Mr. HUNT suggested that these cases bore some analogy to those instances of lumbago and pleurodynia unaccompanied by fever, and usually the result of great muscular exertion.

Mr. HEADLAND, to show the peculiar manner in which the muscular system might be affected by hysteria, related the case of a young lady who was brought to him from school, in consequence of her inability to use her right arm. On examination, the head of the humerus was found in the axilla, the deltoid flattened, and the muscles so rigid that the fore-arm could not be flexed; this was not the case with the wrist. She had received no injury; the limb had become gradually stiffened and useless, without any known cause. The appetite and all the functions were in a healthy state. She had been rather more anxious than usual about her studies. The humerus was returned to its natural condition by force, but returned again to its position on withdrawing the antagonistic power. Judging it to be a case of hysteria, she was placed under some preparation of steel, was well nourished, and gentle friction applied to the limb. A certain amount of improvement followed, and the head of the humerus rested on the edge of the glenoid cavity. A residence of three months at the sea-side, without medicine, cured her. Mr. Headland expressed his opinion that cures were attributed to the use of the medicated vapour-bath, without just or reasonable cause. He could not regard Dr. Bird's cases as rheumatic.

Dr. BENNETT regarded the case as a very peculiar one; no doubt it was hysteria, though the usual symptoms of that condition were absent. He believed the nervous system had been improved by the chalybeates.

Dr. G. BIRD said the case bore no analogy to those he had related. In his cases the cause was cold, the pain in all the limbs severe, and the cure decisive, in a few days, from the vapour-bath. He was inclined to agree with the view taken by Mr. Hunt, and to call his cases by the inexpressive name of "rheumatalgia."

Mr. STEDMAN had seen the case detailed by Mr. Headland. It was very curious to observe the gradual way in which the dislocation was reduced by friction, and the equally gradual manner in which the head of the bone again slipped out of its cavity when force was withdrawn.

Dr. BENNETT related the case of a lady suffering from severe hysteria. She had been put into mechanical training and restraint for a painful affection of the ankle-joint, unattended by swelling. She got well from iron and sea-bathing.

LARVÆ IN THE EVACUATIONS.

Some conversation took place respecting the presence of larvæ in the evacuations.

Dr. BIRD said, that in Ireland, the larvæ of the churchyard beetle were occasionally swallowed by persons who ate portions of earth from the "graves of the saints," and were found in the evacuations. The earth in question was said to cure hypochondria and hysteria.

Dr. CRISP, to show how careful we should be in deciding the character of bodies evacuated from the bowels, related the case of a girl who, after eating repeatedly of snails, for consumption, voided some curious bodies, which were examined by several persons, and puzzled them much. Eventually they were found to be nothing more than the undigested male parts of generation of the snails which the patient had swallowed.

MONDAY, OCTOBER 15, 1849.—MR. HANCOCK, PRESIDENT.

DR. CLUTTERBUCK ON CHOLERA.—DR. AYRE'S TREATMENT.

Dr. CLUTTERBUCK, in rising to make a few observations on cholera, trusted that the subject would, now that the disease was on the decline, be discussed in a calmer spirit than it had been on previous occasions. Firstly, what knew we of the nature and origin of the disease; and, secondly, how should we treat it? In answer to the first question, the origin of the disease was so obscure, that we could only regard it as an epidemic; but this gave us no insight into the real nature of the affection, into, in fact, the physical causes of it. In answering the second question, we might make this inquiry as a preliminary step. Do we know enough of the disease to treat it on any principle? He thought not; and hence he was opposed to vague, uncertain, and violent treatment; such as the calomel treatment in large and often repeated doses, opium in excessive quantities, and, in truth, to all specific modes of treatment whatever. He firmly believed that treatment of this character had conduced to a greater mortality from the disease. Having no certain guide to follow, then, except observation, he had come to the conclusion that one of the most essential points in practice was, not to do harm. The disease was incurable; no remedy could be relied on; and, therefore, all we could do was to palliate, with the hope of bringing the disease to a favourable termination. With these impressions, then, if the disease commenced with vomiting, he should encourage it by mild means; so if purgation existed, he should use the mildest purgatives in assisting that effort of nature to throw off the disease. We should take the symptoms given by nature as our indications of treatment. When the vomiting and purging had existed a short time, he should administer opiates very carefully, and take means to restore the animal heat. He believed that, as a general rule, those cases had done the best in which we palliated with mild means, and gave the disease time to subside without destroying the patient.

Mr. HEADLAND considered that much injury had been done to the medical profession by propagating statements respecting the inefficiency of medical treatment in cases of cholera. He contended that the remedies and measures suggested by our profession had been of the most essential benefit to the public. He regretted that the government had not availed themselves of the assistance of the College of Physicians. It had been said that the profession possessed no remedy for cholera. Why, what remedy had we for any disease, except sulphur for the itch? Did we possess a remedy for scarlet fever, for small-pox, or for any one disease? The answer must be in the negative, and therefore our ignorance of the nature of the cholera poison was not more decided than it was of the poison of other diseases. In each and all of them we only saw the results. We observed that it was a poison, and if the person affected with it was not strong enough to resist its effects, he died, and this in spite of all, and any, medical treatment. But did not the same fact obtain with respect to scarlet fever? Many cases were too sudden in their seizures to allow of available medical treatment, but in others it could be and had been resorted to, with success. He still would assert that the public were under deep obligations to the profession, for the promulgation of a knowledge of those preventive means, in regard to water, sewerage, and other causes, on which the spread of the disease mainly depended.

Dr. GOLDING BIRD remarked that the Report of the College of Physicians on Cholera was about to be published, and he would not anticipate any of its details. He agreed mainly in opinion with Dr. CLUTTERBUCK on the value of doing little in the disease. As with cases of typhus fever, the main point was to get the patient to live until the disease had worn itself out. He agreed, also, with Mr. Headland, that the true mission of the medical profession—a mission which they had faithfully fulfilled—was the prevention of the spread of the disease by the promulgation of hygienic laws. Bad drainage, low situations, filthy and narrow streets, were among the chief causes, in addition to impure water, of the spread of cholera. With respect to the calomel treatment—that by small and often-repeated doses—he had seen much of it, both at home and abroad; he had tried it himself, carefully, assiduously, and constantly; and he had come to the conclusion that it was of no benefit whatever. He could only reiterate the opinion of his colleague, Dr. Hughes, on this point.

Mr. MIDDLETON regarded the diarrhœa, with cramps, and the cholera, to be one and the same disease, but affecting different persons differently.

Mr. HIRD could not agree with Dr. Clutterbuck respecting waiting for a favourable change in cholera; for when cholera was fully developed medicines were of little or no utility. He had, in the epidemic of 1832, employed calomel both in small and large, and often-repeated doses. The plan of treatment was utterly useless. Would not the encouragement of vomiting, in some cases of cholera, keep up the heart's action, and give the patient a longer and a better chance of living? If we failed in this, then we should keep up the circulation by friction, employed over the larger bloodvessels.

Some observations were made on the definition of cholera; and also as to its being a new disease within the present century. It was contended that Sydenham, Celsus, and even Hippocrates, had described similar epidemics.

WESTMINSTER MEDICAL SOCIETY.

SATURDAY, OCTOBER 20, 1849.—MR. HIRD, PRESIDENT.

CASE OF FUNGUS OF THE BLADDER.

Mr. NUNN exhibited a drawing of a fungus of the bladder. The fungus was situated a little to the right of the median line of the bladder, and of the size of a small walnut. He reminded the Society of a case produced by him last session, of a similar nature; in both cases, no other evidences of disease than the passing of blood with the urine had ever existed.

He stated, that in both cases the amount of blood passed was always increased by the introduction of a sound or a catheter. In both cases the ordinary symptoms of malignant disease were not to be observed. The inference that he thought himself justified in drawing was, that where hæmaturia existed without such symptoms as would enable the practitioner at once to determine upon the precise seat of the disease, the discharge of blood being at the same time greatly aggravated by the introduction of instruments into the bladder, there fungus might be reasonably expected. Mr. Nunn particularly wished to express the obligation he was under to Mr. Partridge, for his kindness in lending him the drawing, and for the particulars of the case.

Mr. W. F. Barlow read a paper

ON THE MUSCULAR CONTRACTIONS WHICH OCCASIONALLY HAPPEN AFTER DEATH FROM CHOLERA.

He first detailed two striking cases in which these movements occurred after dissolution, and lasted for a very considerable time. The muscles of the arms, chest, and legs, and, in one of these examples, those of the face, were observed to be affected, some muscles being much more influenced than others. Some of the movements in respect of form were not unlike those of volition. In one of these cases the motions ensued two minutes after death; in the other, a quarter of an hour. In both, the muscles of the lower extremities were first affected, and the movements appeared successively in those of other parts. Two cases, very well marked, accurately observed, and presenting very similar features to the foregoing, and which had occurred long ago, in India, were referred to. The author described those more local and transient forms of the affection which were more commonly observed; the movements might be confined to the legs, the chest, the face, to a single muscle, or even to certain fibres of it. A case of cholera was on record in which paralytic muscles had been affected by spasms. These post-mortem contractions had been stated, by an observer, to admit of excitement and aggravation by "pricking." The writer had endeavoured, in one instance well calculated for experiment, to repeat the observation, but had been unsuccessful. However, this was only a single remark, which he desired might be rated at its proper value. He had used, also, water of the heat of 150°, and of a yet higher temperature, in order to discover if the motions could be either induced or affected by it; no definite result could be obtained. Probably these motions, which had as remarkably narrow a sphere of action in some cases as they had a wide one in others, would have been much more frequently met with had they been oftener sought for. Attention was directed to the terror which they had caused to ignorant persons and persons not ignorant; they had given rise to unfounded notions of persons being buried whilst yet alive. They had been seen by friends, to their extreme amazement, as they were watching the bodies of their deceased relatives; and it was necessary, with the view of preventing groundless alarm and false conclusions, that all persons who might come in contact with the corpses of those who had perished from cholera should be informed that it was by no means extraordinary for such actions to be witnessed after death in this disease. The author had no explanation to offer of the cause or causes of these curious phenomena. For the present, they must be viewed as facts. Groundless speculations would only surround them with unnecessary mystery. He concluded by proposing a careful inquiry into all the circumstances under which they occurred; and some points were specified which it would be interesting to consider. Amongst other things, it was important to note their duration and the most protracted interval which might elapse between dissolution and their commencement.

DISCUSSION ON CHOLERA.

Dr. Sibson resumed this discussion, and considered that hitherto no general and extensive view of cholera had been taken by any observer. He hoped, however, that one master-mind would eventually grapple with the entire subject, and throw some light on the very difficult question of the nature and origin of the disease. One point, it was true, we might put our hands upon, and that was the fact, fully shown in Dr. Webster's paper, that the disease made its appearance and continued its ravages during the prevalence of one particular kind of weather, and diminished as that weather disappeared. It was proved, also, that want of all kinds, and all depressing causes, favoured the spread of the disease. So far we could go, but still the primary cause of cholera was as mysterious as ever. We found it, for instance, at Moscow and St. Petersburg, during the intense cold season, whilst at Paris it raged during the hot months. As to its origin and propagation by water, how could we explain by this cause the appearance of the disease in almost every part of England in one week. It was clear, from this fact alone, that water was not the only means of communication. Did the air act as a means of communication? It was curious if it did; yet we knew that the same state of air produced cholera and typhus, one at one time and one at another; but what cause produced the different result we did not know. He did not agree with Dr. Snow, that the primary seat of the disease was in the mucous membrane of the intestines, for often the complaint set in with the greatest intensity without any intermediate stage of diarrhœa, and, on the contrary, diarrhœa of a very depressive character might exist, and yet no cholera supervene. It had been proved, however, by the researches of O'Shaughnessy and Garrod, that if the blood were not primarily affected, it became so in the course of the disease; for it had been shown that the blood contained matters which ought to have been thrown off by the secretions. That the blood was so affected was also evidenced by the effects of the injection of salines into the circulation when the patient was in a state of collapse. In his own experience this proceeding had always succeeded in rallying the patient, who died, however, eventually, if the secretion of urine was not restored, but lived when the means of carrying off the poison returned. With respect to Mr. Barlow's very interesting paper, he would remind the Society that muscular contractions after death were not confined to patients who had sunk from cholera. Dr. Blake had found that when he injected bismuth into the circulation, the muscles continued to contract after death, and Sir B. Brodie having carried on artificial respiration in a decapitated dog, it continued in motion for an hour and a half. He had produced the same effects also by injecting tobacco into the veins. The irritability of the muscles remained after the death of the brain and nervous system, and in cases of cholera manifested itself in the lower extremities first, as they were the furthest from the nervous centres; and as the nervous force diminished upwards, so the irritability developed itself towards the great nervous centres. In cases where poisons acted at once on the nervous system, more irritability of the muscles remained than when death was slower in its process.

Dr. King had observed, in his labours as district visitor, that when, after the first rice-water evacuation, the patient had a "thin, raspberry-jam motion," he always died; but lived invariably when the motion was of the dysenteric character. He thought the bodies found in the dejections were decayed epithelial cells. He regarded the diarrhœa as distinct from cholera, and at most a predisposing cause of that disease. He thought there were two distinct kinds of cholera—one where the collapse was immediate, and the other where the diarrhœa and vomiting first presented themselves.

Dr. Holland, of Manchester, said, if the cholera depended, as some supposed, on the presence of organized bodies in a state of putrefaction in the water, we possessed a remedy for this in filtration. It had been proved that during filtration the changed bodies imbibed oxygen from the air. Thus nitric acid was formed; and this uniting with the salts in the water, an innocent soluble nitrate was formed.

Dr. James Bird said, that though ready to admit the affection of the intestinal mucous membrane to be prominently influential in the development of cholera, yet he was of opinion, from a careful analysis of successive phenomena, that this was only a secondary and progressive effect of the lost vitality of the blood, and of that congestion which followed in the pulmonary and cutaneous capillaries. While the Society had there heard the lucid statements made as to the extreme fatality of the disease, and the difficulty of investigating its nature and origin, he was not one disposed to despair of seeing a more definite and successful system of treatment introduced, if the profession, instead of expecting to find specifics for a complicated malady, which admits of none, would only seek after well-established pathological facts, from which might be ascertained, by induction, the laws which govern the phenomena of cholera, with the principles of a better therapeutic system. As to the propagation of cholera, whatever might be its origin, he had good grounds for thinking, with Dr. Lankester, that the disease was occasionally communicable from person to person, under favourable localizing conditions; and though he was not prepared to deny altogether the truth of Dr. Snow's views, that it could be multiplied through the medium of water, impregnated with the poisonous dejections of cholera patients, he could not believe that such medium of communication had more than partial effect in spreading cholera. He had now witnessed the endemic and epidemic outbreaks of this disease, in India, for a period of thirty years; and had come to the conclusion, that while endemic influences of low,

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 morbific 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 devolopment 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 $\frac{1}{1000}$th to the $\frac{1}{10000}$ 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 $\frac{1}{300}$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. Brittan 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. Brittan, 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 *ipse dixit* 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.