and a cracked-pot sound. Phthisical bacilli were present in the sputum. He also suffered from anal fistula. His weight, on December 19th, 122 lbs. 4 ozs.; on the 30th, 124 lbs. 8 ozs.

REMARKS.—In this case the cough was much benefited.

CASE VII.—Julia N., aged 23, single, of no occupation, was admitted December 24th. She had been phthisical for three years, and had severe cough and occasional hæmoptysis. The catamenia had stopped for six months. There was dulness under the left clavicle, with moist crepitus. Mullein was given twice daily. The records of her weight are, December 26th, 84 lbs. 8 ozs.; January 2nd, 1883, 85 lbs.; January 9th, 83 lbs.

REMARKS.—This patient is still under the mullein treatment. The relief of the cough is very great.

I have set down the above cases simply in the order in which they occurred, and with no view of supporting any preconceived idea. These cases, although too few to justify any general conclusion, appear to establish some useful facts. The mullein plant boiled in milk is liked by the patients; in watery infusion it is disagreeable, and the succus is still more so. The hot milk decoction causes a comfortable (what our Gallic neighbours call *pectorale*) sensation, and when once patients take it, they experience a physiological want, and when the supply was once or twice interrupted, complained much in consequence. That it eases phthisical cough there can be no doubt; in fact, some of the patients scarcely took their cough mixtures at all—an unmixed boon to phthisical sufferers with delicate stomachs. Its power of checking phthisical looseness of the bowels was very marked, and experiment proved that this was not merely due to the well known astringent properties of boiled milk. It also gave great relief to the dyspnoea. For phthisical night-sweats it is utterly useless; but these can be completely checked by the hypodermic use of from the one-eightieth to one-fiftieth of a grain of the atropia sulphate; the smaller dose, if it will answer, being preferable, as the larger causes dryness of the pharynx, and interferes with ocular accommodation. In advanced cases, it does not prevent loss of weight, nor am I aware of anything that will, except koumiss. Dr. Carrick, in his interesting work on the koumiss treatment of Southern Russia (page 213) says: "I have seen a consumptive invalid gain largely in weight, while the disease was making rapid progress in her lungs, and the evening temperature rarely fell below 101° Fahr. Until then I considered that an increase of weight in phthisis pulmonalis was a proof of the arrest of the malady." If koumiss possesses this power, mullein clearly does not; but unfortunately, as real koumiss can be made from the milk of the mare only, and as it does not bear travelling, the consumptive invalid must go at least to Samara or Southern Russia. In pre-tubercular and early cases of pulmonary consumption, mullein appears to have a distinct weight-increasing power; and I have observed this in several private cases also. Having no weighings of these latter, however, makes this statement merely an expression of opinion. In early cases, the mullein milk appears to act very much in the same manner as cod-liver oil; and when we consider that it is at once cheap and palatable, it is certainly worth a trial. I will continue the research by careful weighings of early cases, and will further endeavour to ascertain whether the addition of mullein to the cultivating solution prevents the propagation of the phthisical bacillus.

THE INCUBATION OF SCARLET FEVER.

By W. TONGE SMITH, M.D.,

Late Resident Medical Officer to the London Fever Hospital.

MR. SWEETING, in spite of the very obvious criticisms which have been showered upon his interesting cases, has inspired me with the boldness of adding my mite to the discussion on the "Incubation of Scarlatina".

Of course, mere exposure to infection does not necessarily mean infection of the person so exposed, any more than that a chemist need be poisoned because he is in constant contact with poisons. In the one case, as in the other, the poison must somehow or other get into the body first, and in a sufficiently large dose, and it must find that body unprotected.

To fix the incubation period of any of the acute specific diseases, it is essential to have a known definite exposure to infection, of short duration, followed by isolation up to the time of the development of the disease; and this is precisely that which it is most difficult to obtain. But though most cases, like Mr. Sweeting's, fall short of this standard, I venture to think that they are not without interest. Unfortunately, I cannot now lay my hands upon all my cases, but

the following series is fairly representative. Some, it will be seen, come up to the above standard, and will, I think, satisfy even Dr. Cullingworth, unless he should ask me to show that these patients had never been exposed to infection in the whole previous course of their lives. The only possible reply that I could give to such an inquiry would be, that neither the patients nor their friends could throw any further light on the subject, although they were questioned closely and set a-thinking. The cases are, therefore, offered for what they are worth.

CASE I.—S. B., certified as scarlatina, was brought to hospital in a scarlet fever ambulance on the evening of May 30th, 1879. He was found to have morbilli, and was isolated. May 31st. The rash of morbilli had almost disappeared. June 1st. Temperature 99°; he felt quite well. June 2nd, 8 A.M. Temperature 103.2°; headache and sore-throat. 11 A.M. Scarlet rash, etc. Incubation, about sixty hours.

CASE II.—H. H., on the morning of October 30th, 1879, met friends at Regent Circus, and rode with them in an omnibus to the Marble Arch. There he left them, and had no further communication with them. He had not had anything to do with them for some weeks previously, as some members of the family were ill with scarlatina. Two of those whom he met had themselves been taken ill with scarlatina about three weeks before. The boy continued well till November 2nd, 1879, when, in the morning, he had sore-throat, followed by rash, etc. Incubation, about seventy-two hours.

CASE III.—J. O'B., certified as having scarlatina, was brought from an important hospital and medical school, and was said to have been isolated some days. He reached the London Fever Hospital at 11 A.M. on November 4th, 1879. There was no sign of scarlatina or other acute specific disease. On faith in the authority (I very soon got over faith in the authority), I myself put the patient in the scarlet fever ward. No symptom of anything wrong appeared until November 7th, when, at 9 A.M., he had sore-throat, and his temperature was rising. At 1 P.M., he had scarlatinal rash, etc. On the 10th, desquamation was beginning. Incubation, seventy hours, not more.

CASE IV.—G. H. was sent in with Case III, at the same time, from the same place, under the same conditions, and by the same authority. There was no sign of illness. In a credulous mood, I myself put the patient in the scarlet fever ward. There was no sign of scarlatina whatever till November 17th, 1879, in the afternoon, when he had sudden vomiting, sore-throat, etc., followed by scarlet rash; temperature 103.4°. He desquamated subsequently. Incubation, thirteen or fourteen days (?) not more, certainly. These were the only two cases sent in; both recovered perfectly.

CASE V.—A. R., certified as having scarlet fever, was placed in the scarlet fever ward on the afternoon of December 24th, 1879. On December 26th, he was feverish at night. Temperature normal. On December 27th, during morning, the temperature was 101°; in the afternoon, he had sore-throat, rash, etc. Incubation, about sixty-four hours, not more.

CASE VI.—J. R., certified as having scarlet fever, had an hour's drive in a scarlet fever ambulance between 5 and 6 P.M. on December 27th, 1879. On arrival at the London Fever Hospital, he was found not to have scarlatina, and was isolated. At 7 P.M. on December 30th, the temperature was 99.8°, followed by sore-throat, vomiting, scarlet rash, etc. Incubation, about seventy-two hours.

CASE VII.—A young lady visited a dressmaker on February 11th, 1880. She saw there a sick child (the mother said it had sore-throat). She went again on February 12th to have her clothes fitted, and again saw the child. On February 15th, she was attacked with scarlatina. Incubation, about seventy-two to ninety-six hours.

CASE VIII.—A sister of the above left home before her sister was attacked, and in consequence stayed with a friend. She visited the same dressmaker on March 10th, 1880, at 5 P.M. On March 12th, she went again to be fitted. On the 15th, she had sore-throat, etc., followed by scarlatinal rash. Incubation, about seventy-two to one hundred and twenty hours.

Since getting the above facts of these two cases, I have been told that possibly there may have been another source of infection common to both sisters—that of a young lady who had an illness which her friends thought might have been scarlatina, but which the medical attendant said positively was not.

CASE IX.—M. P., nurse at the London Fever Hospital, arrived on April 1st, 1880, at night. On April 2nd, at 7 A.M., she for the first time entered the scarlet fever wards, and remained on duty in these wards until April 5th, when, shortly after 7 A.M., she suddenly had sore-throat, etc., followed by rash. Temperature 103°. Incubation, about seventy-two hours, not more.

CASE X.—E. B., certified as having scarlet fever, was brought in a scarlet fever ambulance on April 8th, 1880, at 3 P.M. He was found to have rubella, and was isolated. On April 10th, towards night, he had slight sore-throat; temperature normal. On the 11th, in the morning, his condition was the same. Towards evening, his temperature was 99°; throat much worse, followed by sickness, rash, etc. Incubation, about seventy-two hours.

CASE XI.—M. F. G., ward-servant, began work on entering hospital at 9 A.M. March 28th, 1881, and remained well till 3 P.M. on April 1st, when she had sudden sore-throat; temperature 103.6°, followed by rash, etc. Incubation, one hundred and two hours, not more.

CASE XII.—S. G., certified as having scarlet fever, was brought in the scarlet fever ambulance on March 29th, 1881, at 3 P.M., and was found to have rubella, and was isolated. No symptoms of scarlatina appeared till April 1st, at 7 A.M., when the patient had headache, sore-throat, fever, and then rash, etc. Incubation, sixty-four hours.

REMARKS.—Cases I, VI, X, and XII are all of known, definite, and short exposure to infection, followed by isolation. They show a variation in the incubation period within the limits of sixty and seventy-two hours; that is to say, that towards the end of the third day (the third period of twenty-four hours' length each) after exposure, the onset of the disease took place. I know of no case of the kind which exceeds three days—seventy-two hours. Case II I think as good as any of the above, and in it also the end of the third day is marked out. III, V, and IX show an incubation not exceeding three days.

In Case VII, four days, or ninety-six hours, were not exceeded.

In VIII and XI, five days was the maximum.

In VII and VIII, if the disease were contracted from the child, though they differ in their maximum limit, they strangely agree in their minimum—seventy-two hours; and this, moreover, dates in each case from the most important point in the history—that is, the trying on of the clothes, which had lain for twenty-four to forty-hours in the supposed infected house.

Case IV shows nothing more than it is possible to live in the midst of the infection of scarlatina for a fortnight without showing signs of the disease. Amongst nurses and servants I have known of several such cases, which have little practical importance. In this connection I may remark that I have known some cases lie in bed in a well ventilated scarlatina ward for two or three weeks and not develop the disease, but to do so towards the end of the third day after getting up and coming to closer quarters with the other patients.

Lastly, in ten out of these dozen cases the important points in exposure preceded the onset by sixty to seventy-two hours.

The shortest periods of incubation that I know of were in cases of surgical scarlatina and its fellow *post partum* scarlatina, in which twenty-four hours was the time.

My belief certainly is that in scarlatina the incubation does not exceed three days, and upon this I have founded the practical rule of quarantine for rather more than seventy-two hours in the case of a person exposed to infection, and I have not yet known the practice to fail. Obviously, beliefs are not arguments, but one cannot help getting impressions, after having in three-and-a-half years observed over two thousand cases of a disease. I have been frequently struck, in reading reports of cases with the names, in which this period of seventy-two hours crops up even when these are quoted to show a longer period of incubation. The use of Dr. Neale's *Digest* would probably unearth a good many such instances. Not to go further back than the period of the 2nd December, I would point out that this period was the case with Mr. Jamieson, and also with the baby reported by Mr. Hamilton. Is it unlikely that such diseases as variola, scarlatina, typhus, morbilli, and rubella (which are generally definite in their onset, and, when not modified, punctual in their stages,) exhibit a like definiteness and punctuality in their incubation period? And would it be too much a stretch of the imagination to look to our (as a rule) insidious, indefinite, and unpunctual friends, diphtheria and enteric fever, to show the greatest variations in the length of their incubation.

AN inquest has been held on Mr. Charles Richard Johnstone, resident surgeon at the Cheltenham dispensary, who, suffering from pains in the head, had been in the habit of taking chloral for relief, occasionally as much as ninety grains. One night he took two doses, and his wife afterwards discovered him taking something else, which he said was ether. Afterwards, deceased asked her to pray for him, and he died in a few minutes. Medical testimony showed that deceased had taken prussic acid. A verdict of suicide while in an unsound state of mind was returned.

THE TREATMENT OF SPINAL CURVATURE BY EXTENSION AND ARTICULAR MOTION.

By EDWARD L. FREER, M.R.C.S. Eng.,

Honorary Assistant-Surgeon, Birmingham Orthopaedic and Spinal Hospital.

Dr. M. J. ROBERTS of New York has contributed an article to the October number of the *New York Medical Journal* on the treatment of spondylitis by "elastic tension and articular motion," which is of great interest to all surgeons engaged in orthopaedic practice, inasmuch as a large proportion of their patients are the subjects of Pott's disease. He speaks of the non-uniformity in the principles of treatment as taught by Professor Lewis A. Sayre, who, as is well known, claims for the plaster-jacket that "it affords the means by which absolute rest is secured, motion of the parts diseased not being permitted to the slightest degree, so long as the apparatus is properly adjusted;" but, at the same time, advocates, in cases of cervical or upper dorsal curvature, the addition of the jury-mast, which cannot, by any possibility, afford this absolute rest. Dr. Roberts gives his experience of patients with cervical spondylitis, treated by him, first of all, with the ordinary jury-mast, and more recently, with modifications of it, with elastic webbing, instead of the leather straps for the head-sling, on the principle laid down by him of elastic tension and articular motion, and which treatment, he alleges, is followed by far better results than that of Dr. Sayre. He also describes an instrument by which, in cervical cases, the extension is carried out by a support from below, the occiput resting upon a sheet of iron shaped to the part and padded, the extension being made by a spiral spring and elastic bands on either side underneath, which are attached below to a fork-shaped bar of steel passing on either side of the neck, and fixed to a cuirass in a somewhat different way to Sayre's jury-mast. It allows of extension and free mobility, the support, however, being from below, instead of from above, as in Dr. Sayre's method.

This instrument must commend itself to surgeons for many reasons, not the least being that it renders the patient less an object of curiosity and ridicule. He concludes by saying that, "if speedy recovery without deformity and without ankylosis is a possible, nay, a probable result, of the early treatment of cervical spondylitis by elastic tension and articular motion, are not similar results to be obtained in all other parts of the spinal column by the application of the same mechanical principles?" During the last six years I have applied about 600 plaster-jackets, and my experience leads me to fully endorse Dr. Roberts's views. I would ask—Do we ever obtain that "perfect immobility," on which Dr. Sayre lays such stress? No surgeon can have failed to notice the ample space that is left between the cuirass and the body after the plaster has thoroughly dried, allowing free rotary movement. From this, I consider that the

only real advantage derivable from the plaster-jacket, is the fact that it supports the cone of the thorax, and having been applied during extension, separates the diseased surfaces, so allowing nature to pursue her curative efforts, the sources of irritation having been removed. With this idea in view, and having had many complaints, especially from private patients, of the irksomeness of the plaster-jacket, principally on the ground of cleanliness, I have devised the subjoined instrument, which consists of a pelvic band, similar to that of the