told Dr. Oldham that hot days and cold nights were certain to produce fevers: therefore there is no such poison as malaria. Now, I hold that these are merely assertions without proof; because, in the first place, I deny that Dr. Oldham did test the malarious theory in any legitimate sense of the word by sleeping so many nights on a bed placed in a marsh; for I maintain that his escape from an attack of fever under the circumstances is no more a proof of the non-existence of malaria, than the escape of an individual from an attack of sea-sickness, who may have slept on board a ship in the Bay of Biscay for two months during a storm, is a proof that the motion of a vessel is not the cause of such sickness. In the second place, I deny that only those who work hard in turning up the soil under a hot sun suffer; because, as has been seen, my own case is a notorious instance to the contrary. In the third place, I deny the assertion that "hot days and cold nights" are *per se* the cause of malarious fevers, because my own case entirely disproves it. When Dr. Inman, who, at the commencement of his paper, postulates the non-existence of malaria, makes use of such expressions as "in malarious districts the temperature of those working in the sun", and "every notoriously malarious district is marshy", it is not only a case of confusion, but it is a contradiction in terms.

CHILL AND MALARIA.

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THE question at issue between Drs. Maclean and Inman cannot but create feelings of surprise in the mind of anyone who has had to deal practically with paludal fevers; surprise, not only that the existence of a specific factor—call it by what name you will—has been called in question, but that the gentleman to deny its existence should be a teacher of medicine, who, it may be assumed, has been in the habit of inculcating such doctrines amongst his pupils. However, after the views that have been expressed by one or two members of the profession with reference to the non-existence of a specific virus in such diseases as syphilis and gonorrhoea, we may well cease to feel surprise at anything. As regards the two latter diseases, any individual who is sceptical on the point can probably demonstrate the fallacy for himself; but he cannot expect commiseration, should he have the misfortune to find out that he has been labouring under a mistake.

The case, however, is far different when the belief in, or denial of, the existence of such a poison as malaria is the question to be determined. The health of a ship's company or a regiment is placed in the hands of one man, and the lives of hundreds may be sacrificed to a theory. It is for this reason that I think it incumbent on those who can speak from experience to cast the weight of that experience into the balance of truth, and, in the interest of humanity, to state what facts bearing on the question have come under their own observation.

In the first place, Dr. Inman, although he professes to despise the opinion of those who see for themselves, would, if his ideas were based on personal observation, never have fallen into the mistake he makes in his communication, namely, that "the folks attacked are not those who sleep between decks, but those who sleep in the open air". The history of the outbreaks of fever on board H.M.S. *Eclair*, *Archer*, *Icarus*, and *Bloodhound*, sufficiently refute such a statement. Moreover, he would have learned that there is not a ship in Her Majesty's service where, in tropical climates, awnings are not spread, and the greatest precautions taken to prevent men from exposing themselves at night without a covering. I would, therefore, ask him to account for the following outbreak of disease, according to his theory. In 1861, a cruiser on the West Coast of Africa, whose crew of white men numbered fifty-five, was taken from her cruising ground in the bight of Benin and ordered up the Niger. At this time, the ship's company were in perfect health, having never approached closer to the land than from three to seven miles for a period of three months. The ship remained three months in the river, and while beyond the Mangrove belt was comparatively healthy; unfortunately, on her return voyage, she was detained for a week amongst the swamps in the delta. Ten days had not elapsed after her return to the open sea before thirty-five out of the fifty-five were attacked with a severe form of what is popularly known as "coast fever"; and, out of the whole ship's company, only two entirely escaped. This, I may observe, was not a solitary instance; during three consecutive years, the same duty was performed by this ship with similar results, though in the last two trips the disease did not assume such extensive proportions. I will give another proof of the fallacy of attributing the deadly diseases of Africa to such causes as exposure to the open air at night, *minus* malaria. In all the African rivers where the palm-oil trade is carried on, it was the fashion

some years ago for a ship on her arrival to send down everything from aloft, and *house-in* by building a shed of palm leaves over the upper-deck. Here she remained till her cargo was completed, which generally required from three to four months; the men were not exposed, as the custom is to employ Kroomen for all work requiring exposure to the sun. It was found, however, that, by the time the ship was ready to start, the men were nearly all dead; consequently, she could not leave till a fresh crew could be got out. I do not mean to say that this was invariably the case; but it occurred so frequently that a different course of proceeding was adopted; and a ship now, on her arrival, transfers her crew to the one prepared to start. By this means, the loss of life is diminished 90 per cent. As to the prevalence of fevers of the paroxysmal type in the vicinity of masses of disintegrating granite, explain it how we may, it is a well known and admitted fact, not confined to Hong-Kong alone, but present wherever the necessary conditions of heat and moisture are found; in the tropics, Dr. Inman may depend that the means to be taken to insure immunity from these diseases has occupied the attention of men well qualified by previous training to avail themselves not only of the researches of others, but of what has come under their own observation. I cannot see that a "physician in his study" is, therefore, better constituted in any way to form an opinion on these matters than the "surgeon of a regiment", but, in my opinion, rather the reverse.

CLINICAL MEMORANDA.

NEW FORM OF CATHETER.

IN cases where it is necessary to tie in an ordinary gum-elastic catheter, I have frequently found that it has collapsed from pressure, after having become softened by the heat of the body, and has thus frustrated the object for which it was used; viz., the institution of a free outlet for the urine. To obviate this difficulty, I have devised the following. A spiral of fine wire is made, of similar construction to that used for lining flexible gas-tubing. Over the spiral a gum-elastic coating is put, with the ordinary perforations at the extremity. I find this instrument most useful, and much more durable than the ordinary form, the cost being but very little extra. Messrs. Lynch and Co., of Aldersgate Street, are the makers.

S. W. MOORE, L.R.C.P.Ed.

POISONOUS FUNGI.

IN reply to Dr. Hutchinson's interesting case of supposed poisoning by the edible mushroom, I can scarcely realise that the facts stated warrant the conclusion which he draws. His case had none of the usual and early gastric, or even narcotic, symptoms of fungoid poisoning, such as were shown in Mr. Sedgwick's and Dr. Drummond's cases (JOURNAL, pages 464 and 524, October 10th and 24th), due allowance of course being made for the symptoms varying in different individuals. There were also no tetanic spasms, although there certainly was cerebral excitement after several days had elapsed; but this could not have been caused by muscarine, because the action of the latter on the heart is speedy, which has been clearly proved by Dr. Lauder Brunton (page 617). The patient was in a condition of exhaustion previously, and his system was consequently ready at any time to receive any morbid impression; he partook, as is admitted, of a large quantity of fungi late at night. Surely a heavy meal under such circumstances may have produced cutaneous rash and meningeal irritation, from sympathy with the stomach. Does not the prudent huntsman, when greatly exhausted, first partake of some light stimulating food before he ventures on a rump-steak, and give his horse gruel before a heavy feed of beans? The sister appears not to have been affected, she being more prudent and taking less. If the fungi were of the esculent variety, this would rather tend to show that the effects were due more to a state of idiosyncrasy of constitution rather than to muscarine poisoning. Since the patient survived between two and three weeks, muscarine poisoning could scarcely have been the cause, as in that time the poisonous alkaloid muscarine, if present, would have exhausted itself. Again: if atropia be the true antidote to muscarine, why the increased excitement and the consequent abandonment of the remedy? Dr. Lauder Brunton has shown that "dyspnoea, as well as the other symptoms of muscarine poisoning, disappear in animals almost immediately after the injection of atropia"; and he quotes an instance where this "antidote during the death-struggle completely restored a dog which had been so poisoned". Under the circumstances, I cannot admit that the true mushroom—*Agaricus campestris*—in a healthy condition, and properly cooked, could have acted as a poison in the ordinary acceptance