and by and bye ulceration occurs and perforation, with, it may be, a general peritonitis. In certain cases, however, and this seems to be one, the coats become thicker as the dilatation proceeds, and the secretion is retained while the cyst attains very large dimensions. The thick colloid contents represent the mucous secretion of the glands of the appendage.

CLINICAL REMARKS ON A CASE OF STRANGULATED FEMORAL HERNIA.

By T. H. BARTLEET, F.R.C.S.,
Surgeon to the General Hospital, Birmingham.

FOR the following brief notes of the case, I am indebted to Mr. Shipworth, Resident Surgical Assistant.

Thomas Jones, aged 43, married, a smith, was admitted November 10th, 1874. He had had a swelling in the right femoral region, which occasionally disappeared, for the last seven years. On November 1st, the swelling increased without any special cause. His bowels were constipated; and he took aperient medicine, which produced two motions. On November 2nd, he vomited several times. On the 3rd, the vomiting continued, and he was unable to pass flatus. On the 4th, he described the vomiting as being faecal. Constipation and faecal vomiting continued until his admission on November 10th, when he was found to have a small painless hard tumour in the right femoral region. There was no impulse. The belly was distended, but not painful or tender. The patient's aspect was distressed; his temperature was 99 deg., and his pulse 85. The taxis was tried with great care, but ineffectually. Under chloroform, a transverse incision of one and a half inches was made over the neck of the sac, down to which the intervening tissues were divided on a director. The seat of stricture was the falciform margin of the saphenous opening, a few fibres of which were divided; and the contents of the sac were returned, the sac itself not being opened. The sac, which seemed likely to be drawn up by violent respiratory efforts, was held to the wound by a hare-lip pin passed through the edges of the latter, and including a piece of areolar tissue attached to the sac. The wound was dressed and rolled in the usual way.

November 10th. Vomiting had continued two or three times after the operation. He took twenty minims of tincture of opium, which relieved the vomiting, and was followed by a good night. He had passed flatus. Temperature normal; pulse 75. He had no pain.

November 13th. There was slight discharge from the wound. He had passed a firm and very copious motion.

November 28th. The wound had been dressed with tenax, and was all but healed; it measured now barely an inch.

REMARKS.—The case seems to me to present the following points of interest: the length of time during which symptoms of obstruction had lasted; the lengthened duration of faecal vomiting; the mode of operation; the after-treatment.

Symptoms of complete obstruction had lasted for nine days. The condition of the hernial tumour did not justify the opinion that strangulation had lasted thus long. I believe that the hernia was, at the beginning at all events, *incarcerated*, not *strangulated*. Now, what is an incarcerated hernia? It is a hernia in which the constriction affects only the calibre, and not the walls, of the gut; that is, the passage through the intestine is entirely closed, but the intestinal walls are not sufficiently constricted to stop the circulation through them, and consequently to cause them to sphacelate.

Faecal vomiting was present for four days. This symptom, though a common one in long standing cases of strangulated hernia, is justly considered one of serious importance. It is supposed to be due to the long continued and ineffectual peristaltic action of the intestine above the stricture, inducing a backward current of the contents of the bowel in the centre of the tube.

The Operation.—I have for some time given up the long vertical incision over the whole or greater part of the sac. I make a short transverse incision over the neck of the sac, by pinching up the integuments and transfixing; the incision being rarely as long as two inches. I divide the tissues on a director until I am able to insert a nail under the stricture. I find it rare, even in old standing and in inguinal herniae, to be unable to relieve the stricture without opening the sac. Occasionally the stricture is caused by a few fibres in the sac; and these I have, in one or two cases, very carefully divided from the outside instead of from the inside of the sac.

Herniotomy without opening the sac is an operation free from danger. No important tissues are cut through or displaced. The danger, as far as the operation is concerned, is due to the opening of the sac; for we

then make an open track for the admission of air, and sometimes of fingers, into the peritoneal cavity. I think needless alarm is often felt about the condition of the bowel, or of its strangulation by a band or by omentum, when the sac is not opened. In cases of recent strangulation, it seems to me as great a mark of unwisdom to open the sac to examine its contents, as it would be to trephine the skull to look for a possible clot underneath, in every case of injury of the skull with insensibility. Opening the sac is a *certain* risk; leaving it unopened is an *uncertain* risk. Moreover, in recent cases of strangulated hernia, it is customary to attempt reduction by taxis. Now, if it be safe to reduce a strangulated hernia by taxis, it is surely safe to do so by an operation which is as safe and simple as is extraperitoneal herniotomy. I am aware that the present case is not one of recent strangulation. The symptoms of obstruction and even faecal vomiting had lasted many days. It may be fairly asked why I did not deem it necessary to open the sac and examine the condition of its contents. My examination of the hernia before and during the operation did not favour the belief that it was gangrenous. Pain was not, and had not been, complained of. The integuments were not emphysematous, oedematous, or red; the sac was not flaccid. Had the sac or its contents been long gangrenous, the superficial tissues would have resented the presence of dead matter, as of any other foreign body. Unfortunately, there is no way of telling whether or not the contents of the sac are *just dead*; and I consider it better, in the absence of definite symptoms, to face this risk rather than that of peritonitis, so frequently consequent upon opening the sac. After I had returned the contents of the sac, the violent efforts of vomiting seemed likely to draw the sac itself up into the abdominal cavity. This was undesirable, since, had symptoms of strangulation still continued, I might have wished to open the sac and to bring into view the intestine or omentum it had contained, and which, in the paralysed state of the intestines, would almost certainly be found lying near the crural ring. I therefore kept the sac near the wound by transfixing a tag of areolar tissue attached to it (not the sac itself) by a hare-lip pin, with which I closed the wound. The necessity did not arise in this case, but the manœuvre might be useful in other cases.

With regard to after-treatment, one dose of tincture of opium relieved the sickness, and gave a good night, which the novelty of the patient's position in hospital might have prevented. After this, he had no medicine—above all, no aperient. He was kept to milk and broth diet until his bowels were open, which they were on the fourth day after the operation and the fourteenth day of constipation. The wound was dressed with tenax—an excellent dressing for these deep wounds, as it prevents the accumulation of pus, and consequently favours the contraction and healing of the wound.

THE SLEEPING SICKNESS OF WESTERN AFRICA.

By ALBERT A. GORE, M.D., F.R.C.S.I., SURGEON-MAJOR.

SOME time since, I read, in the "Notes and Queries" of a contemporary, a few brief remarks on this rare disease. As the subject is one of some interest to those who may travel in these distant regions, and to the medical officers who have to treat our West Indian soldiers, I have jotted down the following *résumé*, which may probably contain a summary of all our present knowledge on the matter.

Lethargy, from the Greek terms *λήθη* and *ἀργία*, is placed by the older writers under their division *Neurotica*, and defined as a "mental and corporeal torpidity, with deep quiet sleep". The occasional causes being congestion or effusion in the brain by violent mental commotion, as that of fright or furious anger; by retrocedent gout, or repelled exanthems; but more generally by long continued labour of body or severe exercise of mind, cerebral exhaustion. Celsus regarded it as a nervous affection; Forester and Cheyne as chiefly dependent upon plethora or congestion. According to Mason Good, it is not unfrequently a strictly nervous affection connected with an irregular or debilitated state of the mind. The common causes of sleep, therefore, whether natural or morbid, are in many cases causes of lethargy. The older writers classified the disease under three heads.

(a.) *Lethargus absolutus*, or genuine lethargy, without intervals of sensation, waking, or consciousness.

(b.) *Lethargus cataphora*, or remissive lethargy, with short remissions or intervals of imperfect waking, sensation, and speech.

(c.) *Lethargic vigil*, or imperfect lethargy, perfect lethargy of the body, but imperfect lethargy of the mind, wandering ideas, and belief of wakefulness during sleep.

The first variety of the ancients is the later "constitutional lethargic slumber" of Blanchet and other French observers. Examples have

occurred lasting forty days (Platt), seven weeks (Bang, Collection), months (Blanchet), at intervals during five years (Good). In one instance it was stated to have resulted from insolation, or exposure to the direct rays of the sun. The second variety, the *coma somnolentum* of many writers, has been an accompaniment of many fevers and other diseases of great debility. In a case mentioned by Good, the patient, a young lady of delicate constitution, during the irregular remissions, sighed, ate reluctantly, and instantly relapsed into sleep. The third variety is the typhomania of the Greek writers, and an occasional sequel of fevers and other causes of great nervous debility, where the sensorial power has not recovered its regularity of current or stability of balance. In a case mentioned by Cook, in his treatise on *Nervous Diseases*, the paroxysms ultimately ended in derangement of the mind. Some of the symptoms of that convulsion of the mind known as *melancholia attonita*, or its paralysis, as seen in acute or chronic dementia—viz., an impassive or motionless body, vacant stupid expression, involuntary passage of urine and feces, and a passive resistance to the action of others, are occasionally seen in the last stages of African lethargus.

My attention was first called to this curious disease when quartered upon a solitary island at the junction of the Rio Jeba and Rio Grande, in Portuguese Senegambia, in 1866. I had just returned from shooting, when a native was brought to my bungalow with an enlargement of the glands of the neck, which he was anxious to have removed. My servant, a Portuguese, informed me that he stated he was afraid he was about to get the "sleeping sickness". Upon further inquiry, I found that this was really the case, and that among the people of the neighbourhood this enlargement of the glands was considered a premonitory sign of African lethargus. My servant, who appeared to be quite *au fait* with the subject, further informed me that the native doctors always excised or destroyed the enlarged lymphatic glands as a measure of precaution, a process accounting for the numerous scars which marked the neck and other parts of the bodies of their patients. Upon returning to headquarters, and looking over our medical records, I found a case of the disease, which terminated fatally, noted down as early as 1833, at Sierra Leone, by Assistant-Surgeon McDonald, of the Royal African Corps, the patient being a private of his regiment; and, three years later, the following interesting and graphic description of another, by Dr. William Fergusson, surgeon of the corps, principal medical officer, and afterwards governor of Sierra Leone. It is headed "Lethargus".

Dr. Fergusson goes on to say: "It is a disease rarely met with here, and it occurs more frequently among the class of recently arrived liberated Africans than among any other body of the population. In the course of the last fourteen years, I have seen four cases of it, three of which were liberated Africans, and one a maroon. There was no variety in the symptoms of these four cases, and one only of them terminated favourably. The disease substantially consists of a strong and irresistible desire, or rather tendency, to fall asleep under any circumstances—an inability to keep awake is perhaps the most suitable definition. I have not seen it tested under the circumstances of passion or strong mental emotion, and am rather inclined to think that the passions are obliterated, as it were, in the general tendency to rest which seems to pervade the whole system. The maroon boy above mentioned used to fall asleep standing, amidst all the noise and excitement of a school conducted upon the monitorial system; and I have seen Private George Gannus, 2nd West India Regiment, the subject of this case, lying fast asleep with a mouthful of half-chewed victuals in his cheek; he had, in fact, fallen fast asleep, while eating his dinner. He was taken into hospital on March 1st, 1836; his comrades stated that he was always sleeping; that, whenever he sat down for a few minutes, he was sure to fall asleep. He was well purged, and on the 5th discharged cured. He was again brought to hospital on the 15th with the same complaint of constant sleepiness; his look was stolid. He made no complaint of pain; ate well. The pulse was 72; tongue clean, except a small spot of fur in the centre. A dose of drastic medicine was given, and repeated upon the second day. He was kept in hospital until April 5th, during which time he had a dose of colocynt and calomel about once in three days. He was discharged to duty, not having had any return of the sleepiness. In the early part of May, symptoms of another return of the disease became manifest, necessitating his being again taken into hospital on the 8th of the month; recourse was again had to the system of drastic purging which had succeeded so well on former occasions. It was, however, but too clear that the drastic mode of treatment had entirely lost its power. Five dozen of leeches were now applied to the forehead and temples, and a large blister to the shaven scalp, without any advantage. He slept almost continuously; when roused at meal-times, he made a scanty meal, and sometimes fell asleep over it. He was now put upon the

use of a small daily dose of calomel, and a blister was applied to the nape of the neck; his bowels had all along been kept free, and, up to this period he had made regular use of the night-chair. He now slept constantly; decubitus on his back; his alvine evacuations were passed involuntarily. He sank gradually, and died on June 1st.

"The dura mater was found to be in a healthy state; the arachnoid membrane was red, and highly vascular; this condition was not confined to any particular part of it, but prevailed throughout. The pia mater appeared healthy; the substance of the brain was particularly firm, and not one drop of blood appeared on a section of any part of it; the sinuses were unusually deficient in the quantity of blood; the lateral ventricles contained about a drachm and a half of serum; the thoracic viscera did not present any unusual appearance. In the stomach were found many patches of a red colour near the pylorus; these did not extend into the duodenum. A portion of small intestines, about three feet in length, was of a very dark colour, and the *calibre* of the canal at that part was much contracted; this part of the canal contained a considerable quantity of thick sanguineous serosity, and its mucous coat was highly injected. The liver, spleen, and kidneys were in a healthy-looking state."

The following is the only other case the details of which I have been enabled to obtain: they are given by Surgeon-Major Morphew. "No. 166, Private J. M., 1st West India Regiment, twenty-eight years of age, was admitted into the military hospital, Nassau, Bahamas, June 27th, 1865, for dementia and lethargus. Some months prior to admission, he had been in a singular way. When on guard duty, he was constantly in the habit of falling down in a semicomatose state, and, upon the return of consciousness, he would invariably state that he had seen a "spirit". This delusion had unquestionably something to do in bringing him to the state in which he was upon admission. From his appearance, I was of opinion that he would become a subject of the disease, so common among Africans, called lethargus; and into a lethargy he fell on the 10th instant. For a few days previously, he spent most of his time sleeping, but during the last eleven days of his life he never awoke; he expired about one o'clock in the morning. All medical treatment in these cases is useless as a rule, and in this case was not tried beyond attending to the state of the alimentary canal. Two years subsequently, a private of the same regiment died in the gaol at Freetown suffering under symptoms of lethargus. This delusion of having seen a spirit is a common one among Africans." Dr. Morphew's remarks as to the fatality of the affection is fully borne out by statistics to be presently quoted.

The next contribution to the history and pathology of the disease is from a writer in the *Medical Times and Gazette* of the 24th October of the present year. He goes on to state: "The first intimation is a swelling of the glands about or around the neck, accompanied by a gradual inclination to sleep, which increases; and the person so affected will fall asleep at all times and in all places, no matter what he may be doing—working, eating, walking, talking, or anything else, only awaking at short intervals. Sometimes the body swells in the progress of the disease, and at other times in its first stage. The skin becomes dry and dusty; the strength palpably decays; and the most massive form becomes reduced. This continues for six or twelve months, more or less. Just before death, the patient suddenly ceases to sleep; this is the forerunner of death always. I have never known a Congo die of any other disease. It carries off many natives; and even the Americo-Liberians do not escape from it." The Congos to whom the writer refers are among the lowest type of negro in mind and body inhabiting the shores of West Africa. When they were liberated in large numbers at Sierra Leone, the disease came far more frequently under observation in the colonial hospital, as will be seen from the following returns. For the four years ending March 31st, 1850, 112 cases of lethargus or sleeping sickness were treated; for the seven years ending December 31st, 1866, only 67 were treated. Of the 179 cases admitted, 132 died, 47 recovered—a very high ratio of mortality. Burton, and most other writers on Western Africa, allude to the disease as a medical curiosity; the former states that Europeans have died of it. The cases of African sleeping sickness which I have seen in the colonial hospital at Sierra Leone, two of them in the last stage of the disease, presented similar features to those already described; the patients lay upon a mat sleeping their existence away. One was very much emaciated; he lay quite motionless, and apparently unconscious; only appeared to notice when severely shaken, then merely raised the eyelids and gazed vacantly. The sphincters had already become relaxed; the urine and feces were passed involuntarily; and for some days previous to death he lay quite passive, refused all nourishment, and died at last, as in the cases already described. While recently on the Gold Coast, I had under treatment in hospital a private of the 2nd West India Regiment, who appeared to suffer under lethargic prodromata. He

was constantly sleeping, and with difficulty was made to take food and medicine. He afterwards passed from under my notice.

The treatment of African lethargy has been, as a rule, eminently unsatisfactory in the severer forms. Stimulants, alteratives, depurants, and counterirritants have equally failed to arrest the onward progress of the disease. Celsus, who contemplated lethargus as a nervous affection, confined himself to external and internal pungents, shaving the head, fomenting it, and afterwards applying some rubefacient epithems. Others, such as Good, advised cupping, blisters, purgatives, the voltaic current from the occiput to the sacrum, and metallic tonics. Such a line of treatment appeared to afford a temporary relief. If the glandular enlargement observed were due to leucæmia and hypertrophy of the spleen, conditions sometimes associated with lethargic symptoms, quinine, ferruginous tonics, and antiperiodic medicines might be of value. Except by supposing that the enlarged lymphatic glands in the vicinity of the arteries of the neck pressed upon and deranged the cerebral circulation, thus diminishing and interfering with the blood-supply of the brain, one could not reconcile the Portuguese idea of its pathology with the facts observed. The etiology of the disease is still curiously obscure.

REPLY TO DR. MACLEAN RESPECTING MALARIAL FEVERS.

By T. INMAN, M.D.Lond.,

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Consulting Physician of the Liverpool Royal Infirmary.

IN looking over Dr. Maclean's communication in the BRITISH MEDICAL JOURNAL of December 19th, I find that the author speaks of Dr. Oldham and his followers not attempting to explain the fevers suffered from in the Ashantee expedition. As yet, it is probable that a sufficient time has not elapsed for Dr. Oldham to obtain the necessary information, he being in India. And this cannot be wondered at; for, though anxious to obtain definite knowledge myself upon the subject, I have been unable to find any. As a friend of Dr. Oldham, and one who has taken a deep interest in his theory, I venture to stand forwards on his behalf until he is able to see Dr. Maclean's attack and to parry it for himself.

In writing this, it is not my intention to support Dr. Munro's hypothesis that "certain electrical conditions" influence the productions of so-called malarial diseases; but I most cordially agree with him "that there is no such poison as malaria".

I may, at the outset, state that I have not had any personal experience of malarious disease; but I have travelled more than once through the most malarious districts of Italy, and have had the advantage of conversing on the subject with individuals whose business has led them to reside in Sierra Leone, the large trading stations on the great Western African rivers, the swamps of the Southern United States, and other parts of the world, which are said to be most fatal to human life in consequence of "malaria". My informants have been both medical and lay, and what testimony I have got has not been brought out by the putting of "leading questions". I am not, therefore, simply "a parlour-judge".

As I am perfectly familiar with Dr. Oldham's exhaustive treatise upon *Malaria*, the first thing which strikes me in Dr. Maclean's letter is, that the critic does not know the work of which he speaks slightly. Had he done so, he would not have committed himself to the illogical arguments which he uses. In the paragraph wherein he speaks of fevers breaking out at Hong Kong and Paris, consequent upon turning up the soil for building purposes, there is the expression, "If the soil did not give birth to a fever-generating poison in all or any of the above cases, what did?" This reasoning is on a par with the assertion that "Tenterden Steeple produced the Goodwin sands; and, if it did not, what did?" Dr. Maclean wants an explanation of this difficulty of his. Dr. Oldham has already given it; but I would desire to supplement what he has said by calling attention to the vicious style of observation and argument exposed by Lord Bacon nearly two hundred and fifty years ago. The "idols of the tribe", to which that learned author referred, include a propensity to stick through life to opinions adopted in youth, to explain away everything which militates against them, and to give undue weight to apparent evidence in their favour. Dr. Maclean has not yet learned to avoid this pitfall. Dr. Oldham very justly lays down the law, that the cause which produces "malarious fevers" in rocky and sandy districts, where there is no vegetation and *no soil whatever*, must be analogous to the cause producing the same effects elsewhere. "This is a strictly logical assertion.

If, then, Dr. Maclean declares that "soil" produces malarious disease,

he must show that such complaints never occur in the Sahara, on bare mountain-rocks, and on ships at sea. But at Aden, where there is no soil, and in rocky tropical localities, fevers are as common as and even more severe than on the Pontine Marshes or the Maremma district. *E.g.*, on the march to Magdala in East Abyssinia, Consul Hutchinson, in his recent work on Peru, has mentioned that ague and malarious fevers have occurred more than five thousand feet above the sea amongst the navvies digging a proper bed for a new railway. If Dr. Maclean had seen his observations, he would not have needed to inquire, "If the soil did not produce the disease, what did?" for the consul, himself a medical man, and one who has had intermittent fever nearly forty times, gives a very feasible account of the reason why.

To explain all this, however, to Dr. Maclean and the members of the British Medical Association, would require almost a treatise. I may be pardoned, then, if I try to explain Dr. Oldham's views in a short manner. The gist of his argument is, that the so-called malarious diseases are due to cold or chill after exposure to great heat, especially after it has been accompanied with exhausting labour.

Than this hypothesis, nothing can be more simple. Every doctor in Great Britain is aware how frequently bronchitis, pneumonia, and other severe affections, are attributed to cold—borne by a person who has been previously exhausted by exercise in a hot atmosphere—or by the sweating which labour produces, even in winter. If we see a navvy, teeming with perspiration, lie down and sleep in a draught, or on a cold stone, we say to ourselves, "that he will be lucky if he do not require a doctor".

There is, then, *à priori*, something reasonable in the idea, that an excess of heat and cold will produce analogous and more severe results. Now, in England, the difference between the day temperature and the night, is barely twenty; 90 degs. in the shade by day is about 70 degs. in the shade by night; but, in malarious districts, the temperature by day for those working in the sun (and it is those who get the fever), is about 140-150 degs.; at night, this falls to about 50-45 degs. Again, it is known, that during frost, such as the French Army experienced in the retreat from Moscow, the cold is bearable; but when a thaw sets in, and the cold is moist, not dry, frost-bite is common.

Every notoriously malarious district is marshy; consequently, those who live therein, are subject to more severe variations of temperature than those who live in dry places. During the day, the hot air over a marsh or lagoon is saturated with dissolved water as soon as the sun is low, and still more after it sets. This water is *precipitated*, and we have a cold dew—did ever anyone hear of a hot one?

I have emphasised the word "precipitated" because it involves a most important point. If the word be used correctly, there can be no emanation from the ground during the time the aqueous particles are falling. As I presume that Dr. Maclean is familiar with the cause of dew, I need only point out, that terrestrial emanations only take place by day, and must cease at night; yet night is the fatal half of the four-and-twenty hours. Night is also the coldest part of the day in every country, and the cold is most intense when there is nothing to intervene between the earth and stars.

It is well known that the Pontine marshes are only dangerous after sundown, and only then to anyone who cannot keep himself warm. Dr. Oldham tested the malarious theory, by sleeping, for many months together, on a bed placed in a swamp, and barely two feet from the ground. There were atrocious stinks, and his nostrils suffered accordingly; but he always wrapped himself up warmly, and never got fever that way. He did get it once before, and that was in the room of his superior officer, and under the following circumstances. He had ridden, in the heat of an Indian day, to deliver a report, and was perspiring; when he arrived, the room occupied by the colonel was cooled with a punkah, and the doctor was suddenly chilled, and could not leave the house, for he instantly was struck down by fever. His book contains many such cases.

This will serve to introduce to Dr. Maclean the reason why fever has attended disturbance of the soil. Men work hard, under a hot sun during the greatest part of the day; when night comes they are tired, glad of something to drink, take too much, and sleep too heavily, to be awakened by the cold. The blood has, so to speak, been boiling all day, at night it is chilled almost to the freezing-point—hence the fever, which may show itself as intermittent or remittent; or as the attendant of dysentery, hepatitis, pneumonia, etc.

Consul Hutchinson in Peru noticed these causes in operation, and was not surprised at their results. In some parts of South America, to "catch a cold" is "to get an ague", the expressions being synonymous.

In the Ashantee expedition, the alternation between daily heat and moist nightly cold was enormous; and any one who has read of the excessive exertion, and consequent fatigue, undergone by the men, whilst