nature was not ascertained. Tubercle in the brain-substance occurred in one case. There were several hard masses as large as a filbert in the white matter of the hemispheres, while the corpus dentatum of the cerebellum was almost entirely supplanted by a hard scirrhus-looking mass, through which nerve-fibres appeared to pass. Doubting the nature of this product, I submitted it to Dr. Lockhart Clarke, who kindly examined it, and pronounced it to be tubercle. The child in whose brain this deposit occurred will doubtless be remembered by some of the members of this Branch, as having been pointed out to them when they visited the Asylum. He had never learned to walk, seldom used his hands, though they were by no means powerless; but could make wonderful use of his feet, with which he would throw or catch a small cushion, hold a biscuit while eating it, rub his eyes, or scratch his head. The movements of the eyeballs were imperfectly controlled, but this was the only apparent defect of co-ordinative power. The child died very suddenly, without any previous illness. A case of disease of the knee-joint and another of the shoulder-joint were originally due to a severe attack of scorbatus, the former surviving three years, and the latter about three weeks.

One of the cases of caries of the vertebræ was interesting, from the fact, that, though dislocation of the axis and atlas actually occurred, death did not immediately result from it. The nature of the disease was suspected some time before death, and the patient very carefully attended to in bed. The actual cause of death was intense inflammation of the membranes, consequent upon the entrance of pus into the cranium through the foramen magnum.

The term Juvenile Senility is the only appropriate one with which I am acquainted for describing a not uncommon cause of death in idiots. An undeveloped child (perhaps I ought to say "man", for he was 22 years of age, though only 2 feet 4 inches high), suddenly began to fail in spirits and in appetite. His circulation became gradually more feeble, and his temperature fell, until his extremities were perfectly cold. Death resulted in a few days, and a *post mortem* examination revealed no disease.

THE DETERMINATION OF REFRACTION OF THE EYE.

By W. LAIDLAW PURVES, M.D.

EVERY one who has to determine the refraction of numerous patients, knows the trouble and labour involved in finding the number of lens in the test-box, cleaning the lens when found, placing it in a proper position before the eye of the patient, and replacing it in its proper slit in the box. When to these are added, in the case of patients with astigmatism, the placing of the axes of the cylindrical lenses always in the already determined meridian, no one is able satisfactorily to order glasses in the usual hours of a *clinique* for half the cases of astigmatism which present themselves at any large ophthalmic hospital. I believe that half an hour is about the limit which should be given to a case at a visit. Though the surgeon may not have tired of observing, the patient is generally by that time in a condition in which his answers cannot be well relied on; and, if the surgeon have not by that time satisfied himself of the lens necessary, it is better to hold by the observations already made, and add to them on a future occasion. Any means which will set aside the difficulties above mentioned, and at the same time determine the refraction with accuracy equal to the means already used, will prove of use both to patient and to surgeon; and it is on this account I have had the following instruments constructed.

In two discs revolving from their centres, and placed on a telescopic stand, are cut thirteen apertures, in twelve of which are fixed twelve plano-convex and twelve plano-concave spherical lenses; the discs containing the one kind of lenses revolving, so that any number of lens in that disc can be brought against any lens in the discs containing the opposite kind of lens, as in the ophthalmoscope I proposed in the beginning of 1873. The two remaining apertures are left vacant. By these means, numerous powers are obtained, which will be found especially useful in cases of high degrees of ametropia.

The glasses placed in the discs are as follows: + and - 2, 3, 4, 5, 6, 8, 10, 13, 18, 24, 36, and 48; and by these are obtained the following + or - powers; viz., 2, 2.08, 2.11, 2.18, 2.25, 2.36, 2.5, 2.66, 3, 3.2, 3.27, 3.33, 3.42, 3.6, 3.9, 4, 4.28, 4.36, 4.5, 4.8, 5, 5.14, 5.58, 5.77, 5.80, 6, 6.31, 6.66, 6.85, 6.92, 7.2, 7.5, 8, 8.12, 9, 9.6, 10, 10.28, 11.14, 12, 12.63, 13, 13.33, 13.84, 14.4, 15, 17.14, 17.82, 18, 20, 20.8, 20.34, 22.5, 24, 28.36, 28.8, 30, 36, 40, 43.33, 46.8, 48, 72, 144. Each of these is engraved on one disc, and seen through aper-

tures made in the other disc; but, should there not be sufficient to satisfy the demands of some, the enlarging of the discs and the substitution of other or more glasses is easy.

For the determination of astigmatic cases, the same method of revolving convex over concave cylindrical lenses, or *vice versa*, is used; the placing of the axes in the different meridians being obtained by the same method as in Javal's optometer, or by causing the patient to look through the aperture in the disc corresponding to the meridian of his astigmatism. The two discs are so constructed as to revolve round a graduated circle external to their peripheries, so that any glass is brought before the eye with its axis in any desired meridian; while, being at the same time able to revolve upon a central axis, any single glass or single combination of glasses in the discs may be used as in the discs containing the spherical lenses. The cylindrical glasses used are as follows: 5, 6, 8, 12, 36, and 48 + and - , by which the following powers of each kind are obtained; viz., 5, 5.58, 5.80, 6, 6.85, 7.2, 8, 8.57, 9.6, 10.28, 12, 13.33, 16, 18, 24, 30, 36, 48, 144.

The two instruments may be used separately or together.

For the purpose of controlling the patient's accuracy of observation for the different meridians, I use a disc with varied numbers of lines engraved in different meridians, by the revolving of which one can determine whether the patient sees accurately in any meridian by his counting correctly the number of lines placed in that meridian. I have had lines and letters similar to Pray's cut out in metal, and so placed against a light that patients in whom the circles of diffusion are abolished sufficiently by lenses, can recognise the number of lines or the letters used; but I find the above described method the simplest.

As the calculation of fractions is a trouble to some, I have had a boxwood scale made, which will suffice for calculating the fractions on the ophthalmoscope and optometer, the latter of which would be considerably lessened in price by the using of the card instead of the engraving and boring of the discs necessary to show the combinations.

This optometer fulfils, I believe, all the requirements necessary in determining cases of ametropia, in that by it we can determine at the same time the condition and degree of refraction and the acuteness of vision, the latter of which has, in the optometers of Young, Lawrence, Burow, Graefe, Javal, etc., to be determined as a second step, and all artificial values are done away with.

I trust that, by thus simplifying the determination of ametropic cases, general practitioners may be induced to give more attention than hitherto to the relief and prevention of the numerous and varied consequences which the non-use of glasses in ametropes often entails.

MICROSCOPICAL EXAMINATION OF THE SPUTUM IN PHTHISIS.*

By JAMES SAWYER, M.D.Lond., M.R.C.P.,
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I DESIRE to say a few words upon the method and value of a microscopical examination of the sputum in phthisis. During several years, I have been in the habit of occasionally using the microscope in the examination of pulmonary expectoration. In some cases of chronic phthisis, such a procedure may sometimes give information which no other mode of research can furnish. It is not necessary for my present purpose that I should enter into a discussion of the pathology of consumption of the lungs. My remarks concerning sputum apply to cases of ordinary chronic phthisis. The term "phthisis" has now become generic; it includes several varieties of pathological change, the discrimination of which is of the highest practical importance. But all my hearers—whether, with Laennec and many others, they believe in the existence in the lungs in phthisis of crude lumps of yellow tubercle; whether, with Niemeyer, they hold such masses to be simply inflammatory products which have undergone a cheesy metamorphosis; or whether, following the example of many English pathologists, they take that middle course which is usually held to be the safest—whatever be their own views on a subject which is certainly at present far from clearly worked out, my hearers all know what I mean when I speak of the ordinary form of chronic pulmonary phthisis. I mean the commonest kind of consumption. I mean the commonest of all diseases of the lungs. I mean a disorder which runs a chronic course, and is characterised by the extensive and extending solidification, softening, and excavation of circumscribed portions of pulmonary tissue.

So far as I remember, my attention was first drawn to the importance

* Read before a meeting of the Microscopical Section of the Birmingham and Midland Counties Branch.

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