Part Second.

REVIEWS.

1. Report on the Fever at Boa Vista, by Dr M'William. Presented to the House of Commons, in pursuance of their Address of the 16th March 1847. Folio, pp. 112.

2. Letter addressed by Sir William Pym to the Lords of the Council, relative to a Report on the Fever at Boa Vista, by Dr M'William. Presented to the House of Commons, in pursuance of their Address of May 14, 1847.

The eventful story of the Eclair steamer must still be fresh in the recollection of our readers. It will be remembered that she reached Portsmouth in the end of September of 1845, last from Boa Vista, one of the Cape de Verd Islands, after having lost by fever, within a short period, sixty-five out of a crew of 146 officers and men; that there were still twenty-three sick on board; that she had lost her captain, her surgeon, and assistant-surgeon, besides two surgeons who had subsequently volunteered aboard; and that of the entire ship's company only forty-one had escaped being attacked. It will be remembered, also, that alarming fever first appeared on board this ship during her voyage from Sierra Leone to Gambia and Boa Vista, and that she was permitted by the Portuguese authorities to land her entire crew at the latter place; but that, as the fever still continued to spread among the crew on shore, it was deemed advisable that the crew should be re-embarked, and that she should proceed to England.

The two parliamentary documents before us refer to an inquiry subsequently committed to Dr M'William by the Board of Admiralty, apparently in consequence of information that a fever, resembling that which prevailed in the Eclair, had broken out among the inhabitants of Boa Vista soon after the steamer had sailed from that port.

Dr M'William was sent out to Boa Vista in February twelve-month, with instructions to inquire into all the particulars of the fever which had affected the island, and in an especial manner, whether that fever had any connexion with the visit of the Eclair. The document drawn up in consequence by Dr M'William, consists of several official communications, of the evidence of about 120 persons residing on the island, Europeans and Africans,—officials, merchants, soldiers, labourers, washer-women, and the like,—relative to the kind of intercourse between the crew of the Eclair and the inhabitants, and to the subsequent origin and progress of
fever on the island,—of general observations by Dr M'William on the information obtained—on the climate of Boa Vista—on African fever—and on the fever of the Eclair. The following passage is so important, that we must give it in Dr M'William's own words.

"The main conclusions, from a review of the whole of the circumstances that were brought under my notice relative to the 'Eclair' at Boa Vista seem to be,—

"First, That the fever on board the 'Eclair' was primarily the remittent of the African coast, which is not a contagious disorder, but that the disease acquired contagious qualities in virtue of a series of causes.

"Second, That although there exists on the Island of Boa Vista a physical cause capable of producing remittent fever, yet it does not appear that that cause was in action when fever broke out in September 1845, and that the island was quite healthy when the 'Eclair' arrived there.

"Third, That the disease of which the Portuguese soldiers died at the Fort (Duke of Braganza), on the small island, was that which afterwards ravaged Boa Vista, and the same as that which prevailed among the crew of the 'Eclair.'

"Fourth, That the fever was propagated throughout the island almost exclusively by direct intercourse with the sick, there being only two cases in which there appears any probability of persons being infected in any other way.

"Fifth, That although those who had passed through the fever were much less liable to the disease than those who had not, yet it would appear that a person having had one attack, possesses no absolute protection against a second attack.

"Sixth, That connecting the whole of the circumstances attending the arrival and stay of the 'Eclair' at Boa Vista with those under which the disease appeared on the small island, and afterwards on Boa Vista itself, leaves no doubt of its having been introduced by the 'Eclair.'

"Seventh, That in all probability the mortality from fever on the island was much increased by the want of proper nourishment for the people, as well as by the total absence of medical assistance for some months.

"Eighth, That the disease had in no case spread to any of the other islands of the Cape de Verd Archipelago."—Pp. 111, 112.

We should add, that Sir William Burnett, in a letter to the Secretary of the Admiralty, contained in the document before us, says—

"Dr M'William appears to have taken considerable pains to gain information; but after a careful perusal of the papers he has sent I am compelled to say that I cannot conscientiously arrive at the conclusion the Doctor has done, viz., 'That the fever was occasioned by the intercourse with the Eclair, which is, however, the chief point on which we differ; agreeing with all my late reports to their Lordships on the same subject; and also in conformity with a former report of mine to their Lordships respecting the case of his Majesty's ship Bann, in the year 1824.'

On the other hand, Sir William Pym, in the second document on our list, to use his own words, considers "Dr M'William's Report to be a most valuable document; and that the variety of uncontrovertible evidence brought forward by him, relative to the disease in question, has finally decided and set at rest a most important and long-contested question relative to the nature and history of yellow fever, more particularly as to its infectious power."

From Sir William Pym's letter we quote also the following passages, to remind our readers of the views he entertains on the fevers incidental to hot climates.
For many years a controversy has been carried on by medical men relative to the infectious or non-infectious nature of the yellow fever; a controversy originating from an opinion that the marsh or remittent fever, and the disease known as yellow, Bulam, or black-vomit fever, are the same disease; this last, in the opinion of non-contagionists, becoming, from a vast variety of causes and circumstances, a concentrated remittent or malignant yellow fever. This controversy I trust is now finally settled by Dr M'William's report; and that the question ought not to have been upon the subject of contagion or non-contagion, but upon the existence or non-existence of two very different diseases. At page 75, Parliamentary Papers, I have stated that there are two distinct diseases prevalent on the coast of Africa; the one the remittent fever, from which our seamen suffer so severely in consequence of boat service on the rivers. This is the same disease as the well-known Walcheren fever, the malaria of the Levant, and the jungle fever of India, and exists in all warm climates, in moist and uncultivated grounds. Any person who has had one attack of this fever is very liable to a second, and afterwards to attacks of ague. This fever is not infectious.

"The other disease is a very different one; it is in no way connected with malaria or unhealthy situations. It is unknown in the East Indies, in Egypt, or in Turkey, and is a native of and peculiar to the west coast of Africa, as the Pestis Lubonica is to Turkey and Egypt.

First, It is a disease sui generis known by the name of African, yellow, or Bulam fever, and is the Vomito Prieto of the Spaniards, from its being attended with the peculiar and fatal symptoms of black vomit, a symptom which rarely if ever appears in the marsh or remittent fever.

Second, It is highly infectious.

Third, Its infectious powers are increased by heat, and destroyed by a certain degree of cold.

Fourth, It attacks natives of a warm climate in a comparatively mild form.

Fifth, It has also a singular and peculiar character, viz., that, like small-pox, it attacks the human frame but once.

This disease, from the west coast of Africa, has been at various times imported into different islands and countries, viz., to the different West India Islands, the Island of Ascension, and to different ports of Spain and North America, as in the year 1845 into the island of Boa Vista."—Pp. 4, 5.

The few particulars from the documents before us which we have just given, cannot fail, we think, to interest our readers in a high degree. The whole account of the Eclair, and, in particular, the results of the inquiry into the health of Boa Vista, ought to awaken an earnest attention, on the part of all who have the credit of the profession at heart, to the unseemly violence of contention which still too often marks the opinions even of sensible men, with regard to the origin and spread of some of the fevers of hot climates.

The point which most urgently presents itself for consideration is, why it should happen that diversities of opinion marked by such violence, we had almost said animosity, prevail on what in all strictness is a part of sober medical science. It is true that in ruder times the learned were accustomed to indulge in the utmost license of recrimination, even when their differences ran on what concerned the exact sciences. But it is humiliating to think that any remains of this leaven of barbarism should still attach to debates relative to the conditions under which grave facts like diseases arise. And yet, perhaps, all surprise should cease when it
is remembered how numerous the medical body is, and how many belonging to it are impelled to write on the spur of the moment by the force of unusual circumstances, without any previous preparation by reflection on what constitutes medical probability, logical inference, or certainty of fact, their sole qualification being too often a dauntless spirit and a determination to place the subject of their observation on what seems to them a satisfactory footing. But how few points in medical science are to be carried by a coup de main? The natural course of improvement is at the best zig-zag, and so if a subject be pressed forward in a straight line, the longer will the retrograde direction be, which at last must be entered on. As regards the extent to which fevers are contagious, it is clear to all the world that the acrimonious debates of the last half century (to go no further back) have left the question in pretty much the same state as at their commencement. The enthusiasm of Chisholm, the ingenuity of Bancroft, the self-sufficiency of M'Lean, the zeal and industry of Jackson, Fellows, Pym, Burnett, and a host of others, have had no other effectual bearing on the question than to convince all sensible people of the necessity of some new mode of penetrating such mysteries than disputatious wrangling and sharp encounters with the weapons of sarcasm. Medical facts are seldom of easy proof, and none are less so than those which relate to the rise and spread of epidemic diseases. But when to the inherent difficulties of such points the distortions incident to human passion are added, how is it possible that any progress can be made towards an insight into truth? And besides the distortive vehemence which professional disputations have introduced into such inquiries, there is the more secret influence of the zeal on one side and on the other, of those interested in a pecuniary point of view respectively in the maintenance and in the abolition of quarantine. The quarantine question hardly deserves to be regarded as a medical investigation. It has plainly been hitherto a long struggle between the mercantile interest, whose property is heavily taxed for quarantine dues, in addition to the evils of detention, and the quarantine officers who profit by the number, the extent, and the rigour of the quarantines declared at their respective ports. How must medical facts fare under such an ordeal!

While we confess that we have hitherto numbered ourselves with those who have regarded contagion as unknown among the fevers at least of tropical climates, we cannot but feel our former belief much shaken by the results of Dr M'Williams' inquiry. We feel the necessity of abandoning all decided opinion on the subject, and, if we could, we would willingly persuade the rest of the medical world to do the like, and to regard Dr M'William's inquiry as the first of a new series of observations, on which no final judgment should be pronounced till sufficient data shall have been collected to set the question as much at rest as the question in
Europe regarding the contagiousness of continued fevers, or that of the non-contagiousness of periodic fevers now is.

In the view, then, which we are disposed to take, we think the following propositions nearly represent the logical points of the case.

1. That the whole subject of contagion as respects the fevers of hot climates (including the plague) should be regarded as a *tabula rasa*, as a part of etiology, in which the ascertained facts are totally inadequate to warrant a decided opinion.

2. That no advantage can result to medical science from discussions in which the merits and demerits of existing quarantine laws are debated.

3. That since small-pox, syphilis, and other diseases, are freely communicable in hot climates, there is no *a priori* ground for the belief that fevers cannot be contagious in such climates.

4. That there is no sufficient evidence of the opinion that all contagions are specific, as maintained by Bancroft, that is, that all contagious diseases never arise or are propagated otherwise than by contagion.

5. That though the contagious fevers of Europe lose this property when persons labouring under them are conveyed to hot climates, it does not follow in obedience to any acknowledged principle, that there should be no contagions capable of propagating fevers in such climates.

6. That it does not follow because the ordinary fevers of hot climates are not communicable by contagion, that there are no fevers in those climates which are neither contagious nor capable under unknown circumstances of becoming contagious.

7. That if the contagious fevers of hot climates be of much rarer occurrence than the non-contagious fevers of analogous type, the difficulty of establishing the existence of contagion must be great, as is proved by the example of those parts of Europe where contagious fevers have been less frequent during considerable periods of time, thus giving rise to a temporary disbelief in the existence of febrile contagion.

8. That the more or less perfect protection afforded by an attack of contagious fever against future attacks, is an important element in the investigation of the etiology of fever in all climates.

9. That the effect of the perfect ventilation, due to the openness of the houses in hot climates must be a powerful means of restraining the ordinary operation of contagion in the production of fevers—on the supposition that it really exists.
Beobachtungen und Untersuchungen über den rasch verlaufenden Wasserkopf. Mitgetheilt von Karl Herrich. Regensburg. 1847.

Observations and Researches on Acute Hydrocephalus. By Charles Herrich. Regensburg. 1847, pp. 278. Small quarto.

The work before us, founded upon eighty-one cases of acute hydrocephalus, observed by the author himself, is valuable alike for descriptive accuracy and for the importance of the inferences deduced. Premising that all those cases are to be ranked under acute hydrocephalus, in which, while unusual cerebral symptoms are met with in the last stage of life, at least one ounce of watery fluid is found in the cerebral cavity after death, Herrich goes on to give an account of his various cases, occurring from the age of three months to the seventy-second year. Thus the reader is enabled to study, as it were, with his own eyes, the various modifications which this Protean malady exhibits in different individuals. In another chapter the author gives a very instructive analysis of his cases on the numerical method, in as far as concerns the age, sex, season of the year, temperament, previous condition of health, symptoms after death, and the relation of the functional symptoms to the organic productions. We have room but for a few of these results.

Acute hydrocephalus is known at every period of life; but its frequency, during the first four years, is at the least equal to that throughout all the other periods of life. It is more frequent in the male than in the female sex, and in many instances is congenital. Functional derangements commonly precede the disease, particularly when the predisposition is congenital—chest-symptoms being most frequent, head-symptoms more rare, and abdominal symptoms still less frequent. The period of these antecedent symptoms is generally for one or two weeks only, and seldom extends to three weeks. The commencement of the disease itself is characterized in the majority of instances by headache and vomiting, sometimes by convulsions and chills. The most common symptom is stupor, though in most cases incomplete; among the less common are sleeplessness, restlessness, convulsions from time to time, and these last occur particularly in childhood and in the last stage of the disease. Anormal fluidity of the blood contained in the heart after death appears to stand to the convulsions in the relation of cause. Convulsions seem also to have a connexion with anormal size of the mammae. Tubercles of the brain on the other hand seem but rarely to be connected with convulsions. The most common post-mortem appearances come in their order of frequency as follows; accumulation of water in the cavities of the brain, tubercular deposits and organizable exudations on the membranes, softening of the walls of the cerebral cavities, firm adhesion of the pia mater to the surface of the brain, hyperemia of the cere-
bral membrane and substance. Plastic exudations of the serous membranes occur most frequently on the arachnoid; less frequently on the pleura, still less frequently on the peritoneum; and these in the first mentioned are oftener of recent origin, in the other two membranes of older date. As to the connexion of functional symptoms with organic alterations, paralysis and convulsions were found oftener on the right side; the same being true of tubercles in the chest, while morbid alterations of the cerebral cavities prevailed more on the left side. Thus the head symptoms, paralysis and convulsions, were opposite to the cerebral lesions. The same holds good with respect to the paralysis of the eye-lid and lower extremities.

The whole work is undoubtedly a valuable addition to the literature of hydrocephalus, or rather to that of serous effusion within the cerebral cavities, though we must doubt if the number of cases examined, great as it is, be sufficient to warrant the reception of the results, just enumerated, as general truths.

On Dyspepsia, with Remarks submitted in support of the opinion that the Proximate Cause of this and all other Diseases affecting the general System is Vitiation of the Blood. By John Burdett Steward, M.D., Fellow of the Royal College of Physicians, of London. 12mo. pp. 106. London: 1847.

This little book is not as its title might suggest, a dry argument in behalf of the humoral pathology. We must indeed say, that the chapter devoted to the proximate causes of diseases in general, is but a brief summary of the prima facie evidence in favour of the participation of the blood in the diseases of the general system—a proposition which we have no intention to controvert. But the author has been himself a dyspeptic,—and his book professes to give his experience in the treatment of himself as well as of others. In so far the observations are valuable, though we are far from thinking that dyspepsia can always be cured by one kind of regimen or treatment, and hence are naturally jealous of having what is true as regards one individual applied to all the rest of mankind. If our author can do for dyspeptics in general what he says he has done for himself, we shall account him a most valuable member of society. "He can eat, drink, and sleep like other persons, and no longer fears the moderate enjoyment of those social comforts, which, whilst they renovate the strength and fit us for exertion, stimulate the sullen to gaiety,—make the lively more cheerful—dissipate the cold formality which often enshrines talent; and, in the refinement which attends it, enables us to forget the animal necessity. "The cynic may curl his lip, the wise may moralize, but, as little will the latter be able to dissuade, as the former to deter, the great mass of mankind, from believing, that good eating and drinking form, at least, part of the recreations of life, and that the inability to participate in them is no less a privation."

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