of a microscopic examination of the sputum in some cases of phthisis by a paper from the pen of Dr. Fenwick, which appeared in one of the medical journals in 1868. The method of procedure therein advised I have frequently found in practice to be most satisfactory in its results. If we compress a portion of phthisical sputum on a slide, we can readily see, with a quarter-inch, epithelial cells from all parts of the respiratory passages, mucus and pus-corpuscles, blood-discs, "exudation-corpuscles", etc. But such a plan will not very readily bring into view minute portions of pulmonary tissue, and these are the characteristic marks of destruction of lung-texture. Dr. Hughes Bennett was the first, so far as I am aware, to make the discovery of elastic tissue from the lungs in the expectoration of consumptives. Mainly following Dr. Fenwick, I have found it best to slowly boil about two drachms of the sputum to be examined in a test-tube with an equal quantity of the solution of caustic soda. The alkali soon destroys the ropiness of the expectorated matter, and reduces the mixture to the limpidity of urine. Mucus and pus-corpuscles are disintegrated, and fragments of less easily destroyed tissue, such as minute portions of lung-texture, fall to the bottom of the tube, and may be conveniently removed by a pipette and placed upon a slide for examination. We may find the beautifully slender and curling fibrils of the yellow elastic tissue of the lungs, or scraps of the smallest bronchial tubes, or shreds of the outlines of groups of air-cells. I have rarely seen more attractive microscopic objects than those afforded by phthisical sputa. I have seen in the field at once the outlines of half-a-dozen groups of air-cells as clearly marked as in a diagram, with here and there the traces of red blood-cells, and scattered amongst all, what is a very common object in these cases, minute portions of coal, or other hard, black, and insoluble material.

In many cases of phthisis, there is no need, at least for purposes purely diagnostic, to go to the trouble of making a microscopic examination of the sputum. But, even when the physical and other signs are so unequivocal as to leave no room for doubt as to the nature of the disorder, the discovery of fragments of pulmonary texture in the expectoration will furnish very interesting confirmatory evidence as to the condition of the patient. But do we not all of us sometimes meet with early cases of consumption, in which, though suspecting the malady, and even after frequent and very close investigation of the facts before us, we still remain in doubt as to the actual existence of phthisis? For the further elucidation of such cases, I would suggest the careful microscopical examination of the sputum. The discovery of shreds and fibres of the tissue of the lungs will indisputably demonstrate the results of the breaking down of pulmonary texture. When we are hesitating as to a conclusion, a grain of new fact is worth a bushel of speculation.

MALARIA: REJOINDER TO DR. MACLEAN.

By T. INMAN, M.D.,

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IT certainly is not requisite for a man to have visited Hindostan and China, Ceylon and Sierra Leone, to understand whether an author who writes upon those parts is logical in the conclusions which he draws from his facts. Experience has also abundantly proved that travellers who stay at home, read, and think, know more than those whose pen simply reflects what the eye sees and the ears listen to.

Dr. Maclean is aware, he tells us, that paroxysmal fevers occur in ships at sea; and attributes it to bilge-water. I can tell him that his theory is valueless, for the folks attacked are not those who sleep between decks, but those who sleep in the open air; and that the use of an awning, or even a good blanket, will prevent the spread of the fever, the bilge-water remaining the same. Again, the bilge-water being a nearly constant quantity in ships, it follows that, if it be the cause of fevers, ships must be as bad as marshes, especially when sailing before the wind and almost incapable of being ventilated. But fevers at sea are rare, and are determined by heavy work on a hot day and sleeping on deck during a starlight night. I have not, in the course of my reading, found out that any maritime population sleep in the open air when hot with work in tropical climates. The immunity from fever which Dr. Maclean claims for such, I should say was due to the good sense which has taught them to sleep under shelter.

In Dr. Maclean's fourth paragraph, he allows that ague may arise in rocky districts, where there is little or no vegetation; and then talks vaguely about rotten granite, fungi, water below the surface, old wells, etc. This again reminds me of Tenterden steeple and the Goodwin sands. Why, there is not to be found a spot on the earth's surface where water cannot be found by those who dig for it. Consequently, if

water be the cause of ague, the disease ought to be universal; and, if rotten granite and fungi be a cause of paroxysmal fevers, those affections ought to be found largely in Cornwall, where there are vast moors covered therewith.

But Dr. Maclean not only attributes disease to impossible causes; he ignores those which are known to be in common, almost constant operation—viz., the relative heat of day and night, when a person is necessarily exposed to both without shelter. The worst place for ague which is known in the world are certain black basaltic rocks—I think on the Rio Negro, in Peru. Few parties encamping on them at night escape. Here, however, there is no rotten granite, no fungi, no water near the surface, no soil, no bilge-water, no old wells, etc. Whence, then, comes the fever? In replying to the question, after ascertaining the truth of the "facts", the careful inquirer endeavours to discover what there is in common between this locality and the Pontine marshes, the Maremma, the sandy shores of Walcheren, the parched wastes near the Douro, so fatal in the Peninsular war. He compares these arid rocks with those of Aden; with the sandy Sahara; the deadly rivers in India, Burmah, and Africa; with the swamps of Florida and below New Orleans; with trenches newly dug; and with ships at sea having stinking hides or bilge-water aboard. It is this inquiry which a physician can make in his study, quite as well as a regimental surgeon who knows fever at Hong Kong, but not at Sierra Leone or Gambia. I most unquestionably do not feel a high respect for those who attribute effects to what seem to me to be inadequate if not absurd causes; but I do not intentionally use a sneer in lieu of an argument. Let me for a moment refer to a matter mentioned by Drs. Maclean and Tilt, who attribute certain cases of fever to the formation of excavations for drains and the emanations consequent thereupon. "Fever comes from digging holes", is the proposition. Well, suppose it does: then fever must have been common at the sieges of Sebastopol, Seringapatam, Burtport, Paris, Badajoz, Vicksburg, etc.; it ought to have followed the "navvies" along every one of the railways which exist in Europe. There is not an acre, which has been drained deeply, which ought not to have produced its paroxysmal fever; there is not a well-sewered town the course of whose deep drains must not have been mapped out by fever, or a canal whose makers have not been scourged with agues or remittents. The new Suez Canal, with its saline lakes, its salt marshes, and deep-seated water, ought to have caused the death of myriads; and ploughing and delving on the Maremma ought still to be a fearfully fatal occupation. Is any one of these things so? Does sewerage produce ague—here or in France? If not, surely it is provocative of a smile to see a single accidental case of fever, occurring in a street containing probably five hundred to a thousand inhabitants, attributed to the formation of a drain; and when this bit of reasoning is ushered in by a sneer at the author of inductive reasoning, and by a reference to some glacial theory of the class, *risum tenatis amici*? Equally curious it is to see that coffee-planters at Ceylon are used to support the digging theory hatched at Hong Kong. If trenching could produce agues, the troops at Chatham and labourers at the fortifications about Portsmouth ought to be very unhealthy; and this they are not. Again—to use some more inductive reasoning—let me ask why labourers who used to plough, trench, dig, and excavate in the Fen country and on the Italian marshes were once as subject to ague as the coffee-planters in Ceylon? why those who so delve in England are now free? Is it not certain that there is, or was, a cause in Italy and Ceylon which is not now in existence at home? In all cases, there are soil, water, excavating; nor can the keenest observer find a distinction in the ground thus disturbed. The cause is patent to a close observer, but I need not dwell upon it. I have, without going to Netley, seen men with the stamp of ague on them—some in the dead-house of the Liverpool Royal Infirmary, many more in the wards of that institution, and still more in Southern Italy; but seeing ague-patients alone does not give us information about the agent that worked the mischief. But I have also had—what I shall not find at Netley—the benefit of the experience gained by a civil engineer who has long had to do with fevers occurring in a set of African steamships which trade also to American ports, all notoriously "malarial". The loss and injury to health amongst the men employed was extremely serious, and my friend endeavoured to diminish the unhealthiness. He succeeded. Without knowing even of the existence of Dr. Oldham, he traced the fevers to the same cause as the doctor does; and, by insisting that all who sleep on deck shall put over them a light awning made for the purpose, and thus prevent chill by radiation, the ships under his eye are free, or nearly so, from "malarious" disease.

As I notice that Dr. Maclean does not support his emanation theory by proving that agues are caught by day, when emanation is lively, nor by insisting that emanations are most active when air, heavy with dew, is slowly falling to the ground, and as he acknowledges that, even in

England, chills after exertion in a heated atmosphere may produce some of our most formidable acute disease, I will not make further remarks upon emanation *versus* chill.

ALCOHOL AS A MEDICINE.

By WILLIAM BERRY, M.R.C.S. Eng., L.R.C.P. & S. Ed.

THE interesting papers of Drs. Russell and Sutherland on Alcoholism, as well as the paper of Mr. Lucas, in the JOURNAL of November 14th, will be read by many with the interest and attention they deserve. These admirable papers will cause members of our profession to pause before prescribing alcohol as a remedial agent, especially for women and children, and, to the more inquiring, will cause them to ask themselves what other remedy they can employ instead with equal benefit to the patient. I, for one, do not believe that medical men, as has been asserted, are answerable for the moral degradation of the people from drunkenness, through their indiscriminate prescriptions, but believe, on the other hand, that they are capable of wielding a mighty power against its spread whenever they see a tendency to the development of that habit we call dipsomania.

I am pleased to see that this subject is demanding the attention of eminent members of our profession, that legislation is likely ere long to deal with the subject, and to provide some means of treating this form of mental weakness. That alcohol is a valuable therapeutical agent cannot be denied; and that it has stimulant and narcotic properties, according to the dose, is equally certain, although Mr. Lucas is pleased to term its narcotic effect over-stimulation.

In acute febrile diseases, we see marked benefit derived from its moderate and judicious employment, and have little fear of setting up the habit of its continual employment, if we reduce the dose while the patient is convalescing. It is in nervous people, especially women suffering from neuralgias, faintness, or nervous depression, that we require to be more guarded, and, if possible, to employ a substitute. In these cases, as in all others, the medical man should be definite in his prescription, and should never prescribe it *ad libitum*, but order a certain quantity every three, four, or six hours, or as often as he thinks it requisite; and, as soon as improvement is noticed, he should diminish the dose, finally substituting a nervine tonic in its stead. It is also a good plan to order a biscuit or a small quantity of food to be taken with the stimulant.

Mr. Lucas has arrived at erroneous conclusions from the experiments he has made, when he denies the narcotic effect of alcohol. He admits that alcohol does not pass unchanged out of the body, but is largely oxidised; and he says also that it is to the result of this oxidation that we have its narcotic properties. He believes, moreover, that it is a *pure stimulant*. In large doses, he says, it stimulates to death, giving as an example a person having drunk a quart of rum or brandy falling down dead from over-stimulation, dying from exhaustion of nerve-force; but is it not more likely to be from paralysis of the nerve-centres? The person does not show any signs of stimulation, excepting such as we see in narcotism from other drugs. Opium, for instance, is a narcotic poison, but in small doses it is a powerful stimulant; surely, then, we should not say, in a case of poisoning by opium, that it had stimulated to death.

In admitting that death from a large quantity of alcohol is due to exhaustion of nerve-force from over-stimulation, we must believe that nerve-force is capable of expending itself, after the manner of gunpowder when a lighted fusee has been applied. That its narcotic effects cannot always be explained by its decomposition into carbonic acid and other impurities, thereby charging the blood with the same, is equally certain, and is disproved by the case of the toper who becomes suddenly narcotised after a large dose, and before the blood could possibly become charged with the results of its decomposition. That in cases of drunkenness, the stupor and sleepiness may be theoretically explained in this way, I am not prepared to deny. The effect of alcohol stimulating the animal passions needs no comment. In medicine, it is never used for such a purpose; therefore, this question does not affect us as practitioners. Moral men, of course, ought to keep within bounds, or be total abstainers.

Mr. Lucas also questions the theory of alcohol preventing waste. Dr. Parkes found that urea was not diminished by the use of alcohol in the healthy adult; but the late Dr. Anstie found that the quantity of urea was considerably diminished by its use in a case of typhus. Now, if this were really accumulating in the blood, as Mr. Lucas suggests, its formation not being prevented, we should have uræmia in its worst form; but is such the case in fever-cases, in which large quantities are taken to sustain life? Its action on the albuminous tissues, as shown

by Dr. Ross, and also by Dr. Beale, will explain how it prevents the formation of urea, thus diminishing its excretion when the quantity is large.

To my mind, Mr. Lucas totally fails to show that alcohol is not an useful and suitable medicine when properly employed; neither has he suggested any other remedy in its stead. We have depressing effects after the use of any stimulant, unless its effects be kept up, either by repetition or an equivalent in the form of food or another stimulant substituted; but, in treating diseases which require stimulation, the convalescence of the patient removes all demand for the remedy. It does not matter whether we administer alcohol in shape of spirits of wine, brandy, or wine; but it is well to have a choice, as no one form is suitable for all cases. I have seen drachm doses of spirits of wine, coloured with cochineal, and taken every four hours, produce beneficial results, when the same amount of brandy would have been refused on moral grounds.

How far alcoholic stimulants are necessary to healthy people, either taken with food as an article of diet, or at bed-time as a night-cap, I am not prepared to say. Its habitual use, even in moderation, and that of tobacco, which is also held to be unnecessary, form subjects for the moralist to discuss, and do not affect us as physicians, or its use as a medicine.

REPORTS

OF

MEDICAL AND SURGICAL PRACTICE IN THE HOSPITALS OF GREAT BRITAIN.

GREAT NORTHERN HOSPITAL.

A CASE OF INJURY TO THE FACE, PRODUCING DEFORMITY OF THE FEATURES: THE DISTORTION IN PART REMEDIED BY PLASTIC OPERATIONS.

(Under the care of Mr. W. SPENCER WATSON.)

James C., aged 25, was kicked in the face by a horse, ten years ago. While training the horse, by whipping it round in a circle with a long leading-rein, the animal suddenly backed on him and kicked him violently in the face. He was knocked down, and when picked up the left side of his face was, as he described it, lying on his left shoulder. There was still evidence that both jaws must have sustained compound comminuted fractures; the nose was split open nearly vertically, and the anterior wall of the antrum broken into through the integuments of the cheek. The contraction of the cicatrices formed during the healing of these injuries, resulted in a drawing down of both the left eyelids to a point above the middle of the inner side of the nose, so that the patient was unable to open his left eye by any movement of the muscles attached to the eyelids, though when the palpebral aperture was dilated by the fingers, the eyeball was seen to be perfect, and the sight of the eye itself was unimpaired. The jaws were much distorted, and could only be opened to a limited extent. Nevertheless, mastication and articulation could be very fairly performed. In the middle of the left cheek was an opening communicating with the antrum, and the skin surrounding this opening was drawn down towards it in the form of a funnel-shaped depression.

The above description applies to the condition of the patient when he was admitted into the Great Northern Hospital, in July 1874, for the purpose of having an operation performed, designed to restore binocular vision.

Having several times successfully operated for ptosis, by removing portions of the upper eyelid, and so bringing the eyelid within reach of the fibres of the occipito-frontalis, Mr. Watson proposed a series of plastic operations in which this principle might be rendered available.

† On July 16th, chloroform having been administered, a flap was taken from the upper eyelid, of a somewhat elongated oval form, and inserted in a raw surface left by making a free incision below the lower eyelid, dissecting the skin upwards, and freeing it by making deep cuts into the hard fibrous cicatricial tissue, by which it was bound down to the upper jaw. The edges of the wound in the upper lid were then brought together, and the flap retained in its new position below the lower lid by sutures. The whole of the parts operated on were then covered by a layer of cotton-wool saturated with styptic colloid. In this operation, care was taken to dissect up the lacrymal sac with the inner end of the eyelids. An attempt was made to draw up the flap thus formed, without dividing the lower end of the sac and the commencement of the nasal duct, but this was found to be impossible; the lower end of the sac was, therefore, cut across, and the divided