

## REMARKS ON THE DIAGNOSIS AND CAUSE OF YELLOW FEVER.

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THOUGH the literature of yellow fever be very extensive, and highly controversial, the epidemiological questions discussed in it are but few, and I shall occupy the limited time at my disposal to the greatest advantage, and meet the objects of the Society most effectually, by confining to these the remarks I am about to offer, and endeavouring to indicate where the truth lies between the contending parties. In what follows, particular manifestations of the disease will be referred to, to illustrate the mode of investigation and the conclusions arrived at, but these must not be considered as by any means exhausting the evidence that has been accumulated on the same points.

From the time of the earliest definite records of yellow fever to the present, there have been two opinions as to the nature of the disease, and the manner in which it was propagated. When the early colonists first came in contact with it, they naturally thought it allied to plague, the most deadly disease they were acquainted with, and which was then common in Europe, and it was not surprising that in their endeavours to account for its appearance and spread, they should have assumed it to have been imported in the first instance, and subsequently to have been propagated by contagion from man to man; but a very short experience led many observant people to doubt those positions; and we find Lygon, who arrived in Barbadoes in September 1647, and published his history of the island two years after, while he admitted it was doubtful whether the disease had been carried there in ships, or caused by the irregularities of the inhabitants, added that he had "reason to believe the latter; because, for one woman that died, there were ten men; and the men were the greater *deboystes*" (*i.e.*, the more debauched). Lygon also remarked on the

very unhealthy site of Bridgetown, the first settlement in Barbadoes, as being beside a small creek which he describes as quite a morass, which, indeed, it continued to be until a comparatively recent date. With every succeeding pandemic extension of yellow fever the same question has been raised; and it is worthy of remark, that while the great majority of those residing in the yellow fever localities in the West Indies, both lay and medical, regarded the disease as endemic, and as not communicable from the sick to those in health, the advocates of importation and contagion increased in numbers in places where the disease was only met with occasionally, as in the United States, and may be said to have attained their greatest strength in those parts of Europe where they had little or no experience of it. Nor does this diversity of opinion seem likely to cease; the latest work on yellow fever, by M. Bérenger Féraud, a medical officer of the French Marine, published since the commencement of this year, prophecies an early assent of the medical profession to his views; viz., that the disease is communicated from man to man; and the Commission in America, which has reported on the recent epidemic in the Valley of the Mississippi, has adopted certain opinions, and based on them recommendations as to prevention, which have already called forth considerable criticism, and which there seems reason to believe it will have some difficulty in establishing.

At a very early period of the controversy, supporters of the importation theory accused their opponents of confounding two distinct diseases, the ardent from the ordinary endemic, with the yellow fever, which they held to be of a truly pestilential kind, and highly contagious. This part of the controversy, also, has come down to us in principle, though the details have been gradually modified to meet the increasing acquaintance with the circumstances under which the disease manifests itself. A typical case of ardent or remittent fever can be distinguished readily from an equally well marked one of yellow fever; but as all these may occur at the same place, and even sometimes at the same season, and as there are always numerous cases in which the distinctive symptoms do not come out so prominently as in the typical ones, it was frequently found difficult to determine whether the disease in any particular individual were yellow fever or one of the other forms; this led many, who considered yellow fever to originate from local causes, to regard it as merely a more intense grade of the ordinary endemic fever, due to a more con-

centrated emanation, and appearing for the most part in unacclimatised Europeans. The addition to our knowledge of the characters of yellow fever during the last thirty years, shows clearly that it forms a distinct species, differing from both the ardent and the remittent, though like them it is met with in every degree of severity, from little more than an ordinary ephemeral attack, without danger, to the most intense and deadly fever. So far, therefore, those who advocated the influence of local causes were wrong, but this does not affect the other portion of their position, that the yellow fever was connected with locality, and not due to importation, in support of which they adduced not only frequent sporadic cases between the different epidemics, but even many considerable outbreaks for which no source of importation could be established. To get over this difficulty the supporters of the importation theory long ago described certain cases of the ordinary endemic fever, in which yellowness of surface, and a kind of vomit, closely resembling black vomit, were met with, which, they maintained, constituted the form of disease their opponents referred to as originating from local causes, and not what the former held to be the contagious yellow fever. This explanation was considered sufficient half a century ago, but since then it has been found necessary to be more specific, and the opinion is now set forth that there are two species of yellow fever, the malarial and the specific, which so closely resemble each other in their symptoms, including yellowness of surface and black vomit, that the former can be distinguished from the latter only by its not presenting albumen in the urine, and not extending from man to man. The former of these is said to be the disease which is due to local causes, and to which the sporadic cases are to be referred; and the practitioners in the West Indies are said to mistake this habitually for the specific disease, which, it is maintained, arises there from contagion only, and is but an occasional visitant.

It becomes necessary, therefore, to define what yellow fever really is; and here it must be remembered, that every symptom it presents is met with in other diseases, and one or more of the more prominent may even be absent in individual cases; no single symptom, therefore, can be considered as pathognomonic, but its distinctive characters depend on their grouping, and the succession in which they occur during its progress. The following are its characters:—

1. Yellow fever is a disease which usually terminates in

convalescence or death, from the fourth to the seventh day; but either may occur as early as the second, or not before the tenth or twelfth, or even later.

2. There is generally yellowness of the eyes and surface, commencing at various periods in different individuals and epidemics.

3. On the evening of the third day, or morning of the fourth, the urine usually presents traces of albumen; on the latter a considerable sediment appears in it, consisting almost wholly of scaly epithelium from the bladder; this is succeeded by an equally copious one on the morning of the fifth day, which consists almost exclusively of granular tube casts from the kidneys, with scarce a trace of epithelium from the bladder. By this time the albumen has usually become considerable, the chlorides and urea have been greatly reduced, and the urine, as a whole, is usually scanty, and may even go on to complete suppression; if there be much yellowness, it may contain a variable quantity of the colouring matter of bile.

4. The alvine discharges are devoid of the natural feculent appearance from the third day onwards, becoming greyish or yellowish white, with quantities of black matter diffused through it, when they are formed; when fluid, consisting of mucus tinged with the preceding, or with bile, or blood, in variable quantities. On the approach of convalescence, the evacuations resume their natural colours.

5. As the urinary and alvine evacuations assume these characters there is a great tendency to black vomit, or discharges of similar matter from the bowels, or to the so-called hæmorrhages from the various mucous surfaces, or even, in some cases, from the skin, and, after death, such may be found in the stomach or intestines, when they had not been discharged during life.

Such are the distinctive features of a normal case of yellow fever; but in some the urinary symptoms may occur earlier than here mentioned, and in others they seem delayed for a day or two, but whenever watched from day to day, and properly examined, it is found that the changes in the urine not only embrace the presence of albumen, but indicate desquamation of the bladder and kidneys as regular features of the disease, and when these are fully developed the urea is much diminished, and the chlorides almost, if not completely absent. It is important to bear these facts in mind, for albumen has been found in the urine during the cold stage in certain intermittents and remitents, but in such it went off as the fever came on, and

was not preceded or accompanied by desquamation of the bladder and kidneys, and loss of chlorides, characteristic of yellow fever, but which are not met with in pure intermittent or remittent. After death from yellow fever, the microscope shows acute exudation into the parenchyma of the liver, and when there has been yellowness, the minute bile ducts are found to be occluded; the kidneys also show acute exudation around the convoluted tubes, and the lumen of the latter often altogether obliterated by granular epithelium; and, as a constant feature, there is also acute exudation into the muscular substance of the heart, the fibres of which lose their striæ more or less completely, and show a great disposition to split up into their component fibrillæ, while the voluntary muscles in the same subjects present their natural appearance. The affection of the liver may be met with in cases of the ordinary form attended with yellowness, but those of the kidneys and heart are confined to yellow fever.

There has long been a contention between the two parties as to the form of yellow fever, the Contagionists affirming that it is always continued with a paroxysm of about seventy-two hours, or a little more or less, followed by the characteristic symptoms of the disease, while their opponents maintain that the latter are often found as fully developed in cases in which the fever has been distinctly remittent, or even intermittent, as in those which present the single paroxysm. Previous to our becoming acquainted with the urinary symptoms, when the line of demarcation between the ordinary forms of pure and yellow fever could not be drawn with precision, there might have been sufficient uncertainty on this point to afford ground for ardent controversialists to differ upon, but the trustworthy diagnosis made possible by the present knowledge of these has enabled numerous observers, in different countries, to verify the fact that yellow fever is met with not only in the continued form, but frequently as a remittent and, from time to time, even as an intermittent. Whether the continued form be that proper to the disease, and the other hybrid forms impressed on it when the ordinary malaria, which gives rise to periodic form, is rife, need not be enquired into at this time, it is sufficient for the present purpose that it be clearly understood that a fever, presenting the characters set forth above as those of yellow fever, occurs both sporadically and epidemically in a periodic, as well as a continued form.

The doubts as to the nature of the disease having thus

been removed, the circumstances under which it appears may next be examined. The point for consideration here is whether the disease be connected with a locality more or less circumscribed, in which the cause is generated independently of persons already labouring under it, or whether it depend on a morbid poison given off by the sick, which develops the disease among susceptible persons exposed to it, in whom the poison is augmented, and each of whom may so form a fresh centre for its extension. Before approaching this question, it is necessary to show the nature and bearing of the evidence by which alone it can be decided. There is no test known for the presumed activity of the yellow fever poison but the reaction of the living bodies of men, or of animals that are subject to the disease, and when cases of it arise in any locality where it is a frequent or even only an occasional visitant, all that that proves is that its causes are in operation there. If the persons affected have not been away from the locality for a considerable period, and there be no evidence of its having been introduced from elsewhere, the inference must naturally be that the disease arose from factors in operation at the place. If, however, one or more individuals labouring under yellow fever had arrived from another locality shortly before; and if the first person attacked had been in communication with the sick, whether immediately or mediately, it might be supposed that a poison was conveyed from the latter to which the seizure was attributable; there would be no means, however, in such an instance, of arriving at a conclusion either way, for, to establish the communication between the imported cases and the healthy person, the possibility of local causes having acted must first be excluded; and as the occurrence of the disease in the new locality merely shows the exciting cause to be in action there at the time, there is nothing to indicate whether that was connected with the imported cases, or was quite independent of them. The great majority of the examples adduced in support of the contagiousness of yellow fever are of this description, and when legitimately interpreted, the evidence they afford is quite insufficient to establish the conclusion drawn from it, provided the *de novo* origin is proved to be possible.

There are numerous well-authenticated facts, however, which show that, when yellow fever has sprung up in a locality, persons going to that locality may contract the disease, and should they leave before it become developed and go to a healthy one, they pass through the disease

without affecting anyone about them, and this even when their numbers are considerable. I alluded to a striking instance of this in a paper read before the Society in 1874,\* the salient points of which may be repeated here. There was an epidemic of yellow fever at Sierra Leone in 1865, and the *Isis*, receiving ship there, became affected at the end of the year. The *Bristol* frigate arrived from England late in December, and anchored four or five miles to seaward of the usual place where the *Isis* lay. It being considered necessary to alter the berth of the latter, a party of 116 officers and men were sent from the *Bristol* to her on 28th and 29th Dec. for this purpose; these men left the *Bristol* in the morning, and returned to her each night, without communicating with the shore. Two of them were attacked with yellow fever on Dec. 31st, twenty on January 1st, six on 2nd, three on 3rd, three on 4th, two on 5th, one on 6th, and one on 12th, thirty-eight in all, of whom twenty-one died on board, and two on shore at Ascension. Though the crew of the *Bristol* included many young men fresh from England, and consequently quite unacclimatised, not a single case of the disease occurred in anyone who had not been exposed in the *Isis*. Here the *Bristol* was a healthy locality, for none of her crew had been attacked with yellow fever before this outbreak, nor were any subsequently.

But the same immunity is found, during the healthy season, in localities on shore where yellow fever springs up when the causes of that disease come into operation. Thus the Military Hospital at Barbadoes, which is close to the shore at the south-west part of the island, has some low ground just outside the enclosure, on the land side, where water collects after heavy rain, and the drainage thirty years ago being inadequate, a marshy spot of some extent was formed. As this was drying up, yellow fever appeared among the persons exposed to the emanations on several occasions. On 16th Nov. 1852, H.M.S. *Dauntless* arrived at Barbadoes from St. Thomas, with yellow fever on board, having lost several men during the passage; the sick were sent to the Military Hospital on 17th, part of the crew was landed, and part kept on board; the latter continued to supply fresh cases, which were sent to the hospital as they occurred, while the men on shore soon ceased to be attacked. Altogether, from Nov. 17th to Jan. 5th, one hundred and

\* *Transactions of Epidemiological Society*, vol. iii, p. 481, and *Report on the Health of the Navy for 1866*, p. 226.

fifty-seven officers and men were treated in the Military Hospital, where sixty-five died. The fever cases among the men were mixed indiscriminately with the sick of the troops, yet none of these contracted yellow fever.\*

The military cantonment at Newcastle, in Jamaica, is placed on a sharp ridge about 4,000 feet above the sea. The crest is so narrow in many places that there is room for a single hut only; at others it opens out sufficiently to receive two or more. The extreme length of the cantonment is only 800 yards, and the difference of level between the upper and lower extremities is about 650 feet. Yellow fever appeared among the troops here in the middle of September 1856, when it was prevalent on the low ground, and continued into December. The disease was confined to three localities, which alternated with four others in which there was much less fever, and of a much milder description, though there was uninterrupted communication from one end of the cantonment to the other daily. Including men, women, and children, there were 109 attacks of fever, and 45 deaths, of which 80 attacks and 41 deaths occurred among the people in the three unhealthy localities. The remarkable manner in which the unhealthy localities alternated with those where little fever occurred is very striking, notwithstanding the free communication that went on, and it showed the application of the cause of the disease must have been equally localised. During the epidemic 156 men were employed in the fever wards assisting the attendants, for periods of twenty-four hours, some of them twice, or oftener; altogether there were 210 such instances of exposure to the emanations from the sick in the fever wards; of these persons only eight were attacked with fever subsequently, of whom three died (two of these lived in the most unhealthy rooms at the station, and in the third, 32 days elapsed from his attendance to his attack); while of the remainder of the troops in the cantonment at the commencement of the outbreak, amounting to 523, there were 89 attacked with fever, of whom 38 died, giving a proportion of 5.1 per cent. of attacks, and 1.9 per cent. of deaths, for those exposed to the sick, against 17.0 per cent. and 7.3 per cent. respectively for those who did not come near them.† These instances illustrate the positions laid down above; there is a vast mass of similar facts in the literature of yellow fever, derived from nearly every country in which it

\* Milroy, in *Lancet*, vol. i, 1861, p. 358.

† For full details see *British and Foreign Med.-Chir. Rev.*, vol. xxiv, p. 445.

has appeared, which can only be alluded to on this occasion, but which goes to prove that when the influence of locality is removed yellow fever ceases to appear, however numerous may be the sick, or however close may be the communication between them and those around them.

There is one case which requires notice here and has been held to establish the communication of yellow fever from man to man, in a position free from suspicion of the existence of local causes of the disease. I allude to one of the incidents connected with the *Anne Marie* at St. Nazaire in 1861. This vessel arrived there in July from Havanna with a cargo of sugar, having had nine cases of fever during the passage, two of them fatal. Some days after she commenced to discharge, yellow fever appeared among the people employed in this work, and among a number of others who belonged to other vessels lying in the vicinity. Some of the latter left St. Nazaire while the disease was in the incubation stage, and it became developed only after they reached their destination. A Dr. Chaillon who lived at Montori,  $4\frac{1}{2}$  miles (7 kilometres) from St. Nazaire, and had not been to the latter since the arrival of the *Anne Marie*, visited several of these men, and on the last occasion, on 11th August, finding the patient suffering from cramps, he remained with him and rubbed his limbs for nearly an hour. This gentleman, aged 42, though robust and active, was of a highly nervous temperament, very impressionable, and had a terrible dread of the risk he incurred by visiting these yellow fever cases. On 13th Aug., while 10 miles from Montori, he was attacked with intense headache and felt so unable to proceed, that he left his conveyance, and lay down by the wayside, until some of the people at work in the neighbourhood came to his assistance. He then made one or more visits, and returned home, the headache continuing very severe, with pain in loins, weariness in limbs, and he passed a very disturbed night, with delirium and occasional bilious vomiting. On the 14th he was somewhat better; he took a purgative, which operated very powerfully until the night of 15th, causing great exhaustion; he had himself bled on 14th to from 24 to 35 ounces, and took  $1\frac{1}{2}$  grammes of quinine. On 15th there was little fever, but in the night (the regular tertian period) there was much restlessness, and the eyes were slightly yellow. On 16th the yellowness became more distinct; during the night he had a little red wine and water, which was vomited soon after, and then had a blackish violet colour, and an execrable taste (*goût exécrable*). On 17th there was stupor, convulsive move-

ments of mouth and hands, more yellowness, petechiæ over forehead, arms, and legs. No urine was passed since previous night. Death took place at 11.30 a.m. Throughout the attack the overwhelming presentiment that his connection with the yellow fever cases would cause his death was ever present, and undoubtedly had the most injurious influence on its course.\*

Pernicious intermittents are frequent in the country about St. Nazaire, and in August 1861, when the heat of the weather was almost tropical, intermittents were generally attended with bilious symptoms, and displayed a greater obstinacy than during the preceding years.† And before Dr. Chaillon's case can be received as one of yellow fever, the possibility of its having been merely a bilious intermittent must be excluded. The condition of the urine, which would have cleared up the difficulty, was not observed; the vomit, which has been taken as black vomit mixed with the urine, was described as having an execrable taste, which would lead to the inference that the dark colour it presented was due, not to black vomit, which, if not acid, has no prominent taste, but to bile, and the course of the disease otherwise was not incompatible with its having been a bilious intermittent, aggravated by the mental condition of the patient. It cannot be accepted, therefore, as a case of yellow fever, and does not establish the fact that yellow fever can be communicated from a person labouring under it to a healthy person, which experience elsewhere had already shown. M. Melier himself seems to have been doubtful of the nature of this case, for he says, "If it be not permissible to be altogether so explicit as to the propagation by sick, it must be admitted that the fact, it may almost be called the *experiment* of Chaillon, gives to this second proposition a high degree of probability, not to say of certainty."‡

Yellow fever, then, being clearly connected with locality, its exciting cause must be an emanation given off at such places, but whether this be gaseous, or consist of minute organisms, has not been ascertained. There are a good many facts, however, which throw some light on the condition under which this emanation arises, a right appreciation of which is of great importance as tending to place on a clearer basis several of the disputed points in the etiology of the disease. The first of these is whether the disease be really endemic in any country, and if not truly endemic,

\* Melier, in *Mémoires de l'Académie de Médecine*, vol. xxvi. Letters of Madame Chaillon, Drs. Legoff, and Durand, p. 160 to 164.

† *Ibid.* Statements of De Guallouzo, pp. 129, 146. ‡ *Loc. cit.*, p. 96.

whether it may appear in a country from time to time without importation from some other in which it was already existing. The great frequency of yellow fever formerly in the West India Islands and around the shores of the Gulf of Mexico, led many authors to state that the disease was endemic in that region, though later experience has shown that it is occasionally absent for years from places where it had previously been nearly an annual visitant. The tropical part of the West Coast of Africa, also, has been commonly accepted as a perennial source of the disease, and Chisholm and Fegur attributed the epidemics they encountered in the West Indies to importation from that part of the world. The disease, however, was much less frequently met with in Africa than in the West Indies; thus there were epidemics at Sierra Leone in 1823, 1829, 1837, 1847, 1859, 1865, and 1872, and there was a single case of yellow fever there in 1827, another in 1835, and three in 1845, the year in which the *Delair* suffered from this disease. As the accounts of the commencement of the first four of these epidemics are pretty full, as well as of the cases in 1845, they afford some definite proof on the questions submitted above. Up to 1847, inclusive, the yellow fever in each of the epidemics appeared first at Sierra Leone, and in the three former was preceded by a considerable aggravation of the ordinary remittent fever among the crews of the various trading vessels which were distributed in the neighbouring rivers, collecting their cargoes of wood or other produce. Various vessels were supposed to have imported the disease, but, on examination, each of these was proved to have arrived in the colony in good health, and to have commenced to suffer some time after only, when the general sickness was beginning to show itself. In 1847 there was no suspicion of importation; the first case was that of the colonial surveyor, a gentleman who arrived in the colony from England on 3rd February, in the same vessel with myself. He was attacked on 29th June, and died on 3rd July, quite yellow, and having had black vomit. The second case, a person who had no communication with the first, was attacked about 11th or 12th July, and died on 16th. The third case occurred on 13th July, and died on 18th. There had been no communication between this and either of those that preceded it. The colony had been healthy until June. During that month, fever of the remittent form had shown itself in an aggravated form among some deserters from the shipping, and among a number of Spaniards and Brazilians who had been brought to Free-

town in captured slavers some time before ; but in none of these cases had black vomit been met with, and, though some of them became yellow, these attacks had the usual duration and character of remittent fever.

The epidemic of 1823 extended to many men-of-war and other vessels, and embraced Ascension on the one hand, while one case at least occurred at Bathurst, on the Gambia, in an officer who had just arrived from Sierra Leone. That of 1829 commenced at the latter station in the early months ; many ships were then affected, and it extended to Fernando Po, in the Bight of Biafra ; in 1830, Goree and St. Louis, on the Senegal, were attacked. The epidemic of 1837 also commenced in the early months at Sierra Leone ; the first case occurred at Bathurst on 24th June, and an epidemic followed, commencing about 25th July ; and in August it broke out at Goree. At St. Louis there were a few suspicious cases, which were considered as marsh fever, influenced by the epidemic condition then existing. At the end of 1837, and in the early months of 1838, the yellow fever reappeared at Sierra Leone, and several ships were affected, and, after the arrival of some of them at Ascension, an epidemic appeared there. The epidemic of 1847 seems to have been confined to Sierra Leone and its immediate neighbourhood, no other place on the Coast of Africa, that I have heard of, having been affected ; this year, however, as well as in 1837, there were epidemic outbreaks of yellow fever at several points in the West Indies, and on the neighbouring continents, so that the disposition to the disease at the time was really widely extended.

These facts show that from 1823 to 1847, yellow fever could not be considered as endemic on the tropical part of the West Coast of Africa, and yet that it appeared at Sierra Leone, and became epidemic four times at intervals of a few years, without importation from other places. It is obvious, therefore, that factors requisite for the production of the disease must have become active there, without introduction from elsewhere by shipping or sick ; and this being established for Sierra Leone, when the conditions of the question had not been complicated by previous arrivals of ships from unhealthy localities, what is there to enable us to say that the same may not take place at the places on the same coast, where the same general climatic conditions exist ? This has actually occurred : the British settlements on the Gambia were not affected in 1849 or 1830, but on the 13th June of the latter year yellow fever appeared suddenly in force, without having been preceded by the arrival of any vessel

for a considerable period, and spread over the neighbouring part of the mainland, reaching St. Louis on 4th August, where it caused great mortality.\* The epidemic which commenced at Goree about August 1878, and which has extended very widely through the province of Senegal, seems to have arisen in the same manner; for there was no yellow fever at Bathurst in 1878, and apparently very little at Sierra Leone, the only notice of which I have seen being the occurrence of three cases in March, in black soldiers, all of whom recovered.

Again, when the sporadic cases are considered, they show that the factors may be in operation over a very extensive space about the same time, without any question of importation from one point to another, and yet, at most of them, not in sufficient form to generate an epidemic. Thus, in 1845, the *Eclair* had been at anchor off the Seabar, one of the mouths of the Sherboro river, south of Sierra Leone, from the beginning of March to nearly the end of June, with but little intermission. While here her boats were sent into the river on several occasions; cases of ordinary remittent fever appeared among the crews of these in April and May, but on 22nd May, a stoker, who had not been out of the ship, was attacked with fever, of which he died after five days' illness; the symptoms are not detailed sufficiently to enable the case to be recognised, with certainty, as one of yellow fever, but it was a very suspicious one. Others followed on 4th and 5th June, in one of which the patient became yellow, and in another incipient black vomit was found in the stomach after death, but these had been up the river from 22nd to 28th May.† The *Eclair* left at the end of June, arrived at Sierra Leone on 4th July, and finally left that place on 23rd July. While lying here her crew had leave. The first case of fever among them, since the ship's return to Freetown, occurred on 19th July, this terminated fatally on 27th, on board, and seems to have been the ordinary remittent. A second case occurred on 21st, which was sent to the Military hospital, it also was the ordinary remittent, and recovered. A third case, attacked on 19th, but reported on 22nd only, died on board on eighth day, having had dark grumous vomiting shortly before. A fourth case was placed on the sick list on 23rd, but had been unwell for some days previously; this appears to have been ordinary remittent. All these men seem to

\* *De la Fievre au Senegal*, par L. J. B. Bérenger Féraud, pp. 116, 158.

† Bryson, *Account of the Origin, Spread, and Decline of the Epidemic Fever of Sierra Leone*, pp. 98 to 100.

have been on shore for some days. A fifth case occurred on 29th, and terminated fatally on 7th August, without yellowness or black vomit; and a sixth, put on the list the same day, died on 17th August, with a dusky colour of skin, and some frothy dark fluid was found in stomach.\*

Up to the time of the *Eclair's* leaving Sierra Leone there had been no trace of yellow fever in the colony; but on 6th or 7th August, a clerk named Pringle, who had been in Freetown for some time, had ague; this passed into yellow fever, and he died on 16th, with black vomit. Mr. Babbage, master of the *Ceylon*, an American trader, which arrived on 28th July, from the Gambia and the coast to the northward, was attacked on 15th August, had black vomit, and died on 19th; and, lastly, a seaman of H.M.S. *Star*, named Elliot, who had arrived at Freetown on 2nd August, with six other white men, in an empty slaver from the eastward, was admitted into the Military hospital on 16th August with fever, of which he died on 20th, yellow, and having had black vomit. The officer of Elliot's party was attacked with ordinary remittent on 15th August, the other five white men embarked in H.M.S. *Actæon* on 17th August, where all had fever, and one of them died, but the form is not recorded.† It appears also that, in the last quarter of 1845, a corporal of the Infantry of the Marine was attacked with yellow fever at Goree, which proved fatal.‡

If, now, these facts be analysed, it is seen that up to and during the stay of the *Eclair*, no yellow fever occurred at Sierra Leone; the first case occurred in a resident, at least fourteen days after she left, and this was followed by two others on 15th and 16th August, in persons who arrived from sea, and from different points of the coast, after she left; the crew of the *Eclair*, therefore, could not have received the fever from exposure to *preceding cases* at Sierra Leone on the one hand; nor could the cases which appeared there in August be connected with her visit in July. Whether the case of the stoker in May, then, be considered as the first indication of the disease in her, or whether it be held that that only appeared after she left Sierra Leone finally, it is clear that it arose without previous communication with persons labouring under it, as it has been shown to do at Sierra Leone and Goree on previous occasions, and as actually occurred at both places in 1845. As bearing on

\* Bryson, *Account of the Origin, Spread, and Decline of the Epidemic Fever of Sierra Leone*, p. 110.

† Bryson, *Climate and Principal Diseases of the African Station*, p. 170.

‡ Béranger Féraud, *loc. cit.*, p. 120.

this question, too, it is worthy of remark here that the case at Goree appeared during the last quarter of the year, the time when the epidemic which broke out at Boavista after the *Eclair's* departure was prevailing in that island, which is the nearest of the Cape de Verd group to Goree, and only about 300 English miles from it.

The general outcome of all these facts is that from time to time there is an increased activity of those factors which lead to yellow fever, over a varying extent of the earth's surface, under the influence of which that disease becomes developed where local circumstances are suitable. The latter embrace the state of the weather, the condition of particular spots, often very limited in extent, and the emanation which engenders the disease in individuals exposed to it, while there are many points in their immediate vicinity where they may remain without risk, if only they avoid the emanation in question. The determining condition seems to be a certain amount of moisture of the soil at the point; whether supplied by rain, or from subsoil sources, is immaterial. Too much water obviates the result; whether by checking the formation of the emanation, or preventing its diffusion, has not been determined; while too little seems practically to stop its formation altogether. Thus, while it is found at Sierra Leone that yellow fever epidemics have fallen on years when the rainfall has been low for that place, and even then the disease has been met with before the heavy rains set in, or during a break in these which occurs pretty regularly in the end of July and early in August, or after the wet season has nearly terminated, in all the epidemics heavy and continued rain has put a stop to the yellow form of fever, and the ordinary remittent has taken its place. On the other hand, their vicinity to the desert ensures Goree and Senegal a much drier climate in ordinary years, and it has been remarked that their epidemics have all fallen in years more than usually wet.\* At Boavista it was only after heavy rain, and when the pools resulting therefrom were being dried up under the action of the sun, that the fever became epidemic. The same combination is observed in the West Indies, a certain amount of moisture is always present when the disease springs up, and heavy rain for the time being seems to suspend the influence of the emanation.

What applies to localities on shore applies also to ships. Unless a vessel have her hold in a foul condition, she will not become the source of an epidemic; numbers of her crew

\* Béranger Féraud, *loc. cit.*, p. 298.

may be exposed to the cause of the disease on shore, and embark while it is in the incubative stage, but it is found to cease with them within the recognised period of incubation, and none become affected who had not been exposed in the sickly locality; but if, as in the *Eclair*, the hold be foul, it may supply the requisite conditions for the generation of the miasma under the influence of the pandemic factors, and a most serious outbreak may arise, while neighbouring ships, or even the shore itself, may remain quite free from any trace of the disease. A good illustration of this is afforded by the United States steamship *Susquehanna* in 1858. This ship, while at Spezzia in 1857, received orders to proceed to San Juan de Nicaragua, and left on 22nd October. After touching at Genoa, Madeira, and Key West (where there had been no yellow fever since 1854), she reached San Juan early in December, with her crew in perfect health. She seems to have anchored inside the harbour, where the shipping are within the range of the malaria from the swampy ground surrounding it, and in about ten days intermittent fever began to show itself, then remittent, and diarrhœa, cholera morbus, and scorbutic complaints were not infrequent. This state of things continued until 20th March, when the first fatal case of fever occurred. The ship was then taken outside, and cruized off and on for four days, then anchored sufficiently far off to be out of the reach of malaria; but the fever increased notwithstanding, and the sick list rose to thirty. It was now determined to proceed to the northward, and the ship left on 1st April, and reached Jamaica on 5th. On 6th and 7th, eighty-five cases of yellow fever were landed from her at the Naval Hospital at Port Royal, of whom twenty-three died. The *Susquehanna* left Jamaica for New York on 8th, and arrived there on 15th, fifty more cases of fever having been placed on the sick list during the passage. All the crew were ultimately removed from her, and she remained nearly three months with no one on board. At the end of this period people employed to remove her stores were again attacked with yellow fever. The surgeon of the *Susquehanna* had not heard of any yellow fever at San Juan while she was there, and though the other ships in port at the time had intermittent and remittent, none of them had any yellow fever.\* There was evidently an active source of the disease in the *Susquehanna* herself; her hold contained the

\* Report on fever in the United States steamship *Susquehanna*, by R. J. Macroom, M.D., in *American Journal of Medical Science*, vol. xxxvi, pp. 324-28.

soil suited for elaborating the emanation, and the other factor being supplied during her stay at St. Juan, it began to be evolved, with the result mentioned above; while, both on shore and in the rest of the shipping, for want of a suitable soil, the general factor proved inoperative. The sick from the *Susquehanna* were landed at Port Royal during the healthy season, and the disease died out with them.

A ship with a local cause for yellow fever in her may then, as in the case of the *Susquehanna*, retain the power of generating this for many months, and in that time may proceed from the place where she originally acquired it to a distant country, where those who come within the range of the emanations from her interior are liable to take the disease. Two questions, both important, but very different in their bearing, arise here in connection with the new locality, which require consideration, viz., First, to what distance from the vessel can the emanation prove effective in exciting the fever? Second, should the emanation meet with a suitable soil, can it fertilise it so as to make it give off a similar emanation capable of producing the disease?

The facts in my possession bearing on the first question are not very numerous. One instance occurred at the quarantine station at Pomegues, near Marseilles, in September 1821—a place perfectly healthy and isolated. There were forty vessels in it at the time; these were arranged round the port, their bows towards the centre and their sterns towards the shore, with a clear space between each two, and, being in quarantine, they had no communication with each other. The *Nicolina* arrived, having had some yellow fever on her passage, and was placed in quarantine. On 8th September her hatches were opened, and on the 11th the first attacks of yellow fever took place in the neighbouring vessels, which had previously been healthy. There were twenty-seven attacked, of whom seven died. According to a plan of the port, with the positions of the vessels upon it, the disease extended to the second vessel to the left of the *Nicolina*, a distance of 24 yards, and to the third on her right, a distance of 36 yards, the intermediate ships on both sides being all affected. And there was a single case in the seventh vessel to the right, at a distance of 70 yards, there being three vessels between this and the third which were unaffected. The wind was supposed to have carried the emanation from the *Nicolino* to the other vessels.\* A plan of the dock at St. Nazaire, with the positions of the *Anne Marie* and the various vessels

\* Melier, *loc. cit.*, p. 92, and plan in appendix.

that suffered, accompanies M. Melier's paper, the most distant of these, the *Cormoran*, had two cases, both fatal. These men were said never to have been on board the *Anne Marie*, though it is not mentioned whether they had been nearer her than their own ship, which lay at a distance of 187 yards. A mason who wrought at a point on the opposite side of the dock from the *Anne Marie*, and distant from her 284 yards, was attacked very early with yellow fever, of which he died. It was supposed this man had not been nearer that ship than the position where he was employed, but there seemed to be some uncertainty on this point.\* In the outbreak at Swansea, in connection with the *Hecla* in 1865, described by Dr. Buchanan, two cases occurred on one side at 150 yards from the ship, and on the other side one case was 200 yards from her; but as a quantity of copper ore which she had brought from Cuba was there in a yard between her and the latter, it is possible it might have been the source from which the emanation was given off, which would reduce the distance to about 150 yards. A few days previous to the occurrence of the cases at these opposite points the wind had blown from the vessel towards them.† In 1861 three men of H.M.S. *Rinaldo* (last from England) contracted yellow fever at Halifax, Nova Scotia, which seems attributable to the same cause. H.M.S. *Spiteful* had arrived from the West Indies on 16th August with thirty-four cases of yellow fever on board. The healthy part of her crew were removed on shore to the dockyard, and the sick to a hulk in the harbour, while both parties and the vessel herself were placed in quarantine, as regarded the rest of the squadron. The *Rinaldo* was in the harbour at the time, and remained until 23rd, when she sailed for New York. On 27th August a boy was attacked with what proved to be yellow fever. On 28th a stoker was attacked, and on 29th another stoker. The first and last died, the second recovered. The boy was one of the dingey's crew, and was constantly employed going to and fro between the ship and the dockyard. The stoker attacked first was employed in the dockyard; of course returning to his ship every night. He denied having had any communication with the *Spiteful's* people. The other was a patient in the temporary hospital attached to the dockyard until the day previous to the ship's sailing.‡ Unfortunately there is no account of the relative positions of the *Spiteful* and the *Rinaldo*, or of

\* Melier, *loc. cit.*, p. 125.

† Eighth Report of Medical Officer of Privy Council, pp. 449, 457, 468.

‡ Report on the Health of the Navy for 1861, p. 68.

the distance of the former from the temporary hospital. The *Spiteful* was admitted to pratique on 24th August, and on the 26th her holds were commenced to be cleared; that she continued to have an active cause of the disease in her soon became apparent, for on 31st one of her people, and next day two more, were attacked with it, all of whom died.\* These facts show that the emanations from a vessel may be carried by the wind to considerable distances, and in sufficient quantity to excite the most pronounced forms of yellow fever.

Another mode of diffusion which has been much dwelt on requires notice here, viz., that by clothing, or other porous materials, which have been exposed in localities where the emanation was produced. Reference is frequently made to the behaviour of such articles when subjected to the action of odorous substances as illustrative of this point; but, like many other analogies, this is apt to lead to erroneous views, for the odoriferous element in all these being volatile, first passes into a state of vapour, and being subsequently condensed on the clothing, etc., a considerable quantity may accumulate, and these articles will then continue to give off the odour, so long as any of this matter remains for evaporation. As regards the emanation causing yellow fever, it must either be gaseous, or particulate; if the former, it may pass into the interstices of the dress or other porous material, but in no greater proportion than exists in the air in which it is diffused; if the latter, portions might be detained in the porous material, as a plug of cotton seems to detain fine particles, but the quantity so collected can never be considerable, unless the porous material has been exposed for some time to a current of air containing such particles, and in a thickness sufficiently reduced to permit of the bulk of the current passing through. It would appear, then, that such articles may receive and retain a certain portion of the emanation, and the question comes to be, is this sufficient to develop yellow fever in persons who have not been exposed to it at its original source. Several American authors adduce instances of boxes of clothing packed at New Orleans, causing the disease in those who opened them in New York; but they are not of frequent occurrence, and the every-day experience of men living among yellow fever cases, leads most of them to conclude that the amount of disease diffused in this way is infinitesimally small. It must be borne in mind that these remarks refer exclusively to the

\* Slayter, *Transactions* of the Epidemiological Society, vol. i, p. 356.

emanation from the soil, or from similar sources of the disease in ships, and not to an infective material from the persons of the sick, supposed to be conveyed in their personal clothing and bedding, there being no small amount of evidence to show that, away from a locality where the terrestrial emanation is in operation, exposure to such articles fails to produce the disease.

Coming now to the second question submitted above, which is, in fact, whether the local conditions, giving rise to an epidemic, can be produced by importation, either by ships or fomites, there is, as most people are aware, a great division of opinion, due, in the main, to the parties concerned endeavouring to explain the extended operation of nature they deal with from too limited a view of the facts, and from insufficient analysis of the evidence at their disposal. If the facts connected with the appearance and progress of yellow fever for several years be collocated, it is found that, at its periods of epidemic activity, it has not been confined to one or two localities where it is commonly met with, but has extended far beyond these, sometimes in one direction, sometimes in another; the Atlantic Coast of the United States, and the Valley of the Mississippi, are two of these. In such extratropical extensions, the disease, as a rule, appears earlier in the season at the more southern points, and later at those more to the north; and persons from the former, proceeding north to avoid it, but who may have imbibed the poison before leaving, may arrive at their destination verging upon fever, or actually labouring under it, sometime before the conditions have been matured for the generation of the poison on the spot. When this takes place, and the disease manifests itself among the residents at the place subsequently, many think the evidence of importation complete. In the same way, should a ship, with the cause of this disease active in her, arrive at such a place, the subsequent appearance of the fever in the locality has been attributed to importation by her. Are these conclusions borne out by a full consideration of all the facts concerning the origin of yellow fever now available? I think not. The evidence concerning an outbreak at any given point is often so complicated that it is impossible to arrive at a trustworthy conclusion from that alone, and it becomes necessary to take other evidence, obtained at the same place at another time when the same complication may not have existed, or to have recourse to that from some other point, where the facts bearing on the question may have differed sufficiently to permit of those which

were not essential to the result being eliminated, and so permit of the proper inference being drawn from those that remain.

To give an illustration; in 1854 yellow fever was at Savanna, Charlestown, Portsmouth, and Norfolk, all on the Atlantic Coast of the United States; in 1855 it did not reappear at the two former, but at Portsmouth and Norfolk, where there had been a trace only the previous year, there was a severe epidemic; in 1856, Charlestown was the only one of the four places mentioned where the disease was active again, but this year there were upwards of 300 cases at various points around the harbour of New York; at the same time there was an epidemic in Bermuda, and great activity of the disease in the West Indies; and in Europe there were the first indications of the severe epidemic which occurred at Lisbon the following year. Dr. Vanderpool, health officer, stated there were a good many vessels from yellow fever localities in quarantine at New York when the outbreak commenced, several of which had had the disease on board. These were anchored near some of the districts on shore where the disease subsequently made its appearance among persons who had had no immediate connection with them, and he attributed its appearance to the emanation carried from those ships by the wind, and to portions of it adhering to various articles thrown overboard, which drifted to points on the shore near which the disease showed itself. Among other places where it sprang up was Governor's Island, the Military Station in the upper harbour, where it was confined to a fort at the south-east corner, which looks towards the quarantine station, upwards of five miles from it, but about a mile only from the nearest point on the neighbouring shore where the disease was met with.\* Heavy rain had occurred some time before the yellow fever appeared on shore, and it was as this was drying up that it broke out. Now there is such a combination of circumstances here, that the influence of one cannot be separated from that of another; but when it is mentioned that a second outbreak occurred on Governor's Island in 1870 (in which there were 157 attacks, 49 of them fatal), when the disease was confined to another part of it, leaving the people in the fort it attacked in 1856, and many others untouched,† and where there seem to have been no vessels in quarantine with yellow fever on board, much of

\* Abstract of Report in *American Journal of Medical Science*, xxxiv, p. 471, *et seq.*

† Sternberg, *Ibid.*, lx, 398, *et seq.*

the difficulty is removed, and factors thought to have proved so active in one instance, were found non-existent in the other. If, from New York we turn to Bermuda, it is found the epidemic there, in 1856, commenced at distinct points on the shore, well removed from shipping, and the first cases, in August, could not be connected with those in H.M.S. *Malacca*, which had been in quarantine with yellow fever on board, and had left the islands, with all her sick, on 28th June, or with another vessel, the *Margaret Messer*, from Demerara, which had had no sick on board at all;\* nor could importation by ships be established in any of the epidemics which had appeared in Bermuda up to that date.† As factors similar to those which developed the yellow fever at New York in 1870 cannot be excluded in 1856, there is really no ground for assuming that the emanation from the vessels in quarantine, or from the refuse thrown overboard from them, had any influence in producing the local condition which gave rise to that which occurred in 1856; the experience of Bermuda, not differing much in latitude from New York, and, like it, subject to epidemics at intervals only, and which, in the majority of instances, occur at periods when yellow fever extends more or less along the Atlantic Coast of the United States, indicates the operation of some general factor unconnected with ships or traffic, as has already been shown to be the case on the West Coast of Africa; and it would appear, therefore, that beyond the tropics, as well as within them, yellow fever epidemics may, and do, arise without importation, either by ships or fomites. This conclusion, it may be said, is not incompatible with the active material of fomites fructifying in a suitable soil at some distant point, and so giving rise to an outbreak; but for the present there is no means of establishing this position, save by excluding the general factor already mentioned, and that our present knowledge indicates no means of effecting.

\* Smart, *Transactions of the Epidemiological Society*, ii, p. 59.

† *Ib.*, pp. 30-68.

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