to become visible. The shade of colour is bright when the inflammation is active, dull or bluish when it is sluggish and chronic. The swelling results in part from the distended condition of the vessels, but chiefly from the effusion of the constituents of the blood into the surrounding tissues; it is, therefore, greatest in parts where looseness of texture offers least resistance to the passage of these fluids into them—as the scrotum, the neighbourhood of the eyes, and the face. It is at first soft, being occasioned by the presence of serum, which extends to a considerable distance from the inflamed part; then it becomes firm in the middle, in consequence of the effusion and coagulation of fibrin or lymph; and, thirdly, the existence of a soft central swelling, with an indurated circumference, is a tolerably sure sign of the presence of pus.

I must not omit to mention another important symptom and effect of inflammation, which is nearly allied to the alteration or suspension of nutrition,—I mean, the alteration or suspension of function. The inflamed brain either acts in a disorderly manner, giving rise to erroneous ideas and convulsive movements, or it ceases to convey impressions to the mind, and to exercise volitional sway over the muscular system; and an inflamed muscle refuses obedience to voluntary impulses—it cannot act. The alteration and suspension of secretion is still more obvious, and constitutes one of the symptoms on which the physician most relies, in forming his diagnosis of the inflammation of an internal organ.

I have described to you the symptoms of inflammation, as they commonly occur, illustrating them by the instance of inflammation of the finger, which may be taken as a fair specimen of the disorder in its complete or fully developed condition, where its chief symptoms are presented to our observation, but I should greatly mislead you if I were to induce you to imagine that this protean disorder always presents the same symptoms, or follows the same course. Any, or indeed all, of the above-mentioned symptoms, may be absent, and we are sometimes cognizant of the existence of the process only by witnessing its effects; still, more frequently, the inflammation is seated in a spot concealed from our view, so that we can gain no information respecting the redness, the heat, and the swelling, and have to take as our guide the pain, the most uncertain of all the symptoms, because it is the least likely to be proportionate to the severity of the disease, and because it is communicated to us through the medium of the patient.

In its effects also, inflammation is no less variable than in its symptoms; in one case effusion, in another, ulceration, takes the lead, and in a third the invasion of the disease is quickly followed by mortification, without our being able to offer an explanation of the particular result in either case. I cannot present a more striking illustration of the numerous forms in which inflammation may occur in a single tissue, than by directing your attention to the diseases of the skin. A chilblain, carbuncle, erysipelas, and lepra, are all inflammatory affections, yet how greatly do they differ from one another, and how little do we know of the causes upon which these differences depend. Equally

unintelligible, in the present state of our knowledge, are those peculiar changes in the blood, dependent (as we suppose,) upon the influence of subtle poisons, which attach to inflammation the distinctive characters of scarlatina, measles, small-pox, and syphilis.

THE

CONTAGION OF ASIATIC CHOLERA.*

By E. O. SPOONER, Esq., Blandford.

If we examine the official reports of the cholera in Great Britain for the *first nine weeks*, from September 28th to December 1st, we shall find the number of cases 1,590, of which 741 have died. Of these cases the metropolis gives 413, and the deaths 225. The whole number of cases reported at this period of more than two months, after the first invasion, in the remainder of England and Wales, is only 148, and the deaths 77. The proportion is more than 3 to 1; yet, when we further take into consideration that the metropolis contains two millions and a quarter of inhabitants, while fifteen millions inhabit the cities and villages of England and Wales, (a large number living in towns far worse than the metropolitan district in point of sanitary arrangements,) we must be still more struck with the disproportionate mortality nine weeks after we had been told the epidemic influence had reached the shores of England!

In Scotland, or rather in about seven localities there, are reported in the same time 1,011 cases and 439 deaths, yet, all Scotland does not number three millions. Here again we have 3 to 1 against Scotland. If we take into account the number of pest vessels which have entered the mouth of the Thames and the Firth of Forth from the infected ports on the continent, *we shall be able to explain this anomaly.*

Each focus of contagion has its own radius. The intercourse of the Firth of Forth soon reaches Glasgow and Dumfries. The first nine weeks of cholera in England, however, clearly show that for the most part, those places only had been then visited which have *primarily* had a large tonnage of *northern shipping.*

The *secondary spread* of cholera by inland communication with our own affected ports has not yet penetrated very far into the interior of the country. A tramp arrives at Hertford or Reading, and originates a few cases; or a family with the disease about them, migrate from London to Offchurch, in the neighbourhood of Leamington, and, with indecent haste, Leamington is reported by the Board of Health to be cholera smitten.

Indignant contradications to these misstatements are common enough, and it is scarcely worth while to say that Portland, some eight weeks ago, was reported in the cholera list.

The not uncommon occurrence of the death of a child of three years old, after a few hours illness, induced the Board to make this evidently erroneous statement. They must at that time have been extremely anxious for country cholera cases to put this down as one of Asiatic cholera. They have not been able to report a case there since. It has been evidently the object of the Board, in order to prop up their theory of the local origin of cholera from defective sanitary arrangements, to make it appear that here, there, and everywhere in England it was going to shew itself. The laws of its contagion refuse to bend themselves to their sanitary theories, and country cholera cases in England, after three months importations, have hitherto been rare. The next three months, however, will reverse the picture.

I have already observed that the non-contagion of cholera, though it has been puffed off *pro bono publico* in the companion to the "British Almanack," as a "most blessed" discovery of the sanitary commission, was the general belief of the Indian surgeons previous to 1824. In 1824, Dr. Krabinski, an Armenian physician, residing at Tiflis, in a letter addressed to the Archbishop of the Armenian Church, propounds the same dogma. "This disease is not contagious, like the plague and other diseases." He had seen much of the disease, both in India and Persia, but in the warmer climates of the east so rapid is the diffusion of contagion, that the spokes of its chariot wheels are not visible. Dr. Reumann, however, in the same year, published for the Russian Government two volumes of valuable reports on the "Progress of Cholera from 1817 to 1823, when it appeared at Astrachan." He was one of the most distinguished of Russian physicians, and with the electric scrutiny of a logical mind, he detected what was invisible to Krabinski. He says,—“We deem it, nevertheless, to be contagious, and there is no doubt but that it was brought by the merchandize and people in the boats to Astrachan, yet it is not so infectious as the plague.” Questions of this controverted character are not to be settled on the mere dicta of authorities, and though the Sanitary Commission, and the Board of Health, have adopted the Krabinski dogma, we must be permitted to enquire into the new advices received at the foreign office upon which their conclusions are grounded.

Let us examine the most recent, published in the Appendix to the second report of the Sanitary Commission. Here we find diametrically conflicting testimony; one of the deponents is, however, merely a British Vice-Consul; the other, an experienced British physician. Mr. Vice-Consul Bassam, of Mossul, in a despatch dated January, 1848, says,—“From what I have seen of this disease it does not appear to be in the least degree contagious, and scarcely, if at all, infectious, unless, perhaps, where many sick are congregated together, as in the wards of a hospital; in

fact, I should consider it as an almost true epidemic, such as the influenza which has prevailed here since the last cholera disappeared.” Quite a Coryphæus in the favourite doctrines of Gwydyr House is this Mr. Vice-Consul Bassam, the eminent professional authority at Mossul! Alas! medical men very seldom obtain the Consular or Vice-Consular dignity. Her Majesty's Consul at Archangel is more modest, and requested Dr. Mackenzie, Staff Surgeon, to answer the question. He says, very distinctly and pointedly, —“The majority of facts collected during the present visitation of the cholera confirm the opinion that the disease spreads from person to person, through the medium of the air, and there is every probability that the principle of infection finds its way into the system through these different media,—viz., the respiratory organs, the digestive organs, and the skin.” Such is some of the evidence of “the gradual abandonment of the opinion that the propagation of epidemic disease is mainly dependent on a specific contagion emanating from the bodies of the sick!” Thus, we see then, the most recent advices do not bear out the conclusion announced with so much emphasis by the Sanitary Commission, yet their epitomist, in the “Companion to the Almanack,” (I suppose one of themselves,) bursts into the rhapsody,—“Blessed discovery!” “It is impossible to over-rate the importance of this discovery.” He adds, in the same Io pæan style,—“The non-contagious character of cholera is now then considered sufficiently established, and how many blessings follow in the rear of that one great blessing.” We cannot, for the life of us, understand the consolation which the public can receive from knowing that they may at any moment, in any part of England, breathe this “poisonous gas emitted under certain conditions of heat and moisture, from decaying animal and vegetable matter, unduly exposed to its influence;” that they may meet with it “going up one side of a street,” or “waiting for the opening of a door before it enters the house!” This seems very sorry comfort indeed, though it may be deemed a most wonderful discovery! One would imagine from Dr. Parkes, that the safest place was the bedside of the sick, rubbing their cramped limbs, or emptying their close stools, for “not one of these hospital attendants,” says he, “were attacked.” “The medical officers were constantly on duty, yet enjoyed the same immunity.”

They were unsusceptible enough to escape such a subtle enemy as the epidemic atmospheric influence, and yet we are told their escape proves the disease non-contagious! As reasonably might we conclude that the well-seasoned nurses of our scarlet-fever and small-pox patients, who do not catch the disorder, prove these diseases to be non-contagious. The mild and benignant form of cholera is a simple diarrhœa, which may be quite as effectual as the modified form of small-pox, induced by inoculation or vaccination. A

slight redness in the throat, and a scarcely-noticed rash on the skin, will for ever render a person unsuceptible of the most malignant exhalations of scarlet-fever. Secondary attacks occasionally occur; but the majority of mankind, if they have passed through the milder forms of either of these contagious diseases, are thenceforward *contagion-proof*.

The law is less universal with cholera, but it will be found sufficiently general to solve the enigma of the *occasional immunity* of nurses and *medical men* when in the closest attendance upon the sick. But that this immunity is only *occasional*, dependent on natural insusceptibility or previous seasoning, has been proved by our best authorities.

Drs. Russel and Barry, in their able report on the cholera of Russia in 1831, make the following statement respecting St. Petersburg:—"The number of medical men and hospital attendants attacked with cholera during the present epidemic in proportion to the whole employed, has been beyond all comparison greater than in India, under similar circumstances. Twenty-five medical men have been already seized, and nine have died out of two hundred and sixty." Notwithstanding the early attention to the first symptoms which medical men and hospital nurses receive, *many deaths have already been recorded in England and Scotland during the present epidemic. The number of mild attacks, of course, are unknown; they are considered simple cases of diarrhæa; but they are, in fact, mild attacks of cholera.*

We are glad to find the eyes of the medical profession in Scotland and in Ireland are beginning to open to some of the fallacies of the Board of Health and the Sanitary Commission, and we are gratified to know that a man of Dr. Simpson's acuteness has personally investigated the origin of cholera at Newhaven, though his coachman narrowly escaped the formidable effects of the contagion. It is true four of the nurses of the Edinburgh Cholera Hospital were within twenty-four hours (according to Dr. Sutherland,) poisoned by the emanations of the crowded wards. Three of the four died of Asiatic cholera; but Dr. Sutherland says very coolly "we don't call this contagion!" "I warned them of their danger from the bad smell I detected," and the Sanitary Commissioners have testified to the "general immunity of nurses and other attendants on the affected!"

One of the most recent and graphic descriptions of cholera has been afforded by Mr. Thom, in his Report of the causes, character, and treatment, of the spasmodic cholera in H.M. 86th Regiment, at Kurrachee. Among the arguments brought forward by this gentleman, occurs the following passage:—"It was equally apparent that there was nothing contagious in the nature of the disease, for instead of a few cases appearing first, and the disease gradually spreading, it suddenly burst forth in a few hours in every European regiment,

whether in camp or barracks,—in every tent, and in every house; and it was at its acme in forty-eight hours after, when, instead of spreading further, it gradually declined. Now, it appears that for some days or even weeks, A FEW CASES HAD APPEARED IN THE NATIVE TOWN OF KURRACHEE, but there, about the same period, the malady became suddenly general over the whole place." Now, it appears the few cases appearing for some weeks within the town, were evidently the first *germs of the disease*, which atmospheric causes rapidly ripened and diffused, and entirely do away with the inference drawn from *apparent suddenness* of the general attack. But taking for granted the fact related, that a "few hours were sufficient to spread the disease in all the regiments," we would point out as a complete contradiction to Mr. Thom's deductions respecting the non-contagion of cholera, the circumstances observed in England;—the "few cases occurring first," and "then the disease gradually spreading," is the *universal law of the disease as it has hitherto shewn itself in this country*. The slow progress of the first few weeks of cholera, and the gradual diffusion of the disease at the present time, constitute the very circumstances which Mr. Thom points out as conclusive evidence of contagion. If the argument be a good one as employed by Mr. Thom negatively, it must surely be better still when we find the direct reverse of the picture positively occurring in this country. But the facts are still further explained by Mr. Thom himself. "Long before its visit to Kurrachee," says Mr. Thom, "we heard of its appearance in the Madras Presidency, and subsequently at Poonah, Bombay, and Ahmedabad, Deesa, and finally arriving by the coastward to Scinde, where it first broke out at the sea coast, and from thence gradually extended to Hyderabad and Schwaun."

Now, its gradual advance in the lines of commerce, and its first invasion of the sea-coast of Scinde, are both opposed to the suddenness of the outbreak. The fact is, for a fortnight previous to the arrival of our regiments, the cholera had existed in Kurrachee, in a mild and insidious form. The stagnant atmosphere was loaded with the poison of the pestilence, and no wonder in an Indian climate and in crowded cantonments, the troops were soon rapidly affected. But another remarkable fact which Mr. Thom relates as a proof of its non-contagion, was the comparative escape of the officers. In addition to their being *well fed with animal food*, and better nourished than the men, they were quartered in "houses detached and isolated," and "permitting a free current of air." Can any evidence be more decisive in favour of isolation? If the cause were *wholly* atmospheric, why were not the officers seized as well as the men? They were actually between the two fires,—between the 60th regiment and the Sepoys," both of which suffered severely. Their better nourishment, the isolation, and the

ventilation were the concurrent causes of exemption. The first rendered them less susceptible of contagion, the last two put them out of the reach of infection in such a concentrated form as to produce the disease.

We have read loose statements with regard to the immunity of medical men in India. Putting out of the question the attacks of *diarrhœa*, which we should call *mild cholera*, which were no doubt promptly treated and arrested in the cases of the medical men, we find four out of forty engaged in Scinde, were carried off by the disease. Four out of forty or ten in a hundred is a larger proportion than the number of fatal cases, either in the Sepoy or European regiments. Thus, not only is the fact of the "greater numerical proportion" falling sick with *diarrhœa*, attested by Mr. Thom, but his own statistics clearly prove that the medical officers suffered a greater proportion of mortality than the other officers, and even as great as that of the sepoys, notwithstanding their prompt attention to the first symptoms. This new evidence, then, when carefully sifted, corroborates the statements of Drs. Barry and Russell, whose personal experience, though not so recent, was far more extended. They had witnessed the disease first in the climate of India, where the heat and moisture of the atmosphere combined to render the contagion of cholera most rapidly diffusible and miasmatic, but they added to that experience the most extended observations on the same disease as it appeared in a Russian climate. At St. Petersburg, in 1831, they state, "The number of medical men and hospital attendants attacked with cholera during the present epidemic in proportion to the whole employed, has been beyond all comparison greater than in India under similar circumstances; twenty-five medical men have been already seized, and nine have died, out of two hundred and sixty." The subsequent history of the epidemic in St. Petersburg increased these proportions; while in Paris, where the non-contagious doctrines had taken deep root, the mortality of the medical men was still higher, and even the Prime Minister himself, Casimir Perrier, fell a victim to an insane perambulation of the cholera hospital, which he took with other officers of State, to assure the public mind that the disease was not contagious. Buonaparte, at Jaffa, was more fortunate, when for similar reasons he breathed for a few minutes a well-ventilated plague-ward of his military hospital.

Anticipating the future spread of the Asiatic cholera throughout the land, when the warm sun of the spring and summer shall ripen the latent germs of this contagion into its acme of malignancy, I cannot but view with some degree of anxiety those non-contagious doctrines which emanate from men in high authority, whose position enables them to influence the opinions of the public as well as a large portion of the profession. Though many books and reports have been written within the last three years on the subject of

cholera, very little has been added to our previous knowledge. Cases of collapse are as uncertain in their result as they were in 1832. The importance of attending to the early symptoms was as clearly taught in that day as it is now; the propriety of ventilation and cleanliness was as universally acknowledged as it is at present. The immediate removal of those attacked with choleric *diarrhœa* into a clean and airy apartment, ought to have formed one of the special directions of our Board of Health. We think that such a plan at Tooting would have mitigated the calamity, but the non-contagious doctrines had unfortunately perverted the mind of the medical attendant, and though he has since changed his views on the subject, a fatal experience has been necessary to remove those errors which men of high position and standing had too carefully propagated.

Since the Asiatic cholera emerged from the confines of Jessore, in 1817, it has not ceased to make its appearance in some quarter or other down to the present time. Thirty-two years of its history have enabled us to track its steps from the extremities of Asia, through the continent of Europe, along the shores of Africa, and even in the New World. From the banks of the Ganges it passed into Burmah and China to the East, while towards the West with equal steps it reached Persia, the shores of the Caspian, the Russian empire, and the various countries of Europe, appearing in America, on its first and last invasion, soon after it had reached the Atlantic ports of Europe. No other disease, besides the small-pox, ever took so wide a range, or continued to exist for so long a period. It possesses the same analogies, in time and space, and unless human ingenuity discover some prophylactic, it bids fair to rival its predecessor in fatality also. It has ever travelled with the measured steps of the human traveller; not with the monsoon, but sometimes against its current;—not with the hurricane, nor with the earthquake. It has not taken any track of volcanic action nor electric movement; it passes toward all points of the compass, irrespective of magnetic or diamagnetic lines of force. In Russia it travelled at the rate of between two and three hundred miles a month, (from 400 to 500 kilometres.) It crosses the Atlantic, taking the line and the mode of conveyance of emigrant ships. It was well observed by the *Quarterly Reviewer* in 1831, that it had "a marvellous penchant for easy travelling." It delights, too, in social intercourse; and in India accompanies marching armies or their camp-followers; and is very fond of grand festivals, whether at Juggernaut or Mecca. But though India had its Juggernaut festivals for ages previous, cholera never broke out among its congregated masses till after the *leaven* of 1817. Our armies had often crossed the peninsula of India, but never had they been decimated by the cholera till the forces of the Marquis of Hastings were very nearly put *hors de*

combat, by marching into the focus of its contagion, in 1818; and though since the death of Mahomet pilgrimages to Mecca have been annually made by multitudes of the Moslem population, cholera never appeared among its caravans till subsequent to its appearance in Persia, in 1819. Can we draw any other conclusion from premises so well defined and universally acknowledged, than that a disease thus portable must be also communicable?

It is universally attributed to *human intercourse*, from the boats of Astrachan, to the tramp lately confined in Hertford gaol, after whose introduction it broke out in three days among the prisoners. Belfast and Offchurch, Leith and Staten Island, tell the same story. It is nowhere bred and multiplied till it has been *first imported*. The tell-tale newspapers constantly reiterate this statement; can anything but a *contagious principle* account for such uniform testimony? Can any disease be portable and reproductive without being contagious? Do we ever hear of the agues produced by the fens of Lincolnshire travelling over the Cotswold Hills? Malarious influence is confined to the localities which generate it; *it is the property of contagion alone to defy all climates*, and such has been the case with the Asiatic cholera, wherever its poisonous *leaven* has been imported. Cesspools and stagnant ditches,—the bogs of Ireland,—the back slums and sewer mouths of London, and Glasgow, and Edinburgh, were all as numerous and uncleansd in 1847 as in 1848, and soup-fed pauper children were crowded in masses then as now, but no pestilential cholera was generated under all the conditions of vegetable and animal putrefaction, in that, or in ten preceding years. The efficient cause was absent; the *contagion* was not imported till the autumn of 1848.

That diseases exist which are *both epidemic and contagious*, cannot be a matter of dispute among scientific men who understand the meaning attached to those terms. If an epidemic disease be defined, as some writers have done, as a disease not capable of being propagated by contagion, and if Asiatic cholera be considered as such an epidemic, it is at once begging the question. We deny *in limine*, the major proposition. The definition is erroneous. Small-pox is *often an epidemic*, and yet is invariably propagated by contagion. Cholera we allow to be an epidemic disease, and its whole history from 1817 to 1849, demonstrates that it is a pestilence which is propagated by contagion, or in other words, that the *emanations from the sick are capable of communicating the same disease to the healthy individual*! Now, small-pox has never been absent from the earth since the time of Mahomet, when it first appears to have commenced its destructive career. Since that early period it has always been found, and though it was once fondly anticipated by those who founded the practice of vaccination, that like the dodo, it would by and by become extinct; the

history of its progress and continuance have hitherto falsified the anticipations of the enthusiastic philanthropist. The "sweating sickness," and "the black death," which committed such ravages during the middle ages, have entirely disappeared. They *may have been* epidemics without the contagious property,—without the power of reproducing and perpetuating their kind, and when the conditions of their production ceased they may have died a natural death. It is impossible for us now to settle the question of their contagion upon any satisfactory basis. The facts are wanting on which to build a perfectly satisfactory conclusion. It would have been fortunate for mankind if the cholera had left us equally in the dark.

The epidemic invasion of the Asiatic cholera in 1831, was first reported officially in the first week of December in that year. The last week in March, 1832, (March 29th,) presented the following statistics of the disease:—

Total number of cases	8815
Total number of deaths	3320

This table exhibits the extent to which the disease had extended on its first (generally the most severe,) visitation during the period of four months. The view taken by the Board of Health of that period, as well as by the majority of medical men in England, was, that the disease is contagious. The garments and bed-clothing of the sick were strictly purified if not destroyed, and the communication between the sick and the healthy was as much as possible suspended. No such recommendations have been issued by the present Board of Health, and the community have been taught to believe the whole system of quarantine public and private, national and domestic, quite useless and impracticable.

What has been the result of the new system adopted by the Board of Health now in existence?

The Asiatic cholera began its ravages in the first week of October, 1848. Up to January 27, 1849, *rather less than the same period*, the following results have been registered:—

Total number of cases	10,047
Total number of deaths	4,467

It appears thus pretty clearly, that our present Board of Health have been quite unsuccessful in their plans so far as the arrest or the mitigation of the disease goes. We shall by and by enquire into the sources of their failure. We have had, instead of a diminution of the disease, an actual increase of one-fourth during the first four months.

(To be continued.)