captain of *his privateer,*" (of which we have no information), " went into a *barber's shop* to be shaved," &c. and " that shortly after, they both complained of pains in the head; and they were *immediately* after taken ill. The Captain of the privateer lived in the same house with him, but was removed to the hospital where he *died.* He also deposes that a man belonging to Gibraltar named Santos, who was the first person attacked with the fever *here,* lived many days in the same tavern with him, and the Captain of the privateer; that Santos was in the room with him (Pratt), whilst he was ill of a fever, which however he endeavoured to conceal, for fear (as already stated) of being compelled to go to the hospital; that Santos returned to Gibraltar in the vessel in which he (Pratt) had been refused a passage; and that, whilst he was in the tavern, *several persons were ill.* He

was told that the disorder generally *attacked strangers, and was fatal to them.* That he was attended, when he removed from the tavern, by a man to whom he afterwards gave some of his *clothes;* and that this man was, shortly after receiving them, taken ill and died."*

Sir James Fellowes states that he was present at this examination of Mr. Pratt, on the 8th of March, the day on which he arrived at Gibraltar from Cadiz, and that the affidavit was sworn on the *ninth* of that month (although General Fox attests it as sworn before him the *fourth* of March), and that "Santos, the person mentioned in the above deposition, was then sent for, and confronted with Mr. Pratt, whose examination being read to him, as also his own former statement, he was desired to declare how far he acknowledged what Mr. Pratt had stated to be true, or not. Being thus questioned, he confessed, not without some confusion, that he had lived for about three

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* The inconsistencies in Mr. Pratt's affidavit will soon be so *distinctly manifested*, that I may leave without farther notice the stories of the *barber's-shop*, the anonymous Captain of an *unknown* privateer, who died in a *public* hospital of a disease *not discovered* in Cadiz until *some time after;* the *other* persons ill in the tavern; the information given concerning the fatality to strangers, of a fever *which had not then appeared;* the *clothes* which produced disease and *death to the receiver;* and other similar *incidents*, although they were doubtless intended and expected greatly to promote the belief of contagion.

weeks in the same house with Mr. Pratt and the Captain of the privateer, and that he had also been *three* times in company with the Captain, whilst he was *ill* of the fever, a short time before he left Cadiz for Gibraltar; but he said he did not recollect Mr. Pratt's being *ill*, at the time he was in the house with him; and which" (adds Sir James) "*indeed is possible*, as Pratt said he endeavoured to *conceal his illness* as much as he could, to avoid being sent to the public hospital." Besides all this, Sir James Fellowes gives a declaration, p. 457, by Mr. Bresciano, respecting a man who was found in a dying state under a fig-tree, in the Library Garden at Gibraltar, about the 10th or twelfth of August, and was said to have come from Malaga, in a felucca, with two others, and dying soon after, was *privately* buried, in the Library Garden: which strange story is unworthy of a particular examination, especially as the reporter of it says, page 460, that although "there were some sick in that part of the town, and even one or two deaths, previous to the *alarm* which Santos's illness occasioned,* they were attributed to the warmth of the season; and even after the disorder appeared at Santos's house, no attempts were made to trace it to any other

* This "alarm" seems to have been felt only by Mr. Kenning, since at a meeting of all the medical men in Gibraltar, *so long after as the 15th of September*, he alone imagined that any thing more than the usual remittent of the season existed at Gibraltar.

source; as the circumstances of his arrival from Cadiz, his sickness immediately afterwards, and the progress of the disease in his family, were sufficiently ascertained to account for all the mischief which ensued." "The facts here stated (adds Sir J. Fellowes) *were very satisfactory to my mind*," &c. Whether they *deserved* to be so, must now become the object of a short inquiry; and as *one* of the foundations on which it is to be made, I must state that Dr. Arejula, the physician who was principally commissioned by his government to superintend the treatment, &c. of the sick during the epidemics of Andalusia in that and other years, has, in the sixth and last general Table annexed to his "Breve Descripcion de la Fiebre Amarilla," mentioned expressly the 28th of August, 1804, to have been the day on which the existence of the Yellow Fever at Cadiz was first discovered or ascertained, "se conoció la enfermedad."

According to Mr. Kenning's assertion (which Dr. Pym says "may be depended on"), Santo, or Santos, had returned to Gibraltar on the 28th of August, the *very day* on which, according to Dr. Arejula, the *first* case of the Yellow Fever is *ascertained* to have occurred; but according to Sir James Fellowes (page 102), he arrived at Gibraltar three days earlier, viz. on the 25th, having left Cadiz on the 23d or 24th of August, four or five days *before* the *first* case of Yellow Fever *had appeared* there:

yet he is said by Mr. Kenning to have "lodged in a house at Cadiz where *some persons had died of the then prevailing fever,*" i. e. the Yellow Fever had *prevailed,* and occasioned the deaths of *several* persons in *one* house (besides those which might have occurred in other houses) *a week* or *two* before the time which Dr. Arejula, the Chief Official Superintendent of all matters relating to the epidemic, with all the advantages of the best information, and of a subsequent deliberate enquiry, and mature consideration, has *fixed* as the *earliest* at which the disease had occurred in that city. On the other hand Santos himself declared "that he heard of no sickness prevailing at Cadiz when he left it;" while, in regard to the *deaths* said to have happened where Santos lodged, *all* the evidence that is produced, renders it manifest that *none took place.*

That an affidavit like Mr. Pratt's should have been adduced by Sir James Fellowes, a physician who has seen and treated the Yellow Fever in St. Domingo and Cadiz, must, I think, excite great surprise. This person swears that "he arrived at Cadiz on the 3d or 4th of August," and "remained at the Sun Tavern about fifteen or sixteen days, *when* he was *taken ill,*" i. e. about the 18th or 20th of August; and "having continued so for *eight* days," that is until the twenty-sixth or twenty-eighth of August he had *then* symptoms of *black* or *bloody vomitings,* at which he was much alarmed; and fearing lest he

should be sent to the public hospital, *he removed to another quarter of the town*, and ultimately *recovered.*" Probably this will be allowed to have been the *first* case in which a patient labouring under Yellow Fever, *with black vomiting*, was able to "*conceal*" the fact of his being ill *during eight days*, and then, *in the very worst stage of the disease*, upon the supervention of a symptom, *which almost invariably terminates in death within a few hours*, could *remove himself to another quarter of the town*, and, after all this, *recover!* How much time was consumed in this recovery we are not told; but it must have been one of singular rapidity, since he declares that he applied for a passage in the same vessel in which Santos returned to Gibraltar, but was "refused," on account of his "very yellow look."—Unfortunately for Sir James Fellowes, and his witness, and the cause of contagion, this vessel, as Santos and Sir James assert (page 116), and public records prove, left Cadiz *at the latest* on the 24th of August, *several days before* this occurrence of the *black vomit* in Mr. Pratt.

To me it seems evident, that when Sir James Fellowes *assisted* at the examination of Mr. Pratt, and the composition of his affidavit, and when he afterwards stated it to be "*very satisfactory*" to himself, and as such published it in his work, he must have laid aside the exercise of his reason; because a minute's consideration would have made him sensible of the *obvious and irreconcileable contradic-*

tions, which it contains, and of the absolute *impossibility of its being true.* This however is but one of the many lamentable instances which have occurred in my enquiries respecting the Yellow Fever, of the infatuation under which men, whose minds are strongly predisposed, will write, without appearing to have reasoned on this subject. —The different accounts given by Santos are so much at variance with each other, and with the other statements, particularly that of Dr. Arejula, that I deem them unworthy of notice; and this is so completely the case with every thing else alledged to prove the importation of contagion at Gibraltar in 1804, that I will not waste another thought on the subject.——I had at page 480 of my Essay, *concluded* that possibly some persons who had imbibed the noxious exhalations of Cadiz or Malaga, might have arrived and sickened at Gibraltar; and that this might have happened about the time when miasmata, similar to those which occasioned the epidemic in those cities, were *beginning* to operate at Gibraltar; and that this was the utmost, which could be said with truth, to countenance the supposition of an importation of contagion, as the cause of Yellow Fever at Gibraltar in 1804: and nothing has since occurred to weaken my confidence in this conclusion.

It appears that the same local and atmospherical causes which produced and *matured* the fever-exciting miasms at

Cadiz, produced a similar effect about the end of August in some parts of Gibraltar, particularly in a crowded filthy spot, called Boyd's-Buildings; where it is said that Santos (who kept a shop there), was attacked within a day or two after his arrival from Cadiz. But as strangers who landed at Gibraltar during the epidemic, were sometimes attacked within the same *short space* of time, by inspiring the noxious exhalations from particular places *there*, subsequently to their landing, it may be matter of doubt, whether the fever of *this person* was occasioned by the miasmata of Cadiz, or by those of Gibraltar. His attack was followed by several others, to the amount, according to Mr. Kenning, of nearly fifty before the 12th of September. Sir James Fellowes employs ten pages in endeavouring to ascertain the order in which these attacks succeeded each other, presuming that by so doing he should be able to convince his readers that they were the effect of contagion; although the fact of their having been during all that time confined to persons who lived on, or who very much frequented, the spot called Boyd's-Buildings, renders it much more probable that they were the effect of miasmata exhaled there, than of contagion, which last would certainly not have been thus *limited* in its operations, while such limitation is eminently characteristic of fevers produced by *local* causes.

In the latter part of September several persons, in

other parts of the town, and particularly in the south, were attacked with this fever, and Sir J. Fellowes endeavours to account for its production there, by saying that a young man named Sterico, who "kept a wine-house not far from Boyd's-Buildings, went to his mother's house on Scud-hill, sometime in September, where he died about the 18th or 19th;" and that "the disease shortly after appeared in the sheds, and small confined buildings adjoining Sterico's house, and it continued to occasion great mortality in all that quarter," &c. In a note Sir James Fellowes says, "I was never able to *trace* the progress of the malady directly from Sterico's house, nor could it ever be ascertained whether other infected persons, flying from the town, did not go to this neighbourhood about the same time." But these occurrences more nearly resemble the operation of miasmata than of contagion.*

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* The following may serve as one instance and proof of the *local* causes of fever at the places in which it first became prevalent. Sir James Fellowes, having noticed a letter written by the Honourable Colonel Colville, respecting the three first persons belonging to the 13th regiment, who were seized with the fever in 1804, in which it is said that "they did not live in the barracks, for they were married, and inhabited from necessity *hired huts*, in a *most unhealthy situation*, where there were already sick inhabitants," adds the following note at page 139, viz.

"The unhealthy circumstance to which Col. Colville alludes, was the public drain or sewer, running from the barracks, which from being uncovered, and from the *want of water* to cleanse it, was during the heats of summer extremely offensive; it was at

Dr. Pym at page 21 quotes Mr. Kenning as making this declaration: "A meeting of Medical Gentlemen, Civil and Military, was assembled by order of the Governor on the 15th of September, when *I pronounced* the disease to be malignant and highly contagious: I was the only person that ventured to give this opinion, and standing single, I was of course overruled, as the highest medical authorities pledged their honour and reputation that no other disease existed *than what prevailed there every year.*" And Mr. Griffiths, late Surgeon to His Majesty's ship Triumph, in a Report made to Dr. Burnett on this subject, published by the latter at page 210 of his work, says, "In a medical consultation held about this time (September 1804), for the purpose of enquiring into the nature of the fever, *it* was declared to be the *usual remit-*

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this time particularly so; the barrack necessaries having been emptied into it, and the contents not having run off. The huts in which *so many sick inhabitants*, and some of the married people were living, were built *adjoining to this sewer*, and some *immediately over it*, with a *single boarded floor intervening*. The *disorder prevailed particularly* in *this spot;* and from its being so near the barracks and the canteen of the regiment, it is not surprising that the soldiers would frequent it when they could."

As very mortal Yellow Fevers have in many instances spread through ships in warm climates, containing collections of filth in no degree comparable to those here described, and which were but a part of those existing in the garrison, there is not, in my judgment, any reason to imagine that contagion had produced the fever in the huts of the 13th regiment.

tent incident to the inhabitants of the place at the same season; but deaths becoming more frequent than at any other of its former visits, this opinion was soon relinquished; and the disease announced to be contagious, and of the same type with that which was then prevailing at Malaga." He adds, "During the greater part of September, the Triumph, of which ship I was then surgeon, was at Cadiz, where the same fever was also *commencing;* and upon my return to Gibraltar in October, I found the daily mortality greatly increased; averaging from sixty to seventy. The disease had at this time assumed a very aggravated form; often terminating fatally in forty-eight hours."*

This violence and increased mortality of the fever at

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* Dr. Pym at page 36 mentions Dr. Nooth's having been attacked with this fever, adding that, after "all his arguments at the bed-side of the patients, to convince them of the true nature of the disease, or the non-existence of contagion, we could never have expected that he, or General Barnet, who fell a victim to his zeal, could have been attacked with the disease." Without taking upon myself to ascertain how much of unnecessary exultation belongs to this observation, I must say, that I discover in it little either of knowledge or reflection; for whether the disease were produced by contagion, or by noxious miasmata, it was reasonable to expect that Dr. Nooth and General Barnet should be as liable to be attacked by it as any other persons; especially since one of them *lived*, and the other was very much employed, in that part of the town, where these miasmata were *very prevalent*.

Gibraltar, with the prevalence of a similar fever supposed to be contagious at Malaga and other places in Spain, appear to have alone produced that change of opinion, among the Medical Gentlemen at Gibraltar, which Dr. Pym mentions to have taken place in the space of twenty days; a space too short for any adequate examination or enquiry; and especially at a time when they must of necessity have been incessantly occupied in the duties of their profession, and too much influenced by the new and alarming mortality, for any calm investigation or deliberate reflection: nor indeed could it have been possible, where so many were daily attacked within a narrow space, to distinguish between the effects of miasmata, suspended in the almost stagnant air of the streets, &c. and those of personal contagion; and *this last* might therefore naturally, though erroneously, *seem to be the probable cause*, as the disease had not before occurred with so much violence. Dr. Nooth however, and Mr. Glasse,* Surgeon to the Forces, continued to reject this

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*Mr. Glasse, according to Dr. Burnett, page 323, had served in Gibraltar ten years, and witnessed the fatal fever of 1804, as well as those of 1810 and 13; and he says of the former, "I think the same atmospheric influence which produced it in other parts of Spain, caused it at Gibraltar; and it was there most fatal, from the crowded and *filthy* state of the garrison."—Mr. Amiel in his answers to Mr. Fraser, says of the epidemic at Gibraltar, that "a *milder* form of it has long been known here under the denomination of Bilious Remittent, *as is averred by all the medical men*

cause, and the facts which the recurrence of this fever in subsequent years has made known, have since induced a majority of the medical officers at Gibraltar to reject it likewise.

As the time when I must again embark and return to my duties in Jamaica is at hand, it is necessary for me to abridge the remaining part of this publication as much as possible; and I shall therefore say no more respecting the epidemic of 1804. Dr. Pym (at page 46) says, in regard to the troublesome and useless system of purification, which took place at the end of 1804 in Gibraltar against the supposed fomites of the disease, that "whether it was absolutely necessary or not, it however proved *successful in* 1805, as the garrison enjoyed the most *perfect* health from that time until 1810." But this is very fallacious, unless the prevalence of severe Remittent Fevers be compatible with the *perfect* health of *that garrison,* for I have indisputable evidence of their having occurred, and prevailed to a considerable extent, at least in the ten years

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acquainted with this place. This fever became frequent in 1803," (when it prevailed in a more violent form at Malaga) "and in 1804, when it assumed, about the end of August, a very aggravated form; the character of Bilious *Remittent*, being however yet *perfectly distinct;* as was declared by a medical consultation, held to report on the *nature* and progress of the disease"—i. e. in September, 1804.

which followed 1804, as they had done for several preceding years: and in 1810, 1813, and 1814, they were (probably from the usual causes) aggravated into the form of Yellow Fever, in a great number of cases.

In the year 1810, the Remitting Fever, which had occurred *in every preceding summer and autumn* since the commencement of this century at least, and very probably in all the antecedent years since it became a populous town, again appeared; but without any unusual aggravation until after a considerable fall of rain, about the autumnal equinox, which brought down the vegetable and animal matters from the mountain, and left them in the town, the outlets being mostly impervious; and being followed by a dry easterly wind, and very warm weather, with an almost stagnant atmosphere in and over the town, soon produced an additional number of attacks from the fever, and with increased violence.* But Dr.

* Mr. Doughty, who was both at Cadiz and Gibraltar in the summer and autumn of 1810, says of that year, "To my knowledge there was a continued drought and heat for three months; the rains set in about the autumnal equinox; then, and not till then, did fever of the *order* we are speaking of, develope itself both at Cadiz and Gibraltar." Mr. Amiel also in answering Mr. Fraser's questions says, "I was in this garrsison (Gibraltar) in the year 1810, and I had the opportunity of observing the Epidemical Fever which then made its appearance in this place. Bilious Remittents were prevalent in July and August; especially in the

Pym, instead of ascribing the *aggravated* cases of this remittent, to which he gave the name of *Bulam*, to an augmentation in the number, or in the force, of the local and atmospherical causes, has endeavoured to maintain that they were produced by contagion imported from Carthagena. He says (page 47), "During the prevalence of contagious fever at Carthagena in 1810, four transports from that port (two of them having on board deserters from the French army), anchored in the bay of Gibraltar on the 10th of September; they were immediately put under the restraint of quarantine." He adds, that on going along-side, he was informed "that one (man) was already dead, two were dangerously ill, and several others had been attacked with fever, which shewed itself in all the four transports in the course of a few days." He then gives an account of his precautionary measures against contagion, particularly that of procuring two

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hospital of foreign recruits, of which I had the charge: in the month of September some cases of that fever appeared likewise in the same hospital, but they were *attended with more malignity*, and two ended fatally."—He adds, "it appears to me evident from *facts* which came directly under my knowledge, that atmospheric causes more or less powerful, according to the different changes of seasons, gradually directed the form of those fevers from the mild Bilious Remittent in July and August, to a more aggravated one in September and October, attended with great irritability of stomach, *but with remissions* still distinct; and lastly in November to that malignant type which brought on dissolution in four days, preceded by black vomiting."

hulks, into which seventy-seven soldiers were removed from the transports, and, excepting fourteen who were afterwards attacked on board the hulks, escaped the fever; while the sailors who remained on board continued to be attacked by it, as was Mr. Arthur, Assistant Surgeon to the 4th Veteran Batallion, who most humanely and courageously volunteered his services on board the transports, &c. Here Dr. Pym asks (page 42), "how Dr. Bancroft will attempt to account for this attack of Mr. Arthur, or the escape of the men, when removed from the transports?" adding, "he can only do it by acknowledging contagion, of which we have here a positive proof, and that the disease in question was not produced by marsh!!" For an explanation however of the facts in question, I may, without "acknowledging contagion," refer to my fourth chapter, where many instances and proofs may be found of the Yellow Fever's prevailing more extensively, with greater aggravation than it did on board these transports, in ships in which there was not the smallest reason to suspect the presence of contagion, and where the fever itself was manifestly destitute of any contagious quality. In the case before us we have only to presume (what, if the truth could be ascertained, would, as I confidently believe, prove to be true) that these transports were sufficiently *foul* to produce, and that they did produce, the fevers in question, without the aid of any contagion from Carthagena, (where there was none) and we shall then easily

understand how Mr. Arthur, by going on board of these foul transports, and breathing their morbific exhalations, was, like others on board, attacked with the fever; and we shall also understand how the soldiers, by being removed out of the reach of these exhalations, escaped the fever, excepting the fourteen who, previously to that removal, had inspired sufficient quantities thereof to cause fever afterwards. We are not told that these gave the fever to any other person; and, from the invariable experience in regard to fevers produced by that cause, I am confident they could not give it *out of, or without, the foul atmosphere* by which it had been *exclusively produced.*

But as these transports, and the fever which occurred on board of them, are *indispensably necessary* to enable Dr. Pym, *in his way*, to account for the aggravated cases of Bilious Remittent, or Bulam Fever, which afterwards occurred *on shore*, it is probable that he will not admit my explanation, so long as a single person can be found to countenance him in rejecting it. These transports, however, were confessedly put under the strictest quarantine, and continued under it during the whole time of their stay at Gibraltar; and they were anchored, says Dr. Burnett, "at the distance of at least half a mile from the garrison, and probably more:" and, if quarantine regulations in such a case could be frustrated, *even*

when enforced by Dr. Pym, as their superintendent, and when he was fully convinced of the presence of a most dangerous contagion, it certainly must be useless to employ, and highly dangerous to rely upon, them at any time. He however says, at p. 49, that on the 20th of October, forty days after the arrival of the transports, "in consequence, as I *must suppose,* of a breach of quarantine regulations, *which, however, could not be detected,* a Minorcan family in the south district, belonging to the dock-yard, was attacked with the disease."—Here, as in some other instances, by a *gratuitous* supposition, and without the slightest attempt at proof, he endeavours to deprive the anti-contagionists of a fact, which demonstrates the aggravated cases of fever at Gibraltar in 1810, to have occurred without any importation of contagion, and to convert it, by the help of this most improbable supposition, into a proof that contagion was actually derived from Carthagena; and that notwithstanding all *his* precautions and regulations, it was smuggled into the garrison at Gibraltar. But of this event, he says, he was only informed on the *26th of October,* when, upon visiting the family, he found "that the disease which we so much dreaded, actually existed: six out of seven persons had been attacked in one house; and three other families, *neighbours,** who had visited them, as also a *Spanish*

* We always find that the persons first attacked by this fever live in the same neighbourhood, a circumstance which proves it

priest," &c. But the *subsequent* part of Dr. Pym's account of the progress of the disease has been minutely noticed, *and abundantly refuted,* by Dr. Burnett, between pages 442 and 456 of his work; and to these I beg leave to refer my readers, because, as he has anticipated every thing which I could offer on the subject, and with some facts, of which I had no knowledge, it becomes me to allow him the credit which is his due, for the ability with which he has supported the cause of truth, and especially as it enables me to abridge my own performance. Dr. Burnett proves unquestionably, by affidavits, attestations and other evidence, " that the Minorcan (whose name is Jacinto Rey) was not the *first* person attacked with the Bulam or Yellow Fever in 1810;" and that he had had no " communication with the transports from Carthagena"* (of which, and the

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to have originated from *local* causes; but my opponents endeavour to convert this into a proof of the operation of contagion, by supposing that persons in the same neighbourhood must always be very intimate with each other, and that they *are visited only by their nearest neighbours.*

* Dr. Burnett says (in the London Medical Repository, vol. vi. p. 459) " I have proved, by the solemn declaration of Jacinto Rey upon oath, and also by the concurrent testimony of Messrs. Donnet and Amiel, that this person had no communication, directly or indirectly, with the transports from Carthagena; and also that he was not the *first* person attacked. But, independent of the cases of Mrs. M'Lean and Mrs. Vaughan, mentioned by the gentleman

origin of their non-contagious fever, I have already given a sufficient account). Dr. Burnett also proves "that Dr. Pym *must* have been mistaken in stating that *he* had had not any knowledge of the Yellow Fever's having appeared in the garrison of Gibraltar before the 26th of October, 1810;"* that "no Spanish priest had (as was

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just named, I have proved by the communication of Mr. Kidstone, Surgeon, and Mr. Waters, Assistant-Surgeon, of the Seventh Veterans (whose testimony Dr. Pym thought of so much importance, that he published their Letter in the Gibraltar Chronicle), and who, as I believe, were the only medical officers of the garrison who supported his (Dr. Pym's) opinions respecting the fever of 1810, that Thomas Nottage, a soldier of that corps, was taken ill on the 11th of October (nine days before Jacinto Rey), and died on the fifth day thereafter;—that Darby Ferguson, another soldier of the same regiment, was attacked on the 19th, *one day before* the above-mentioned Rey." And in answer to Dr. Pym's assertion, that only the Spanish priest and a lady were attacked within the *walls* of the town, Dr. Burnett says in the next page, "instead of *two only* being taken ill *within* the walls of the town, I have proved that no less than *ten* soldiers belonging to the Seventh Veterans were atacked in the cooperage-barrack, at the extremity at Water-Port Street, within the walls of the town, and having a very populous neighbourhood contiguous."

* This relates more immediately to the case of Mrs. Vaughan, of which Mr. Donnet, who was then in charge of the Naval Hospital at Gibraltar during the absence of Dr. Gardiner, states, that *the first* instance which he saw at Gibraltar in 1810, of the "disease in its aggravated form," was that of Mrs. Vaughan, wife to the Commissioner's clerk, "to whom," says he, "I was called in the evening of *the 12th of October*," at the north end of the south barracks, and "found in a most deplorable state," with "black

asserted by Dr. Pym) attended Jacinto Rey during his illness, and consequently none could have received infec-

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vomiting," &c. She expired the next morning, having been attacked by the disease on the 5th. The body after death, he says, became "of a very dark yellow."—Of this event, Mr. Donnet declares that he *immediately acquainted Dr. Pym*, i. e. thirteen days before the 26th of October.—See his Report to the Transport Board. In regard to this case, Dr. Pym says (in the London Medical Repository, vol. vi. p. 203), "I was not so much alarmed; 1st. because I knew that Mrs. Vaughan and her husband *had both had the disease in* 1804; 2d. because there was no appearance of disease in the neighbourhood; and 3d. there being circumstances attending the history of her case, which led me *not* to suspect contagion, but that some *deleterious* medicine had been administered." To repel this insinuation, Mr. Donnet wrote on the 20th of September, 1816, to Dr. Burnett as follows: "it will be matter of surprise to poor Mrs. Vaughan's relations, when they happen to read the paragraph Dr. Pym has published in the Medical Repository as to the cause of her dissolution. At the time I made my report to him, he observed that Mrs. Vaughan must have taken something hurtful; and I remember well, that spirituous liquors were mentioned—but I never supposed, until I met him the other day in the Strand, that he suspected poison had been the cause of her death. *I have no reason on earth* to consider her case in a different point of view from that in which I have reported it." And, in a letter to his friend Mr. Whitmarsh, of Haslar Hospital, Mr. Donnet expresses his surprise at Dr. Pym's publication in stronger terms, adding, "I wonder that Dr. Pym, entertaining a supposition of that nature, did not cause the body to be examined, instead of desiring Mrs. Vaughan's mother to have it buried the morning she expired, while she was supplicating a delay of the funeral until four o'clock." In this case Dr. Pym could not, with a chance of obtaining belief, pretend that Mrs. Vaughan had broken his quarantine regulations, and caught the fever from the transports; and unfortunately there

tion from this source:"—" that all the men taken ill in the barrack-room of the 7th Veterans, were *within* the *walls* of that town, in a very populous part;" " and that the fever existed in the hospital of the 7th Veterans, from the 11th of October till the 30th of the same month." Whoever will attentively peruse that part of Dr. Burnett's work, to which I have just referred, in addition to the facts which I have recently stated, must, I think, be fully convinced that Dr. Pym has entirely failed in his attempt to make it probable, or even possible, that any imported contagion, or any other than local or atmospherical causes, should have produced any of the cases of fever at Gibraltar in 1810, or that, when produced, they manifested any contagious property* in a single instance: and in

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was *no person on shore* from whom she could have caught it, or to whom she gave it; otherwise, as Dr. Burnett presumes, " we should have heard nothing about poison, or the introduction of the disease by the Minorean carpenter. But, he adds, there is still another reason for his silence on this occasion: to have publicly acknowledged this as a case of " *Bulam*," would have shaken his favourite hypothesis to its foundation; for, on the authority of himself, and his evidence Mr. Donnet, this is an indisputable instance of *a second attack*," and of one which *proved mortal*.—See note, page 80.

* Dr. Burnett adduces facts at page 464, which prove that " the disease in question (of 1810) did not radiate from any particular spot, but arose simultaneously in several persons living at a distance from, and perfectly unconnected with, each other." Mr. Amiel, Acting Surgeon to the Foreign Depôt at Gibraltar, in his answers to Mr. Fraser, confirms this statement by saying, " the

this persuasion I will proceed to the fever in the same garrison in the year 1813.

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rise and progress of our epidemics have never been traced, in a satisfactory manner, from a single point of contagion to a gradual number of individuals or families; and instead of creeping slowly from one district to another, cases have appeared unconnected and scattered at different points; and in some instances it has spread with the rapidity of the electric fluid, attacking persons who never approached the sick, nor any assignable source of contagion." It appears by the return published by Dr. Burnett, page 287, and signed by Mr. Kidstone, Surgeon to the 7th Royal Veteran Battalion, that between the 11th and 30th days of October, 1810, Bilious Remittents occurred simultaneously with cases of fever called the Bulam; and after the latter had ceased without any appearance of contagion, five cases of the Bilious Remittent occurred subsequently to the 30th of October, in that Battalion. Of the former cases, thirteen are stated to have been the Bulam, of which, six proved fatal; and four to have been Bilious Remittents; and of these, two were fatal. It appears, however, by the descriptions of some of these cases by Mr. Waters, the Assistant-Surgeon of the Battalion, that several of those denominated Bulam in the returns, had the same symptoms with those called Bilious Remittents; and that, excepting in two cases with black vomitings, it was so difficult to distinguish between the former and the latter, that Mr. Waters considered some of those called Bulam in the return, as being Bilious Remittents. But, supposing that only those with black vomiting were cases of Bulam, it is not easy to conceive that the fever should not have attacked others, if it had been as contagious as is represented.—That it was not so, is proved by the fact asserted by Dr. Burnett, of its having in that year attacked none of those who attended the sick; and also by many others, particularly that mentioned by him at page 256, of Ann Barlow, on the authority of Staff-Surgeon Glasse, who had been in the garrison during the whole of the epidemic of 1804; he states that she remained ill four or five days within the town, in

In this year, Dr. now Sir Joseph, Gilpin was at the head of the Medical Department at Gibraltar, and as it is requisite that I should mention, and notice the opinion which he has published in regard to the epidemic he then witnessed, I think it also right to make my readers acquainted with the *prepossessions* with which he went to that garrison; because these have manifestly influenced his views of the local circumstances of that place, as well as of the epidemic itself; and likewise his judgment in regard to the inferences to be drawn from these views. And for this purpose I must refer them to a letter from Joseph D. A. Gilpin, M. D. Deputy-Inspector of Military Hospitals at Gibraltar, to Colin Chisholm, M. D. printed in the Edinburgh Medical and Surgical Journal for January, 1814; in which it will be found, that Sir Joseph makes this declaration respecting the ship accused

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a small shed, which was crowded with visitors by day and night, and was removed in the last stage of the disease to the neutral ground, where she died with the *black vomit*. But no communication of the disease followed to any of those who had been with her during her illness. Mr. Glasse makes this farther observation, viz. "during the spring and autumn, I have been in the habit of seeing *solitary* cases of fever attended with *black vomiting*, and other severe symptoms, both in town and in the *South*, without the disease being communicated to others confined in the same building." Mr. Amiel also saw two cases of Yellow Fever with yellowness of skin and black vomiting in the garrison of Gibraltar, "while there prevailed no epidemic, nor any suspicion of an imported disorder."—See Dr Burnett, page 326—329.

by Dr. Chisholm of having imported the Yellow Fever from Bulama into Grenada in 1793, viz. " of the infected state of the *Hankey I never did, nor ever shall, entertain the least doubt.*" Certainly he, who never has entertained the *least doubt* upon any particular question, never can have had any reasonable motive to examine or ascertain the facts concerning it; and there is good reason to believe, that, in this respect, Sir Joseph Gilpin was completely inactive, and I may add, uninformed, except by vulgar report. Indeed, he only alledges, as the foundation of this his *unalterable* belief, that soon after the arrival of the *Hankey*, a fever appeared, attended by the symptoms described by Dr. Chisholm; that Captain Cox of that ship, as he (Sir Joseph) was informed, had admitted to the Lieutenant-Governor, " that he had kept the clothes and bedding of his passengers:" which information, however, was not correct in regard to the *bedding*, this having been *all thrown overboard* on the coast of Africa; and in regard to the *clothes*, they, after being properly cleansed, or aired, had been closely packed in chests, and were *stowed in the hold*, completely out of the way of communicating infection, even if they possessed any; while the clothes of those who died on board the *Calypso*, in which ship the supposed infection was alledged to have originated, had been all landed and dispersed in England without the least harm to any one, as before mentioned, p. 282. " That those," adds he, " who

visited the Hankey, brought the contagion into the town of St. George, and that it spread from thence into the country, I have as little doubt, as I have of my present existence."* It is a melancholy truth, that those who

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* If Sir Joseph Gilpin had not determined to retain through life his original opinion in regard to the *Hankey*, I might hope that the following extract from Inspector Fergusson's very valuable Inspection Report of the Island of Grenada to the Army Medical Board, would point out to him a much more reasonable cause of the fever in that island, than an importation of febrile contagion from a part of the world in which it did not exist: viz. after describing the ridges of Richmond Hill, seven hundred feet above the sea, Cardigan Heights, and Hospital Hill (the base of which touches upon the carenage, and is "foul and swampy," and most unwholesome), Dr. Fergusson says, "the carenage itself differs little from the pestiferous creeks which bear that name in the West Indies. Its channel winds for about nine hundred yards round the bluff of Fort George, so as to be invisible from the sea till you are actually in it, when it presents an oblong *cul de sac*, enclosed on every side by the high grounds of the Hospital and the Richmond ridge, *hanging directly over it*. The marsh below Hospital Hill is within less than half a mile on the north-west. The swampy margin of the Lagoon, as it is called (a conglobate appendage of the carenage, but separated from it by a reef under water), is within a short distance of the south-east, and the bowsprits of the shipping, even where they lie at anchor, overhang in many places the muddy uncleared banks of the carenage."

"No winds but such as come in eddying gusts through the chasms of the hills, can ever fairly sweep the surface of the waters at the anchorage, which, from the shape of the harbour, derive scarcely any movement from without; and when the port is crowded with vessels, obstructing all ventilation by lying after the manner of a carenage, as close together as they could be supposed to do in a dock, it is difficult to conceive, amidst the stag-

are least accustomed to exercise their reasoning faculties, and least *informed*, are commonly the most *credulous* and the most *tenacious* of their *unfounded opinions*. I hope that this cannot be true of Sir Joseph Gilpin; but I must assert, that those *suppositions*, of which he declares himself to be as *confident as of his own existence, never had, or could have had, the smallest foundation in truth*: that it never was proved, or made probable, that any individual whatever took the fever on board the Hankey, after her arrival at Grenada—*where* she was a *clean* ship, and probably the *cleanest* and *most healthy* of all the ships at that island; and that the fevers which occurred on board, or among the settlers, while she was on the coast of Africa, were *exclusively* the remittents or intermittents of that coast, *on which contagious fevers are unknown*. And if he had really taken proper pains, he must have been able *to ascertain* that the fever at Grenada in 1793 was not contagious; that Captain Remington, who was most confidently selected as the *first* person said to have caught the fever on board the Hankey, (although it appears to have been excited in him by exposure to wet and cold), and who *died* of it after a

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nation of the elements and combinations of pestiferous sources, a spot more likely to generate, under a tropical sun, the most aggravated species of Yellow Fever."

week's illness at* Grenville Bay, did not communicate any fever to Dr. Stewart, or to any of the persons by whom he was visited and attended on *board his own ship*, or afterwards on *shore;* and that the disease was so far from being propagated from the town of St. George to the *country*, as Sir Joseph Gilpin firmly believes, that those who, after imbibing the noxious miasmata in the town, went into the country, and sickened, and even *died there*, did not, in a single instance, occasion fever in any other individual; and that the bedding, clothes, &c. of such persons proved in all cases perfectly harmless, which could not have happened if any such effects, secluded in the hold of the Hankey, had been able to produce the fever at St. George's. If proofs of this are wanted, they may be found at page 116, and also in abundance in the Seventh Appendix to my Essay.

With this prepossession, which Sir Joseph Gilpin has

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* Dr. Fergusson, in his Inspection Report concerning Grenada, describes this part of the island in the following words, viz. "Within a few miles of Grenville Bay, on the windward shore, all the sea-borders become extremely foul and swampy. I have scarcely seen in the West Indies a more unwholesome quarter than the village of Grenville, or the post which was once occupied by our troops in its neighbourhood."—"Even our unthinking and most incautious sailors dread to sleep in the village; and the clerks who are employed, after the crop season, to ship the sugars at the harbour, are generally allowed a double salary."

avowed to be *invincible,* though founded *solely in error,* it may naturally be apprehended that his mind, after his arrival at Gibraltar, was *inaccessible* to every species of evidence *against* the supposed contagion of the fever, said to have been derived from Bulama; and that he would have seen *proofs of that quality* in things which, to impartial observers, must have suggested very opposite conclusions. And accordingly we find it stated in the answers of Sir Joseph Gilpin to certain "Queries" proposed by the late Army Medical Board, and printed in the fifth volume of the Medico-Chirurgical Transactions, that the persons *first* attacked by the epidemic at Gibraltar in 1813, "were *strangers,* who had come from Cadiz, where, at that period" (as he says erroneously) "the epidemic in question prevailed."* These strangers were

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* In this communication, Sir Joseph Gilpin seems as desirous as Sir James Fellowes and Dr. Pym to represent the production of fever-exciting miasmata at Gibraltar as being impossible. He says, "the heats are so excessive during the summer months, as to leave no sort of moisture on the surface of the ground in the town, even for a few hours; and the garden grounds become so hard, as to admit the insertion of a *hoe* with difficulty."—"The disease first appeared (i. e. in 1813) in a part of the town by no means particularly exposed to noxious effluvia." It will soon be shewn that other gentlemen think differently on this subject. But what seems the most extraordinary is the declaration of Sir Joseph in this very communication, that "bilious *remittent* fevers, and other autumnal complaints, are seen *in this garrison every year.* In 1812, we had in the hospital, particularly of the foreign

the persons mentioned by Mr. John Cortes, a medical practitioner at Gibraltar, as having arrived there, in a vessel called the *Fortune*, from Cadiz, *on the 11th of August*, one of whom, a seaman belonging to that vessel, being ill, as we are told, "was sent to the Catholic Hospital, and died on the 19th of this month, with every symptom of the Yellow Fever." It is added, that a Frenchman, a native of Paris, died on the 3d of September, in Government Street, near the City Mill Lane, with nearly the same symptoms as the former man, and who had been a passenger with him in the vessel above named." (See Sir Joseph Gilpin's Letter in the Edinburgh Medical Journal for July, 1814.) We are not told *when* this vessel left Cadiz, nor is it pretended, so far as I can discover, that either of the persons said to have died as before mentioned, were the cause of fever in any other individual; and I really believe, that if Sir Joseph Gilpin had not, from his prejudices on this subject, concluded that the epidemic, which afterwards occurred at Gibraltar, *must* have been produced by an imported contagion, and that

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corps, and 7th Royal Veterans, some *very bad* cases of bilious *remittents*, attended with the *worst* symptoms."—"Intermittents do not *prevail* here." Does Sir Joseph Gilpin suppose that his observation, that intermittents do not *prevail* (he does not say *occur*), will invalidate his preceding declaration? And will he allow it to be understood to be his belief, that intermittents, but not remittents, are caused by marsh miasmata?

it was necessary to point out some vehicle by which the importation was made, mention would never have been made of the Fortune, or of the illness of any person who had been on board. Certainly no suspicion in regard to this vessel, and her crew or passengers, was entertained by Sir Joseph Gilpin, until *long after* the time at which the supposed contagion, had it existed, *must have manifested itself in many persons;* for Sir James Fellowes states (at page 273), that the late Lieutenant-General Campbell, then Lieutenant-Governor of Gibraltar, declared in a letter which he "addressed to Sir James Duff, the British Consul at Cadiz, dated September 13th, 1813, that some cases of fever had lately been observed in the garrison, which gave rise to various reports respecting the public health of the garrison; but that there was *not one of a contagious nature,** as *they were peculiar to the season only.*" This was *more than a month* after the supposed importation from Cadiz is said to have taken place; and as Dr. Pym confidently asserts that the contagion of the Bulam produces disease *in four days,* at least in Gibraltar, its existence must have been made

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* This accords entirely with the statement officially made by Mr. Amiel, Acting Surgeon to the Foreign Depôt at Gibraltar, in his answers to Deputy-Inspector Fraser's questions, viz. "bilious remittent fevers were very prevalent during the summer of 1813; but, as the autumn approached, I observed they were assuming a more aggravated character."

manifest by the occurrence of very many attacks within that interval; while, if it had been known to have produced *even one*, Sir Joseph Gilpin must have been highly culpable, had he not informed the Lieutenant-Governor thereof.* Sir James Fellowes adds, that Sir Joseph

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* In the same communication Sir Joseph Gilpin adds, "*we* might have been *thrown off our guard, had we not seen, at an early period after their arrival, two* of the *sick strangers* from Cadiz; for the Spanish government declared that, after the most serious and *fullest* investigation, no contagious disease existed in that city. One man from Cadiz died in the Catholic Hospital a week or ten days before I was informed, by an intelligent medical officer residing at Cadiz, that the disease did exist in that city at the time when the declaration above-mentioned was made."—Here, as I think, Sir Joseph Gilpin either has taken to himself the merit of having, by his great watchfulness, escaped the being deceived by the declaration of the Spanish government, and of having detected the introduction of the supposed contagious or Bulam Fever, *three or four weeks before he could have had the smallest suspicion of its existence in Gibraltar*, or must very culpably have neglected his duty as chief medical officer, in not revealing the truth to the Lieutenant-Governor, and in allowing him to remain in ignorance, or under a delusion, on a matter of the highest importance. The dilemma seems unavoidable; but I leave the alternatives to his choice.—In regard to the "intelligent medical officer," whose alledged information is taken as the foundation of a charge against the Spanish authorities, of suppressing the truth, and publishing a dangerous falsehood, I must think it to have been highly incumbent on Sir Joseph Gilpin to mention his name, and the date of the communication in which such a charge was made. Had he done this, we might have been able to discover whether this *intelligent* officer really deserved that character, or whether he is one of those who shut their eyes against truth, and see contagion where it does not exist, and with a deter-

Gilpin in a letter to himself, dated the 15*th* of September, wrote as follows: "*Within these last few days*, several cases of fever of a *serious* type have appeared amongst the civil inhabitants, which we (the Surgeon-Major and myself) have reported to the Board of Health." The term *serious* does not here necessarily imply that contagion was, *even then*, suspected to have had any influence

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mination never to alter their opinions, or to become wiser; and we might have been able to ask for the facts on which his communication was founded, and how it happened that he could, and did, convey this information to Sir Joseph Gilpin at Gibraltar, and yet concealed it from his immediate superior officer, Sir James Fellowes; who certainly, when he wrote and published his volume nearly two years after, must have been completely ignorant of any case of contagious fever having occurred at Cadiz until at least three or four weeks after the time, when one of the persons said to have imported it from that city was attacked, and sent to the Catholic Hospital in Gibraltar.—For, in treating of the epidemic of this year at Cadiz, he says (page 254), "the complaints of the natives" (i. e. of Spain) "maintained the same *bilious* character, and followed the order of the season, *without shewing any peculiarity* of symptoms until the *end of August*." He adds, "these observations were made by the Spanish practitioners in Cadiz; but they were equally applicable to the British troops, and to our military hospitals, for the disorders of the soldiers admitted there were so mild, as to yield readily to the simplest plan of cure," &c. Again, at page 256 he says, "in fact, until the *end* of August, the people collectively were, according to all the reports at the time, in a healthy state." And it farther appears from his statement at page 261, &c. that it was only on the 14th of September that he observed any case in the British hospitals that excited his suspicion.

in producing these cases, or that they were any thing more than severe remittents, which in truth was the case: but, whatever they were, they had only occurred *very recently*, probably after General Campbell's letter, and nearly five weeks after the 11th of August; and therefore the recurring to the vessel from Cadiz is altogether a suggestion ex post facto, especially as at the time of this vessel's sailing, and indeed at a much later period, the Spanish Authorities and the Board of Health at Cadiz, had declared, after a full investigation, that no contagious disease existed in that city; and also as it was not until the 14th of September that Sir J. Fellowes, although resident there, and anxious, vigilant, and predisposed in regard to contagion, was able, as Dr. Burnett has observed, to " discover a few cases *under lock and key* in a Spanish hospital."

Leaving such inconsistent accounts to their merited fate, I proceed to give briefly other and better explanations of the cause of the epidemic at Gibraltar in 1813, referring for more ample details to the statements contained in the 4th and 5th Appendixes to Dr. Burnett's work. It seems that, although in that year " the garrison of Gibraltar was in strict quarantine *for several months before* the malady made its appearance, and a Board of Health was sitting *almost daily* on account of the plague which had broken out at Malta," as is stated by that

gentleman, page 479, the vigilance and precautions of this Board against the introduction of contagion were of no avail towards hindering the occurrence of the Yellow Fever, when the temperature of the atmosphere excited into action those local causes which were competent to produce it. During the month of August and in the early part of September the thermometer commonly stood at about 80° in the shade; but the mortality occasioned by the disorder was not considerable until about the 20th of the latter month, upon which day the deaths increased from seven to twelve in number, and the attacks from six to thirty-three. On the 13th of October the epidemic appears to have attained its greatest augmentation, twenty-one deaths and one hundred and fifteen new cases of fever having that day occurred; but after the 8th of November it rapidly declined.

The following short extracts from the account of this epidemic (given by Mr. Humphreys, Assistant Surgeon to the Royal Artillery, in the 46th number of the Edinburgh Medical and Surgical Journal) will, I conceive, afford a just and satisfactory explanation of its nature and causes. After stating that "the Bilious Remitting Fever, which has long been prevalent at Gibraltar, during the summer months, assumed a malignant form in the month of August, 1813;" he adds, that on its first appearance this year "in its aggravated type, a report was spread, and

generally believed, that it was contagious; the garrison was immediately placed in a state of the strictest quarantine, and families became so exceedingly alarmed from a supposition of its being highly so, as to shut themselves up, refusing admittance even to their friends. I do not however concur in this opinion, and hope to shew in a satisfactory manner, by *many* instances which I shall adduce, that *no contagion existed.* The disease at its commencement, attacked persons *in different parts of the town,* at the *same time,** who were remotely situated from each other; and those who *shut* themselves up, and considered themselves secure, being perfectly *insulated,* were attacked as readily as those who mixed indiscriminately with the people as usual; shewing clearly that the disease was produced by general *local* causes, acting on the whole population of the place, and *not imported,* of which no satisfactory assignable source has ever been traced. Nor do I think it probable that contagion is of that peculiar nature as to shew itself in different years precisely at the *same season,* having lain dormant and inert nine months of the year, and then burst forth to commence its devastating influence.

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* This is confirmed by the declaration of Dr. Gardiner, Surgeon to the Naval Hospital at Gibraltar, as given at page 312 of Dr. Burnett's work: "the disease (in 1813) did not spread from any focus, but broke out in fifty different places at once."

In regard to the causes of this fever Mr. Humphreys (besides the crowded population, and wretched construction and state of the houses, precluding the access and change of air), ascribes it to "the immense collection of animal and vegetable matters arising from so great a population, during the dry summer months, remaining stagnant from May until the end of August, and which became roused into action in the autumnal season by the partial rains, when the quantity of water is not sufficient to propel them through their respective drains, which are too narrow for their evacuation, being frequently choaked, and bursting: at this season also (says he), the heavy night fogs, succeeded by a parching sun, occasion exhalations extremely noxious and unwholesome. To these may be added the perpendicular situation of the town, which is on the western foot of a steep rock, about 1400 feet in height, running north and south, the air remaining nearly stagnant during the prevalence of the easterly winds, which continue with but little variation the whole summer." He then proceeds to state a considerable number of cases of individuals, who in 1813 and 1814, after secluding themselves for three, four, five and six weeks from all intercourse which could introduce contagion, were attacked with the fever, and either recovered or died without any communication of disease to those by whom they were surrounded, or attended during its continuance.—"In these cases the patients had the characteristic symptoms of vio-

lent Yellow Fever."——Mr. Humphreys adds, "It is now pretty well established as a fact, that the fever of Gibraltar is *local*, not imported or propagated by contagion; that all the regulations, quarantine and precautionary measures, have had but little effect in either preventing or lessening the ravages of the disease; that the fever is alone influenced by the seasons of the year, and gradually lessens in severity, as the cold weather, with wind and heavy rain, succeed to the autumnal heat; and that the only measure found decidedly efficacious, has been the removal of the people from the *western side* of the rock; that is to say, from the range of action of the local causes."*

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* Dr. Burnett mentions p. 335, that in the fever in Gibraltar in 1813, the South Pavilion was particularly unhealthy; a number of officers with their servants, who were quartered there, being attacked with the fever, especially those who resided on the *ground-floor*; and he adds, in a note, that in 1804 the first men attacked in the 13th regiment were precisely in a similar situation. I have already noticed similar facts, as proving that miasmata emitted from the surface of the earth could alone occasion this difference, between the upper and lower stories. See the note at page 185.

It is stated by Mr. Humphreys (see Dr. Burnett's work, page 354, that Dr. M'Millan, Physician to the Forces, came to Gibraltar in 1814, after having been recently employed in the West Indies, "and, after an examination of the sick in the town and garrison, and the appearances on dissection after death, pronounced the disease to be the Yellow Fever of the West Indies, and not contagious." Dr. M'Millan, in a communication printed in the Edinburgh Medical and Surgical Journal (No. 37), treating of the controversy, whether the Yellow Fever

As the Fever at Gibraltar in 1814 (the last year in which the disease prevailed epidemically) has been thus brought under the consideration of my readers; and as I believe that no person has even pretended to account for its production by any specified *importation of contagion* in that year (which, with the similar instances of 1801 at Cadiz and Medina Sidonia, and of 1804 at Malaga (see p. 308, 309 and 317), affords a convincing proof that the epidemical existence of the Yellow Fever is altogether independent of any importation of contagion) I shall finish my observations on this subject, by subjoining certain extracts from the answers delivered by the Medical Officers at Gibraltar, in the

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differs in *kind* or in *degree* from the common remittent," says, "he was at first disposed to view them as distinct *kinds* of fever; but more extended experience has inclined him to coincide in the opinion now pretty generally entertained by those engaged in military and civil practice in the West Indies, that *they differ only in degree of severity of symptoms.*" He adds, "This may be judged of more clearly in the wards of a large military hospital, where fever may be viewed in all its degrees, at the same time, in different subjects; varying from the intermittent to the mild remittent, and by increasing stages of severity to what has been denominated Yellow Fever. In Europeans *newly* arrived, of full habit and robust health, the exciting causes will no doubt act with greater force than with those accustomed to their action, and produce the disease in its most aggravated form. The fever is often at first continued; by evacuants breaking the force of re-action, it becomes remittent; and frequently in the same patients, before they are discharged from the hospital, it may be observed to change to an intermittent."

autumn of 1814, to the questions proposed to them by Deputy Inspector Fraser; and by a very few additional extracts from some of the Reports made to the Commissioners for the Transport Service, as already mentioned. It becomes me however previously to say, that the medical officers to whom Mr. Fraser's questions were severally addressed, were seventeen in number: and that, in regard to the origin of the epidemics in that garrison generally, and particularly of that of 1814, twelve of the respondents (viz. Dr. Lea, Surgeon to the 26th regiment; Mr. Weld, Surgeon to the 67th regiment; Mr. Short, Surgeon to the 60th regiment; Mr. Playfair, Surgeon to Dillon's; Mr. Donnet, Surgeon to the Naval Hospital; Mr. Amiel, Acting Surgeon to the Foreign Depôt; Mr. Humphreys, Assistant Surgeon to the Royal Artillery; Mr. Martindale, Assistant Surgeon of the 67th regiment; Mr. Thompson, Hospital Assistant; Mr. Gamellaro, Assistant Surgeon to Dillon's; Mr. Brady, Assistant Surgeon of the 26th regiment; and Mr. Foote, Assistant Surgeon to the 26th regiment) all declared it to be their belief that the disease *originated from domestic or local causes at Gibraltar*, and *not from imported contagion;* that on this question *three* were neutral, (viz. Surgeon Sproule, of the Artillery; Assistant Surgeon Williams; and Hospital Assistant Sutherland): that Mr. Barker, Surgeon to the 11th regiment, declined giving any opinion on the question, whether the disease was of domestic origin or had

been produced by imported contagion?* and that Assistant Surgeon Considini *alone* considers it as having been imported. It might have been expected that all those who professed to believe the disease to have originated from local and atmospheric causes at Gibraltar, would have also believed it not to have been contagious, according to the general principle, which I have endeavoured to establish between pages 95 and 156 of my Essay (and pages 195 and 203 of this Sequel), that, contagion being *ingenerable*, no fever which is not naturally contagious can ever acquire a contagious property; but here the ancient, and as yet but imperfectly exploded, doctrine, of the schools, which taught men to believe that all fevers might by crowding, filth, &c. become contagious seems to have operated in the minds of four of these (twelve) gentlemen, viz. Mr. Donnet, Mr. Amiel, Assistant Surgeon Brady, and Assistant Surgeon Foote: of these indeed, Mr. Donnet appears to allow the disease to be contagious only in a very limited degree; and says, "the contagion is rendered *inert* by a removal to a pure atmosphere;" and Mr. Amiel, although he thinks the disease may acquire a contagious quality in extraordinary circumstances, such as crowding, want of cleanliness and ventilation, &c. does not consider it as *naturally* contagious, but

* Mr. Barker's words are, "with regard to its foreign introduction, or formation in the town, I have nothing to say."

"*only accidentally*" so, and therefore in truth he ought to have been reckoned among the non-contagionists. Besides these, there are *five* on the side of contagion; viz. Surgeon Sproule, of the artillery, who thinks the disease contagious "from its running through whole families" (a reason which, in my judgment, is quite as favourable to the belief of its resulting from *local* causes): Assistant Surgeon Foote, who thinks it contagious *for the same reason*; and Assistant Surgeon Brady, who, though he thinks that the disease originated at Gibraltar, and was propagated in consequence of a crowded population and a vitiated state of the atmosphere, declares his belief in its contagion, but without assigning any foundation for that belief: Assistant-Surgeon Considini, who inclines to think that the disease was imported and contagious, "from its *sudden* appearance" in 1804 (which shows that he was not acquainted with the history of the disease), and because his opinion "holds forth a hope that, by persevering exertion, it may be finally eradicated," founding his belief, as we see, on a *hope*, and not on any fact: and lastly, Mr. Barker, who, by the contagionists, is reckoned as a believer in the contagious property of the fever in question, although he has abstained from giving any opinion concerning its origin, and has offered no reasons on the side of contagion which do not better accord with the opposite doctrine. He conceives that "Gibraltar is too clear of wood, and certainly has not sufficient vegetable matter, to account for the production of so formidable a disease," but says that "the

disease appeared to exist only in the town, or in cases brought from it to the south," which indicates miasmata as its cause; he also conceives that "atmospheric vitiation, as applied to an epidemic, ought properly to be considered synonymous with the term contagion, although he is aware that on this point there is still much controversy." The only facts which he mentions to support a belief that the fever was contagious are, that "the 11th regiment was encamped at Europa" ("having a detachment in the town," as he states elsewhere) "and out of 162 cases affected with fever this year, 130 had communication with the town or hospital," i. e. with the places in which miasmata were most prevalent; and that of his "own family, six in number, five had the epidemic," although "as little intercourse as possible was kept up with any other place;" but it is obvious that the disease, if contagious, must have spread through the regiment at Europa, where however, as well as on the neutral ground, there is abundance of proof that contagion did not operate. See p. 166, 394, 396—7, 399, &c.

From this review it will appear that, on the subject of contagion, eight of the respondents, besides Mr. Amiel, were decided in their disbelief of it, as connected with the epidemic at Gibraltar, and that Mr. Amiel ought strictly to be considered as a ninth. That Mr. Donnet ascribes to the disease a very limited degree of contagion. That

Mr. Sproule and Mr. Foote believe it to be contagious, for a reason, better suited to produce a different belief, as does Mr. Barker; that Mr. Brady believes this, without alledging any reason, and in opposition to facts stated by himself, which appear incompatible with it, and that Mr. Considini believes the fever to be contagious, because that belief best accords with one of his hopes or wishes. Of the value of the preceding opinions, my readers will judge from the facts or reasons on which they rest. The most important result of this appeal by Mr. Fraser, to the Medical Officers at Gibraltar, is this, that while *twelve* of the respondents have *denied* the importation of contagion, as a cause of the epidemic, and have almost all of them stated *facts of decisive importance* to support their denial; *one only* (Mr. Considini), professes to believe this importation, without alledging a *single fact* to warrant his belief. And according to all the arguments of my chief opponents, and of the principles which I have endeavoured to establish, a denial of the *importation* of *a specific* contagion, as the cause of this fever, involves a denial of its contagious property, and a complete subversion of all their doctrines. Having premised these observations, I now proceed to introduce the extracts lately mentioned, as the conclusion of what I propose to offer relative to Gibraltar.

Dr. Gray, late Physician to the Mediterranean fleet, in his Report to the Transport Board, says of the epidemic

in that garrison, "I could not upon reflection hesitate in considering the disease to be the endemic of the country, the Bilious Remittent Fever, but displaying a type of greater malignity than is usually observed; and this I thought might arise from peculiar *local* circumstances, inducing this *aggravated* grade, on a population crowded to excess." Here he describes the local causes, &c. which he thinks likely to have occasioned this aggravation, adding, "It has been remarked that the summers preceding its attack in 1804," (1810) "1813, and 1814 were excessively hot and oppressive, accompanied with a long prevalence of easterly wind: the town being situated at the western foot of a high mountain, and surrounded by walls on the north and south, the free circulation of air is *completely* obstructed when the wind blows from this quarter, rendering the atmosphere over the place almost stagnant. The inhabitants can always foretel the approach of an easterly wind, from the offensive odour which arises from the privies, sewers, &c." "The *shallowness of the sea-beach along the line wall,* is often a source of mischief; the sewers do not discharge their contents sufficiently into deep water, and when mixed with other materials thrown over it, the air must necessarily be vitiated, from these putrid fomites."——"The Bilious *Remittent* Fever is more or less *endemic* in the summer and autumnal months along the southern coasts of Spain, Italy, and the Mediterranean islands. The inhabitants of sea-ports, and

towns in the vicinity of marshes, swamps, &c., are most liable to this disease, when the heat is unusually great and oppressive. *Strangers, particularly those from northern latitudes, are first attacked, and in a more violent degree than the natives.*"—" Whilst serving in the Mediterranean, as Surgeon of the Dolphin *hospital*-ship, and of the naval-hospitals at Gibraltar and Minorca, from 1794 to 1802, and of *Malta* hospital in 1803 and 1804, I have had under my care upwards of two thousand cases of the endemic of this country, the Bilious Remittent Fever, from His Majesty's ships cruizing on the coast of Spain, Italy and Sicily. Some cases of a very severe type I received from troop-ships, employed on the coast of Genoa, and afterwards in Egypt in 1800. I also received from the Leviathan and Genereux upwards of three hundred men, attacked with the fever, the chief symptoms of which were great prostration of strength, excruciating head-ache, referring to the forehead and orbits, eyes turgid, flushed countenance, oppression at the præcordia, general pains of the back, loins, and calves of the legs, great irritability of stomach and bilious vomitings; yellow suffusion of the skin; if proper evacuations are not resorted to in the commencement of the attack, such as bleeding and copious purging, delirium shortly supervened, and towards the fatal termination of the disease petechiæ, hæmorrhages from the nose and mouth, with *black fœtid stools*. I have also observed in some instances, the matter ejected from

the stomach (in the last stage) of a *dark* colour resembling *the grounds of coffee.*" Dr. Gray adds (as before stated), "during this long period of serving, I can not recall to my recollection a single instance of fever's being communicated from one person to another, either amongst medical attendants or nurses."

Mr. Whitmarsh, late Assistant Surgeon in the Naval Hospital at Gibraltar, in his letter to Dr. Gray (of December the 6th, 1815), communicated to the Transport-Board, No. 24, says,

"The appearance of this fever (at Gibraltar) in the two successive years of 1813 and 1814, was after many weeks of intense hot weather, mostly with a Levant-wind. The thermometer of Fahrenheit's scale generally ranging from 82° to 88°, within doors; the night developed thick fogs, so as to leave a dampness on the earth, to all appearance the next morning as if rain had fallen: between the interstices of the rock is to be found mould, which nourishes the roots of various shrubs; soon after the rains set in, many of these vegetate; others not so firmly attached are washed down by the torrent, great portions of which will occasionally be obstructed 'ere they reach the drains below; the rains subsiding, many weeks follow of oppressive solar heat, and other atmospheric vicissitudes, as above-mentioned; the vegetable productions remain-

ing, as well as those obstructed in their passage down the mountain, are left to be acted upon by the rays of the sun, during very many successive days; night setting in, this vegetable matter, imbibing the nocturnal moisture, expands and extricates vapours which, from want of ventilation, in the course of time vitiate the surrounding atmosphere; this, if we may so call it, local atmosphere, so contaminated, is rendered still more obnoxious to the health of man, from an excessively crowded population, the bulk of whom are of the lowest class of all nations, who huddle themselves in incredible numbers into filthy ill-ventilated habitations: which with the want of some other requisite attentions to cleanliness and order," made him consider the disease to be of "*domestic* origin." He then proceeds to mention other facts in support of this opinion, which the reader will find quoted at page 166.

To the preceding extract I will add one from the answers of Mr. Martindale, assistant Surgeon to the 67th regiment, viz.

"I believe this fever to be of endemic origin, and not imported from any foreign part; it is *precisely the same as the fever at Cadiz*, and the same which rages at certain times along the *whole coast*. This fever cannot, in my opinion, be produced casually, whilst the disposition to it exists in the climate, aggravated in Gibraltar by local cir-

cumstances of a badly ventilated and dirty town, very dirty inhabitants, very crowded houses, narrow streets, and from these a vitiated atmosphere. The dwellings, except in the principal streets, are mere wooden huts or hovels, where dirt and filth are rapidly accumulated."

In regard to the supposed contagion of the epidemic, Mr. Martindale answers Mr. Fraser, " If this fever were contagious, it is reasonable to suppose that *orderlies* in the hospital, who constantly attend the men attacked with it, touching their bodies, their bed-clothes, and dirty linen, removing their excrements, and breathing the same atmosphere *day and night*, would, by being thus exposed to the worst source of the contagion, be liable to take a fever of the highest degree of malignity; but out of seven attacked with the fever, not one died, and only one had it severely. Medical Officers also are constantly exposed to this contagion, if it may be so called, not only during the life-time, but after the death of the patients; but *no one* took the disease from opening a dead body: I myself opened several, and once cut my finger in doing so, but no consequence followed; and of the Medical Officers taken ill, *not one died.* Both of these descriptions of persons are exposed to the *local* causes of this fever, and it is much more probable that they took it in this manner than by contagion. The very circumstance of a number of men in a regiment being attacked with fever at the *same time*, is to me the greatest proof of the *non*-con-

tagious nature of this fever. Dillon's regiment was quartered in the *Blue* Barracks near the Moorish Castle: a great number of the men took the fever, and several died: in consequence of which, the regiment was sent out to the *neutral* ground, and encamped; and immediately the fever stopped. Now, if these men had caught the disease by infection or contagion, would it so soon have ceased? Would not many seeds of it have been carried into the camp, and the fever now and then, if not generally, have shewn itself as before? But nothing of this kind took place; they remained perfectly healthy. The 8th battalion of the 60th regiment arrived from Cadiz in a healthy state, and were encamped on the Governor's Meadow; shortly after, they went into town, and were quartered in the Cooperage-range; the fever instantly raged among them; both officers and men suffered severely; they were then sent back to the encampment, and the disease, as in Dillon's regiment, immediately ceased, as if by magic; the seeds of contagion, if they did exist, were *blighted.* Are not these instances proofs of the *locality* of the disease."

Mr. Martindale farther observes, "If it (the fever) should run through a whole family, this does not shew its contagious nature, but that the *spot* is unhealthy. On the rock there are healthy and unhealthy places: the town is less healthy than the south; but even here the fever breaks out, when neither contagion nor communication with the

town can possibly be traced; and several men in the Regimental Hospital, in the south, took the disease, where they had not *even seen* any one sick with fever. This also shews its *endemic* origin. I likewise have known several instances where only one in a family, and even in a *whole* house, was taken ill. Although *ten* officers in the regiment had the fever, not one of their servants took it."—"The woman who *washed* the *dirty* linen of *all* the men in the *Fever* Hospital *escaped free* from the effects of *contagion*."

The following is a short extract from Mr. Amiel's answers to the questions of Mr. Fraser, viz.

"The temperature of Gibraltar in the summer months differs very little from that of the islands between the tropics, the thermometer rising from 80 to 90 degrees of Fahrenheit; the weather is usually very dry from the beginning of May to the latter end of August; and the town, protected by high walls on the north and south, stands at the western foot of a *steep* mountain, whose elevation is about 1,400 feet, *completely obstructing all easterly* breezes, and rendering during their continuance the atmosphere of *this side* of the rock nearly stagnant; and therefore the *exhalations*, which a *scorching sun raises* from the many unwholesome substances above enumerated, accumulate from the want of ventilation, and be-

coming a very powerful cause of disease, have most probably aggravated the Bilious *Remittent* Fever, which *has long been known here*, in its mild form, to that *malignant type* which has lately constituted our epidemic. In fact, the summers preceding the epidemics of 1804, 1810, 1813, and this last summer (1814), have been chiefly remarkable for *a long continuance of easterly winds:* it is consequently highly probable that the epidemic of Gibraltar has a *domestic* origin, is produced from *local* causes, and has not been introduced from *abroad* as a specific contagious disease." *

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* A long and, as I believe, good account of the *local* circumstances and peculiarities of Gibraltar, by Mr. Amiel, may be found between pages 335 and 340 of Dr. Burnett's volume.—This gentleman in answering Mr. Fraser, farther states that " an individual labouring under our Epidemical Fever, on being removed to a pure air and ventilated place, such as the Neutral Ground or *Europa* Point, did not communicate the disease to those in the closest contact with him: this observation was confirmed in many instances during the epidemic of *last year*, amongst the foreign recruits quartered at the Brewery Barracks. The depôt consisted of between five and six hundred men, sixty of whom were permanently employed in the different departments in town or as servants to officers; these men on being attacked by the epidemic (and I believe not one escaped it) generally came to the barracks, where they lay *all night in a crowded ward*, and sometimes, by concealing themselves, they continued two days in the same place; yet I never observed that either their breaths, or the effluvia of their bodies and clothes, had proved infectious to their companions. Forty women of that depôt, who had been prohibited passing Europa gate, remained perfectly healthy, although I had seen

I will next subjoin an extract from the answers of Dr. Lea, Surgeon to the 26th regiment, to Mr. Fraser's questions :

" I first observed this fever in the month of *June* 1814, both in the 26th and 67th Regimental Hospitals. I consider this fever to *originate here ;* and to be aggravated by the *local* disadvantages of the rock."—Its diagnostic symptoms were " the usual diagnostic symptoms of Bilious Remittent Fever."——He farther answers, " I consider this fever to be the *true* Bilious Remittent :"—adding, " I have seen the Bilious Remittent in Holland and England, and in Gibraltar in 1804 and 1814 ; and had it myself in *both* those years." He farther observes, that the *fatal* terminations generally took place " from the *third* to

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some of them sitting on the same bed where a man was lying in a fever. Out of four thousand Spaniards removed last year to the neutral ground, a few died of the fever then prevailing in the garrison ; but it is a well-known fact, that those *only* had the fever there, who were *already* sick on going out ; and that they did not communicate the disease to any of their neighbours or attendants. If this disease cannot be transplanted from this to such a *small* distance, could it have been transplanted thousands of miles from America to Cadiz, and then from Cadiz to Gibraltar? And if ventilation so effectually destroys the infectious quality of this fever in a place so contiguous to our atmosphere, is it to be believed that it would have less efficacy on board a vessel, and especially in a *boat*, where a man, or foul clothes, must stay some time before they can reach our shore."

the fifth day;" that the mortal symptoms were "great prostration of strength; constant vomiting; low muttering delirium, hiccup, petechiæ, and black vomit;"—and that "of three hundred and fourteen men with this fever, twenty died."

The following is an extract from the answers of Mr. Playfair, Acting Surgeon to Dillon's regiment, to the questions of Mr. Fraser, viz.

"I first observed the epidemic in Dillon's regiment, on the 19th of August, 1814."—"It is in my opinion an endemic disease: and the reasons which induce me to think so are as follow, viz. the recurrence of the disease at *one fixed season of the year,* best adapted to favour the exhalations from putrid animal and vegetable remains; the diminution of its violence and final disappearance at the end of this season: the unhealthiness of particular houses and quarters in comparison with others; the immediate stop put to the disease by preventing communication with these quarters, as stated in answer to No. 12; the prevalence of the disease in places well calculated by local situation and neglect of cleanliness to produce endemic fevers, as Carthagena and Gibraltar."

Mr. Playfair does not think the Gibraltar epidemic of 1814 to have been *contagious;* among other reasons he

says, " It is very striking that, even after the removal to the camp in the Governor's Meadow, while the men of Dillon's regiment were allowed to enter the town on fatigue duty, the fever still continued prevalent; but from the day of their confinement to the neutral ground, they were quite free of it; although they had equal intercourse and communication with *the inhabitants coming from town*, in which the disease was at that period very destructive." He adds that " the Blue Barracks and Blue Barrack sheds were decidedly more unhealthy than the *Castle* sheds or the town range."

Mr. Donnet, in his answers to Mr. Fraser, delivers the following opinion; " I conceive the malady to have been generated in this garrison, and attribute it to an atmosphere vitiated by noxious exhalations from animal and vegetable matters, which, by a long continuance of easterly winds, the height of the rock screening the town and part of the south, and consequently a want of a due ventilation, has exerted its deleterious properties on the system in a manner peculiar to the season and climate of Gibraltar, differing in summer and autumn very little in temperature from the islands within the tropics, where the Bilious Remittent rages with severity until the month of August, when it assumes a different type, announcing itself in all respects with symptoms similar to those of the existing epidemic."

Assistant Surgeon Gamellaro, of Dillon's regiment, in answering Mr. Fraser's questions says,

"I believe the epidemic to be an endemic disease: the facts which induce me to think it such are, the *season* during which it manifests itself; the *places* which it most generally attacks; and the character of its progress, which evidently shews that it is produced by a particular miasma, exhaled from putrid fermented animal and vegetable remains surrounding the habitations, in a season of the year regularly when the temperature of the atmosphere permits it; spreading itself more rapidly during the prevalence of certain winds; and finally developing itself almost always in places where the cleanliness of the houses, streets, harbours or bays is neglected, and where filth of all descriptions is allowed to putrefy in the neighbouring stagnant waters; such as the bays of Cadiz, Malaga, Alicant, and above all Carthagena, and lastly Gibraltar; to which the easterly winds blow the pestiferous exhalations, arising from the sea-weed deposited in abundance on the beach near the Moorish Walk, where it lies and rots, and from the accumulated filth constantly putrefying on the neutral ground; a large extent of which it is impossible to pass without disgust and nausea."

He thinks "the single febrile paroxysm, without remission; the apparent short amendment and the *sudden*

general debility which form the three stages of the Yellow Fever, are sufficiently marked to distinguish its singular type."—He thinks there is a difference in the miasmata which produce the Bilious Remittents, and those which occasion Yellow Fever, adding that, when the latter has prevailed, "the appearance of the Remittent is always a favourable sign, as indicating that the nature of the miasmata is already changed; in the same manner as is the appearance of the Intermittent in *Sicily* when the *Remittent* begins to yield;" and it is to this difference in the nature of these miasmata that he attributes the occurrence of the epidemic in some years, and its absence in others.

He adds that the Yellow Fever "*is not contagious.*" —"Many examples may be brought of persons who, touching and assisting the sick, have not been attacked: of which I myself am a witness and example."

Several extracts from the answers of *other* Medical Officers are before me, but I omit them from my persuasion that they must be unnecessary for the purpose of convincing those, whose minds are open to conviction, that the fever which has prevailed epidemically several times within the present century at Gibraltar originated from domestic and atmospherical causes, and was destitute of any contagious property. It may indeed by some be thought that too great a number of my pages has been al-

lotted to this purpose; but I trust that the singular importance of that fortress to the best interests of Great Britain, and an extreme anxiety to point out the true causes of the Epidemic Fever, which has occasioned so much mortality among its defenders and civil population, and to remove the dangerous delusions in regard to the supposed contagious property of that disorder, will upon consideration be deemed sufficient excuses for the length to which the present chapter has been extended.

CHAP. VII.

AFTER the facts and opinions which I have recently stated in regard to the epidemics in different parts of Spain, in the years 1800, 1803, and 1804, and at Gibraltar in the latter year, as well as in 1810, 1813, and 1814, and which abundantly warrant the conclusion stated at the end of the preceding chapter, that the disease *originated in the several places* in which it appeared, and was completely destitute of any contagious quality, it would, I think, be a kind of tautological supererogation, were I to enter upon a similar refutation of the allegations which have been advanced by the contagionists, in regard to the epidemics at Cadiz, and other Spanish cities, in 1810, and some of the subsequent years.—For, as they were confessedly the *same disease*, the decisive proofs already adduced are equally applicable and conclusive in regard to the latter, as to the former returns of it. I will, however, shortly notice some of the facts and observations which have been mentioned by Sir James Fellowes and others, respecting the more recent appearances of the disease at Cadiz, &c.

Sir James Fellowes, at page xix of his Introduction to

his Reports, says, " I cannot but regret that he (Dr. Bancroft) had not been with me in Spain during the prevalence of the disorder in that country, where he might have seen enough to induce him to draw very different conclusions, both from personal and local knowledge of the subject." Concerning the *sincerity* of this *regret*, I wish not to express any opinion. But, in regard to the consequences of my being in Spain at the times mentioned, I have no hesitation in saying, that, in my judgment, they would have been very different from those which he appears to think probable. It may be presumed, that, with his predispositions towards contagion, no discoverable fact or circumstance indicating the presence of it, was *unseen* by him in Spain, or *unnoticed* by him in writing his volume; and I can conscientiously declare, after maturely considering all that he has stated, that, if I *had seen the whole, it* would not have been " enough to induce me to draw" any *conclusions similar* to those which he has adopted. I firmly believe, however, that, with *my predispositions*, I should have *seen* many facts and circumstances which were either overlooked, or rejected by him, and which, to my understanding, would have appeared to warrant inferences, the very reverse of those which he has published respecting the epidemics under consideration. In fact, Sir James Fellowes came to England, in 1806, so prepossessed on the side of contagion by the stories he had *heard* in Spain and at Gib-

raltar (not by any thing which he had *seen* of the disease) that he was then absolutely determined to write and publish on the subject; and, to *my knowledge*, he continued in this determination when he embarked for Cadiz in the beginning of 1810; and it may therefore be reasonably presumed (as will be made evident), that his eyes and attention in Spain were not so much directed to an *impartial* acquisition of truth, as to the discovery of *incidents* and arguments favourable to that side of the question, on which he had for several years been resolved, and preparing to *contend*. And accordingly we find him everywhere, although without any thing like reasonable proof, *assuming* the *presence* and *operation* of contagion, and explaining every occurrence concerning the fever in question, by a reference to that cause—notwithstanding that he has not, in a single instance, been able to *account* for its *introduction*, or make its *existence probable*. But, persisting in *this assumption* and in *this* mode of explanation, his readers, if not well acquainted with the subject, are liable to be misled, and ultimately to *forget* that *nothing* of all which they are taught to believe, has been *proved;* and that every thing rests upon gratuitous and *confident assertions*, and *unwarranted suppositions*. Thus, in regard to the epidemic at Cadiz in 1810, he tells us at page 227, that Dr. Mellado, Physician to the Board of Health, published an account of this fever, entitled "Historia de la Epidemia padecida en Cadiz el año de

1810," adding, " the *ingenious* author has *proved* its contagious nature by numerous well-attested facts; and he has *endeavoured* to *shew*, from official documents, that it did not *originate* in Cadiz. But he candidly acknowledges the *difficulty* of *tracing it to its source*, and of ascertaining who were the first individuals attacked;" an acknowledgment which at once demonstrates that he *could not have proved* any of the things here mentioned; and as Sir James Fellowes has abstained from giving the slightest *intimation* respecting the *place* whence, or the *vehicle* by which, any contagion was *even suspected* to have been introduced, this omission may be considered as an admission that the " endeavour" of Dr. Mellado had completely failed.* Sir James Fellowes has been

* Dr. Mellado's endeavours to prove that this epidemic did not originate at Cadiz, seem to have afforded but little satisfaction even *there*, since it is stated by Sir James Fellowes himself, at page 228, that " there appeared to be a difference of opinion among the practitioners of Cadiz in regard to the origin of the disorder; that some persons attributed it to the passage of a comet, *the increased temperature*, and other atmospheric causes, all of which were so enveloped in obscurity, that they afforded *him* no satisfactory explanation." Had Dr. Mellado been able to point out any means or channel to account for the supposed introduction of contagion at Cadiz in this year, which were in any degree probable, or capable of bearing even a slight examination, it may be presumed, from the laborious, though abortive, efforts of Sir James Fellowes to uphold the story regarding the Dolphin, that he would not have withheld all information concerning these means, or this channel, and contented himself with only vague and general expressions, against which no counter proof can be directed.

almost as reserved in regard to " the *numerous well-attested facts* by which the *contagious* nature of the disease was proved:" at least, the only ones mentioned by him, and probably the only ones which he thought capable of *bearing the light,* are altogether *equivocal.* He tells us, page 216, that this same physician to the Board of Health, on the 11th of September (1810), " first discovered some persons labouring under symptoms of fever, similar to what he had observed in 1800 and 1804; and it appeared from the reports which he made to the Junta, that the disorder was *contagious,* having *spread gradually* on the quarter of the town where it *broke out, four* out of five of the first family attacked having died; and particularly as the only individual who survived was not ill, having passed through the disease in the epidemic of 1800." We are here told *what,* in the judgment of the Spanish physicians and of Sir James Fellowes, was deemed *a sufficient proof* that the disease was contagious, viz. its attacking *four persons out of five* in the family in which it first broke out, (which, *as usual,* was in the *low* filthy Barrio de Santa Maria), a fact quite as likely to result from the action of miasmata, as from that of contagion; and in regard to the *escape* of the *fifth* person in this family, which is considered as a farther proof of the *presence of contagion,* I have, in my second chapter, pointed out other, and much better, ways of accounting for it. If there was any other evidence of *this quality* in

the disease, Sir James Fellowes has not divulged it, so far at least as I can discover. He tells us, indeed, that the disease was confined within the walls of Cadiz; that, although within these limits it occasioned the deaths of between two and three thousand persons, yet "not one case of it was reported as having occurred amongst the troops at the Isla de Leon," and that only a small portion of those who were quartered in the barracks within the Landgate (and close to the most unwholesome part of the Barrio de Santa Maria) were attacked,* of whom "twenty-five" died, "including two officers, and a clerk in the Commissariat Department." The exemption of the troops at the Isla de Leon is uniformly ascribed by Sir James Fellowes to "the *precautionary measures*" which, upon his recommendation, were taken "to cut off *as much as possible* the communication with Cadiz," by "prohibiting any officer or soldier from going into Cadiz, *except* on

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* On this occasion, as upon all others, the morbid effects of the miasmata were manifested almost exclusively upon individuals possessing that accumulation of excitability, which is produced in cool or temperate climates, with the habit of generating a proportionate quantity of animal heat. Sir James Fellowes says, page 219, "it was a well-known fact, that the prevailing disease was principally confined at this period to *strangers*, and to persons from *northern* climates, and even to the Spaniards, who had fled to Cadiz from the *northern provinces of Spain*, and to whom the disorder was most fatal." The causes and reasons of these effects have been already repeatedly explained by me, and they are absolutely incompatible with the agency of any thing like contagion.

the *most urgent* public service:" but there is good reason to conclude that they were beneficial, only by hindering* *some* persons from frequenting the spots in which noxious miasmata were prevalent, which might in them have produced the fever. Those, however, in whom it was thus produced, would not have been capable of communicating it to others; because, wherever the Ardent or Yellow Fever has prevailed most violently, as at New York, Philadelphia, Charleston, and even in Grenada, during 1793,

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* It must not be supposed that the communication between Cadiz and the Isla de Leon was *really intercepted*, however this may seem to be *implied* by the statements of Sir James Fellowes. Staff-Surgeon Doughty, who served there at that time, gives the following information upon this subject at page 196 of his Observations, &c. "In consequence of the blockade by the French army, it was necessary to convey all supplies for the sustenance of the combined forces stationed at the Isla, amounting to more than fifteen thousand men, from Cadiz; also for the inhabitants, consisting of near ten thousand people. *This required a daily and constant communication between the two places.* Not only the *muleteers and others* employed in the transport of provisions, &c. but *soldiers and inhabitants were constantly passing and repassing. No cordon* of troops was stationed *to cut off* the communication, and for a very obvious reason, because such an interruption of intercourse at that time would have led to the starvation of the Isla; and its being allowed of necessity to continue, *was attended with no bad consequences to this latter place*, as to the *dissemination* of the principles of *contagion*." We perceive from this statement how effectually Sir James Fellowes succeeded "by vigilance and unremitting attention," in cutting off all intercourse between the Isla and Cadiz; and likewise how seriously those suffered who disregarded his "precautionary measures."

it has always been found impossible to communicate the disorder to any person living but a single mile from either of those towns, who had not actually visited the town, and there imbibed the febrific miasms (see page 194).—A fact so notorious, and so convincingly demonstrating, that the non-communication of the disease at places in which it does not *originate from local causes*, cannot justly be ascribed to any of the *precautions* constantly extolled for this purpose by Sir James Fellowes and other contagionists, could not, I think, have been overlooked by any, who were capable of perceiving and appreciating evidence unfavourable to the belief in contagion.

But while Sir James Fellowes is unable to adduce any thing deserving the name of proof, to support his assertion of the presence and operation of contagion at Cadiz, Mr. Doughty, Surgeon to the Forces, who was there on duty at the same time, has, in his "Observations," &c. furnished sufficient testimony to shew that the epidemic of that place (in 1810) was destitute of any contagious property. Between pages 91 and 146 of his work, he states minutely the appearances and results of eight dissections of the bodies of soldiers who had died of the fever in question, with their names, and the symptoms of each case, which were those of the most aggravated forms of the Ardent Fever, accompanied with the black vomit, in the stomachs of whom portions of coffee-coloured matters,

and marks of inflammation, were found. These dissections, however (as those which have been many hundred times made in the West Indies, at Philadelphia, and New York, &c. have invariably done), proved completely harmless not only to Mr. Doughty,* but to several other persons who assisted in performing them.

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* It appears from Mr. Doughty's statement, page viii of the Preface, that those examinations after death gave great dissatisfaction to, and were at length hindered by, Sir James Fellowes, who was then Senior Medical Officer. "A difference of opinion in regard to the *qualities* of that fever," says the former, "*must have* been the occasion of *his* placing obstacles to the inquiry by dissection, on a request that I made for the purpose, by instituting arrangements, which rendered it afterwards *not possible* for me to open a dead body in the *British* military hospitals"! I hope that Sir James will be able to assign, for this extraordinary hinderance, some reason which may be compatible with the liberal investigation of truth, some better motive than the desire to prevent the decisive conclusions against contagion, which *must* be drawn from *innocuous* dissections. As such dissections have long been not merely countenanced, or encouraged, but *enjoined*, and occasionally *required*, by the Army Medical Board, he could not, as head of the medical department at Cadiz, have had any instructions or authority from *his superiors* at home, for prohibiting or hindering them; and it must be presumed that, personally, he does not disapprove of them for scientific purposes (as in Mr. Doughty's case), because he has himself had recourse to them (as he says, page 350), and has acknowledged to have derived *much instruction* from them, viz. "The dissection of those who died discovered to *us* a series of morbid appearances of which *we had no suspicion;* and they enabled *us* to *account* for *many* of the phenomena of the complaint, and to form a *more rational* plan of *treatment* than *that* which *we had at first adopted.*" The dissec-

A farther proof that the epidemic in question was not contagious, is offered by Mr. Doughty at page 192, in the following words: "In the season of 1810, to which my own particular observations relate, there were doing duty within the walls of Cadiz, and at the Aguada (Hospital), two physicians to the Forces, two surgeons to the Forces, one deputy-purveyor, an apothecary, five or six clerks, and twelve or fourteen regimental and general hospital-assistants, not one of whom had ever been in the West Indies (for I do not include myself, Sir James Fellowes, or Mr. Bowles the Purveyor, who had been there), or where this fever had before prevailed; yet *not one of them was attacked,* although several attended patients labouring under the disease, as well as assisted me in the dissections I have given:" and in addition to this, he likewise mentions, at page 196, that "*not one* of the *orderlies* in the hospicio (hospital), where several cases of this fever were admitted, *nor one* of those in the Aguada, where the greater number of the British troops affected

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tions here mentioned were instituted by Sir James Fellowes at Colchester, in cases of the Walcheren fever, which had proved fatal; and as they did not there "excite in the minds of the soldiery the strongest prejudices against their medical attendants, and a belief that they were *prematurely consigned to death* for the purposes of dissection, and being anatomized," the apprehensions which Sir James publicly avowed, lest dissections by Mr. Doughty should excite the prejudices and ideas just mentioned in the minds of the soldiery at Cadiz, appear to have been at least groundless.

with the disease were placed and treated, and where many of them died, *fell sick* of the same order of fever." *

As it appears that in 1810, as in 1800, 1804, and 1813, the epidemic fever first became manifest in the notorious Barrio of Santa Maria, it may not be improper to quote from Mr. Doughty's work some observations upon *this* quarter of Cadiz, and likewise upon the weather during the autumn of 1810. Having noticed at page 179 " the crowded and filthy state of this Barrio, and the impropriety of " *shutting our eyes* to so obvious a focus of disease," he adds in the next page, " from the time I took up my residence in the St. Elena barracks, situated in the above quarter, and not more than fifty yards from the back of its several lanes and buildings, it was my custom, in my daily visits to the Hospicio and the San Juan de Dios hospitals, to pass through many of the narrow streets of this quarter, and I particularly observed the striking

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* It is, I think, seriously to be regretted that the Chief Medical Officer of the British Forces at Cadiz should have *closed his eyes* against proofs so clear as those recorded by Mr. Doughty, occurring, too, *immediately before him*. Had he seen facts, and *reasoned* upon them with impartiality, he might have given different and more *faithful* and valuable accounts and explanations of what he witnessed; and he might also have rendered an essential service to humanity, by contributing to remove, or at least to diminish, the *erroneous prejudices* of the Spanish physicians, instead of *confirming them to the utmost of his power*.

difference as to cleanliness in this part of Cadiz, where the poor classes dwell, to what was found in the open streets facing the (Atlantic) sea, where the mercantile and higher orders of society reside. In the latter there was nothing offensive to either sense, whilst in the former the olfactory nerves were assailed with the most noxious exhalations, and the eyes disgusted with every sort of filthy and excrementitious matters, thrown indiscriminately into the streets. Fish, bones, rotten vegetables, and rotten matters of every description, mixed together with the contents of the receptacles of the night, formed the covering of most of those extremely crowded and ill-ventilated streets." "Will any one tell me (he asks) that if Cadiz was built on a rock of adamant, and its streets to be covered from time to time with matters of this description, on which the *solar* influence might operate a degree of heat equal to 95° or 100°, often experienced out of the shade in that city in the summer months, there would not be just grounds to expect the generation of fever?"——Again, at page 180, he says, "from what I have stated it will be seen, that I consider the fever which has so destructively prevailed at Cadiz, to have had its source *within the walls of that town;*" and "I am equally disposed to give full credence to the excellent and luminous statement of Mr. Amiel on the cause of the epidemic in *Gibraltar*."——— ——"In 1810, to my knowledge, there was a continued drought and heat for

three months, and the rains set in about the autumnal equinox; then and not till then did fever of the order we are speaking of develope itself, both at Cadiz and Gibraltar."

In the same year of 1810, the Yellow Fever prevailed at Carthagena, and as we are told by Dr. Burnett, caused the deaths of three thousand persons, being one fourth of the inhabitants of that city, in the space of six or eight weeks. We are told by Dr. Burnet (page 274), that Dr. Riseuno, Physician of the Spanish Royal Hospital there, " positively asserts, that the fever was brought from Cadiz and Gibraltar in 1810:" while Dr. Pym as positively asserted it to have been carried from Carthagena to Gibraltar. This last assertion has already been proved to be erroneous (see p. 359, &c.); and the former must be so, because the Ardent Yellow Fever, or Bulam, did not appear at Gibraltar (except in the transports), until near the middle of October, a month after the disease had been prevalent in Carthagena; and this observation is also applicable to Cadiz, which continued healthy till the middle of September, " before which time many deaths had occurred at Carthagena;" and these contradictory assertions serve only to manifest the readiness with which the contagionists, who believe that an Epidemic Yellow Fever must always proceed from imported contagion, hazard tales to account for it. That it originated in this and in

succeeding years at Carthagena from local and atmospherical causes is a fact, which will presently be made manifest.

In the following year (1811), the Yellow Fever again became prevalent at Carthagena, and many persons were its victims. How it was produced this year none of the *Spanish* physicians, as Dr. Riseuno declared, would venture to explain; but Mr. Vance, Surgeon to the *British* forces, who was sent by Dr. Pym's advicē to that city to *examine* and *inquire*, says in his Report (published by Dr. Pym at page 60), that he had "good reason to *suppose* that it *might* have remained *dormant* during the winter, till roused into action again by the summer's heat." Upon this occasion Mr. Vance cannot claim even the merit of invention, unless he will deprive Sir James Fellowes of that of accounting in the same way for the fever at Malaga in 1804.*—(See p. 318.)

Dr. Burnett says (page 218), "In the month of February, 1811, a British garrison had arrived at Carthagena, which during the prevalence of the epidemic, I un-

* In this report, Mr. Vance mentions a fact which affords the means of accounting much more rationally for the recurrence of the fever in this year, which is, that "the winds *for several months* have blown from the Levant, and the thermometer has generally ranged from 82° to 86° of Fahrenheit's scale."

derstood had suffered much." Having obtained permission to visit Carthagena, to ascertain facts respecting this disease, Dr. Burnett, at page 219 & seq. takes a view of the medical topography of the place, from which it appears that the sources of marsh effluvia *adjoining* that city are more extensive, and likely to be more productive, than at almost any other in Spain; that in August and September the thermometer of Fahrenheit commonly stood at about 85°; and that the troops which had been quartered *nearest* to these sources, were those who suffered *earliest* and *most severely;* while a part of the garrison encamped in a situation remote from these sources, i. e. "under Mount St. Julian, although they ascended it twice in the day to work, continued exempt, notwithstanding they suffered considerable fatigue, and were necessarily much exposed to a scorching sun. *Not a man* there was attacked with fever during the continuance of the epidemic."—"In the Castle of Attaleya" (on another neighbouring hill) "the troops were remarkably healthy, only two or three very slight cases occurring."—"Men on board the Leyden to the amount of upwards of forty, who had been on shore, were taken ill; but in no instance was the disease communicated to any of the other soldiers on board, or to any of the ship's company of the Leyden, not a case appearing among them."

"In 1812" (says Dr. Burnett, at page 274), "this ill-

fated city was again afflicted; and it was *officially* stated in the Gibraltar Chronicle (27th February, 1813), on the authority of the Board of Health of Carthagena, that it had been characterized by its usual symptoms, and that *two thirds* of those taken ill *died.*" Dr. Riseuno states that the fever was this year brought from Mazaron."—Dr. Burnett however thinks that "its progress among the troops quartered at this time in Carthagena, leaves no room to doubt as to the cause of the disease. The *whole* of the sickness of the 67th regiment took place *at Galleras,* which has *uniformly* been remarked as the *most unhealthy* situation about Carthagena. Every evening during the summer the miasma is carried by the land-wind from the marsh directly on Galleras; to which are added in its course the exhalations from the *mast ponds,* and that which will be extricated by partial rains, succeeded by a hot sun, from the general scite of the arsenal. So offensive were these exhalations, that some of the officers declared to me, that they could scarcely respire out of doors after sun-set. In the morning the castle was enveloped by a dense vapour, which did not disperse till the sun attained some height; the effluvia from these sources was even perceptible at the entrance of the harbour. St. Julian being on the opposite side of the harbour, is entirely out of the line of the marsh air, and *there* the men, though much exposed to causes which often of themselves produce *sporadic* cases of fever, continued perfectly healthy.

Attaleya, which is also out of the line of the marsh air, remained very healthy during the whole time. In the Leyden lying in the bay, nearly fifty of the 67th regiment were attacked, and, except one, they all recovered on board; only such men as by going on shore, were exposed to the influence of the marsh air, were taken ill, and they did not communicate the disease to a single person on board.* In the fleet, *in no instance* have the Surgeons considered and said that the disease was contagious."—"With such powerful evidence of the non-contagious nature of the disease before me, I cannot but conceive that it is neither propagated by persons, nor goods, but is the genuine product of miasmata. It is useless for one to say in support of its contagious qualities, that whole families have been attacked; this only proves that whole families have been exposed to the same remote cause. In

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* The following is an extract from the communication of Mr. Rae, Surgeon of His Majesty's ship Leyden, on this subject, printed at page 227 of Dr. Burnett's volume, viz.

"I do not think it (the fever), contagious: otherwise it is hardly possible that the men constantly on board would not have suffered, while unavoidably exposed to those who brought it from the shore. I considered it merely as a violent *remittent*, and the remote cause marsh miasma, arising from an extensive marsh with which the city is almost surrounded; and where not only a putrefactive process of vegetable, but also of animal matter is constantly going on. During a period of nearly four months, which we lay at this place, our *ship's* company were always in a tolerable state of health, and no deaths occurred except such as were almost to be expected by the course of nature."

Zealand nearly whole regiments were taken ill with the fever, yet no one was hardy enough to assert that it was contagious."

Mr. J. Proudfoot, Assistant Surgeon to the 27th regiment of foot, in a communication with the perusal of which I was favoured by Sir James M'Grigor, respecting the Yellow Fever at Carthagena in the autumn of 1812, gives the following description of that place, viz. "Carthagena is situated on the sea-shore, having a high hill on each side, which after *diverging* for a certain way, then *incline* towards each other, thus forming a kind of irregular circle, which surrounds a low damp marshy plain, nearly twenty miles in diameter." "The effluvia arising from the stagnant water, and the putrefaction of vegetable matter obviously taints the constitution of the surrounding atmosphere; as the stench of the marsh can be distinctly perceived at a very considerable distance. This readily accounts for the prevalence of Intermittent Fever at Carthagena, and unquestionably contributes in no inconsiderable share to produce the disease in question; from the very squalid sickly appearance of the natives all over the marsh."

"The death of Major-General Ross, which happened on or about the 26th of September, 1812, was perhaps what first excited general alarm; and accordingly the

Spanish Physicians were consulted," &c. and after going through the British Hospital with Dr. Wright, were " of opinion that several men in the hospital had what they called the *Epidemia,* or Yellow Fever."

Mr. Proudfoot then proceeds to state his reasons for believing the fever in question to have been produced by local and atmospherical causes, and that it is void of contagion ; he adds, however, that, while this point is contested, " people were unfortunately labouring under all the inconveniences of a long and strict *quarantine,* obviously as unnecessary as in cases of Pneumonia and Hepatitis."

He also describes the symptoms of the disease, and the appearances upon dissection after death, which in general are similar to those described by most authors who have written concerning this fever.

Soon after the death of Dr. Wright, Staff-Surgeon J. A. Campbell arrived at Carthagena and officiated as Principal Medical Officer to the British Forces there. And I shall conclude my observations on the fever at this unfortunate city, by some extracts from his Report to the Army Medical Board, which was kindly put into my hands by Sir James M'Grigor. Mr. Campbell says,

"Being ordered to Carthagena in the latter end of 1812, where an epidemic fever had for some time prevailed, which had committed considerable ravages amongst the natives as well as our troops, I found on my arrival that as many of the latter, as could be spared from the duties of the garrison, were *embarked* and detained in strict *quarantine* outside of the harbour. The women and children were crowded together in one of the forts situated on the western side of the harbour, *where* the disease *first* made its appearance, and where they daily fell victims to it. The sick soldiers were scattered among temporary hospitals and huts along the beach, *outside* the town walls; there being no convenient building for their reception: and no communication with the town or inhabitants was permitted." "In this state of things, I had a large Spanish hulk moored in the harbour, on board of which (although ill adapted to the purpose) I received all the worst cases. The women and children were encamped near me on the beach, and separated as much as possible from each other. Up to this period (6th October), I had no opportunity of forming any exact opinion, as to the *nature* of the disease."

He then proceeds to describe the symptoms, and afterwards the treatment of the disease: adding, "from the sixth of October to the 21st of December there were admitted into the hospital, including those that had *re-*

lapsed after having been discharged, 1278; of these 136 died, including seven who died on the first day of my arrival; and a number who were subsequently lost from dysentery and *visceral obstructions.*"

"The Spanish Physicians whose hospitals I visited, termed this disease Fiebre Amarilla, or Yellow Fever;" and always considered it as imported from Africa; they were induced to adopt this opinion, from its differing so much from the usual autumnal fever they were in the habit of seeing, both in regard to the rapidity with which it ran its course, and the particularly distressing affection of the head and stomach; the *continued* form of the disease; and the great yellowness of the skin. I am however willing to attribute its origin to *local* causes; a marsh of considerable extent, which is *partly dry* during the summer season, is *close* to and *nearly surrounds*, Carthagena; the animals that die, and all the rubbish and filth of the town, of which there is no small quantity, have from time to time, been thrown on different parts of it; a great number of *mast* ponds, or stagnant water tanks, are also to be found here, for the purpose of preparing timber for building ships: the heat of the sun, acting on these animal and vegetable bodies spread over so moist a surface, would naturally be supposed to forward the process of putrefaction, and consequently a vitiated state of the atmosphere would be produced, particularly at Carthagena; its situa-

tion being very low and confined, and the summer of 1812 being unusually and remarkably *close* and *hot*."

"In support of this opinion I must state that an *easterly feeble breeze* generally prevailed, for *weeks together*, during the months of September and October; which in its passage, swept a great portion of the marsh I have mentioned, previously to its reaching the quarter of the town and fort, where the disease was most prevalent; and it is worthy of remark, that the men who were at work and encamped on the south-eastern point of land forming the harbour, and consequently removed from the influence of this impregnated breeze, *did not at all suffer*."—"I am also of opinion that the disease was *not contagious;* as neither orderlies nor nurses were more liable to its attack than persons not attending hospitals, where a due regard to cleanliness and ventilation was observed and enforced."

"With regard to the opinion that individuals once undergoing the disease, are not liable to a second visitation, I have to observe, that Mr. Reid, the Assistant Deputy Quarter-master General at the station, Capt. Morris of the 67th regiment, and an officer of Watteville's regiment *died of second attacks*."

As it might be expected that, before I quit the subject of Ardent Yellow Fever *in Spain*, I should notice

the fourth and last Report made by Sir James Fellowes concerning the epidemic at Cadiz in 1813, I will therefore briefly comply with that expectation, notwithstanding my reluctance, after the decisive contrary evidence which I have already adduced, to descend to the consideration of supposed proofs of contagion, which are all either equivocal, or nugatory and absurd. I call those equivocal which admit of being at least as well explained by a reference to fever-exciting miasmata, as by a reference to contagion; and such are almost all those *

* Of the numerous equivocal proofs which might be cited, one may suffice as an example: at page 275, after having mentioned the removal of most of the British troops in 1813 from Cadiz to the *heights* near the Isla de Leon, Sir J. Fellowes adds, that they "were *narrowly* watched; for it was to be expected that *contagion was lurking amongst them;* and *this was actually the case;* for on the 18th of September, two days after they had left Cadiz, five of the Germans were taken ill," &c. Here we have not merely a *fallacious* explanation, but a positive, yet most unwarranted assertion, that *contagion* had produced the illness in question. It happened that three only of these men were attacked with fever, and as they had *in Cadiz* been *exposed*, like others affected by the epidemic, to the cause of it, Sir J. Fellowes could not possibly know that miasmata rather than contagion, had not (as was undoubtedly the case) produced the fevers in question, and he might have found good reason to believe the *contrary* of what he asserted, by attending to the fact mentioned immediately after at his page 277, where he says that, in this new situation; "after the 28th there were few admissions, and the *type* of the disorder *seemed to be entirely changed.* The ground of the encampment was dry; the sea-breezes rendered the situation *cool*, and refreshed the men," &c. Such a *change* as this would naturally be produced by *these causes*, in a fever arising from *miasmata* or *local* causes; but certainly not in one produced by the supposed "pestilential" contagion.

which are produced by Sir James.—Of the absurd proofs I will only mention one, which is stated by him at page 294, concerning two English labourers belonging to the Store-keeper-general's department, who died of the Yellow Fever in the Regimental Hospital of the German battalion, after having, as was reported, "been in the habit of looking through the gratings" into "a space which had been railed off (at one extremity of a church), for the reception of dead bodies brought thither from the different parts of the town during the day or night previous to their interment." "As the two men above-mentioned," says he, "had never been in the *barracks*, and had not had, as far as I was able to ascertain, any communication* with *the sick*, there *could be no doubt* but that their illness *must have originated from this source;* and it became the *more* probable, as their disorder was of *short*† duration, and as it was attended with all those

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* This is a fair instance and example of the manner in which Sir James Fellowes *observes and reasons;* he states, as the *foundation* of his belief, that the individuals in question had caught the fever by *looking* through the *gratings*, &c.; that they had not, as far as he could ascertain, "*had any communication with the sick;*" thus *rejecting all consideration* of the *true* and *only cause of the disease*, (an exposure to those *miasmata* which *alone* had produced it *in others*), and assuming, as an *indubitable* fact, that it could only be the result of *personal* contagion, proceeding either from the *sick* or from the *dead*.

† Not so short as many cases of fever which *indisputably* have arisen from fever-exciting miasmata; as at the island of *Edam* (see page 131), and many other places.

marked signs of *malignancy* which *usually* accompany diseases arising from *contagion*, and which had been observed in so many of the fatal cases of the prevailing malady in Cadiz."—To minds *strongly* and *suitably* predisposed, " trifles light as air" afford irresistible evidence: but after the thousands of well-known proofs, that the bodies of persons who had died of Yellow Fever were incapable, either before, or after death, or even when *dissected*, of causing the disease in any one, I conceive that, without such predisposition, no person would assume, as a *fact established beyond all doubt*, that *looking* through the gratings before-mentioned "*must*" have caused the illness and deaths of those men. In regard to the latter part of the above quotation I would ask if Sir James meant seriously to inform us that all kinds of contagions are "*usually* accompanied with *marked* signs of *malignancy*," and that *mild* cases of small-pox, scarlatina, measles, &c. *seldom* occur? Or did the writer intend to apply this *general* observation *only to his imaginary contagion* of Yellow Fever? in which, however, *mild* cases often occurred, particularly in the late epidemic at Gibraltar, where some of the Medical Officers declared, in their answers to Mr. Fraser, that they had not lost one patient in more than thirty, and even forty.

After this example, I trust that I shall not be expected to continue the serious examination of relations which must

be mis-named, if called proofs; and which doubtless would have carried a very different appearance, if the particulars of each case had been all *fairly* stated.* The only statement

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* The *want of fairness* in many of the statements, made to induce a belief that the Ardent or Bulam Fever possesses a contagious quality, has been to me a frequent cause of regret. But I will here only notice one instance of it, in the work lately published by Sir James Fellowes, between pages 254 and 298, respecting the present epidemic. After admitting (what it seems to me was an obvious cause of it) that the *heat* had been excessive throughout the month of August, he draws the attention of his readers from that circumstance, by observing that "the state of the public health continued as good as it had ever been at that season." This, however, is an usual occurrence, the morbid effects of great heat rarely manifesting themselves until a considerable time has elapsed. He then notices the commencement and progress of his "*pestilential* fever" in the month of September, *assuming* that it was produced by *contagion*, although without even the smallest probability, and without any attempt to explain how, or whence, the contagion had been introduced; and he afterwards descants on the supposed good effects produced by separating the sick, and burying the dead at a proper distance from the camp, after *burning* their *bed*-clothes, &c. precautions to which he ascribes the non-communication of the disease, though we have a thousand instances in which it has been found, that neither living bodies under the fever, nor those dead of it, nor their clothing, or bedding, were capable of reproducing the disorder. In no instance has Sir James Fellowes mentioned so much as *one* person attacked by this fever, who had *not previously been exposed to the local miasmata at Cadiz;* but I am sorry to be under the necessity of thinking, that it was intended his readers should conclude, that such attacks had occurred in individuals *not* so exposed; for, at page 290, after mentioning the removal of the Regency and the Cortes, with upwards of twenty thousand other persons" from Cadiz, he adds, "the Cortes assembled at the Isla

in regard to the cause of the Yellow Fever in this report which deserves notice, is that at page 255, in which the tem-

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de Leon, and many respectable families crossed the Bay to Port St. Mary's, where the disorder had not yet appeared, but which afterwards prevailed *there*, as it did in other places, whither the emigrants from Cadiz had retired." In using this vague ambiguous language, the writer must have known that, on a multitude of occasions, persons going from places where the fever in question was prevalent, had (in consequence of their previous exposure at those places to the cause of the fever) been attacked in other healthy situations, without communicating the disease to any one; or without its extending to any person who had not come from, and been in like manner exposed to, the same original source of the disease; and there is the strongest reason to believe, that this was true in regard to the emigrants from Cadiz on the present occasion; for otherwise he certainly would not have omitted to state in distinct terms a fact of so much importance, and so unexampled—since he is not backward in using such terms to support the most unwarranted *suppositions;* e. g. he says, p. 290, "that within a few days after M. Mexia's return to Cadiz, he went to visit a friend of his, another deputy of the Cortes, who was at that time in the *last* stage of the disorder, and embraced him but a few moments before he expired: *from this source Mexia caught the infection;* he was himself soon after attacked with the fever, and died on the *fifth* day."—This *pathetic* story is well suited to vulgar apprehension; but it must have been absolutely *impossible* for the writer to know, that the deputy Mexia *caught any infection* from the other deputy; and there is the strongest ground to presume that *he did not*, and that *in both* the fever was produced by *local miasmata*, which produced it in others, and to which *he must have been exposed during his stay in Cadiz.* It is by such means that Sir J. Fellowes endeavours to induce a belief, that the fever in question is contagious; in all his accounts of it, this quality is either *asserted*, or *presumed*, to be actively present, but without the least proof or probability. He invariably ascribes every occurrence of

perature during the month of August at Cadiz is described as having "varied from the 81° to the 91° of Fahrenheit," and that "the highest point to which the mercury rose was 95°, with an * *easterly wind* and a cloudy atmosphere." Now, since every recurrence of the Yellow Fever as an epidemic has been preceded by a continuance, during several *weeks*, of *unusually hot weather*, it must appear at least very extraordinary to those whose minds are not entirely warped by prejudice, if the disorder be produced by *imported* contagion, that the importation should be *always* made *so opportunely*, and in the summer and autumn of *those years only* in which the summer's heat has been *excessive*.

It seems likewise extraordinary that contagion should always be so *opportunely* imported, as to reach places *north* of the *tropic* of Cancer only at *that season* of the

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fever to *this* cause, overlooking *every other*; and so constantly and confidently explains every thing by a reference to contagion, that his readers, taken by surprise, are apt to forget that nothing like proof has been adduced, and to conclude that these assumptions and explanations are really founded in truth.

* "It has been shewn from experience," says Sir James Fellowes, page 14, "that the Levant wind has always had a considerable influence over the Andalusian fever; the disorder has usually made its appearance in Spain *after* the prevalence of *easterly* winds." Does not this testify strongly against contagion?

year in which miasmatic fevers are *uniformly* * *most prevalent;* and that *between the tropics,* its effects should only become prevalent at times and places, in which great numbers of strangers are brought by the occasions of war; and that it should *then,* contrary to every other known species of contagion, *confine* its ravages almost exclusively to those who are comparatively *strangers,* leaving the native and seasoned inhabitants harmless, or only exposed to their ordinary remittents or intermittents.

Little has indeed been said of any importation of contagion into Cadiz in 1813, although Sir James Fellowes mentions that *one* contagionist had no doubt of its having been imported, an opinion in which all other contagionists will naturally agree; yet there is no one, so far as I can discover, who has ventured *even to hint* at any particular place whence, or any vessel by which, the

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* Dr. Currie of Philadelphia, treating of the Malignant Yellow Fever, says, "to infer that the Yellow Fever is only a variety or higher grade of the indigenous intermittent" (he ought to have added, *remittent*) "fever of this country, because it generally occurs, progresses, and terminates at the same time of the year as the intermittent, is confounding or mistaking *coincidents* for causes." This observation however is by no means correct.—No person has supposed that one of these fevers, or co-incidents, was a *cause* of the other; but, *being co-incidents,* it was very natural to presume that they result from *one common cause:* especially as, at pages 406—408 of my Essay, after a *general* review of the history of Yellow Fever, I have ascertained and stated the fact of its exhibiting *all the characteristic marks,* which confessedly belong to fevers from miasmata.

contagion was *suspected* to have been brought in that year. There was, in truth, no other place, either in Europe or in America, in which the Yellow Fever prevailed in 1813, Gibraltar excepted: but as Sir Joseph Gilpin had, at a very early date, derived the epidemic at the *latter* place *from Cadiz*, even before it had appeared there (see page 373), it would have been too much for ordinary credulity to represent its importation into Cadiz as having been made from Gibraltar; although a similar ingenuity was exercised respecting this garrison and Carthagena in 1810.—(See page 415.)

It appears, moreover, from the Report of Sir James Fellowes, page 256, that the epidemic of 1813 commenced "in the well known Barrio of Santa Maria;" and, considering that not only at Cadiz, but likewise at Seville, Malaga, Gibraltar, New York, Philadelphia, &c. the Yellow Fever has *always* made its *first* appearance *in the same description* of low, dirty, narrow, ill-ventilated streets and lanes, it behoves those who ascribe it to an imported contagion, to explain how and wherefore this contagion, when imported, happens *uniformly* to be *deposited* and *left* to operate *first in these particular situations?*

Were any proofs required, after the great number already adduced, in order to demonstrate that the epidemic

of 1813 in Cadiz was produced by local causes, the following fact, related by Sir James Fellowes at page 296, might alone suffice to establish the point. After mentioning that "from the number of men belonging to the corps left behind *in Cadiz*, who were *taken ill*, it had become necessary to replace them by fresh troops from the Isla de Leon," and that "these, on their arrival, were in perfect health," he states that the latter "were put into *clean tents* on the *glacis*, and they never went into town except to take the duties of guard mounting; but," adds he, "there were *few* of those who were fairly exposed that were not attacked by the disorder." Had the fever been contagious, it is to be presumed that the various "*precautionary measures*," the efficacy of which is so often praised, could not have failed, especially when employed with "vigilance and unremitting attention" by a zealous contagionist, of preserving the *British* troops *in Cadiz* from the epidemic with equal certainty, as the *Spanish* dragoons of Maria Louisa were said to have been preserved in 1800, while quartered in the midst of places *supposed* to be infected (see page 327): but they confessedly were most unsuccessful, because such measures and active talents must be of little avail against the effects of exposure to local causes. *

* In a note to page 195, Sir James Fellowes remarks, that, "whilst the pestilential fever prevailed at Cadiz in 1810 and 1813,

It had been my intention, upon bringing my observations concerning the Yellow or Ardent Fever *in Spain* to

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there was no appearance of it at Malaga."—" I am totally at a loss," adds the ingenious writer, " how to account for this exemption, unless by reference to the *precautionary measures* adopted." It is but natural that those who have obtained promotion and rewards from the benefits supposed to have resulted from *their* precautionary measures, should be the last to acknowledge a doubt of their utility. But if Sir James had not been strongly inclined to overlook local causes and facts unsuitable to his purpose, he would probably not have so soon forgotten those which he had himself stated, from which he might have obtained a different solution of his doubts. Thus, at page 158, after mentioning that " even with the least torrent of the Guadal Medina, the streets (of Malaga) are *overflown*, which, upon the waters retiring, are left full of mud and clay," he quotes from Dr. Mendoza's " History of the Epidemics at Malaga of 1803—4," that " *since that period* measures have been taken to *prevent* the water entering the lower part of the town." Again, at page 15, having stated the oppressive and injurious effects of the Levant winds *at Cadiz*, he says, in a note, " *at Malaga*, the Levant wind is rather *refreshing* than *oppressive*, for it blows due east, and seems to pass over a greater extent of sea, and is consequently moister; but *there* the north and north-east wind, called the Terral, or Land wind, is very hot and oppressive during the summer months;" which he repeats at pages 158 and 159: and, as the epidemic has never appeared at Cadiz or Gibraltar until after the prevalence of a dry and heated easterly wind, which, *at Malaga*, would, as Sir James observes, have been " rather refreshing than oppressive," a little *recollection properly directed* would have enabled him to account for the supposed difficulty, without recurring to those " precautionary measures" to which every exemption from the epidemic is by him constantly attributed, although they have often proved completely unavailing. If, however, such precautions could really have availed, as the contagionists agree in confidently asserting,

a close, to notice at some length the account published by Dr. Pym (between his 66th and 77th pages) of a sickness among the 54th Regiment at Stony Hill, in Jamaica, during the autumn of 1808; which he cites as "the last proof he shall bring forward of the Bulam being contagious—of the disease which prevailed at Gibraltar being the same as that which prevails in the West Indies:" but the evidence already adduced from every other quarter of the globe to disprove the existence of any contagious quality in that fever, is so abundant and convincing, that any addition thereto seems to be superfluous; and my extreme reluctance to extend this volume farther, together with the obvious internal defects of the supposed proofs given by Dr. Pym in support of the above assertion, induce me to forego my intention, and to offer only some brief remarks on his account, although I had, in fact, prepared a full refutation of it from numerous official documents before me.

It appears that, after the 54th Regiment had been twenty months in Jamaica, and chiefly stationed at Stony Hill, "a violent continued fever" broke out among them,

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do the repeated recurrence and prevalence of the Yellow Fever among the British troops at Cadiz and Gibraltar reflect credit or discredit on the officers who were at the head of the Medical Department on those occasions?

"attended with considerable mortality," in October, 1808, "the rains having been heavy and incessant for a fortnight" previously; and it is this sickness (which Deputy-Inspector Rockett considered as "their seasoning fever," for they had until then been *healthy*) that Dr. Pym, and Mr. Redmond, Surgeon to the Regiment, have represented as being the contagious Bulam Fever.

In favour of the existence of contagion in this disorder, I can discover nothing in Dr. Pym's account and documents, except an "opinion" of Mr. Redmond, and this gentleman confesses that "*he stood alone* in declaring his opinion of its distinct and infectious nature to Mr. Rockett, who told him it was *contrary to the generally received opinion in the West Indies.*" It was equally contrary to the opinion of Mr. Rockett, who has been dead some years, but who formerly joined in the belief expressed against contagion in the Yellow Fever by the Hospital Staff in the Windward Islands, as mentioned at page 335 of my Essay. The sole cause to which he ascribed the fever in the 54th Regiment, whether in his letters to the Surgeon-General, or in his reports to Major-General Carmichael, then Commander of the Forces in Jamaica (with certified copies of which I have been favoured by Dr. Muttlebury, Mr. Rockett's successor as Head of the Medical Department in that island), was "heavy and incessant rains." The "*proof*" upon

which alone Mr. Redmond has founded his opinion is, that Stony Hill is "in general healthy," and that "a detachment of his regiment was sent thence to Fort Augusta, and quartered together with others in barracks with the 2d West India Regiment, where several men contracted the disease, and upon the detachment's return to the Hill, the fever passed progressively through the regiment." Mr. Redmond, however, has not explained *whence* the supposed contagion was "contracted;" nor, in fact, did any contagion then exist at Fort Augusta—certainly not in the barracks of the 2d West India Regiment (a body of seasoned negroes, among whom a single case of Yellow Fever has probably never occurred), and as certainly not among the "others" (white soldiers) who were quartered with them, because it is expressly stated that "other regiments in the same quarters with them (the 54th) had not been affected by the fever." If, therefore, no contagion existed in Fort Augusta, none could have been carried thence to Stony Hill.

In Mr. Redmond's account of the sickness in question, I perceive nothing which does not entirely accord with the ordinary course of nature, as marked by the experience of both preceding and succeeding years at Stony Hill, without any reference to contagion. Whenever long continued and heavy autumnal rains have fallen there, the troops at the station have suffered greatly from fever

of a severe and dangerous character, which was always aggravated into the worst forms of the Ardent Fever, if the weather was likewise unusually hot, and the troops at that post were unseasoned; all which circumstances concurred on the present occasion. Mr. Doughty, alluding to the account of this particular fever, very justly observes (at page 54 of his publication), that "the soldiers of the 54th were susceptible of its influence, whilst those of the other corps were not, in the same degree; because one of these latter regiments (the 18th) had been in the island, to his knowledge, not less than *three*, and the other (the 55th) six years, and a great part of that time in quarters annually visited with Yellow Fever." Accordingly it appears from Mr. Rockett's official letter to the Surgeon-General, dated the 14th of November, 1808, that these and other regiments suffered from the same fever-exciting cause as the 54th, although in a *less violent* form of the disease. As Dr. Pym has omitted the passages in that letter to which I allude, it is proper here to insert them, viz.

"The 18th Regiment at Up Park have again become sickly, and lost twenty-six men during the month. This corps was removed from Spanish Town some time since, with the hope that the change would be beneficial, but it has not proved so.

"The disease most prevalent with them is the remittent and low continued fever. The constitutions of the men being previously debilitated by former attacks, and long residence in the country, are enabled to make but a feeble resistance, and many of them consequently drop off.

"The 55th and other regiments have felt, but in a less degree, the effects of the sickly season."

In the autumn of 1812, a similar sickness occurred at Stony Hill, among six companies of the York Light Infantry Volunteers, which Dr. Muttlebury, Inspector of Hospitals, in his Official Report to the Lieutenant-General commanding, described as being "the seasoning or Yellow Fever, which occurs among men on their first arrival in the West Indies from a colder climate." The Army Medical Board having desired Dr. Muttlebury, "to inform them of the causes that had led to the mortality of Stony Hill in that year, as they had hitherto been led to believe Stony Hill one of the most healthy quarters in the island," this officer stated in answer as follows, viz. "although troops stationed there are *generally* healthy, yet, in very heavy *seasons*" (the periodical rains are usually called "seasons" in Jamaica) "they are occasionally otherwise; as was the case with the 6th Battalion, 60th Regiment, in the autumn of 1810, and also with the 54th Regiment in the autumn of 1808, when that regiment lost

one hundred and eighty-three men in forty-three days;"— "there were no observable circumstances in the atmosphere" (during the autumn of 1812) "different from what is usual, except that *the rains were greater*, and of *longer duration*, and *the heat more powerful* than ordinary;"— "so far as my observation extends, it is only when very heavy and long continued rains have been followed by extreme heat that the Yellow Fever now appears *there*," i. e. at Stony Hill.

Believing that the opinions of two medical officers, who had served so many years in the West Indies, as Dr. Muttlebury and Deputy-Inspector Adolphus (the latter of whom had also witnessed the fever at Stony Hill in 1812), were entitled to particular attention, and would contribute to elucidate the subject under consideration, I thought it my duty to apply for them, and I here insert the substance of the answers with which they favoured me.

Extract from Dr. Muttlebury's Letter, dated "Bath, September 12, 1817."

"The fever which I witnessed among the York Light Infantry Volunteers at Stony Hill in 1812, I considered, as I have stated in several official reports, to have been caused by the heavy and long continued rains of the autumn of that year; and I never observed it to possess a

contagious quality. I may add, that the result of my experience during the seventeen years I have served in the West Indies, in which space of time I have visited most of the colonies from Surinam to Jamaica, and have had ample opportunities of seeing the Yellow Fever, has convinced me that this disorder is not contagious."

Extract from Dr. Adolphus's Letter, dated "September 2, 1817."

"In answer to your question, whether I conceive that the fever which prevailed at Stony Hill among the 54th Regiment in 1808, was contagious or not? I do not hesitate to say, that all the information which I could obtain concerning it at that time (for I was then stationed at Port Antonio, Jamaica, as Surgeon of the 6th Battalion of the 60th, where we had much sickness from fever, especially among some lately arrived recruits, which Deputy-Inspector Rockett informed me was exactly similar to the disease prevailing at Stony Hill), has led me always to consider it as not having been contagious. Subsequently, in 1812, I frequently visited, in my capacity as Staff-Surgeon, numerous cases of fever at that post among the York Light Infantry Volunteers; and I have always had reason to consider the latter fevers as similar to those which occurred there in 1808, and as being equally devoid

of contagion. In answer to your other inquiry, I have farther to state, that the result of my experience during fifteen years of military practice in Jamaica (besides that from considerable private practice), have satisfied me that the Yellow Fever is not a contagious disease."

I will only add, that during the autumn of the last year, 1816 (while I was in charge of the Medical Department in Jamaica), there fell heavy and long continued rains at Stony Hill, which produced considerable sickness and mortality from fever among the York Chasseurs; but, as the heat scarcely equalled that of ordinary years, the disease seldom assumed the violent form of Yellow Fever; but, as Staff-Surgeon Storey described it in one of his reports to me, was generally "of the remitting form, occasionally alternating to intermittent." In a subsequent report, dated "Stony Hill, 4th of October, 1816," Mr. Storey wrote to me as follows, viz. "I was quartered here more than fourteen years ago, when there were one thousand men and upwards, besides women and children; and at that time a similar fever raged as is now prevailing, and the weather was exactly similar, raining almost every day.—Many of the fevers here run a most insidious course; for when the patients are apparently better, and likely to do well, on a sudden they become worse, and then sink rapidly, in spite of every remedy." That the fever upon this occasion was not contagious, I had good reason to

know; and I also know that this was the sentiment of Mr. Storey.

As it has been necessary to draw the attention of the reader to the West Indies, I conceive that, before quitting this part of the world, he will be gratified by perusing the following highly interesting extracts from two manuscript papers (obligingly put into my hands by Dr. Fergusson), which afford valuable additions to the information concerning the late epidemic in Barbadoes, already given between pages 182 and 189, from Dr. Fergusson's Report, but necessarily left imperfect, because his report scarcely reached the middle of that sickness. The first of those papers, entitled "Memoranda relating to Yellow Fever, as it appeared in the Queen's Regiment at Barbadoes in 1816," is dated "June, 1817," and composed by Mr. Alexander J. Ralph, Assistant-Surgeon of that regiment; and the second is an unfinished account of the same fever, drawn up by Dr. Wray, Physician to the Forces, who fell one of the last victims of the epidemic he describes, and whose loss to the service is very sincerely to be regretted.

Extracts from Mr. Ralph's Memoranda.

"On the 2d of September, and three following days, rain fell in torrents; the rich alluvial soil, contracted by the heat

speedily absorbed and tenaciously retained much of the water; every excavation on the parade ground (St. Ann's) became filled; the north-east steady and refreshing breezes, which had so materially contributed to our health and comfort, ceased to blow; light southerly winds succeeded, when the atmosphere became still and sultry. This considerable change in the state of the weather was the prelude to a most melancholy alteration in the health of our corps."——"We now ceased to breathe an atmosphere purified by cool and wholesome winds, but existed in one which had been contaminated with exhalations emitted from the swampy soil, and rendered stationary from the feebleness of the winds. Fever changed its character, became more generally prevalent, was more rapid in its course, and was attended in its earliest stages with excessive debility and violent *gastric* symptoms, and frequently terminated fatally."

"This change in the features of the prevailing fever was noticed in the first week of September in the married families who resided in the huts."—"*There* the causes which favour the vitiation of a stagnant atmosphere were most abundant; *there* the disorder raged speedily, with great force, and many fell victims to it."——"The huts are low ill-ventilated buildings, constructed with a single deal board, and roofed with shingles; each hut consists of a single narrow apartment, which was commonly

occupied by two families, composed of six or more individuals; from their construction these huts afforded the people inhabiting them but a feeble protection from the direct influence of the sun's rays; and from their crowded state, no attention was paid to personal cleanliness by the women and children."

"Finding that we had to contend with a most formidable disease, which was rapidly becoming epidemic, our attention was particularly attracted to the huts; from the number of fatal cases which had occurred there, and from the disease having been first noticed in that situation, we attempted to trace its spreading from the individuals first attacked, but could not do so: progressive propagation of the disease from the person affected, to those who had constant communication with that individual, had not taken place; the attacks were diffused, and occurred in several, and remote situations at the same time; and very many were seized who had not held any communication whatever with the sick. Whole families were swept off by the disease; but it rarely happened that the members fell sick in succession; indeed it was no unusual occurrence to have *all* the inhabitants of the same hut brought to the hospital together labouring under the fever: and here I may remark that I never heard any one attribute the production of his or her illness to contagion:"—"Further it appeared that the disease was confined to particu-

lar situations, favourable to the production of miasmata; and that it selected individuals pre-disposed by debility, for its operation; that certain spots were free from it, although the individuals living there held free intercourse with the sick; and finally that the sick who were removed to habitations on an *elevated* and *dry* soil, did not communicate the disease to those attending them."

"Thus the huts were found to be the *fons mali;* the fever had made comparatively but little progress amongst the men in the *barracks;* and in these buildings a remarkable difference was observed in the respective salubrity of the two floors; the soldiers who slept in the lower apartments were seized in much greater numbers than those who slept in the upper." See page 185.

"The officers, in obedience to the regimental orders, visited the hospital and huts daily, and were continually in the habit of conversing with the men, in order to inspire them with confidence; they notwithstanding, as well as the whole of the hospital attendants, were exempted from the prevailing fever during the months of September and October; and not a single instance of any one of these being attacked, occurred during that period; clearly proving that those affected with fever possessed no power of communicating that disease by *personal infection.*"

"Anxious to lessen the misery which evidently resulted from a residence in the huts, an order was issued to have the whole white-washed; and every thing they contained was directed to be exposed to the open air daily, between the hours of ten and two; the health inspections were made more frequent and particular; a military and medical officer visited the huts daily, to enforce a general attention to cleanliness, both in person and habitation, and to separate such families as might appear to be in any degree crowded."

"The disorder scorned these efforts made to arrest it, and continued to rage with unabated severity: the mortality with which it was accompanied gave rise to a general and alarming apprehension in the minds of the people, and thus gave an increase of activity to its further development."

The month of October is stated to have passed without any fall of rain, and the trade winds blowing steadily during the latter part, "the fever evidently was declining," but "in November the fall of rain was heavy, the dried surface became covered with superficial sheets of water; unusual heat succeeded to these falls, and continued throughout this month, and the greater part of December; the winds being faint and invariably from the southward."

—"The cause of disease was now more diffused; the virulence of the noxious atmosphere increased; and, as might be expected, the additional power of resisting the operation of the causes of sickness, which their habits, intelligence, and situations afford to *officers*, above that which they possess in common with the private soldier, proved insufficient to secure them from the ravages of this fatal disease. Officers, hospital-attendants, and soldiers, under every peculiarity of situation, were seized."

"The predilection which the fever had for particular *local situations*, was at no time, however, more clearly manifested, than in this month, when it might have been expected that the general diffusion of the attacks of fever would have concealed this peculiar and striking feature of this disorder."

Those officers who resided on the basement floor of the barracks, who had not the protection of a jealousied gallery, with which the upper rooms were furnished, were first attacked; indeed to the lower apartments the fever appeared early in November to be confined. It was not however to be supposed, that, unassimilated as we were to the climate, even those who by their elevated residence, were in some measure removed from the palpable sources of the disease should escape: the disease ultimately at-

tacked a few of the officers living in the upper-rooms." In the military hospital situated on a low spot of ground, "it was observed that those servants, whose duties were most remote from the fever wards, were more frequently attacked than those orderlies, who were employed in performing offices which obliged them to be in *contact* with the sick. This partial exemption which the orderlies of the sick-wards enjoyed, compared to that which the other servants experienced, appeared evidently to arise from the orderlies of the sick-wards sleeping invariably in the upper apartments of the hospital, which were those into which the fever cases were received and treated."

"At the *Naval* Hospital, situated on an elevated rock, where many of the Queen's regiment were employed as orderlies to the sick-wards, and a large proportion of the men affected with fever, belonging to that and other regiments," (and, as Dr. Fergusson says, 'all the sick of the navy, comprehending many of the *worst* cases of Yellow Fever') "were treated, the orderlies, with two exceptions, escaped; an escape which they unquestionably owed to the healthy site on which they dwelt."

"The increasing spread of fever in the huts, and the alarming mortality with which it was attended, particularly in November, had not been diminished by any means of purification, and separation which had been adopted;

it seemed to threaten the sacrifice of the lives of all who inhabited them."

"On the 10th of November it was determined to have the whole of the *married families* * removed to a certain building situated on a *dry sandy soil*."—"The beneficial effects which were the consequence of the adoption of this measure, (the necessity of which had been long urged) were speedily rendered evident, by the disorder's ceasing in a class of individuals, who had previously been particularly prone to its attacks. They carried with them their bedding, and every article of apparel, belonging to many who had fallen victims to the disease; and surely had these articles been infected, the disease would have continued its destructive career; but as the cause of it was purely *local*, removal to another situation differently circumstanced, as to soil, &c., afforded a release from this terrific scourge. Unfortunately for those of the troops who lived in the barracks, they remained in the focus of the pestiferous emanations, and continued *there* to suffer most severely, until the 10th of December; when a healthful change took place in the constitution of the atmo-

* Upon this passage Dr. Fergusson remarks, that the Queen's regiment carried to Barbadoes more than double the number of women allowed by the regulations, and almost all of them had families.

sphere; the soil became dry: a north-east wind set in and blew steadily, and the fever abated for a time; indeed so long as the soil continued *dry*, and the cool, refreshing breezes prevailed, so long did we experience a manifest, and daily decrease in the numbers attacked with fever: we hailed the favourable omen with thankfulness, as the precursor of our deliverance from this affliction; and that gloom that hovered around the minds of every one, was gradually giving place to more agreeable ideas, and brighter prospects. We passed the period from the 17th to the 28th of December, without one admission of a fever patient into the hospital." But "profuse falls of rain on the 27th, 28th, and 29th of December, attended with great heat, and a stagnant atmosphere, announced the return of the fever with its usual malignity. That combination of causes which is essentially necessary to the generation and deleterious action of mal-aria reappeared, and brought again with it its direful attendant, Yellow Fever; which continued to prevail until the second week of February, when the soil again became dried, and the trade-winds setting in, afforded us a final deliverance."

"Had facts been wanting to evince most satisfactorily the cause of this fever, the disappearance of it in December, its return at the end of the same month, and final cessation in February, afford evidence highly conclusive." —"To insinuate that contagion gave rise to it, when its

close and obvious connection with, and dependence upon, the state of the soil, winds, &c., was so manifest, would be more than absurd."——" During the whole of the sickly season, a barrack in which several officers of the Queen's regiment resided, standing on a *rock*, a few feet below the level of the parade, as well as many ill-constructed huts erected on a *sandy beach*, on the same level with the sea, were peculiarly healthy."—" When the southwardly winds blow, these barracks may be considered as being the *windward* boundary of the parade-ground; and standing on a dry rock, which does not admit water, and the south wind blowing from the sea across a sandy beach, rendered them free from noxious effluvia." The huts on the sandy soil are stated to have derived similar benefit from the southwardly winds which " were scarcely felt on the upper table of land."

Mr. Ralph distinguishes four forms of the before-mentioned fever; and in two of these he describes the black vomit, together with hæmorrhages, suppression of urine, and other symptoms belonging to the Ardent Fever or Bulam of Dr. Pym.

Dr. Wray generally agrees in his account of the weather at Barbadoes in the last half-year of 1816, with Mr. Ralph. He remarks as an effect of the excessive heat and moisture, and the consequent miasmata, that " the two driest

and most healthy islands in the West Indies, Barbadoes and Antigua (at former periods) have this year been peculiarly the reverse."—"The reason," he adds, "is obvious. In the wet, swampy, alluvial islands, the quantity of rain which has fallen, has been such as to render their marshes complete sheets of water, necessarily preventing the emanation of miasmata."—"Whereas in the dry islands alluded to, the light soil covering the porous coral rock (that easily imbibes the moisture, with which in a great degree it gets charged, allowing the purer water quickly to filter through, but retaining the more dense and noxious particles) when exposed to the burning and unclouded rays of the sun, has afforded a hot-bed of pestiferous miasmata, without the slightest breeze to disperse it. The comparative degrees of health observable between the officers and soldiers inhabiting the upper stories of the barracks, with those unfortunately obliged to live on the ground floors, has been strikingly conspicuous this wet season; of twelve officers belonging to the 2d regiment living on the ground-floor, one only escaped an attack, and of the others six died: of an equal number occupying the upper story in the same building, two only had fever; both of whom however died."

Dr. Wray adds, "A similar and palpable distinction may be exhibited from the servants and people employed about the old and new hospitals: the former being

situated to leeward of a low wet flat; the other on the side of a firm dry elevated rock. Eleven of those attached to the *lower* general hospital were attacked with fever, whilst only two of those at the *upper*, who had the additional fatigue of frequently going to the lower hospital for extras; the purveyor's store being then at a greater distance."

"A considerable mortality prevailed amongst the women and children of the Queen's regiment, such as to awaken the attention and call for the interposition and advice of Dr. Fergusson, Inspector of Hospitals, ever vigilant in watching the health of the army entrusted to his care, relative to a mistaken indulgence granted to that class of people in the corps. The married families were allowed to lie out of barracks in miserable wooden huts, resting on the very *ground;* many of them not water-proof, and all defective of jealousies, ("venetian blinds.") The rent of these wretched habitations was so great, that two or three families were not unfrequently obliged to join, to pay for one; so that they were extremely crowded."——"Whole families were swept off by the fever; affording a plausible argument to the shallow contagionist for the generative cause of the disease, without looking into the real source. But strange to say, this contagion ceased immediately on the women being ordered into barracks, where a little more regularity and cleanliness were imposed upon them; the

facility of selling new rum diminished; and, though last not least, a fine *dry* well-ventilated elevated apartment was afforded them to live in, with galleries shaded from the influence of the sun to wash in: after this salutary change not an instance of fever occurred amongst the women and children, nor was there the slightest increase of sickness in the men, though evidently more crowded in their accommodation; sharing the supposed infection with their bedding, or mixed among the different companies. Be it as it may, the magical contagion vanished: the fever had ceased (which for the month of November and part of December was almost exclusively confined to the Queen's); about the middle of December several days passed without an admission: the wind blowing steadily from east north-east, and the weather assuming its natural temperature and salubrity, the spirits began to resume their wonted hilarity, and the gloom of the closing year, memorable for its fatality to the inhabitants of the island, to disperse; when a severe shock of earthquake was felt on the morning of the 23d of December, accompanied with, and succeeded by, an extremely unpleasant sultry state of the weather, the atmosphere loaded with electricity; which state of weather continued to the 26th, when rain in heavy showers fell and continued all that day and the following, deluging the country with water. Several days of very hot weather succeeded: what was the consequence? a renewal of the fever, but with minor mor-

tality; the wind blowing fresh, and from the right quarter; than which nothing can more distinctly prove the cause of the disease."

It was my desire to have here offered some observations concerning the treatment of the Ardent or Yellow Fever, (in addition to those contained in my Essay) founded on my own subsequent experience, and on that of others, which has been made known to me; but the limited time in which I am compelled to finish this Sequel, does not allow me to accomplish this desire with the care and exactness, which the superior importance of the subject requires; and I must therefore defer this undertaking until a season of greater leisure.

CONCLUSION.

IN concluding this Sequel I beg leave to revert to some of the principal facts and arguments which have been employed in it to disprove the existence of contagion in the Ardent or Yellow Fever.

Those who ascribe this contagious quality to a particular form of fever, called by them Bulam, or Pestilential Andalusian Fever, and contend that it is a peculiar disease (sui generis), are bound by all the laws of evidence, as well as the rules of logic, to prove the *affirmative*. This indeed has been attempted, particularly by Dr. Pym; but my readers will, I trust, have been convinced that his, and all other, attempts have wholly failed, and that there is not a single diagnostic symptom, or other peculiarity, among all those which have been alledged to give a *distinct* character to this disease, which is not either illusive, or capable of being applied with equal truth to other forms of marsh or miasmatic fever. With equal justice may we require that those, who assert this supposed form of fever to possess, and to have been imported and propagated by, a contagious power, should prove the *affirmative*, and both explain and ascertain the time *when*, and the means or

vehicles *whereby*, as well as the places *whence*, such importations were made. But all endeavours to do this have only served to betray a lamentable want of truth in the explanations and allegations employed for this purpose. It must have been highly incumbent on those, who contend for such importations, first to establish that the disease actually possessed a contagious quality *at the places* from which the importations are asserted to have been made; because, without this quality, any importation would have been impossible. But this never has been done, in regard to the importations supposed to have taken place by vessels going from the British or French West India colonies to Philadelphia, New York, &c.; and still less in regard to those going to Cadiz, &c. from Spanish America; where Dr. Arejula declares (see page 267 of this Sequel) that the fever in question, or vomito prieto, has been proved by the experience of centuries to have no contagious power: a declaration which Sir James Fellowes appears to admit, from his incredible supposition that such power may have been acquired by it on the passage to Spain through some incomprehensible operation (see page 280 of this Sequel). In short, the uncandid evasions, and the inconsistent suppositions, which have been employed in order to create and support a belief of the several alledged importations of the Yellow Fever, would only excite compassion, if the dangerous consequences of that belief did not render it most expedient

that these evasions and suppositions should be palpably exposed. Equally discordant are the accounts given by Drs. Arejula, Chisholm, Pym, Fellowes, Riseuno, Hosack, &c., of the force and effects of the supposed contagion of this fever, which they could not have been, if they had any real existence, or foundation in truth; because they must have manifested themselves so distinctly and so repeatedly as to obviate all such disagreement. And even when their allegations are not liable to this objection, their supposed proofs of the existence of contagion might, *if fairly stated*, (see p. 428, &c.) be explained with more reason and probability by a reference to local exhalations or miasmata; which are known and admitted to be the cause of fevers so perfectly resembling that called Bulam, that no mark or symptom capable of distinguishing the latter from the former has been, or can be, adduced. It is notorious that the latter fever has never prevailed in any place, or at any season, in which remittent fevers do not occur, and that it is invariably preceded, accompanied, and followed, by them; and as they are the acknowledged offspring of those fever-exciting exhalations called marsh-miasmata, and as these miasmata are known, at the times and situations in which *a high temperature* prevails, to aggravate the remittents, and convert them into violent continued fevers, similar in every respect to that form of fever called Pestilential or Bulam (which only occurs as a *consequence* of this *high temperature)* would it not ap-

pear natural to all unprejudiced minds, when the latter becomes prevalent simultaneously with what are called Bilious Remittents, to refer *both* to *one and the same cause* (a cause *notoriously adequate to produce both)*, rather than adopt the improbable and strained supposition of two *very different*, if not opposing causes, occurring and operating with *epidemic violence* at the same time, and place ; and one of them at least (contagion) *never manifesting itself* at any time, or at any place, in which the other does not prevail.

As the same causes, cæteris paribus, must always produce the same effects, it is *most certain* that, if the Ardent, Yellow, or Bulam Fever had *really* possessed a *contagious* power, this power would have been *constantly* manifested in all suitable circumstances ; and the proofs of its existence would in their nature have been most decisive and unquestionable, and in number they must have amounted to many thousands. Instead of this result, however, we find the medical practitioners of a large community (the state of South Carolina, see page 263), *unanimously* declaring that the Yellow Fever had not, in any instance within their knowledge, ever manifested a contagious property; we likewise find almost all the medical officers of the navy, and a great majority of those of the army, as well as of the medical practitioners in civil life, who have had the best opportunities of becoming conversant with

this fever, declaring their conviction that it is not contagious: while those of a contrary opinion are compelled to rely on cases, the *best* of which are but *equivocal;* because they are capable of being as satisfactorily explained by a reference to fever-exciting miasmata as by contagion; and because there is much greater reason for this reference to a *well-known* and *widely-extended cause of fever,* than to one, of which the existence, as connected with Ardent Fever, has *never been proved,* or *even made probable.* Some *few* instances have indeed been alledged, in which the agency of contagion *seems* to have been more plainly indicated; but there is the strongest reason to presume, in regard to *these,* that facts of importance have either been *omitted* or *unfairly stated;* because it is at least incredible, if not impossible, that *the same disease* should have been *contagious* in a *few* instances, and *non*-contagious in an immensely * greater number.

To suppose the existence of contagion, is convenient for those who are too indolent to inquire, or who want the

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* On this subject see page 429 of my Essay; and pages 488—9, concerning the difficulty of distinguishing the effects of febrific miasmata from those of personal contagion; and also pages 92—4, where I have adduced the authority of Dr. Haygarth, a decided assertor of the supposed contagiousness of the Yellow Fever, to show that much greater weight ought to be allowed to *negative* than to *affirmative* proofs of this quality in diseases.

qualifications necessary for accurate discrimination: it is an obvious, easy, and imposing expedient to remove in appearance almost every difficulty, as it accords with the prejudices and apprehensions of the greater part of mankind, who are prone to believe that all diseases are contagious when they become generally prevalent; and, however erroneous the belief may be, it is often extremely difficult to prove the contrary, because contagion depends upon a cause or quality not cognizable by the senses.

I have long been, and continue to be, most anxious to ascertain and establish the truth upon this subject, only because it is of very high importance, not merely to individuals, but to this and other governments, from its *present* intimate connexion with quarantine regulations, and with every precaution or expedient which may be employed to preserve health, whether in fleets, armies, or cities. If, however, it has been decisively proved in the preceding chapters, as I hope and believe, that the Yellow Fever is but a more acute and violent form of the disease produced by febrific miasmata, and is *destitute of any contagious power* or *quality*, it follows, as an obvious and necessary conclusion, that all quarantine regulations intended to prevent an importation of the contagion *supposed* to accompany and reproduce *that* fever, are not only, at best, absolutely useless, but highly detrimental to the commerce, and naval operations of this great maritime

country, by the expensive delays and restraints which they impose both on persons and things. The evils, however, occasioned by the precautions taken against *foreign importations* of this ideal contagion are, perhaps, inconsiderable, compared with those which have been produced by the supposed danger of its *domestic communication* from the sick to the healthy, in places where this fever occurs, which, in a multitude of instances, particularly in Spain, has induced persons to abandon their homes and their occupations, and even their nearest relations and dearest friends, when attacked by this disorder; and, by thus deserting the sacred duties of humanity, and withholding that precious assistance and those consolations which might have been afforded *without the smallest danger*, they have rendered these visitations of disease incalculably more afflicting and destructive than they would otherwise have been. I have, at page 495 of my Essay, stated on the highly respectable authority of Don Rodriguez Armesto, "that the barbarous and anti-social belief of the importation and contagion of the Yellow Fever has, from its baneful influence in Spain, caused many unfortunate victims to be abandoned, and to *starve* in their beds; that others have been *shot at the very doors of houses* within which they had endeavoured to find an *asylum;* and that many others were carried *alive* to their *graves*." Deplorable as such consequences are, fewer persons probably have suffered by even *these*, than by those injudicious

measures of the Spanish government, which prohibited, under severe penalties, individuals residing where the Yellow Fever happened to prevail, from removing to *more healthy* situations, least by so doing they might carry, and there introduce, the supposed contagion: they have consequently been compelled to remain within the reach of the fever-exciting miasmata, and thereby to undergo, and often to perish by, the disease. The first measure of the Spanish government at the commencement of the epidemic of 1800 at Seville was, to oblige the inhabitants of the suburb of Triana, where the fever began, to remain in their habitations; and by doing so, more than nine thousand people were attacked by it in that suburb, who might, perhaps, have *all escaped by a timely removal.* Similar restraints were extensively employed in many other Spanish cities and towns, which were in consequence nearly depopulated by the epidemic of 1804. Extensive and enormous as these evils are, they are yet but a part of those which result from the belief of contagion in this fever. For, while this belief subsists, it naturally produces an entire neglect of the proper means for obviating a recurrence of this fever, by meliorating the condition of the places in which it originates, and removing the materials which afford miasmata; and it often, besides, occasions an adoption of measures which, if not hurtful, are at least useless. Dr. John Hunter (Observations on the Diseases of the Army, page 320) remarks 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;" and certainly a knowledge of the true cause of this fever may generally enable those, who have the direction of fleets and armies, in a great degree, to avoid it, by a judicious choice of situations for anchorage, encampments, &c.; and even when the fever occurs where a judicious choice has not been made, its ravages may still be greatly diminished by such knowledge, if properly employed in selecting better situations for removal, &c. Even in a limited garrison like Gibraltar, it has been repeatedly found that a change of situation (see pages 166, 394, 396—7, 399, &c.) has at once procured an immunity to those who, without it, would almost all have undergone the disease.

While I think it my duty, for the reasons just mentioned, earnestly to recommend to His Majesty's government the suspension or abolition of all quarantine regulations, so far as they respect the *Yellow Fever*, it is not less my duty to state distinctly, that I see no reason to advise any relaxation of the restraints and precautions hitherto employed to hinder the importation of *the Plague*, of the eminently contagious quality of which I have, from personal observation, the firmest conviction.

In regard to the fourth clause of the first report made by the Royal College of Physicians to His Majesty's most honourable Privy Council, concerning Dr. Pym's publication, in which the College state their belief that "the cold of our climate would not prove a preservative against the contagion" (of the Yellow Fever), because "it appears that, during the months of October and November, when the fever raged at Gibraltar, Malaga, and Leghorn, the temperature was greatly below the average heat of our summer," it may be observed that this fever has never appeared at these or any other places, without a *previous* occurrence and *prevalence during a considerable time* of a degree of *atmospherical heat*, which is *rarely* felt *here*, and which, when felt, *never continues a sufficient time to produce the disease;* which degree of atmospherical heat seems indispensably necessary to the preparation and maturation of the fever-exciting miasmata. This explanation (which, through an erroneous belief in contagion, has been overlooked) will, I conceive, upon due consideration, remove all apprehension of the *possible* existence of the Yellow Fever in the British Isles.

POSTSCRIPT.

I FORMERLY noticed at pages 377 and 378 of my Essay, and with merited commendations, the candid and magnanimous retraction, which the late Dr. Rush published in the year 1809, of his error in believing the Yellow Fever to be contagious; and it now becomes me to notice what I firmly believe to be a most injurious, and groundless imputation upon his memory, which that retraction seems to have produced. It is contained in the Dictionaire des Sciences Medicales, tom. xv. p. 351, where it is pretended, on the authority of M. Moreau de St. Mery, that Dr. Rush when dying (" *en mourant*"), had declared that he was induced by *particular motives* (" des considerations particulieres") to make this retraction; and that he had never ceased to believe that the Yellow Fever is contagious. It is added that, in his last moments, he had disavowed every thing written by him in favour of *non-contagion* (" Il a désavoué à son heure suprême, tout ce qu'il avoit écrit en faveur de la non-contagion"). How M. Moreau de St. Mery acquired a knowledge of this disavowal or confession, we are not told, as we certainly ought to have been, considering its

importance, as connected with medical truths, and with the otherwise unimpeached character of an eminent physician, who is no longer able to repel an injurious accusation.

M. Moreau de St. Mery appears to have been in some of the French West Indian Islands, but not, so far as I can discover, in any part of the United States, and much less at *the death bed of Dr. Rush;* and therefore, if the confession in question had been made, it probably would have been known to many thousands in America, and to many persons in Great Britain (who correspond almost daily with others in Philadelphia, New York, &c.), before M. Moreau de St. Mery would, in the ordinary course of events, have been informed of it: and it may be presumed, from the interest which has been generally taken throughout the United States, in the question whether the Yellow Fever be or be not contagious, and the warm discussions which have there occurred respecting it, that such a confession as that which has been ascribed to Dr. Rush, would have been a subject of frequent animadversion, in every newspaper and periodical publication of that country. But, instead of this, I cannot find that any person either *there* or *here* had ever heard of this pretended disavowal or confession, until it was announced in the way which I have just stated. I find, indeed, and I am sorry to find, that Dr. Chisholm,

after this imputation on the memory of Dr. Rush had found its way into some of the London journals, took the first opportunity to give it currency, and convert it to his own use. In a note to his Statistical Pathology of Bristol and Clifton, at p. 276, of the last, or 50th Number of the Edinburgh Medical and Surgical Journal, he says, "we are enabled to appreciate the value of the observations of those men, who have given their opinion against the existence of contagion, in the malignant, pestilential, or Bulam, called by them Yellow Fever, by adverting to what, we are told, were their *real* sentiments on this subject"—and he then proceeds to *repeat* the before-mentioned charge, or calumny, against Dr. Rush; admitting, indeed, that we have "no grounds for believing, that Dr. Rush was guilty of the insincerity of denying the contagion of Yellow Fever, for a particular reason," but, at the same time, giving his utmost support to the remaining part of the charge, by saying, "I have information which I cannot disbelieve, that the death-bed *confession* and *contrition*, stated by M. de St. Mery, are true; and that his last, and we may suppose his firmest, because his first, opinion, was that the fevers which devastated New York, Philadelphia, &c., were highly contagious, and not derived from domestic or local causes." Now, I have no hesitation in declaring that, for reasons to be presently stated, I have not the smallest belief in this assertion; and, in my judgment, common prudence, if it had been

possessed by Dr. Chisholm, would have restrained him from thus *connecting himself* with this calumny, after *the many inexcusable misrepresentations* by which he has endeavoured to induce a belief, that the Yellow, or Ardent Fever, is contagious, and of which he has been unanswerably convicted in the Seventh Appendix to my Essay, and in my additional communication published in the 39th Number of the Edinburgh Medical and Surgical Journal.

The onus probandi, in regard to this charge against Dr. Rush, ought in reason, and according to all the rules of evidence, to fall on those who have made it. I cannot be expected to adduce *negative* proofs, where no proofs are advanced in the *affirmative*. I will, however, undertake to do this, so as to satisfy all who are unbiassed on this subject.

Dr. Chisholm knows (for the fact was *eagerly* published by himself) that some years ago, a periodical work was undertaken at New York, by Dr. Hosack and others, principally for the purpose of supporting the advocates for the alledged contagion of the Yellow Fever; and that this work was continued to a fourth volume, under the title of the American Medical and Philosophical Register; and as Dr. Rush died before any part of the last volume of this work had been published, it is impossible

to believe, that such a confession, as that which has been imputed to him, would not have been triumphantly made known, and repeatedly brought into notice, *in that work*, by the contagionists of Philadelphia and New York. I have, however, diligently looked over all the pages of this Register, without finding a syllable to countenance the belief of any change of Dr. Rush's opinion, that the Yellow Fever is not contagious; but I have on the contrary discovered sufficient evidence to warrant a confident belief, that there could not have been the smallest foundation for the charge under consideration. At the very commencement of the 4th volume of this Register, I find "A Sketch of the Life and Character,"—"of that ornament of science and of human nature, the late illustrious Benjamin Rush;" in which, after an account of his studies, his professional exertions, and various writings, mention is made of the 3d edition of his works, which appeared in 1809, with this remark, that it contained "the recantation of his opinion of the contagious nature of the Yellow Fever." And it certainly is not credible that, after having noticed this recantation, the writers of that Sketch would, in concluding their account of his life, have omitted to mention such an extraordinary event, as his subsequent dying disavowal of this recantation, if any such had occurred. But far from giving the slightest room even to suspect, that any change whatever had taken place in the opinion published by Dr. Rush

in the 3d edition of his work, these gentlemen proceed to narrate the remaining incidents of his life, until the moment of his death, and with the highest encomiums, not only on his literary and medical attainments, but on his *religious and moral rectitude.* Encomiums which certainly would not have been *prostituted* to any one who, on his death bed, had accused himself of such mischievous dishonesty, such unprincipled deception, as M. de St. Mery appears to have imputed to him. In regard to Dr. Rush's *moral* character, it is added in this Sketch (at p. 14) that, "to inculcate the principles which flow from the source of all truth and purity, and impart them as a legacy to his children, was an object dear to his heart, and which he never failed to promote, by constant exhortation, *and the powerful influence of his own example.*" In the same volume (at p. 305, & seq.) I find "a Discourse, introductory to a Course of Lectures on the Theory and Practice of Physic, &c., delivered at the College of Physicians and Surgeons," (in New York) "the 3d of November, 1813, by David Hosack, M. D. &c. &c.," in which that Professor, addressing his auditors says, "your feelings, I trust, will be in unison with mine, while, in addition to the numerous offerings of public and private respect, which have been paid to *the memory* of Dr. Benjamin Rush, we devote a few minutes to the contemplation of the professional attainments, the public services, the *moral and religious character,* which make up the por-

trait of that distinguished philosopher and physician." He then proceeds through ten pages, to detail in terms of the highest commendation, the merits and virtues of the deceased, and, with expressions, which certainly Dr. Hosack would never have applied to one, who had been in the slightest degree guilty of the turpitude, which M. de St. Mery has endeavoured to fix upon Dr. Rush, whose *last act*, Dr. Hosack declares to have been, not the confession unjustly imputed to him, but an act of *charity*, and that the *last* expression which fell from his lips, was this injunction to his son, "be indulgent to the *poor*;" upon whom the same Professor declares, that he had "in the latter and more prosperous years of his life, expended one seventh of his income."

The Sketch of his Life before-mentioned, gives the following account of his death, viz. "He was on the evening of the 13th of April (1813), seized with symptoms of general febrile irritation, which were soon accompanied with considerable pain in his chest. His constitution was naturally delicate, and he had acquired from previous illness, a predisposition to an affection of his lungs. He lost a moderate quantity of blood, by which he felt himself considerably relieved. But his strength was not sufficient to overcome the severity of his complaint; the beneficial effects resulting from the most skilful treatment, were but of a temporary duration. His disease rapidly

assumed a typhous character, attended with a *great stupor, and a disinclination to conversation.* In other respects, however, he retained his faculties, and the perfect consciousness of his approaching dissolution. On Monday evening ensuing, after a short illness of five days, and in the 69th year of his age, he ended his truly valuable and *exemplary* life. His death was the subject of universal lamentation, and he was *followed to the grave by thousands,* who assembled to bear testimony to his excellence." I need not add, that these facts are absolutely irreconcileable with the allegations which I have here undertaken to refute.— How or where the authors of them will find an excuse for their conduct, in endeavouring to uphold the nearly vanquished cause of contagion in the Yellow Fever, by unjustly aspersing the posthumous character of a very meritorious physician, I am unable to conceive.

The printing of the present Work was very nearly finished, when some passages were pointed out to me, in a recent publication, entitled, " Results of an Investigation respecting Epidemic and Pestilential Diseases," by Dr. Charles M'Lean, in which the Author supposes that certain doctrines published by him, in the autumn of 1796, at Calcutta, must have been " widely circulated," and have produced " the principal changes," which have since occurred, " whether in the theory or the practice of medicine;" and accuses me of unjustly taking great

credit to myself for having "shewn fifteen years afterwards, what he had already proved in 1796, that Yellow Fever does not depend on contagion."

If Dr. M'Lean had not wilfully shut his eyes against the facts stated in my Essay, he must have seen (at page 335) that, *previously* to this publication in the *East* Indies, I had, with the medical officers of the army under Sir Ralph Abercrombie in the *West* Indies, delivered in writing, for the information of the Army Medical Board, a similar opinion, *founded upon our own personal experience* in the Military Hospitals. He would also have seen, that I was so far from considering this as a *new* discovery, and taking any improper merit for making it, that I adduced the strong and decided testimony delivered by Dr. Hillary fifty *years before* against the existence of contagion in the Yellow Fever, as well as that of Dr. John Hunter and others of the same import, likewise long previously delivered: so that if I had even known the doctrines, claimed by Dr. M'Lean, (which, however, I do not recollect ever to have read) I could have had no reasonable motive for referring to it, after the many more respectable authorities which I had already quoted on this subject.

Although Dr. M'Lean avails himself largely of my facts, against the supposed contagion of the Yellow

Fever (misapplying them indeed to prove that the Plague, a disease essentially different, is not contagious) his work abounds with hostility against myself, chiefly because my Essay does not comprehend the entire refutation of the doctrine of contagion, as connected *with Epidemics generally;* thus he appears to consider the refutation, contained in my Seventh Appendix of Dr. Chisholm's Allegations concerning the Hankey, to be unobjectionable, yet thinks it mischievous; for, says he, "as any refutation, given not as an example, but as an exception to the rule, involves the virtual acknowledgment of the doctrine of contagion generally, it can be considered, if that doctrine be erroneous, as only misleading."

Of such a writer even this notice will probably be thought more than sufficient.

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