

readers to judge whether for paraplegia Sir Benjamin Brodie "*prescribed indiscriminately*"; nor will I presume to raise the question whether he was or was not able to recognise a case of syphilitic disease when before him.

I am, Sir, your obedient servant,
Savile-row, W., Feb. 20th, 1875. CHARLES HAWKINS.

CROUP AND DIPHTHERIA.

To the Editor of THE LANCET.

SIR,—Any teacher who has felt difficulty in proving a doubtful question to his class will gratefully recognise a certain advantage in the authoritative decision of that question. But gratitude is well known to be a temporary sentiment, except so far as it has an eye to the future; and all who decide doubtful questions by authority will find the doubtful points still alive, and crying against extinction by weight of matter instead of by sufficient proof.

It has often chanced to me to have to demonstrate the differences between croup and diphtheria in the dead body in the post-mortem theatre at Guy's Hospital during the time I had the conduct of affairs there. I tried the several criteria presented in the books. But, first, the limitation of the disease to the trachea in croup by no means corresponded with what we found; so that I once put together observations showing patches of false membrane in the fauces in "true croup," with the intention of asking you to publish them, but was diverted from that purpose. Also I found very unsatisfactory the study of the character and adhesion of the false membrane, concluding after my examinations that the semi-purulent character in diphtheria was due to intensity of the action, and the greater adhesion due to the character of the surface, no false membrane ever adhering to the trachea, and none ever readily parting from the fauces. I found more reason to rely on the implication of the glands and the oedematous swelling of the implicated parts; but though these facts seemed to me to have very important analogies in favour of their significance, yet they were not so obvious to the class, especially to those few and valuable members who had a turn towards seeing for themselves the reasons of our conclusions.

Only a teacher under such circumstances can fully feel the satisfaction there is in now being able to say, "True membranous croup and diphtheria are identical; Sir W. Jenner and Dr. Johnson say so." And when one can add, "So does the Editor of THE LANCET," I will leave you, Mr. Editor, to estimate the entire reliance one is able to obtain amongst one's listeners.

If, in my present teaching, I am obliged to rely on these great authorities rather than on my own convictions, I will be short in humbly stating the reasons of those not yet effaced convictions,—even though I hold the relations between membranous croup and diphtheria to be exactly the same as that of fatal English diarrhoea and Asiatic cholera, and should think of a practitioner who enforced in a family the same stern measures of caution for such croup as he would use in epidemic diphtheria in much the same way as the general public think of that doctor who annually raises the cry "The cholera is amongst us" on the faith of his bad diarrhoea case.

My reason for believing that true membranous tracheitis is not, as Dr. Johnson appears to hold, always caused by diphtheria, is that I have seen false membranes identical with those in question caused by a child drinking scalding water. I only inspected two such children, and both had "croupy" membranes in their tracheæ. Dr. Wilks has also recorded such cases. Now, unless we are to believe that in these little things the diphtheria was in them when they went to the teapot, and was only waiting for the boiling water so that it might kill them, we have evidence in these children that common irritations will produce the membranes which Sir W. Jenner and Dr. Johnson say are only produced by diphtheria. If these physicians allow the croupy membrane to be produced by any other cause than diphtheria, then the whole force of their present argument is yielded by them, and they fall back amongst us into the question, how many membranes are diphtheritic and how many croupy,—a question I should not think new or promising enough to be worth discussing.

But, Mr. Editor, you are bolder, and, if in your presence it

were allowable, I could praise much the courage of your convictions. But, excuse me, do you really mean that when the little things drink out of the teapot the diphtheria hitherto unsuspected—they were healthy children—is lying quietly in them waiting for the boiling water? And that because most false membranes are caused by diphtheria? If a sportsman who rarely fired were seen to bring down his bird, one would stare if another, out of sight and hearing at the time, should claim the bird on the score that he usually shot all the game.

To assert the diphtheria between the boiling water and the tracheitis seems to me a most violent proceeding to secure a monopoly, and such as in no other field than in this vague chaos of pathological speculation would obtain a hearing for an instant. Frankly, having seen the cases, I cannot but believe that scalding water causes membranous tracheitis in healthy children; and hence, when I am told all membranous tracheitis is due to diphtheria, I must ask (1) why, if scalding water will cause it, will other severe irritants, such as very cold air, not cause it? and (2) how often do we find fatal tracheitis with the well-known characters of membranous croup attack one member in a household and show no trace or tendency to spread as diphtheria. The answers to these questions must, I submit, show, first, that it is theoretically to be expected confidently that severe cold will cause membranous croup; and, secondly, that facts observed without prejudgment show that such membranous croup is constantly occurring around us indifferent to the traceable agency of diphtheria. It seems to me that the position recently taken by Sir W. Jenner needs explaining by the remark he makes to the effect that he is induced to take it by having taught the opposite view with confidence. But the returning from beyond the line of direction is apt to carry beyond the line in the opposite direction. And it is not only in the mountains of Navarre that, when untenable positions are taken up under a new enthusiasm of authority, steps have to be retraced.

I am, Sir, yours, &c.,

Finsbury-circus, Feb. 23, 1875.

WALTER MOXON.

BLOOD-STAINS.

To the Editor of THE LANCET.

SIR,—Whilst thanking you for the very high compliment which (owing to absence from home) I have only just observed you pay me in the *Annus Medicus*, I beg leave to call your attention to an inadvertent omission, by which I am credited with more honour than is justly my due.

My claim is only to have advanced the science of forensic medicine, so that we can now discriminate between "the blood-cells of man and of other mammalia" *apt to be confounded with them in criminal trials*, especially the ox, pig, and sheep, animals from which murderers generally pretend to have received suspicious blood-spots.

Unfortunately there are some creatures, such as the dog, elephant, and monkey, from the blood of which I believe no such distinction can be made; yet as these are so rarely slaughtered, the diagnosis has hitherto seldom been embarrassed by their interference in the problem, although I fear the present discussion of the subject may lead to more hindrance of justice from this failure of our science in the future.

I am now preparing a reply to some strictures printed here upon this point, of which I will forward you a copy when it appears, and to which I respectfully ask your attention.

I remain, Sir, yours truly,

JOS. G. RICHARDSON.

Philadelphia, Pennsylvania, Feb. 10th, 1875.

PRESENTATION.—On Thursday, the 18th inst., there was a large attendance at St. John's school-room, Portsea, to witness the presentation of a handsome marble and gilt twenty-one day clock to Mr. Brietzke, late assistant-surgeon of Portsmouth Convict Prison. The clock was uncovered by the Rev. Mr. Innes, who made the presentation. It bore the following inscription:—"Presented to Henry Brietzke, Esq., L.R.C.P., M.R.C.S., &c., by the officers of Her Majesty's Convict Establishment, Portsmouth, in appreciation of his kindness and attention to them and their families during his short stay amongst them, and as a token of their esteem and of regret at his departure.—February, 1875."