

damp situations, vegetable and animal effluvia, bad water, imperfect ventilation, and deficient food, acted as predisposing causes, in giving rise to this intractable malady among the people, epidemic atmospheric constitution was necessary for its very general diffusion. The atmosphere is the principal channel by which cholera is disseminated, though the human recipient of the morbid miasm occasionally becomes, as in yellow fever and influenza, a secondary agent in propagating it. That it was so propagated sometimes, even in India, he had every reason to believe. When cholera was prevailing at Tannah, in 1818, the soldiers of a confined, ill-ventilated barrack-room in the garrison were attacked by it, in succession, as they lay along side of each other on their beds; and here infection seemed to act a subordinate part in the diffusion of an epidemic disease, not primarily infectious. Such, too, seems the nature of infectious yellow fever, arising out of endemico-epidemic fever of malarious countries, as that of Sierra Leone, where, as shown by Dr. Bryson's convincing evidence, the infectious fever which prevailed at different times on board her Majesty's ships, *Bann, Eden, and Eclair*, grew out of, as it were, the common endemic of the country. Cholera, moreover, in India, is admitted on all hands to attach itself to masses of the people assembled at religious festivals, and to be disseminated by them to persons previously free from it. In the extensive district of deep black alluvial ground, called the Southern Mahratta country, cholera, in 1841 and 1842, so invariably attacked the Madras regiments marching through it, that it came to be considered endemic to this part. It appeared to creep at this time from village to village, and was carried by bodies of religious pilgrims from district to district; yet, in the face of such strong characteristics of infectious disease, some have endeavoured to explain away the evidence by supposing that a specific poison, the essential cause of cholera, can lie dormant everywhere till accessory causes give it activity; but when not propagated by human contact, there is no satisfactory evidence to prove that this disease has any other source than a malarious and epidemic origin. He would only make one more remark on the subject of the disease becoming transmissible, under favourable conditions, from the sick to the healthy—namely, that having observed how cholera continued prevalent among the men and followers of native regiments attacked by it on their march, and allowed, immediately after arrival at a new station, to occupy the regimental lines of native mud huts, he recommended to the general commanding the division that all such infected regiments should be encamped in some dry and healthy locality, outside the cantonment, till all traces of the disease had disappeared, after which they were allowed to occupy the regimental huts. This precaution was followed by the happiest results; for, after its adoption, the men and followers of regiments which had suffered from cholera on the march were altogether exempt from it in the lines. A combination of conditions may be necessary for the development of infectious cholera, but that it cannot frequently be self-multiplying in the human body seems an assumption contrary to fact.

#### THE BODIES FOUND IN CHOLERA EVACUATIONS.

Dr. LANKESTER said that Dr. SNOW's theory of its progress and development involved the necessity of its being something generated in the mucous membrane, and capable of being diffused by handling, and especially through drinking-water. It was not more unlikely that the mucous membrane in cholera should produce a poison, than that the skin should in small-pox. No such poison had, however, yet been demonstrated to exist, and the only approach to it was the announcement of the presence of fungi in the evacuations and vomited matters of cholera patients, more particularly mentioned by Dr. SWAYNE. These bodies might be divided into two classes—the definite and indefinite. The latter consisted of all the bodies found in the air and the water, and which were probably organic substances of various kinds, and the smaller bodies from the evacuations and the vomited matters of cholera patients measuring from the  $\frac{1}{10000}$ th to the  $\frac{1}{1000}$ th of an inch in diameter, and which consisted of various organic and inorganic matters. The definite bodies were such as those exhibited by Dr. SWAYNE at the last meeting of the Society; they were probably from the  $\frac{1}{100}$ th to the  $\frac{1}{1000}$ th of an inch in diameter. Amongst these bodies, his friend, Mr. BUSK, had succeeded in making out three forms. First, there were spores of a species of *uredo*—a fungus which produced smut in corn, and was often found in bread. These bodies appeared to be only drawn in Dr. SWAYNE's illustrations. Secondly, portions of vegetable membrane, of a dark colour, which resembled the membranous portions of a grain of wheat, and which were seldom absent from the finest flour, but were very abundant

in the coarser kinds. Under a high magnifying power and deficient light, these bodies resembled the last. The third form of these bodies resembled starch granules. The two last bodies were evidently not independent organisms. He had examined Mr. BUSK's preparations, and compared them with those of Dr. BRITAN and Dr. SWAYNE, and he felt convinced of the correctness of Mr. BUSK's inference, that no new organism had yet been demonstrated to exist in the body of those affected with cholera. All the bodies that had been observed by the microscope were evidently introduced by the food or were the natural products of the mucous membrane. He thought we must look in some other direction for the poison of cholera.

Dr. WEBSTER and Dr. SNOW having replied, the Society adjourned.

#### THE BRISTOL MICROSCOPICAL SOCIETY, *VERSUS* THE PRESIDENT OF THE MICROSCOPICAL SOCIETY OF LONDON.

To the Editor of THE LANCET.

SIR,—In the *Athenæum* of last Saturday, there is a notice of a communication from Mr. BUSK to the London Microscopical Society, which notice commences with the flippant remark, that "it would seem that Mr. BUSK has performed the funeral obsequies of the cholera fungus," and concludes with a warning to young microscopists, who are informed "that the use of the microscope is not to be learned in a few weeks."

Now, SIR, as a member of the Bristol Microscopical Society, I feel called upon to notice such an imputation upon the skill and experience of three of our most industrious members, two of whom—viz., Drs. BUDD and SWAYNE, have belonged to the Society since its formation in 1843, and have each successively filled the offices of vice-president and president, the other gentleman, Dr. BRITAN, being at this time the honorary secretary.

Mr. BUSK, in his communication, disposes of the so-called cholera fungi in a very off-hand manner, by resolving them into their three elements of smut, starch, and bran, before consigning them to the tomb of "all the Capulets." And he states—1st, that the more perfect cells which are rarely met with are merely specimens of the "*uredo frumenti*" from bread; 2ndly, that the more imperfect cells usually found are nothing more than the inner coating of bran; and 3rdly, that the smaller and more delicate bodies are merely broken grains of starch: thus he endeavours to account for the bodies found in cholera evacuations without condescending to notice those found in the air or water of cholera districts. Now, SIR, I think it would not be very difficult to show that Mr. BUSK's "bran new" theory is entirely unsupported by facts. I have seen specimens of the "*cholera cells*" first mentioned, and have compared them with the several kinds of *uredo*; the only one at all resembling them is the "*uredo caries*," which, like them, has external projections and thick coats. It differs from them, however, in every other respect, not being above one-tenth part of the size—in fact, bearing about the same relation to them as a pig does to an elephant; and I suppose that even Mr. BUSK would hardly consider these animals to be identical, merely because they have both thick skins.

With respect to the other cells, they have been compared with both bran and starch, and are not found to agree in any one respect, not to mention that the polariscope and iodine serve effectually to distinguish starch grains from any of the bodies that may resemble them. But then, Mr. BUSK accounts for a fancied resemblance between them, by insinuating that they appear similar "when viewed with a sufficiently high power and a sufficiently bad illumination;" just as if good oil or gas were not to be had in Bristol, or as if the sun shone less brightly there than through the murky atmosphere of Greenwich or Blackwall. Such are the statements and insinuations put forth by so accomplished a microscopist as Mr. BUSK, and they are only to be accounted for by supposing that even his optical instruments are not always perfectly achromatic, but are apt occasionally to impart a tinge of green, especially when he is looking at the labours of his brother microscopists in the provinces.

But, SIR, (joking apart,) the question as to the nature of these bodies, and their relation to the fearful disease of malignant cholera, is not to be set at rest by the supercilious *ipocritiz* of any one individual, however talented he may be, but will require for its solution the patient and persevering labours of many well-qualified observers, who have a sufficient distrust of their own powers to examine with care and caution every avenue that might be likely to lead them into error and self-deception.—I am, SIR, your obedient servant,

A MEMBER OF THE BRISTOL MICROSCOPICAL SOCIETY.