

of the kidneys, etc., there are four times as many deaths from the same causes as in the abstaining section; still more, against the proportion of 32 in every 1,000 of the ordinary section who have died of liver disease, not one of the abstaining section has died of such affection. Again, deaths by accidents (including suicides) are in the proportion of 13 in the ordinary section to only 1 in the abstaining section. These figures are gathered from large numbers of assurances; persons, when proposed, known to be intemperate are rigidly excluded." The report further states that: "Of all diseases among the intemperate, cancer shows the greatest preponderance. Total abstinence augments considerably the probability of a death from old age or natural decay only."

Mr. Bass does not quote the editorial article of March 17th, 1888. It gave a summary of statistics drawn from life assurances and the duration of benefit-drawing sickness in the ordinary friendly societies on the one hand, and in societies consisting exclusively of abstainers, as the Rechabites, on the other. It concludes with these remarkable words: "There can be little doubt as to the general tendency of these striking tables in favour of the healthfulness of abstaining temperance."

Years ago innkeepers, finding life assurance offices unwilling to assure them, started an office of their own; the claims were unusually heavy, and the Court of Chancery dissolved it after an experience of about seven years. Mr. Bass said: "The condition of the licensed victuallers and of the wholesale trades was one of great jeopardy," but consoled his hearers by saying that, "whenever threatening measures were before the House the agents of the trade were always in the lobby to supply the members with the true facts." I have compared his quotations with the authority whence they were drawn, and I will leave your readers to judge whether the so-called "facts" given to our M.P.'s are either fair or trustworthy.—I am, etc.,

February 23rd, 1889.

MEDICUS.

"ALBUMINURIA OF ADOLESCENTS."

SIR,—The correspondence in the JOURNAL between Dr. Johnson and Mr. Lucas respecting the interpretation of certain passages in a paper by the late Dr. Moxon on the "Albuminuria of Adolescents," seems to indicate at least one important thing—that doctors, at any rate when writing on professional subjects for doctors, would do well to convey their meaning by plain words rather than by "delicate" hints.

Now, despite Mr. Lucas's opinion that "the author's views as to causation, though delicately put, were clear to everyone except to Dr. Johnson," I venture to confess that on reading the paper in question some years ago, I found the same difficulty that Dr. Johnson found in understanding exactly what was meant; and it was only some time afterwards, on looking up the same subject in Dr. Dickinson's excellent work on *Renal and Urinary Affections*, that I learnt what Dr. Moxon's views really were. Moreover, I learnt that Dr. Dickinson agreed with Dr. Moxon that most cases of this form of albuminuria were due to (pray pardon the "indelicate") masturbation.—I am, etc.,

Old Burlington Street, W.

ARTHUR COOPER.

OBSTRUCTED LABOUR FROM GROWTH ON FETAL HEAD.

SIR,—Dr. Laird Pearson's case reported in the JOURNAL of March 2nd is of a rare kind. In the forthcoming *Transactions of the Obstetrical Society*, a case under my own care will be found described and depicted. The tumour was a sarcoma, and of the size of a small cocoa-nut, situated in almost exactly the same place as in Dr. Pearson's case; yet it caused no dystocia, owing probably to the mother having a very capacious pelvis. Some of the secondary growths which developed as the case progressed had much the appearance described by Dr. Pearson, and I should be inclined to infer that the tumours in his specimen were probably malignant in nature. The majority of head tumours enumerated as affecting the newly-born fœtus (apart from encephaloceles) are of a malignant nature, being chiefly of the sarcomatous or myxomatous type. I believe his specimen and the mode of dystocia to be unique. I am, etc.,

Harley Street, W.

JOHN PHILLIPS, M.D.

PUERPERAL SEPTICÆMIA.

SIR,—It appears to me that writers like Dr. Barnes tend to confuse men's minds on the question of puerperal septicæmia. He talks of "the blind adherents of the dogma that puerperal fever is septicæmia and nothing else" in his paper published March 2nd.

The subject, indeed, is not one that will be advanced by dogmatic assertion on either side.

The first question really is this: What are we to include under the head of puerperal fever? And it would be well if Dr. Barnes were to clearly state what he includes. If we look at the question from the point of view of fevers occurring in the puerperal state, cannot the following be included?—

1. Any of the exanthems, pneumonia, or, indeed, any disease that may occur in or out of the puerperal state which is accompanied by pyrexia, for it will now be generally allowed that such diseases are the same diseases, whether they occur in or out of the puerperal state.
2. Fever in connection with lactation.
3. Septicæmia or septic intoxication.
4. Septicæmia and pyæmia.
5. Peritonitis (a) simple, (b) septic.

It is clear that some of these may be autogenetic and some not. Does Dr. Barnes assert that all may be autogenetic, or is the question simply whether puerperal peritonitis may be simple as well as septic, or whether septicæmia, sapræmia, or pyæmia may arise from the absorption of normal lochia as well as from septic? The question, Sir, is one of such extreme importance to the lying-in woman that I make no apology for attempting to obtain a clear statement from Dr. Barnes.

The recent discussion at the Royal Medical and Chirurgical Society upon Dr. Smith's paper, dealing with the germ theory, showed what very loose ideas many obstetricians hold on the subject, and the impossibility of the general adoption of antiseptic principles in the practice of midwifery until the question is settled. Without definite and clear teaching on the subject from those who are responsible for the education of doctors, the time is far off when a doctor will be held guilty if he does not carry out antiseptic treatment to the very utmost of his ability.

I am one of those, however, who still have faith that, notwithstanding partisanship, such teaching, the result of impartial, careful work, will be forthcoming, and that before long.—I am, etc.,

Hatfield, Herts, March 3rd.

LOVELL DRAGE.

RAISING THE EPIGLOTTIS.

SIR,—Repeated communications have appeared in the JOURNAL from Dr. R. L. Bowles, under an apparent impression that in my recent short paper on "A new and only way of Raising the Epiglottis," in the JOURNAL of November 17th, supposed work and writings of his own received inadequate recognition.

As Dr. Bowles has in his last communication generously acknowledged, unasked, that one alleged fact of leading consequence in his misapprehensions he now finds to have been impossible, I am encouraged to make a brief statement of facts, some of which are only known to myself, and which may, I trust, allay any misapprehensions which possibly yet remain. Briefly, then, I state (1) Dr. Bowles's name was quite unknown to me until he was kind enough to send me a pamphlet, as he may perhaps remember, in 1881. As he now knows, my discoveries referred to were, as he says he has just found, completely published in the *Proceedings of the Royal Medical and Chirurgical Society of London* in 1878. (2) Since Dr. Bowles's article of January 12th, I have, with the aid of two experts, faithfully investigated the question, and find nothing which has been referred to which, had I known it before 1878, could have helped me in any way towards the results I then published. My findings of publications on this subject by Dr. Bowles are as follows: from 1853 to 1888 inclusive, one paper only. Its title was "On the Treatment of Apnoea in the Drowned; the relative merits of the Marshall Hall and the Sylvester method," read before the South-Eastern Branch, East Kent district meeting, November 28th, 1863, and published the following year. My usual index searches on this subject had been under the headings—*anæsthesia*, *anæsthetics*, *asphyxia*, *artificial respiration*, *resuscitation*, *apnoea*, *drowning*. This solitary paper, under any of these headings, had certainly been overlooked, and from being in connection with the proceedings of a local provincial society, this does not seem to me, even now, at all remarkable. This is my explanation of the fact in statement (No. 1). Now for statement (No. 2), as indicated by its title. The sole purport of this solitary paper was to show the superiority of the Marshall Hall method. In this paper is the only published case of apnoea treated by Dr. Bowles. Here is the treatment: "I at once, though a stranger, introduced my finger into his mouth, and hooked up the base of the tongue." This is all that is said to have been done, and the treatment was consistent. The whole paper is an en-