

tactile sensibility, analgesia of certain spots, and retardation of sensibility. The sense for temperature and weight was normal; the muscular sense was considerably affected. The patellar tendon reflex was quite absent. Both lower extremities were often the seat of the lightning pains. As regards (2) trophic changes, (a) the muscles of both thighs were flabby and atrophied, but especially the muscles of the left thigh. (b). There was marked dislocation backwards of the left thigh, so that when the patient stood the upper surface of the tibia could distinctly be felt under the skin. There was no atrophy of either of the articulating surfaces, nor any new deposit of bone round the joint, as far as could be made out on manual examination. The head of the right femur was dislocated on to the dorsum of the ilium, and could be felt as a distinct round projection; it was freely movable and could easily be reduced, but very soon slipped out again from the acetabulum on to the dorsum. Owing to this dislocation the left knee was situated four inches lower than the right knee, which difference disappeared as soon as the reduction of the head of the femur was effected. The head of the dislocated femur did not seem to be atrophied, nor were there any bone deposits to be felt about the joint cavity. A mass of bone, however, of more than one inch in length was felt, situated in the sheath of the sartorius muscle, totally unconnected with the joint, but moving freely with the muscles during this contraction. As regards (3) motility, there was considerable diminution of motor power in both legs, but more in the right than in the left; the patient was, however, able to stand and to walk with the help of two sticks; his walk was characteristically ataxic; he was unable to walk with his eyes shut, and with his eyes open his gait was very unsteady, owing to the ataxy and the dislocations.

Remarks.—Though the joint affections in nervous disorder, but especially in locomotor ataxy, are now sufficiently well recognised, yet not many cases of this nature have been recorded in English medical literature, and it is only very recently that Dr. Buzzard has again drawn attention to this subject and brought forward an interesting series of cases (*Medical Times and Gazette* Feb. 14th, 1880). The chief points of interest which are attached to this case are briefly these: 1. The presence of a mass of bone situated in the course of a muscle. Such a condition is noticed in two of Dr. Buzzard's cases, but is not usually found in the arthropathies of ataxic persons (see Talamon, *Revue Mensuelle*, 1878). 2. The gradual development of the dislocation of the knee, whilst in almost all cases reported the joint affections follow an acute effusion into the joint. 3. The absence of any atrophic condition of the articulating surfaces of the bones concerned in the dislocation, at least as far as a careful surface examination could show. 4. The absence of the "crises gastriques" which usually accompany the arthropathies.

WEST NORFOLK AND LYNN HOSPITAL.

COLLOID CANCER OF MAMMA.

(Under the care of Mr. WILSON.)

FOR the notes of this case we are indebted to Dr. Blomfield, house-surgeon.

Mary Ann B—, aged forty-two, married, was admitted on March 16th, 1880, with a tumour in the breast. Her previous history was good; her mother was said to have cancer. The patient gave the following history of the mammary swelling:—About sixteen months before she first began to have pain in the right side of the neck; this was followed by pain in the right breast. She first noticed the swelling in the right breast as a small lump the size of a nut. It increased gradually, and with little pain. She thought she had lost flesh. She was, however, a healthy-looking woman. Examination of the right breast showed a hard, circular, somewhat nodulated, and fairly movable tumour, the size of a chicken's egg. There was no retraction of the nipple. The tumour occupied the posterior part of the right breast to the axillary side of the nipple. There was a suspicion of slight enlargement of a gland in the axilla.

The growth being regarded as malignant, Mr. Wilson removed the entire breasts by the usual operation. The naked eye appearance of the tumour after removal was characteristic. It consisted of a somewhat circular mass of jelly-like semi-transparent material of a greyish hue, surrounded by no very evident capsule, with delicate shreds of connective tissue between jelly-like masses.

Dr. Creighton, of Cambridge, kindly examined the tumour, and reported: "The sections show the most typical colloid condition, roundish spherical masses concentrically laminated; the whole field is made up of such round laminated bodies, and there is hardly a trace of any cellular formation."

The wound did well, healing rapidly, and the slight glandular enlargement in the axilla disappeared.

The patient was discharged cured on May 8th, 1880.

STAMFORD AND RUTLAND INFIRMARY.

LITHOTRITY, WITH RAPID EVACUATION (BIGELOW'S METHOD); TWO SITTINGS; CURE.

(Under the care of DR. NEWMAN.)

IN the early days of any novel procedure it is always well to bring under the notice of the profession cases which may yet throw some light on the value of the special treatment in question.

W. W.—, aged sixty-five, labourer in a maltkiln, was admitted April 20th, 1880. Since August, 1879, he had had every indication of stone in the bladder; blood had passed with the urine, thick matter was at times present, and he had very frequent calls to pass water with even considerable pain. If he sat very still in his chair for some time the pain was not so severe, and the need for micturition became less urgent.

On admission he was a stout, flabby-looking man, and declared that he had very good general health, as a rule, but had of late lost nearly a stone in weight from broken rest and continual pain. The urine showed sp. gr. 1025, normal acidity, cloud of mucus, no blood, no albumen, no sugar; crystalline deposit of uric acid in moderate quantity.

On April 21st Dr. Newman sounded him, and readily struck a calculus, lying rather to the right side. The next day, wishing to measure the calculus, it was caught, and measured with Thompson's scoop lithotrite. It was half-an-inch across; but when an attempt was made to shake the stone from the grip it was found that the blades had taken too firm hold of a soft stone, and the stone had therefore to be crushed before the instrument could be withdrawn. Nine grains of debris were removed in the jaws.

On April 24th ether was given, and profound anæsthesia kept up for twenty minutes. The bladder was partially filled with water, and three introductions of a screw fenestrated lithotrite for the first time, and three of the ordinary scoop lithotrite for twice more, were practised. After each crushing Bigelow's elastic exhaustor, attached to a No. 16 slightly-beaked catheter, was employed.

Twenty grains of fragments were removed. The instruments were actually in the bladder for nine minutes and three-quarters. There was frequent micturition and some urethral smarting after the operation. Twenty-four hours after the sitting he was hot and cold for three hours, and temperature (normal before) was found to be 101°. This feverish attack soon, however, passed by, and his local suffering became much less. Thirteen grains of fragments were passed within the next few days.

A week after first dealing with the case, on May 1st, the same crushing and removal of fragments were put in practice. The man was under ether for forty-five minutes, and little over thirty minutes was given to the actual operation. Eighty-three grains of stone removed at the time. Twenty-one grains were subsequently passed with urine—in other words, one hundred and fifty-six grains of lithic acid were collected within twelve days. (A single fragment, weighing twenty-one grains, was removed at the time of second operation through the evacuating catheter.)

The man had no subsequent feverishness and no trouble. The urine was noted to be perfectly clear on the fourth day after the second sitting. No remaining fragment could be detected by very careful sounding, and on May 11th, seventeen days after the commencement of the surgical treatment, the patient went home quite well.

Remarks by Dr. NEWMAN.—Few words of comment are needed. The stone was thought probably to be not very large, and lithotritry, on the older lines of working, would most likely have ended in perfect cure, but with much more of time, suffering, and greater risk. The operation proved to be more tedious than difficult, and there was no hitch in the working of the elastic evacuating bulb. I would only add that when, as in this case, there is prostatic enlarge-

ment, it seems very desirable to keep the catheter well home within the cavity of the bladder.

Medical Societies.

EPIDEMIOLOGICAL SOCIETY.

A SPECIAL meeting of the Society was held—President, Sir Joseph Fayrer—on June 15th, to discuss certain papers which had been read during the past session on the “Continued Fevers of India and Tropical Climates.” Three of these papers, one by Deputy-Surgeon Dr. J. Ewart, one by Surgeon-General Dr. C. A. Gordon, C.B., and one by Dr. W. G. Don, were printed and circulated amongst the members, together with others known to be interested in the subject, and they dealt specially with the questions whether the common continued fever of those countries was enteric fever, and if so, whether it was due, as in this country, to a specific poison associated with conditions of excremental filth.

After a few preliminary remarks by the President, the debate was opened by Surgeon-General MURRAY, who expressed an opinion that continued fever in tropical countries differed materially in its signs and type according as those attacked were natives or Europeans.—Sir WILLIAM SMART, K.C.B., referred to the marked mildness of the various forms of continued fever in the islands of Bermuda, and he gave the result of his experience in China, where, he alleged, enteric fever was recognised as a distinct disease in 1859; but as no history of its incidence upon the natives prior to that date could be ascertained, it was inferred that it had been generated amongst the British forces who at that time were exposed to local conditions of excremental poisoning.—Inspector-General LAWSON held, as the result of experience at Mediterranean stations and in Cape Colony, that simple climatorial causes did not lead to enteric fever, unless, indeed, as a predisposing cause in special localities where faecal causes also obtained.—Surgeon-Major WHITE referred especially to Assam, where fever of the enteric type had prevailed until conditions, which up to that date had led to the excremental fouling of water, were abolished, and then the disease subsided.—Dr. BUCHANAN could only profess to look at the subject of Indian fevers by the light of experience gained in the study of continued fevers in England. Referring to the papers specially under discussion, he pointed out that attempts seemed to be made to identify a disease now by means of some anatomical character, now by means of clinical symptoms, or thirdly, by an appeal to considerations of cause. The last means of identifying disease appeared to him questionable. In England we were content to know enteric fever by the total of its symptoms and pathological appearances, and to proceed from that standpoint to the investigation of cause. We had long known the clinical disease, and had not yet become fully acquainted with the circumstances of its production. Given a continued fever with more or less diarrhoea and spleen enlargement, with special characters of temperature curves, with successive crops of rose spots on the skin and with certain anatomical characters in the fatal cases, we have known it as enteric fever. In England, having come to know our disease, and not till then, we have gone on to learn lessons about its association with one and another external condition, and then by degrees we were learning of its causation. But the numerous and valuable results that inquiries about causation had heretofore yielded did not prevent our having, but rather led us to have, ever fresh doubts whether we know all the conditions for the appearance of the disease, and assuredly had taught us that we did not yet know the conditions for its production. Inquiry as to the way in which fever had made its appearance in a household—to say nothing of inquiry, in a stricter sense, as to “cause”—had been found to involve a process of living over again, as it were, the life of each patient before his attack, with differentiation of his life from that of other people. That process was difficult enough of accomplishment under the conditions of English society, but must needs be incomparably more difficult where a fever occurred in indi-

viduals of an alien race having traditions and caste-notions separating them by wide intervals from the people who were endeavouring to investigate the origin and spread of disease among them. The impression that Dr. Buchanan had received from a study of the three papers was that it was perfectly permissible to regard much of the described fever of India as being identical with European “enteric fever.” It was modified, no doubt, by circumstances of Indian life, yet did not appear to differ more from our own enteric fever than one case or one outbreak might differ from another case or another outbreak of this multiform disease when it was witnessed in Europe. It was not from any single one of these three papers that he had formed this opinion, but it was essentially from observation of what the three papers, taken together, appeared to him to agree upon.

Mr. NETTEN RADCLIFFE dealt in detail with many points specially referred to by Surgeon-General Gordon, who, though admitting that, so far as symptoms and pathology were concerned, a disease resembling enteric fever did prevail in India, yet denied that it was etiologically the same, and even threw doubts as to the views held regarding the etiology of the disease in England. Mr. Radcliffe pointed out that Dr. Gordon had implied that the continued fevers of India were but one single form of fever, whereas in his paper he at once proceeded to divide them into as many as twelve groups. Dr. Gordon had further quoted in support of his views as to the unity of “fever” an opinion of a distinguished author, which was penned as far back as 1843, since which date that author had, as was well known, not only changed his views, but had accepted the classification of the continued fevers enumerated by Stewart and Jenner. Dr. Gordon’s use of the term causation was most misleading, points being referred to as causes which in reality were nothing more than conditions, associated with the prevalence of disease. The study of climatology in connexion with disease was of the utmost importance, but the assertion that an infectious fever was of climatic origin, when even the local circumstances under which it existed were not recorded, was damaging to the cause of epidemiology; for, although it was obvious that climate governs every condition of life, health, disease, and death, yet that did not necessarily constitute it a cause of any special disease. Dr. Gordon’s materials were also misleading, and having been obtained as the result of the issue of circulars to a large body of medical officers spread over a vast area, they were almost necessarily untrustworthy. Anyone having experience of the difficulties associated even in this country in investigating causes of epidemics must know how little value is to be attached to data got together in such a manner. But even accepting the facts recorded in the returns issued, they, in some important respects, led to conclusions diametrically opposed to those embodied in Dr. Gordon’s paper. As to the 175 cases on which Dr. Gordon chiefly based his conclusions, that gentleman had submitted that *seven* only were real cases of enteric fever, the others being, as he argued, so-called “climatorial” fever. Of what value it was asked could seven cases, scattered over several years, and distributed in a wide territory, be in determining a question of this kind. But Mr. Radcliffe proposed to accept the diagnosis as given by the recorders, and was not prepared to admit a greater margin of error in diagnosis than would have occurred if a similar number of medical practitioners had been applied to by circular in England. What followed? Why, 25 per cent. of the total cases furnished no evidence whatever either in one way or another with reference to the question of etiology, and, deducting these cases, it was found that the greater proportion of the remainder (53·5 per cent.), according to the recorders, had occurred under conditions of “faecal effluvium from latrines,” or the patients had been exposed to chances of infection or to local conditions which are known to be chiefly associated with enteric fever. He was quite unable to reconcile these data with Dr. Gordon’s statement, that in not a single one of these cases was there any evidence of the existence of such “causes” of enteric fever as govern the prevalence of that disease in Europe.

Dr. W. DICKSON considered that in tropical climates the continued fevers were attended by many more complications than in Europe, and to this was due many of the differences of opinion which were held with regard to them.—The PRESIDENT, quoting from official statistics, drew attention to the fact that enteric fever was recognised as the principal fatal fever in many parts of India; but at the same time he expressed a conviction that there is a form of “fever” in tropical climates which, as regards its cause, is not identical with