

tubercles were distinctly seen to be seated within the seminal tubes, which, in many places, were dilated by the deposit. I have observed tubercles within the ducts, in other specimens of this disease. In some instances, the tubercular matter, after softening and suppuration have taken place, becomes discharged from the epididymis, or from the testicle, and the sinus closes, the disease taking the same course as it does when affecting lymphatic glands. But when true tubercle is largely developed in the testicle, and is not thus got rid of, and the gland is tunnelled by fistulous passages, no proceeding short of castration is satisfactory and effective in curing the local disease; and the operation involves only the removal of a gland already lost for all the purposes of its office. But, as I have already remarked, the operation is not often called for; because it is not often that we meet with extensive tubercular disease of the testicle unaccompanied with similar formations in the lungs, which, I need scarcely say, would render castration inadmissible.

### CLINICAL NOTES.

By MARSHALL HALL, M.D., F.R.S., &c.

(Communicated by J. RUSSELL REYNOLDS, M.D.)

#### NOTE X.—PROGRESS OF MR. MACKARSIE'S CASE OF EPILEPSIA LARYNGEA TREATED BY TRACHEOTOMY.

To the Editor of THE LANCET.

SIR,—I have received the subjoined note from Mr. Mackarsie, containing the further details of his interesting and instructive case. It contains the *proof* that the supposed effect of the tracheotomy was not a mere coincidence, for the tracheal tube, which was always too small,—as is too usually the case,—having become filled with mucus, so as to be impervious to air, and the patient being in circumstances similar to those in which he was before the operation, *laryngismus* and a fit of *epilepsia laryngea* occurred! The tube being now removed and replaced by an *amplifier* one, in the form of a wire-cage, not even the *threatening* of a fit has occurred.

It is perfectly obvious, indeed, that *all* that is *laryngismus*, and dependent on *laryngismus*, is obviated by a *free tracheal orifice*; and that the former is the true indication for the practice, and that the latter must be *adequate* to the fulfilment of this indication.

In my next Note, I may probably send you two most interesting communications, which I have recently received from Edinburgh and Plymouth, on the subject of the use of tracheotomy.

These Clinical Notes will henceforth be communicated by my friend Dr. J. Russell Reynolds, who has kindly undertaken the task of revision. In doing this, Dr. Reynolds will not be *pledged* to any of my views, with which he is, however, very well acquainted. The appeal must still be to future research, to enlarge, confirm, or correct those views, and Dr. Reynolds must be left at liberty, like the rest of us, to form his own opinions.

I am, Sir, your obedient servant,

Grosvenor-street, Dec. 1852.

MARSHALL HALL.

[COPY.]

MY DEAR SIR,—My patient continued in much the same state, as described in my note to you of Oct. 10th, for some days, but had several *threatenings*. On Oct. 27th he had two fits, but they were much altered in character, being decidedly *milder*, and continuing but a very short time. The opening at this period was undoubtedly *too small*, and I felt convinced that unless it could be dilated, there would be a recurrence of the fits.

On the 29th my patient had two fits; but they were of very short duration, the last being accompanied by a slight noise, as of *laryngismus*. On Nov. 1st he had another fit, as *severe* as any previous to the operation. On *examining the tube*, I found it *firmly plugged with mucus*, the patient being placed in precisely the same position as if the operation had not been performed! *Laryngismus* was, as before, a *prominent symptom*.

Feeling satisfied that the tracheal opening was not sufficient, I submitted my doubts to you, and in reply received from you a silver wire "cage," to be introduced as a substitute for the tube. As I have before said, *prior* to using the "cage," my patient had occasional fits, though not of the kind designated "epilepsia gravior," but such as you have termed "epilepsia abortiva." Subsequently to the receipt of the cage, I introduced it, surrounded by a small belt of vulcanized India-

rubber, *gradually* dilating the orifice. Since effecting this, the patient has not to the present time had even *an attempt* or threatening of a fit. I have now dilated the opening to a sufficient area, and the orifice is as patulous as I could wish. He has latterly suffered from cough, and there has been a blush of inflammation of the skin on the upper part of the thorax, occasioned, as I imagined, by the mucus which is forced through the tube irritating the skin. I have used collodion, which has removed it, and the cough has been allayed by calomel and sedatives.

The *general* health is now MUCH improved, and the countenance changed from what the neighbours supposed to be jaundice to quite a healthy appearance; the *mind* is also decidedly improved.

One circumstance I may mention—viz. that while dilating with the "cage," on removing it, as I did every second morning, I for three mornings found a small portion of the tracheal rings adhering to it. To prevent this, I have thought, now that the opening is sufficiently dilated, of introducing a *solid* silver tube, which, when prepared, I shall have much pleasure in sending for your inspection, and trust it may obviate this little difficulty.

Waiting any further suggestions you may kindly make in the treatment of this increasingly interesting case,

I remain, my dear Sir, yours most faithfully,

Marshall Hall, Esq. M.D.

W. J. MACKARSIE.

### ON CERTAIN IMPORTANT POINTS IN THE CHEMISTRY & PATHOLOGY OF THE URINE.

By ARTHUR HASSALL, M.D. Lond., M.R.C.P.

(Continued from p. 567, vol. i. 1852.)

#### OBSERVATIONS ON THE PRINCIPAL TESTS EMPLOYED IN THE DETECTION OF SUGAR IN THE URINE.

In previous articles I gave the results of certain observations and experiments on the action of the potash and copper tests for sugar in the urine.

I showed, contrary to what had been previously stated, that potash, when boiled with non-saccharine urine, almost always deepened the colour, and hence I inferred that this test cannot be relied upon for the detection of small quantities of sugar in the urine.

I showed, likewise, that diabetic sugar, in quantities by no means inconsiderable, might be introduced into many urines, and yet not afterwards be detected by the most careful application of the copper test; further, I began the attempt to trace out to what causes this very frequent failure was to be attributed.

With this view I proposed to experiment with all the principal salts and substances proper to the urine, in order to ascertain which of them affected most the action of the test.

I showed that urea, in the quantities in which it is ordinarily present, did not affect the test, but that carbonate of ammonia, the principal part of which, contained in the urine, is derived from the transformation of the urea, exerted an influence over its action by no means inconsiderable, although not in itself sufficient to explain the decisive failure of the test. I therefore first endeavoured to determine the conditions which occasioned the transformation of the urea, with the object of ascertaining whether, during the application of the copper test, carbonate of ammonia was really evolved or not, and I found, as the results of numerous experiments—

1st. That the simple act of boiling an aqueous solution of urea is sufficient to determine the gradual dissolution of that substance, and its conversion into carbonate of ammonia; a result at variance with statements made on this subject, particularly with one advanced by Dr. Bence Jones, and which was particularly adverted to in a former of these articles.

2nd. That this conversion of urea takes place, after a time, in distilled water, even without the aid of the spirit-lamp.

3rd. That the decomposition of urea is effected, either with or without heat, much more readily in fluids which are alkaline, and especially in those the alkalinity of which arises from the presence of lime in any form.

4th. That the conversion of urea is retarded, and sometimes altogether prevented, by an acid condition of the fluid in which it is present, and this is equally the case whether the solution be subjected to the heat of the spirit-lamp or not; the more acid the fluid, the greater its power of resisting the decomposition of the urea.

5th. That animal matter in a state of decomposition exercises a powerful influence over the transformation of urea; and this