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 leuchæmia 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.

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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

the sun was above the horizon, and the cold which they had to endure when that source of heat had set, can readily understand why the fevers which the soldiers had were so severe. Had the men not been partially sheltered, by the forests which they traversed, from the radiation of their heat into space, matters would have been infinitely worse. Even horses who sleep in the open air in Arabia become diseased; all the valuable ones are sheltered during the night by some sort of cover, which prevents chill. Dr. Maclean, had he read Dr. Oldham's book, would readily see how forcibly his own statements support the latter's doctrine; *e.g.*, Dr. Maclean quotes from Colonel Gordon how the blacks of the Soubate go indoors at dusk, light fires, and try to live in the smoke. Surely, he will acknowledge that the smoke of a fire is warm, and that the effect of the fires, kindled in every hut, kept by the nearly naked Africans, is to make them, as respects warmth, somewhat comfortable. This warmth it is that prevents their getting fevers. A friend of mine, who lived at Sierra Leone for years, never had even a touch of fever; for, during the wet season, he habitually warmed his rooms with a stove, and always was warmly clad.

These observations have a practical bearing; for, if there be such a poison as "malaria", we cannot absolutely guard ourselves against it; but, if it be true that the diseases attributed hitherto to malarious miasms are really due to severe chill after great heat or great exertion, we can arm ourselves against them. How severe and sudden may be the effect of chill, the following will show. A medical friend of mine, living in Cheshire, saw, one day in May, a maid of his, who had been engaged in washing, and was bathed with perspiration, standing in a draught of air to cool herself. He sent her away from the door, but, ere the day was over, the woman had general dropsy; the urine passed in twenty-four hours amounted only to half an ounce, and was full of tube-casts, though not albuminous. Surely, no one would say that she had imbibed a poison from the soap-suds; nor, had she been delving, would one say that she had been poisoned by the terrestrial effluvia. Until the teachers of our youth have a larger capacity for inquiring into old facts, with the light of modern science, and for basing their instruction upon logical deductions, rather than what are called "time-honoured facts"—because they have been asserted for a century or two, though not facts at all—the practice of our profession will not improve, not being based upon sound knowledge.

If a soldier be taught that agues, etc., come from "soil", he takes no precaution when no "soil" is present to produce them; and then, when a *medico* finds his detachment ill with intermittents or remittents, he feels helpless to prevent the spread of the disease. On the contrary, had he been taught truly, he would study the air as well as the earth, and remember the dictum of the Indian colonel, who said to Dr. Oldham, "You doctors may talk of marsh-poison as you like; but my experience has taught me that hot days and cold nights are certain to produce fever".

THERAPEUTIC MEMORANDA.

IODIDE OF POTASSIUM IN ASTHMA.

THE notes of the following case, showing unusual results from the administration of iodide of potassium, may be sufficiently interesting to warrant me in asking for their insertion.

Last summer, I was called to see A. B. She complained of cough accompanying attacks of asthma, coming on regularly about 2 A.M., and lasting for an hour or two. I ordered the iodide in four-grain doses, with ipecacuanha wine and squills, every three or four hours. The asthma was better during the night; and the next day the cough and breathing were also better, and the medicine was taken less regularly. The same evening, my patient was seized with intense pain across the loins, more severe on the left side; also inability to pass urine unless in very small quantities, and accompanied with very great pain. Relief was obtained from the use of hot baths, diluent drinks, and large doses of the bicarbonate of potash and liquor potassæ. During the next fortnight, no medicine was given, the asthma continuing better; but at the end of that time a fresh attack occurred. The iodide was given in the same doses, and was followed in a few hours by threatenings of the same pain and discomfort. A short time afterwards, two-grain doses of the iodide were tried; but a few doses were sufficient to show that the system would not tolerate the medicine, although after its use the asthma was invariably better. My patient complained of no other symptoms peculiar to a state of "iodism", unless a very salt taste in the mouth while taking the iodide. The urine was highly acid while the pain lasted, and relief was only obtained as this secretion became alkaline. This bears out the theory that rheumatism, eczematous eruptions,

or gravel, often replace asthma, or may in turn be replaced by it. My patient has inherited a strong rheumatic tendency. She also suffers from a patch of eczema on one arm, which always followed the fortunes of the asthma as it got better or returned. This case seems one in which asthma and eczema act as safety-valves for a system having a strong rheumatic diathesis; and the curative effects of the iodide on the former serve to intensify the latter by increasing the amount of uric acid to be eliminated by the kidneys. Iodide of potassium acts by promoting the action of these glands, most probably by its primary power of causing the absorption of additional waste nitrogenous matters into the blood, which are eliminated partly as uric acid. Hence we can understand how this additional work, thrown on glands already injured by constant work at high pressure, should have the effect of paralysing their efforts at elimination—should be followed by a severe attack of gravel.

J. MITCHELL WILSON, M.B., Chatteris.

THE THERAPEUTIC VALUE OF IODIDE OF POTASSIUM.*

AS I have for several years paid considerable attention to the action of iodide of potassium, I venture to offer the following remarks as supplementary to Mr. Spurgin's article in the JOURNAL of September 5th, 1874. This medicine has been accredited with many modes of action: thus, in struma as an alternative, in asthma as a sedative, and in diphtheria as an antidote. To all these titles it may have a claim which different observers may think fairly borne out; but certainly the one distinct and indisputable action of iodide of potassium which I have noticed, is that of *stimulating the mucous membranes*; thereby influencing their action and promoting their secretions. Thus, as the results of its use, there are pain and sense of fullness across the eyes; increased secretion from the nares, mouth, fauces, and bronchi; leucorrhœa and menorrhagia are greatly aggravated; and in persons very susceptible of its influence, diarrhœa is induced, not so much of a cathartic as of a dysenteric kind; that is, rather an increase of mucus with tenesmus than of serum with catharsis.

In a person suffering from an attack of chronic winter cough, the first symptoms are great difficulty in breathing, amounting to a sense of suffocation; hard, dry, racking cough, which the patient says he cannot subdue; while he expresses a belief that relief would be obtained if something could be brought up. The suffocation complained of has been attributed to a swollen state of the air-passages, obstructing the respiration; but there is a fair probability that the dry congested condition of the membranes is unfavourable to the interchange of gases requisite for blood-aëration, and the situation of the patient such that, however he may fill his lungs, his sufferings remain unrelieved. Whatever the actual state of matters at this point, certain it is, that as soon as expectoration sets in, the breathing is improved; and, although the disease has by no means gone, the patient is so far better. Many hours of severe suffering may be obviated by taking advantage of the power of iodide of potassium to restore and promote the secretion of the bronchial membranes, thereby greatly relieving the congested blood-vessels, producing comparative tranquillity of breathing, and getting the patient over the first stage of the disease much sooner than he otherwise would. This, however, is possibly not its only value. For, here again, however opinions may differ as to the cause of the emphysema which from an early period exists in these cases, no one can have witnessed the severe and straining cough at the onset of the attack, without feeling that it is at least possible for either dilatation of the air-cells or rupture of the tissue of the lung to take place—complications much less likely to occur, so far as the cough is concerned, when the sputum has been rendered easier of expectoration and the irritability of the congested membranes removed by free secretion. It is further to be remarked that the action of the iodide of potassium changes the purulent character of the sputa in chronic bronchitis to a much healthier appearance. From this view of its operation, it follows, as a matter of course, that when free secretion of mucus has set in the medicine should be used with caution or altogether abandoned; and, therefore, when in the treatment of bronchitis—capillary or chronic—moist râles are fairly established, the further management of the case should be on the principle of preventing a too abundant secretion, at the same time employing such means as may assist expectoration and maintain the strength.

In asthma, iodide of potassium is recognised as a valuable medicine. Here the explanation of its action generally given, is that of a sedative relieving bronchial spasm; evidence of the presence of spasm being found in the wheezing and whistling sounds heard in auscultation,

* Extracted from a paper read before the Glasgow North-Western Medical Society in the winter of 1872.