

4. The PRESIDENT stated a difficult case of Strangulated Hernia, which elicited similar interesting cases from Dr. Hughes of Mold, Dr. Williams of Mold, Mr. Eyton Jones, and others.

5. Dr. WILLIAMS (Wrexham) mentioned a case of Compound Fracture, in which Mr. Lister's plan of treatment with Carbolic Acid was adopted. It was successful so far as preventing suppuration in the wound; but it produced troublesome superficial sloughing of the surrounding integuments when used of the strength recommended—one part of carbolic acid to three parts of linseed-oil. Dr. Williams suggested a weaker application, of one part of the acid to five of oil.—Dr. WILLIAMS (Mold) called attention to the fact of his having, a few years ago, and before Mr. Lister published his cases, introduced, at one of the meetings of the North Wales Branch, the subject of carbolic acid, more particularly its adaptability to several commercial uses, showing at the time several beautiful specimens.

6. Mr. LODGE (St. Asaph) had a case of Twins. The principal point of interest in the case was, that the first child presenting the arm necessitated turning; and, in doing so, the presence of another child was not detected.

7. Mr. LODGE also related a singular case of Self-Pollution.

8. Mr. JONES (Ruabon) brought forward the subject of Insurance Fees, which drew a long and animated discussion.

Dinner.—All the members present, with Rev. Thomas Williams of Berse and Rev. J. James of Mold as guests, then dined together at the hotel, and were very agreeably and hospitably entertained by Dr. Hughes and Dr. Williams of Mold.

CORRESPONDENCE.

DEBATING COLUMN FOR DISCUSSION OF PAPERS, ETC., PUBLISHED IN THE "JOURNAL".

THE ANCIENT AND MODERN TREATMENT OF INJURIES.

SIR,—In No. 421 of our JOURNAL, page 74, is a case, under the care of Mr. Lane, which astonishes me, and brings to my mind similar or apparently more serious cases treated by me by the most strident antiphlogistic method possible, and with the happiest effects. I will give the particulars of three cases, and allude to probably a hundred more.

The first was that of a young man, who, with his companions, went to a wake about two miles hence; and, on his return home drunk, fell down in descending a steep part of the road, and was brought to me in a very ghastly state, the whole of the hairy scalp turned inside out, and hanging over the face, and covered with dirt. After cleaning the parts, the pericranium was detached here and here. I replaced the scalp, and kept it *in situ* by a few stitches. Seeing that he had not lost much blood, I took away from the arm about sixteen ounces, and enjoined not only abstinence from all stimulants, but the most meagre diet, quietness, and rest, for the next three weeks. With this, a few doses of calomel, and the bowels kept lax with sulphate of magnesia and rhubarb, he was quite well and at work at the month's end.

The second case was that of a boy ten years old, who, in the absence of his parents, was amusing himself by putting a lighted stick into a stone blacking-jar in which his father kept gunpowder. After a few trials, it exploded, and burst the jar in pieces, a piece of which, about an inch and a quarter long, and of the same breadth for about a quarter of an inch, when it rather gradually ended in a point, entered, in the middle of the forehead, into the brain, the broad part sticking out. With some difficulty I extracted it, and about two teaspoonfuls of the cerebrum followed it; the boy all the while perfectly sensible. The same mode of treatment for three weeks cured him.

The third case was that of a boy about the same age, the son of a blacksmith. He was kicked by a horse in the middle of his forehead; and a piece of the os frontis, about two inches long and half an inch broad, was driven into the brain, part thereof sticking out. On its extraction, about the same quantity of brain followed as in the preceding case. The same treatment as above was followed by the same happy result.

I dare say that I could adduce a hundred cases apparently as severe as that of the poor young man at St. Mary's Hospital, all treated as above, and all cured; but, if iron, wine, chloric ether, and probably diet accordingly, had been the *methodus medendi*, God only knows the sequelae. Such a mode of treatment I consider illogical, unorthodox, and thwarting our great friend, the *vis medicatrix nature*.

I am, etc., THOMAS POPE.

Clebury Mortimer, Salop, February 1869.

THE CAUSE OF PHTHISIS.

SIR,—Having read the letters which have passed between Drs Leared and Mac Cormac on this subject, I would make a few remarks which appear to me fatal to the theory—for it is nothing more—of the last named gentleman; who assumes he has proved that the "invariable result"—I use his own words—of the habitual respiration of pre-breathed air is phthisis. Where, I ask, is his proof? I have read plenty of his assertions; but none of his proofs. In truth, he starts with an assumption which I distinctly deny. For, if his theory were correct, in place of sixty or one hundred thousand dying annually of this disease the numbers would be increased one-hundredfold. For myself, I have known a large number of persons who have been confined to the house or bedridden for years, and who have done their best to prevent draught of air from reaching them; and yet they have not got phthisis. Many, very many too, of our poor sleep constantly with their head covered over by the bed-clothes. To say that none of these get the disease, would be going too far; but most assuredly all do not; and yet according to the theory, every one should.

Again, is it not allowed by all that it is quite the exception when phthisis attacks a fat person? If the theory were true, however, fat persons should be only the more obnoxious to the disease; but while they do not get tubercle, they store up fat. And this reminds me that Dr. Mac Cormac, through the whole of his writings, speaks as if carbon and tubercle were identical. This, I consider a grave error; capable in itself of upsetting the entire hypothesis. If tubercle consisted of nothing but carbon, there might be some plausibility for the theory. But as it is not so, I need not pursue the point further here.

It seems to me that the question of Iceland being free or not from phthisis is only of secondary moment. It affects the argument much more to observe that here we have a whole people, living in a way that, according to the theory of Dr. Mac Cormac, must engender phthisis. Yet they have not died out; and are, I believe, as numerous as ever they were. Before going further, I call for direct answers to this and the other points brought forward in this letter.

I am, etc., HENRY KENNEDY.

Dublin, March, 1869.

SIR,—I shall only occupy your valuable space with a few lines in reply to Dr. Mac Cormac. It would be simply useless again to go over the same ground, to point out the one-sidedness of his views on the production of phthisis. "Why," he asks, "should we remove the trial of the question to Iceland?" It is, no doubt, inconvenient as regards the re-breathed air theory. "I myself," says Dr. Mac Cormac, "assert that Iceland is not less the seat of tubercle than the country in which we live." Yet I have quoted statistics from his own book, in my last letter, which prove that Iceland enjoys a remarkable exemption. Since this controversy commenced, Dr. Mac Nab has published a pamphlet on *Immunity from Consumption in the Hebrides*, to which I would wish to draw Dr. Mac Cormac's attention. Dr. Mac Cormac speaks of the "possible inhalation of tapeworm-germs." This is certainly novel, more novel than the inhalation-of-phthisis theory. I still pin my faith to Dr. Hjaltalin's statement; and, if Dr. Mac Cormac will be good enough to let this controversy lie over for a short time, Dr. Hjaltalin will, no doubt, answer for himself, and, unless I am much mistaken, will contribute valuable information on a subject which, *pace* Dr. Mac Cormac, is by no means settled.

March 1869.

I am, etc., ARTHUR LEARED.

VESICAL ABSORPTION.

SIR,—In your issue of February 20th, appears a letter from Dr. Campbell Black, of Glasgow, upon "Vesical Absorption," in which he brings forward a case to prove "the great absorptive power of the bladder." The cause of his publishing this case, was a passage in one of Sir Henry Thompson's lectures on "Affections of the Urinary Organs," which states, that "the mucous membrane of the bladder appears to have no absorbing power, unlike the neighbouring tissue which lines the rectum." When I read Dr. Black's letter, two things occurred to me—either that the absorptive powers of those bladders, which I, as Sir H. Thompson's house-surgeon, have had the advantage of observing, have been peculiar, or that Dr. Black had attached too great importance to some soreness of the gums. The following case, I think, ought to satisfy Dr. Black, and prove the "fallaciousness" of his doctrine, and substantiate Sir Henry's statement: On the 10th instant, Sir Henry, after reading out Dr. Black's letter to the class, injected into a man's bladder half an ounce of the liq. opii sedat. This was retained for two hours and a half, without producing the least effect, although I carefully watched for it. No drowsiness, contraction of the pupils, or subsequent constipation. Before this, ʒiiss, ʒij, and ʒij had been injected