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 muscles were affected in assisting, in consequence of her inability to use her right arm. On examination, the head of the humerus was found in the axilla, the dislocation was reduced by friction, and the muscular rigidity 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 strength were in a healthy state. She had been on previous occasions. Firstly, what knew we of the nature of the affection, into, in fact, the physical causes of it. 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 opiate very carefully, and to restore the heart’s action, and, therefore, we could do as we 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; if it purged, he should use the mildest purgative means. 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 contended 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 attended with the most excellent results. 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 death was attributed to the use of the reme
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good iron and sea-bathing.

Larve in the evacuations.

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

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

Dr. Case, to show how careful we should be in deciding the character of the bowel evacuations, gave 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. Archer’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? 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 cause. 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 death was attributed to the use of the reme
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He stated, that in both cases the amount of blood passed was generally considerable, and that the introduction of instruments into the bladder, there frequently produced by far the greatest effect. 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 particular discussion on cholera. 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. The post-mortem contractions had been observed in living patients 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 with which the movements were sometimes accompanied, the movements being 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 experiments, to repeat the observation with some success. However, this was only a single remark, which his desire might be rated at its proper value. He had used, also, water of the heat of 100°, and of a yet higher temperature, in one or two of these motions, which might be either induced by it; no definite result could be obtained. Probably these motions, which had so remarkably narrowed 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 often 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 they were alive. They had been seen by friends, to their extreme astonishment, as they were watching the bodies of their deceased relatives; and it was necessary, without such symptoms as would enable the practitioner at once to determine upon the precise seat of the disease, that all persons who might come in contact with the corpses were to be informed 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 should be informed, by induction, the laws which govern the phenomena of filtration. It had been proved that during filtration the collapsed bodies inhaled oxygen from the air. Thus nitric acid was formed; and this uniting with the salts in the water, and parting soluble matter, it was formed. Dr. Holland, of Manchester, said, if the cholera depended, as some supposed, on the presence of organized bodies in a state of putrefaction, and this was swept away, we must expect this in filtration. It had been proved that during filtration the changed bodies inhaled oxygen from the air. Thus nitric acid was formed; and this uniting with the salts in the water, and parting soluble matter, it 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 the disease, yet he was of opinion, from a careful analysis of successful 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 directions with regard to the extermination 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 curious if it did; yet we knew that the same state of air produced cholera and typhus, and at one time produced the one and at another time the other, 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 then the constitutional set in with the greatest rapidity, and many intermediate stage of diarrhoea, and, on the contrary, diarrhoea of a very depressive character might exist, and yet no cholera supervene. He agreed with Dr. Snow's 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 material which might be thrown off by the affected parts. 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. Sir B. Brodie, from that observation, and the action 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 resulting from the death of the patient was observable 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 greater 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 progress.

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in the coarer kinds. Under a high magnifying power and under the dissecting-lens these fungi, as well as the other third form of these bodies resembled starch granules. The last two bodies were evidently not independent organisms. He had examined Mr. Busk's preparations, and compared them with those of Mr. Swayne, and he thought the producements 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 him were evidently introducing the same thing that the natural products of the mucous membrane. He thought we must look in some other direction for the poison of cholera.

Dr. Swayne 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 am called upon to set before the public, in a circumstantial manner, the facts and experience of three of our most industrious members, two of whom—viz., Dr. Budd and Swayne, have belonged to the Society since its formation in 1843, and have each successively occupied the offices of president and secretary, and the third, Mr. Busk, Mr. Busk, 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 starch, and bran, before consigning them to the tomb of "all the Capulets." And he states—"that the most perfect cells which are rarely met with are merely specimens of the 'uredo frumenti' from the bran, and also that the more imperfect cells which he found were nothing more than the inner coating of bran; and 3dly, 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 "uran 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 of the size—in fact, bearing the same relation to them as a pig does to an elephant; and I suppose Mr. Busk would hardly confound 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 brother microscopists in the provinces. But, Sir, (joking apart,) the question as to the nature of the particles of starch, and bran, found in cholera evacuations, is not to be set at rest by the supercilious ipsedixit 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.