

SOUTH-EASTERN BRANCH: WEST SUSSEX DISTRICT.—The autumn meeting of the above district will be held at the Marine Hotel, Worthing, on Thursday, November 22nd, at 4 P.M., Dr. Kelly in the chair. The following gentlemen have promised to read papers. Dr. Kelly, on House-Drainage; and Dr. Uthoff. The dinner will be served at 6 P.M.—G. B. COLLET, Honorary Secretary, 5, The Steyne, Worthing.

CORRESPONDENCE.

PYOSALPINX.

SIR,—Will you kindly permit me to publish the following extract from a letter just received from Dr. Lusk, professor of surgery at the New York Hospital?

“As the first-fruit of my visit to Birmingham I have to report the removal of a tube distended with pus to such a degree that at first I could hardly believe it was not a portion of the large intestine. It was over two inches in diameter, and I was obliged to enlarge the abdominal wound to five inches before I could extract it. The operation was performed two weeks ago, and the patient appears to be relieved of the source of a great deal of suffering.”

This letter goes to prove the points upon which I laid some stress at the last meeting of the Obstetrical Society, that these cases are not at all rare, that the patients are in constant danger of their lives, and that the disease can be dealt with only by abdominal section.—I am, etc.,

Birmingham, Nov. 13.

LAWSON TAIT.

NITRITE OF SODIUM.

SIR,—As a recent paper by Dr. Ringer and myself on “Nitrite of Sodium as a Toxic Agent” has excited adverse criticism in the daily press, I venture to ask permission to say a few words by way of explanation. It has been said that we made experiments on our patients with the view of investigating the action of the drug. This is entirely untrue; and no experiments or experimental observations of any kind have been made on any one. The circumstances of the case are briefly these. In the early part of last year a paper appeared in a well known journal of therapeutics recommending nitrite of sodium as a remedy for epilepsy. It was stated that this drug was allied to nitroglycerine, but was superior to it, as its effects were more lasting. For some years I have used nitroglycerine with marked success in the treatment of many complaints, including angina pectoris, pseudo-angina, asthma, neuralgia, sick-headache, dysmenorrhœa, and the various forms of Bright's disease. When I heard of a remedy which was said to be superior to nitroglycerine, I hastened to give it a trial, for I was anxious that my patients should have the benefit of the latest discoveries in medical science. I was cautious, however, and gave it only in half the dose originally recommended, ten grains, that is, instead of twenty. The first patient who returned at the expiration of the week said that it had done him no good, and expressed an opinion that it was not strong enough. I then gave him fifteen grains, and this was the only case in which the dose was increased. The second patient, who had also taken ten grains, said it made him giddy and feel sick. He complained of other symptoms, which I thought could hardly be attributable to the drug, for I knew that it had been recommended in twice that dose, and I myself had just seen a man who had taken the ten grains three times a day for a week without the production of any symptom whatever. I asked the man how many doses he had taken, and he said only one. I urged him to try it again, but he declined, saying he had a “wife and family.” A great deal has been made of this expression, but it was said jokingly, and without any intention of conveying the idea that it was a serious matter. As the other patients returned one by one and complained, I saw that the drug had really disagreed with them. This was the first intimation I had had from any source that nitrite of sodium possessed toxic properties. I at once reduced the dose first to five and then to three grains. It was at this stage that I communicated with Dr. Ringer. He thought the matter of such importance that he made two experiments on cats, forwarding me the results. I, in drawing up the paper, in deference to his seniority, placed these experiments first, but, in point of time, they were not made until long after I had observed the untoward effects of the drug on my patients. This is a plain, straightforward statement of facts. There has been nothing in the shape of experimentation; and I have prescribed nitrite of sodium only in those cases in which, from my previous experience with nitroglycerine, I was convinced that it would prove useful.—I am, sir, your obedient servant,

WILLIAM MURRELL, M.D.

38, Weymouth Street, W., November 15th, 1883.

SIR,—It is much to be regretted that the recent investigation of Drs. Ringer and Murrell into the physiological action and dose of sodium nitrite should have been the means of eliciting hostile criticism from the lay press and public institutions. Of so potent an addition to our *materia medica*, too much cannot be known; and, in the interests of public safety, it was eminently desirable that the untoward effect of large doses of the pure drug should be prominently brought forward after careful investigation of the subject by competent observers. I feel anxious that wrong notions as to the dose of sodium nitrite should be corrected in all quarters, since to me, I believe, attaches the responsibility of introducing this agent to the notice of the profession in this country as a remedy. In the *Practitioner* for June 1882, I have described a case where I gave it in twenty-grain doses, with no ill-effects whatever. At that time, however, the drug was not in use as a medicine; and, though some was specially obtained at my request from a leading London firm, it was very largely adulterated with *nitrate*, and hence the harmlessness of scruple doses. I was not, however, aware of this impurity until some months afterwards, when Dr. Ralfe, who, at my suggestion, was using it at the London Hospital, discovered a most perplexing and dangerous variability of purity in a large number of specimens obtained from the principal makers, and to this circumstance is largely attributable the occasional ill effects following its administration, brought forward by Dr. Ralfe and Dr. Ramskill at the Royal Medical and Chirurgical Society in October 1882. It cannot be too widely known that it is unsafe to begin with a larger dose than two grains of Morson's sodium nitrite (less will often produce headache). Unfortunately, in the latest edition of Dr. Ringer's work on *Therapeutics*, the dose is given as 20 grains; and, as my name appears in connection with the statement, I am desirous of explaining that my conclusions were based upon the employment of what eventually proved to be a very impure specimen of the salt. To the mistakes of respectable wholesale firms is chiefly due the danger that has attended the ordinary use of sodium nitrite in medical practice; and it is of the greatest importance that the facts should be made known, and thus future mishaps avoided.—I am, sir, your obedient servant,

W. T. LAW, M.D., F.R.C.S.

St. Leonard's-on-Sea, November 14th, 1883.

SIR,—In the course of a review of the tenth edition of Dr. Ringer's *Therapeutics*, appearing in the *JOURNAL* of November 10th, there occurs the remark: “On the succeeding page, a terrible mistake is made, nitrite of sodium being recommended on the authority of Matthew Hay, Mitchell, and Law, in angina pectoris, and epilepsy, in *twenty-grain* doses.” If the reviewer had either consulted my paper, or perused more carefully the work of Dr. Ringer, he would have found that I never recommended so large a dose of the nitrite. I employed it in small doses; and was the first to point out the great impurity of ordinary preparations of the salt.

Yours faithfully,

MATTHEW HAY.

230, Union Street, Aberdeen.

COATS'S MANUAL OF PATHOLOGY.

SIR,—It is not customary nor desirable for authors to reply to criticisms of their works appearing in the public journals, but an exception may surely be made when matters of fact are in question. In the notice of my *Manual of Pathology* in your last week's issue, your reviewer, in illustration of his principal criticism, makes two statements, both of which I hope to show to be incorrect. He states (1) that I make out the whole pathology of tuberculosis to be expressed in the bacillus discovered by Koch; and (2) he goes on to state that, with the exception of a line and a half, in which it is stated “that it is only persons who have natural or acquired weaknesses that become the subjects of tuberculosis,” that is the sum of my teaching.

As to the last mentioned statement, the quotation given is from p. 167; but at p. 165 there is nearly a paragraph devoted to the enunciation of the view that, “in order to the development of tuberculosis, there must be some existing condition of the body predisposing.” The part of the book in which these quotations occur is that dealing with general diseases; and in this department the various local peculiarities did not call for prolonged consideration, these two references being judged sufficient to prevent the student inferring that the bacillus is everything. At pp. 543 to 546, where the causation of phthisis pulmonalis is treated of, the local peculiarities receive the greatest prominence, while the bacillus is scarcely more than mentioned.

The other statement of the reviewer is that, in my book, "the whole pathology of tuberculosis is expressed in the bacillus." Now, it is a singular commentary on this statement, that the whole of the article on tuberculosis, with the exception of the page devoted to Koch's discovery, was written more than a year before that discovery was announced. My notes show that this part was written before February 1881, while Koch's discovery was published in March 1882. I have just referred to the manuscript written at that time, from which the article was printed, and I find scarcely a word altered so as to conform to Koch's discovery, the account of that being intercalated and paged separately. It has struck me as remarkable, that Koch's facts could thus be inserted without dislocating anything. The main description of tuberculosis is based on the view, which I have held and taught for years, that true tuberculosis is, in its essence, an infective disease, owing its origin to a specific virus. Koch's discovery exactly met this view; and, considering the importance of this discovery, surely one page was not too much to devote to it. As a matter of fact, this is all the space it occupies.—I am, etc.,

JOSEPH COATS, M.D.

Glasgow, November 5th, 1883.

** An author is naturally sensitive and critical, and, therefore, liable to be unreasonable, in his treatment of his reviewers; and we venture to think that, in making complaint of inaccuracy of statement as to matters of fact, of which we cannot admit the justice, Dr. Coats is under a misapprehension derived from this source. There is no inaccuracy of matters of fact, or only a seeming one. The writer had no means of knowing what Dr. Coats had been in the habit of teaching in regard to the pathology of tuberculosis prior to the discovery by Koch of the bacillus tuberculosis. All that could be said was, and we see no reason to amend it, that Dr. Coats is a thorough-going champion of the bacillus as a cause of tuberculosis. If anyone doubt the accuracy of the opinion, let him see for himself. We find it stated, for instance, that Koch's observations "distinctly prove that tuberculosis is a specific infective disease." One would be at a loss to know what more positive acceptance of Koch's views could be held. Nor is it easy to see what objection can be taken after it to the statement in the review that the whole pathology (not morbid anatomy, an attempt was made to distinguish between the two) of tuberculosis is expressed in the bacillus. If the bacillus be the cause of tuberculosis, there is but little room for any other pathology. What little room there may be left is occupied by the qualifying clauses concerning resistance of the tissues. The writer of the review happens to hold the opinion, not altogether singular, that it is much too early as yet to shake the bacillus tuberculosis so warmly by the hand, least of all in a text-book, the statements of which are copied, without question, everywhere. That all Dr. Coats's previous teaching led up to, and fitted in with, some such extraneous origin, is, no doubt, very valuable independent testimony to the importance of Koch's observations, but it hardly constitutes a sufficient body of evidence, to render it advisable to teach the student that the question is a settled one.

On the second head, all that was intended—it must be allowed that the wording admits of some objection—was that, concerning heredity, the power of resistance to the disease which some types of configuration appear to possess, its non-contagiousness, the extraordinary chronicity which many cases exhibit, the relations to soil, to climate, to race—concerning all these things, which bear upon, and constitute the pathology of, the disease, in the widest sense of that term, where not explained by the vagaries of growth of the specific bacillus, the sum of Dr. Coats's teaching seems to be fairly expressed by the line and a half quoted, "it is only persons who have natural or acquired weaknesses that become the subjects of local tuberculosis."

A conscientious reviewer must sometimes say things which appear a little harsh; but Dr. Coats may be assured that we desire to do him justice; and he must have seen that we have said some not altogether unkind things of his book. It has our best wishes, and we shall be glad to hear of a speedy call for a second edition.

FRACTURE OF THE PATELLA.

SIR,—I was present on Monday evening, November 5th, and heard with much interest the discussion on Professor Lister's paper at the Medical Society. It appeared to me that everyone was of opinion (as, indeed, I think all surgeons are) that bony union of the patella, after transverse fracture, is to be desired; but the general conclusion arrived at was, I think, that the risk incurred by opening the

knee-joint is too serious a one, in ordinary cases of transverse fracture, to justify the operation proposed by Professor Lister. Now, in all probability, in most cases of transverse fracture, the fractured portion communicates with the knee-joint, and, therefore, the operation of uniting the two ends of the bone must, if performed immediately after the accident, necessitate opening the joint; but, by sufficient rest, the communication between the joint and the fracture would probably be closed, and then, by a careful operation performed antiseptically, might not the fractured bone be cut down on, the fractured edges pared, and brought together by silver wire suture, without opening the joint at all; and, at any rate, if this could not be done in recent cases, might it not be in those in which, after long treatment, the leg was found to be practically useless, and in which most surgeons seemed to consider Professor Lister's operation justifiable.

I may add that, during Monday evening, I mentioned this to two distinguished surgeons. One thought this operation not practicable, the other seemed to think it feasible. Under these circumstances, I think it worth while bringing this idea to notice, and am, sir, your obedient servant,

E. D. TOMLINSON,

Brigade-Surgeon (half-pay).

Junior Army and Navy Club, November 6th, 1883.

UNFOUNDED CHARGES AGAINST MEDICAL MEN.

SIR,—It seems to me high time, after the deplorable and lamentable deaths of two able and useful members of the profession, that we should wake up to the great danger to which we are always exposed, of being criminally accused of some assault or malpractice by nervous, hysterical, lascivious or evil-disposed women, and at once start a defence fund, so that in the event of any member of the profession being so accused, he might at once be defended by the most skilled legal advice in the country, and supported out of the purses of his fellow medical men.

Only this year a young lady, by birth, called upon me for advice, as her natural epoch had ceased, and positively assured me that she knew of no cause except that she had taken cold. I therefore innocently gave her medicine, until I was struck by the fact that she always came to me dressed in an ulster. I, becoming suspicious, changed her medicine to syrup of senna, that time might prove her condition. She, not being satisfied, went to another physician, by the advice of her friends, without first consulting me; and from his prescription I am certain he did not suspect that she was pregnant. Of course I refused to have anything more to do with the case. Not many weeks after this, she was confined, and was attended by another medical man, without even communicating with me, so my refusal did not inconvenience her in the least.

Now, I should not have been at all surprised had she aborted; and, had I obtained a warrant against her, which I certainly should have done, no doubt she would have met the charge by saying that she told me of her condition. How different it would have been had I been the guilty party. I think it would be only charitable on our part if we, after the trial in which the late Mr. Haffenden was accused, sent a deputation to his widow conveying our heartfelt sympathy in her sad bereavement, and offering to refund all the legal costs to which she may be put.

I trust also, although this is outside the scope of this letter, that the profession and students of University College, with the help of the outside public, will raise a memorial to the memory of the poor "Khan" who so courageously, and with such success, came to this country to master the difficulties of a profession which, even to a Britisher, are many, and after he had gained two of the most honourable diplomas cut short the good he might have achieved in a moment of despondency.

I will gladly forward my guinea for either of the above proposed funds.—I am, sir, yours faithfully,

THOMAS DUTTON, M.D., M.B., M.R.C.P.

Sidlesham, November 3rd, 1883.

SIR,—As the cases of Messrs. Bower and Keates, Mr. Haffenden and Dr. Edwards, are still fresh on our memories, could we not, as members of the British Medical Association, form a society for mutual protection, more especially as we find the Treasury with the public prosecutor arrayed against us? A small additional subscription, say 2s. 6d., would amply suffice to form a fund to engage a solicitor whom the members would have the right to consult if, on investigation, their case merited support.

As in Messrs. Bower and Keates' case, the expenses must have