ON EPIDEMICS OF DENGUE FEVER; THEIR DIFFUSION AND ETIOLOGY.

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(*Concluded from page 28.*)

The characteristic features of the epidemic were the slowness of its growth during the first two months of its existence; the rapidity and universality of its ravages during the succeeding three months—nearly the whole population having been attacked; and, lastly, the gradual mode of its decline, apparently owing to the absence of fresh and susceptible victims. The epidemic, as observed by me from the very beginning, never presented the appearance of being produced by any cause operating upon the population generally; but, on the contrary, it had a period of growth, maturity, and decay; and, in this respect, was precisely analogous to the cholera epidemic which preceded it. It differed from it, however, as to its mode of propagation; for, while two sections of the community escaped the cholera epidemic, dengue spared none, nearly all being attacked irrespective of rank, nationality, sex, and age. I am not over-estimating its ravages when I say that, at least, 75 per cent of the population were attacked, and I am certain that at least one half of the attacks took place in the months of October and November.

The period of incubation appeared to be short, from two to four days; the attacks were generally sudden and without premonitory symptoms, verifying the accuracy of the Spanish designation, *trancazo*, a *stroke*, and the no less expressive East African designation, *denga*, a *sudden cramp-like seizure*. The mode of propagation was not like that of cholera and enteric fever, through the intestinal excretions; but more closely resembled that of scarlatina and measles—its range, however, being wider, and its infective power more intense. No fatal cases occurred. The mortality of uncomplicated dengue seems to be absolutely *nil*; but were it accompanied by even a small percentage of fatal cases, it would be the most destructive disease known. With regard to the diffusion of the epidemic from Zanzibar to the mainland, I can only state generally that it took place along the lines of human intercourse, and that the coast towns were infected; but to what extent the natives in the interior were affected I am unable to state. I am under the impression, however, that it did not prevail to any great extent, else I would have heard of it.

During the period of its prevalence in Zanzibar as an epidemic, the Red Sea ports, Southern Arabia, the Persian Gulf, Cutch, the Bombay Presidency, Ceylon, and the Seychelles Islands, were all exposed to infection, as vessels and native craft sailed to these places; but, as a matter of fact, with the exception of the coast of Africa and adjacent islands, the epidemic did not extend except to Aden and the Red Sea ports until the early months of 1871, when the disease had become all but extinct in Zanzibar. In so far as I am aware, the first vessel from Zanzibar to Aden, in 1871, was the American barque *Essex*, which sailed on the 11th of March, and which would probably reach Aden about the beginning of April. By what vessel, or native craft, the disease was actually carried to Aden is not known; but it was recognised there by Drs. Turner, Reade, and Welsh, about the end of June. Dr. Turner, in his account of the epidemic at Aden, expresses his opinion that it was introduced by the dusty wave whirlwind (*Shumal*) which burst with great violence from the south-west African coast, sweeping Aden in its flight, on 26th June, 1871. I need scarcely, however, state that there is no necessity whatever for the introduction of this hypothesis.

Aden has been generally regarded as the sole secondary starting point of the epidemic for the Persian Gulf, British India, and the Straits Settlements; but, while this is generally correct, there is reason to suppose that Bombay was infected, direct from Zanzibar, in August 1871, as cases were observed at that date by Dr. Da Cunha. That Aden played an important part in the dissemination of the disease is evident from the fact that Port Said was infected in September; Muttra and Calcutta in the same month; and Batavia in November. Professor Charles ascribes the infection of Calcutta to Aden, and he states that many cases were observed in October among the Jewish community, who have direct commercial intercourse with Aden, though the greatest prevalence of the epidemic was not reached till February, March, and April, 1872. Bombay was subjected to a second infection in the latter part of December 1871, by the troopship *Dalhousie*, and it was considered, at the time, that this was the first entrance of the epidemic into British India. There is abundant evidence, however, that British India was infected at different points, and almost simultaneously, from Zanzibar and Aden, before the *Dalhousie* reached the port of Bombay.

I shall not attempt to trace out the diffusion of the epidemic throughout the Presidencies of British India, the Independent States, Burmah, Assam, Bhotan, Nepaul, Thibet, the Straits Settlements, and China. The facts communicated by a number of distinguished and zealous medical practitioners are scattered throughout the pages of our current medical literature, and will, when collected, form one of the most interesting narratives in epidemiology.

From a perusal of nearly all that has been written on the subject, I have been much impressed with the similarity of each branch of the vast epidemic, a similarity so striking that the history of one, in so far as the main features are concerned, might be adopted as the history of all. The epidemic followed the lines of human intercourse; when it fixed itself in a large community, it had its periods of growth, of greatest intensity, and of decline; while nearly every observer used the same language—"It spared neither rank, nationality, age, nor sex."

The epidemic had, in so far as it had been traced, about a six years' life; but sporadic outbreaks have occurred up till 1880, which were probably indirectly connected with the original outbreak in 1870.

I shall now consider the epidemic of 1823-29, as read by the light of the diffusion of the epidemic of 1870-75-80.

Systematic writers have sadly distorted the chronology of this epidemic, each after the other having copied the mistakes of his predecessor, representing it as having had its origin in Burmah; or in Burmah, the Bengal and Bombay Presidencies simultaneously.

The first appearance of the disease was, as I have already mentioned, in the island of Zanzibar, or somewhere on the east coast of Africa in 1823, when it was called Denga. This I ascertained in 1870, and I may state that I have every reason to believe that the date is accurate. I cannot, however, state positively whether the disease had its origin in 1822 and extended into 1823; or in 1823, extending into 1824. The next that we hear of it is in Guzuratti, in the Bombay Presidency, a district which has the closest commercial connection with Zanzibar, by means of native craft, during the south-west monsoon, which begins to blow in April and continues till November. Dr. Kennedy, surgeon to the Residency at Baroda, says—"The epidemic passed through the whole province of Guzuratti during the hot months of 1824, and was most severely felt at Baroda during the last week of May and beginning of June. "According to the invariable laws of this epidemic disease, the general infection of Guzuratti in June would imply the entrance of the disease in April, or even at an earlier date.

We next hear of it in Calcutta, where exhaustive accounts of the epidemic were written by Drs. Twining, Cavell, Mellis, and Mouat. Dr. Twining saw his first cases on 23rd and 24th May, and the disease reached its greatest intensity in June, July, and August; but it is not at all probable that he saw the first case which occurred. He says—"An ephemeral fever, somewhat similar, prevailed at the time at Rangoon." Dr. Mellis says also—"A medical friend writes that the troops at Rangoon were attacked with fever on the 11th May, after twenty-four hours' exposure under incessant and heavy rain." These reports, embodied in the communications of Drs. Twining and Mellis, were evidently the foundation of the statement that the disease first appeared at Rangoon, in Burmah.

Regarding Rangoon, we know that it was taken by storm on the 11th May, 1824, and that a great amount of sickness prevailed. Dr. Hamilton, of the 13th Regiment, under date 20th July, says—"The type of fever which so generally pervaded the troops *during the latter part of June and the commencement of July* was purely inflammatory, ushered in by more than usual articular pains." Dr. Waddell, in an article "On the diseases which prevailed among the British Troops at Rangoon," says—"The pyrexial epidemic which visited Calcutta in May, and spread over a great portion of India during the two succeeding months, also *prevailed at Rangoon in June and July*. It chiefly affected the officers of the army, of whom but few escaped."

The main features of this branch of the epidemic are as follows:—Denga was prevalent in Zanzibar in 1823, probably extending into 1824. At the end of May, 1824, it had reached its maximum intensity in the Bombay Presidency. Towards the end of May the earliest cases of the disease were observed in Calcutta, and towards the end of June the disease appeared among the troops at Rangoon. I may observe here that the earliest cases among the soldiers at Rangoon must have been brought under medical notice, so that the exact date of its appearance is fixed. The outbreak must have been but trivial, as Dr. Walsh, in his *Memoir of the Diseases of the British Troops during the Burmese War*, makes no mention of it.

This epidemic in India was as widespread as that of 1870-75; and its available chronology indicates that it extended from Zanzibar to the Bombay Presidency. During the period of its prevalence there, there was a general movement of troops from all parts of British India towards Burmah, so that every facility existed for its rapid transit from the east coast to the west coast of India. In every district where it was described, it had a period of growth, of maximum intensity, and of decline; and it was delineated as having attacked nearly all without regard to nationality, rank, age, or sex.

The next that we hear of the disease is at the Danish island of St. Thomas, in September 1827. No history of the outbreak in St. Thomas was written by any medical practitioner resident there; but Dr. Stedman, of the neighbouring island of Santa Cruz, gives a valuable and interesting account of it, written immediately after the occurrence of the event. He says:—"St. Thomas is the great entrepôt for the trade of Europe, and the U. S. of America, to Columbia and the Islands. Being a free port, and possessing one of the best harbours in the West Indies, it is always crowded with vessels from every part of the world, and from every civilized nation." The population of the town was about 12,000 at that time. Stedman says:—"*The English negroes in St. Thomas called it the Dandy Fever*, and by this name it became generally known throughout the islands wherein the English language was spoken." Dr. Squaer, Assist.-Surg. 39th Regt., then stationed at St. Christopher, gives an account of the epidemic as it appeared there, and states that "the stiffened form, occasioned by the pains in the head connected with the shoulders, and the dread of motion, obtained for it the fantastic name of "The Dandy." Out of about twenty-five local historians of the epidemic in the various West Indian islands, most of them having been permanently settled there, Dr. Squaer is the only one who attempts an explanation of the meaning of the term "Dandy;" most of them stating that they cannot say wherefore it was so named. The explanation of the term puzzled all except Dr. Squaer; and I am certain that no one who has ever seen the disease would accept this derivation of the word. The aspect of those who have suffered from this painful affection is anything but dandyfied; had it been called "the cripples," the designation would have been more appropriate. When the disease appeared in the Spanish island of Cuba, it was called Dunga: but was afterwards changed to Dengue, which means *fastidiousness* and *prudery*,* the name by which it has been ever since known.

I am of opinion that both the disease and its designation were imported to the West Indian Islands direct from the East Coast of Africa. In 1824 Denga was prevalent in Zanzibar, and it is impossible that the coast towns on the mainland, north and south, could have escaped. On the hypothesis that the Mozambique territories were infected, the mode of the extension of the epidemic to the West Indian Islands is obvious; and from my personal knowledge of the traffic of the district, I can certainly say that it would be much more easy to account for the infection of Mozambique than for its immunity. Dr. Stedman, the earliest historian of the epidemic, and the only one who writes of its introduction into St. Thomas, says—"The contagion was supposed to have been brought by a vessel from the coast of Africa, which touched at St. Thomas;" but whether this vessel was from the west or from the east coast of Africa Dr. Stedman does not say.

At the time of the appearance of the epidemic—1827, and long after, an extensive slave trade was carried on between the Portuguese territories, in the Mozambique channel, Brazil, and Cuba. Captain Burton, in his interesting volumes on *Zanzibar—City and Island*, refers to this slave trade. The vessel from the coast of Africa, which *touched* at St. Thomas, would probably be a slave ship from the Mozambique channel, bound for Cuba. At what time the disease first appeared in Cuba I am unable definitely to state, but it was certainly present in March, 1828, and was then called *dunga*.

If it be accepted as a reasonable explanation that both the disease and its designation *denga* were imported from East Africa to the island of St. Thomas, the latter would be changed by the English speaking negroes into the familiar term *dandy*, and by the Spanish speaking negroes of Cuba into the equally familiar term *dengue*. I have endeavoured to get information regarding the beginning of the outbreak at St. Thomas from the local newspapers, but unsuccessfully, as all were destroyed during a rebellion. The only description which I have found is the following:—"*An epidemic of the most painful nature, which the oldest inhabitants did not remember to have seen or heard of before, has broken out.*" This is quite inconsistent with the statement which has been made that the disease was prevalent in the West Indian islands in 1824.

I shall not attempt to follow out the progress of the epidemic throughout the West Indian Islands; but I may state that the chief centre of dissemination was the island of St. Thomas, and that Havannah infected the ports of the Southern States of America as far north as Philadelphia.

The literature of the diffusion is very complete, and there can be no doubt whatever that the disease which prevailed in Zanzibar and the East Indies from 1823 to 1826 was precisely the same as that which prevailed in the West Indian Islands and the Southern States of America from 1827 till 1829. With the exception of Professor Dickson of Charleston and Dr. Nicholson of Antigua, none of the numerous recorders of the epidemic seem to have had any knowledge of the East Indian epidemic, and his information regarding it was derived from an article in the *Edinburgh Medical and Surgical Journal*.

The accounts which we have of the epidemic of 1779-84 are of a fragmentary nature, and it is by no means certain that they really refer to an epidemic of dengue. I may state, however, that Gaberts describes an epidemic as having been prevalent in Cairo in 1779, and that Brylon refers to a similar epidemic, which was called knockelkoorts (bone fever), which prevailed in Batavia in the same year. Mr. Persin, an Indian missionary, mentions an epidemic of a disease resembling dengue which was prevalent in 1780 on the Coromandel coast, Africa, Arabia, Persia, and Thibet, while Fernandez de Castilla and Nieto de Piña describe an outbreak of a similar disease called *la piadosa*, which occurred in Cadiz and Seville in 1784-85. Probably these are but the fragments of a great, though partially recorded, epidemic which existed from 1779 till 1785 or 1788.

I shall now consider the etiology of the disease, and I may state, at the outset, that nothing beyond hypothesis can be advanced.

Regarding the epidemic of 1870, I may state that it was not introduced to Zanzibar from without. I have exhausted every possible means of investigation without having been able to discover a single case of dengue on the mainland of Africa, or in any of the southern ports which had commercial intercourse with Zanzibar prior to July 1870. The disease appeared at the height of the south-west monsoon, and for at least three months before, dhow communication from the north was impossible.

On the 19th of July the barque *Florence* arrived after a month's passage from Aden, but there was no sickness on board; and on the 2nd August the steamer *Millbanke* arrived from Aden, having made the passage from Marseilles *viâ* the Suez Canal; but the epidemic had previously appeared in Zanzibar. I was compelled, contrary to my preconceived opinions, to come to the conclusion that the disease originated *de novo*, the hypothesis that the germs of the disease might have lain dormant during the previous 48 or 49 years being too absurd to be entertained for a moment.

There was nothing unusual in the meteorology of the season; the monsoon rains had been copious, but not unusually so, and the south-west winds had set in early and strong. Altogether, the climatic conditions were favourable as regards health. The sanitary condition of the city was bad; indeed,

* *Andar en dengues*: to be over nice; to be too punctilious. *No andar en dengues*: not to mind trifles.

I may say as bad as bad could be ; but except in one respect, it was in its normal state.

During the previous eight months the population of the city had been affected with a remarkably severe epidemic of cholera, and out of a population of about 100,000, the estimated deaths had been from 15,000 to 20,000. During the months of December and January the chief occupation of the living consisted in burying the dead. Interments took place in the immediate vicinity of houses ; in the public market place ; in the outskirts of the town ; and when the disease was at its height, dead bodies were exposed in the bush and left on the sea-beach. In the case of the slave population there was scarcely the semblance of burial, the body being hardly hid from view. When the violence of the epidemic had somewhat abated, I had leisure on one occasion to walk over part of the suburbs devoted to interments, and I saw the entire space red like a newly ploughed field. Thousands must have been buried and exposed there during the preceding two months ; fresh bones and skulls were scattered about on the surface of the ground, and fragments of humanity in every stage of decomposition were lying in the bush, emitting a sickening odour. I can testify, from my own experience, that the effluvium from decomposing human remains was sometimes, especially at night and in the early morning, when the atmosphere was still, quite distinguishable in the city.

I wish to avoid the *post hoc, ergo, propter hoc* arguments ; but it may be stated as at least a remarkable coincidence that the denga epidemic of 1823 was also preceded by a cholera epidemic in 1821, which may have extended into 1822. All that we know as to the precise date of this cholera epidemic is from a passage in the "Narrative of a Journey into Korasan," in 1821, by Mr. J. B. Fraser, who says :—"A ship with slaves from Zanguebar, which had lost a number on the passage [from cholera], had come to Muscat [1821], but not until after the disease had appeared there." Zanguebar may mean either the city or the territories of Zanzibar. We know, however, that about that time a severe epidemic of cholera prevailed in Zanzibar, and we may take for granted that the sanitary condition was the same as in 1870 ; for Captain Smee, writing in 1811, says—"It is a habit all over the town to bury amongst the houses. The poor are only wrapped up in a mat, and have scarce sufficient sand thrown over them to hide the corpse from the view. Indeed, some part is generally seen sticking through ; and, as to the slaves, they are often laid out to putrify on the beach. In consequence of this disgusting practice, the stench in and about the town is most intolerable." Captain Smee's description refers only to the normal condition of the place, and one may imagine what it would most likely be during a fatal epidemic visitation.

That this condition of matters must have had a deleterious effect upon the health of the people is obvious. In the month of May, 1870, fever of the most severe type I had ever seen was prevalent, and I find the following in my notes :—"In nearly every case the fever attacks the patient every second night, and, in nearly all the cases on hand, the patient suffers most severely during the night." Cases of erysipelas appeared in my practice for the first time ; wounds and sores were generally unhealthy and inclined to slough, and I saw cases of gangrene and trismus. Indeed, the first case of dengue which I saw, I mistook for erysipelas of the face. In my original notes of the symptoms of the disease, I observe regarding the eruption :—"It neither resembles that of measles nor of scarlatina, but is much more like that of erysipelas, with this exception, that the colour is much less intense, and spreads over the entire body within forty-eight hours. In regard to the wavy outline, the boundary between the affected and the unaffected parts, the resemblance is complete. Its course was always from the head and face downwards. More or less swelling and tenderness of the lymphatics were invariable. The symptoms indicated systemic poisoning." I do not find that these symptoms characterised the disease invariably during its progress throughout India, but I can state most decidedly, both from observation on others and on my own person, that the eruption did not appear as a rash, but after the manner of a rapidly spreading erysipelas.

From the very first I regarded dengue as a hybrid disease, though I could not account for the nature of the hybridism. Scarlatina I had never seen nor heard of in the island ; and I could not satisfy myself that I had ever seen a case of measles. Prior to the cholera epidemic I had seen only one case of erysipelas; and acute articular rheumatism was almost equally rare. In short, there was not, prior to the epidemic, any instances of acute diseases which I could imagine might result in the production of a hybrid disease such as dengue. The disease, however, must have had some cause originating from conditions existing prior to July, 1870.

As regards the etiology of the disease, the question may be considered—Is it not possible that the cholera germ, or the materies morbi of cholera, may be so modified by the products of human decomposition as to give rise to a hybrid disease such as dengue ?

It will be admitted that putrid emanations from the *cadaver* cause ill health, and may even cause death ; and it has been shown that cadaveric alkalies have toxic properties, some of them being equal inactivity to the strongest poison. If chemical changes, of such a nature, take place in the *cadaver*, may not physiological or pathological changes also take place? The hypothesis is not unscientific ; for, in the vegetable kingdom, we have illustrations of hybrid plants dissimilar to either of their immediate progenitors, morphologically and physiologically. In plants, the production of first hybrids, where species have been intercrossed, takes place very frequently ; and the results of Mr. Darwin's investigations tend to show that every degree of fertility probably exists in hybrid races "from zero to perfect fertility." It is generally admitted, however, that sooner or later the fertility of hybrids becomes impaired, there being no evidence that any one race of plants or animals represents the results of permanent hybridisation originally induced between two distinct species.

If the germ theory of disease be admitted, the possibility of hybridisation must be admitted also. The physiological properties of the cholera germ, and of certain products of decomposition are known ; but, reasoning from known facts, it would be impossible to predicate, *a priori*, the physiological properties of the hybrid product. So also with the yellow fever germ, the plague germ, and the typhoid germ. Each and all might be hybridised, and each would have its own distinctive features, a family likeness pervading the whole. We might thus have a cholera dengue, a yellow fever dengue, a typhoid dengue, &c., &c. Following the analogy of hybridism in plants and animals, we might have every degree of fertility from zero to perfect fertility ; which means sterile or sporadic cases ; limited outbreaks and great epidemics. According to the same analogy, we would have a necessarily interrupted existence, there being no permanent hybridisation ; so that sporadic outbreaks, and great epidemics would die out, from sheer want of vitality. In dengue, but in no other disease, in so far as I am aware, are these peculiarities observable. It has been stated that the disease is endemic in certain places, but this requires further investigation and confirmation.

I shall only mention the epidemics of dengue, or break-bone fever, that have occurred in America ; but I may observe that no epidemic seems to have followed the parallel of that of 1827-29, which, in my opinion, was imported. The epidemics described by Rush, in Philadelphia ; by Pezet, in Lima ; by Lallemant, in Brazil ; and by Smith in Peru, seem to me to have been very doubtful as to their nature. The Polka fever, described by Lallemant as having been prevalent in Brazil in the summers of 1846-47-48, I could not recognise as dengue. It was certainly not the dengue which I have seen. There is the same element of doubt about the Savannah epidemic of 1862.

The epidemics of 1850 and of 1880, which occurred in the Southern States of America, were both preceded by unusually severe epidemics of yellow fever ; and, in both epidemics, symptoms were noted as common which were, to say the least of it, very unusual in the oriental epidemics which I have described.

Limits of time prevent me from discussing this part of the subject ; but I may mention, that a study of the epidemics which have appeared in the western hemisphere force the conviction upon me that they had the same connection with the results of yellow fever epidemics, that those of the eastern hemisphere had with cholera epidemics. Each were distinct varieties of the same disease, but not identical.

If this view of the etiology of dengue be correct, the measures to prevent the diffusion of the disease must be directed towards its origin; and special care should be exercised in the disposal of the dead, more especially in tropical climates, during outbreaks of epidemic disease. Cremation would be the most effective preventive ; but until public opinion is ripe for this mode of disposal of the dead, disinfectants such as lime, which is available in most places, should be freely used.

The mode of communication is evidently similar to that of

scarlatina or measles; but when an epidemic has attained full force it would be impossible to arrest its progress by any known means. The disease being in many cases mild, and the infection spreading with great rapidity, isolation would be impracticable, and quarantine quite inefficacious. The only mode is to deal with the disease at its origin; but quarantine might limit its spread by bringing it under early observation.

Dengue is the only epidemic disease with which I am acquainted, the *de novo* origin of which can be asserted on reasonable grounds admitting of discussion.

Though a non-fatal disease, the universality of its ravages, in the countries where it prevails epidemically, give it national importance. Of 2,324 persons employed by the Government and East India Railway Offices, 1,636 suffered from the disease, over 70 per cent being attacked during the epidemic of 1871-72. Epidemics of dengue have resulted in the temporary suspension of business, and they might disable an army during war.

ON THE PREVALENCE OF ENTERIC FEVER AMONG YOUNG SOLDIERS IN INDIA.*

By Surgeon-General Professor Maclean, C.B., Netley.

Mr. Chairman and Gentlemen,—The subject to which for a brief space of time I am about to invite the attention of this section of the Congress is one that of late years has much occupied the attention of military medical officers in India. It is the prevalence of enteric fever among young soldiers during the early years of their service; the nature of this fever; its causes; and the most rational means of prevention. It is not in India only that this subject has awakened attention, as I shall presently show; the French army serving in Algeria has in like manner suffered, and very notably also during the occupation of Rome.

It was Dr. Bryden who first seriously awakened the attention of the profession in India to the wide-spread distribution of enteric fever in that country, and brought out the fact that youth and recent arrival there are intimately related as predisposing causes of this disease.

M. Léon Colin, one of the most distinguished physicians in the army of France, and professor in the great military hospital of the Val-de-grâce, has referred me to a valuable *brochure*, originally published by him in the March and April numbers for the year 1878, of the *Archives Générales de Médicine*. It appears that enteric fever was fatal to the French army in Algeria to the extent of 4.63 per 1,000 in the year 1868, and that the death rate in that country from the same disease was in 1872, 2·2; in 1873, 2·23; 1874 3·2 per 1,000 effectives. As in India, so in Algeria, the young and the newly arrived were the victims. When the Pontifical States were occupied by the French army the mortality attained in the year 1868, from the same fever, to the enormous figure of 20·3 per 1,000 effectives. It is a notable fact to be afterwards referred to, that along with this great mortality from enteric fever, the same regiments serving in Rome and Algeria simultaneously lost severely from pernicious malarial fevers, and from that fatal malarial form of pneumonia from which the French army, during its stay in Italy, suffered so severely. For example, in 1868 the 35th regiment of the line lost in Rome 70 men, a mortality of 41·15 per 1,000 present; of these 48 died from enteric fever, 8 from intermittent, and 10 from pneumonia; while another regiment serving at the same time in Algeria lost in one year 13 men from enteric, and 15 from malarial, fever. I may be permitted to say with reference to pneumonia, as seen in malaria poisoned men, that the fatal rapidity with which consolidation of the lungs takes place in this disease in malaria-struck subjects is a fact with which at Netley we are quite familiar.

Up to the year 1861 enteric fever never appeared in the medical statistics of the army of India, European or native. Fevers were registered under three heads—intermittent, remittent, and continued. The two first were regarded as without question due to malaria; under the heading *Febris communis continua*, were classed all fevers not distinctly paroxysmal. Few now doubt that hundreds of cases, formerly regarded as the adynamic form of remittent, would now be classed as enteric, and still fewer that an enormous proportion of those entered in the returns as common continued fever, more particularly those with a history of bowel complication, as the diarrhœa of such cases was often phrased, were nothing else than enteric fever, as we now understand that term.

We must bear in mind that it was only about the year 1861, or thereabouts, that clinical thermometry in India was brought to aid in the diagnosis of fevers. In the General Hospital at Madras, so far back as the year 1838, the year of my first connection with India, in the old infantry barracks of Secunderabad, in the Deccan, of dysenteric notoriety, and on service in China, as far north as Nankin, I saw and treated cases of continued fever, extending over twenty days in duration, with the bowel complication above referred to, that, without hesitation, if I saw them now, I would diagnose as enteric fever. The symptoms were the same, the mortality exceeded that of fevers distinctly malarial, they were not amenable to quinine freely given, and death from hæmorrhage from the bowels was frequent, and the intestinal lesions were what we now recognize as a characteristic of enteric fever. It was on my return to India after two years' residence in Europe that I first satisfied myself that such cases, in all their essential particulars, were none other than enteric fever as that term is understood among you here. I then saw, treated, and dissected cases as distinctly enteric in their symptoms, progress, and in their post-mortem lesions, as any to be seen in the London Fever Hospital. It is useless to add that all the attempts since made to persuade the public and the profession that enteric fever is not a disease of India have had no weight with me. I now beg to hand round drawings from the facile pencil of my friend, Surgeon-Major Gillespie, made in the dead-house of an Indian hospital, from the intestines of men dead of fever of a distinctly non-malarious type, and I request you to examine the preparation in this jar, taken from the body of a young soldier who died in India, of a disease not in any essential particular to be distinguished from the fever with which in your hospitals and various fields of practice you are so familiar.

The question has arisen, Is enteric fever a new or an old disease in India? I regard this as an unprofitable discussion, and believe we might as well ask, Does the existence of enteric fever in Great Britain date only from the researches of Dr. A. P. Stewart and Sir William Jenner? No attentive student of Indian medical literature can doubt that Clark, Annesley, Twining, and many others, saw, treated, and dissected cases of enteric fever. There are more Europeans now in India of the enteric-fever-liability age than in the days of the men I have named; we study disease from better stand-points, with more light, and better means of research, and, as a natural result, our diagnosis is better. This, I believe, in few words, is the whole matter.

And now as to the all-important question of causation. It has been said—I maintain on very insufficient data—that enteric fever is not a disease from which the natives of India suffer. We know that "fevers" kill more of the native races than all other diseases put together. I have just seen the annual report of the health of Calcutta for the year 1880. I observe that 441 deaths among the native population are put down to enteric fever; the health officer doubts whether all were in reality due to that disease, but I notice a significant entry—viz., 2,072 fatal cases of continued fever, many of them with diarrhœa. How many of these were cases of enteric fever? The deaths of thousands of natives all over India are put down as due to fevers, but the exact nature of the fever is known only in the comparatively small number of cases diagnosed by competent observers. I am not here to dogmatize, and to assert that enteric fever in India has no other factors than those with which sanitarians have made us familiar at home; but this I do say, that those who assert that the fever, so fatal to our young soldiers in India, cannot in any case be traced to pythogenic origin, have a difficult thesis to maintain. Few in this country doubt that this fever, in the language of my colleague, Professor Aitken, is generated by a specific morbid cause, fostered by, if not actually born of, decomposing nitrogenous matter. I ask, is there any country in the world in which decomposing nitrogenous matter is more abundant than in India? If so, what is there in the nature of the climate, in the various soils, to hinder the birth of the *contagium* of this disease, that elsewhere owns decomposing nitrogenous matter for its parent? It is said, and truly said, that the present sani-

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