

combination, and not being set right until typical small-pox lesions made their appearance among those of acne. The rash of glanders *per se* is certainly very like pustular small-pox, and vomiting and pain in the back are common to the two diseases. In glanders, however, the eruption does not appear till well on in the second week; it comes out in crops, very rapidly pustulates,<sup>13</sup> and is generally distributed without any predilection for face and wrists. More important than all, glanders is a discrete eruption, but exhibits constitutional symptoms as grave as if it were confluent small-pox. Once glanders is suspected, the history of association with horses and, if needs be, a bacteriological examination will clear up the case.

The only case of iodide rash with regard to which I have notes of any difficulty was in the instance of a patient suffering from acute Bright's disease. The renal pain and vomiting of nephritis simulated small-pox, but the grouped pustular eruption situated on an inflamed base was easily recognisable as due to iodide or bromide. Inquiry soon elicited the fact that he had been taking iodide of potassium for chronic pleural effusion.

It is customary to lay stress on the vaccinal state as affording presumptive evidence. Personally, I prefer to be unbiased and make up my mind apart from any such consideration, as it is impossible to lay down any absolute rule as to the duration of the immunity enjoyed by the vaccinated. Still, it may be remembered that a copious vesicular eruption in a well-vaccinated child or occurring more than fourteen days after revaccination is very unlikely to be variola. Vaccination rashes, especially vaccinia lichen, are now and then mistaken for small-pox. More important is it to remember that successful vaccination performed a few days previously to the appearance of a given eruption does not exclude small-pox. I have notes of one such case in which the presence of normal vaccine vesicles led to the diagnosis of generalised vaccinia—a diagnosis which was even maintained when the "cow-pox" spread to several members of the patient's family. It is not, however, my intention to dwell on my own or on others' mistakes. "We are none of us infallible, not even the youngest of us," and I will conclude by bearing my ungrudging testimony to the conscientious and unselfish way in which the members of our profession have endeavoured to aid the sanitary authority by the early notification of small-pox. Early notification is of vast importance in prophylaxis, but adds immensely to the difficulties of those on whom the responsibility is made to rest.

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## ON THE CAUSE OF ROTATION IN SCOLIOSIS.<sup>1</sup>

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IT is acknowledged by all except a few pathologists who would seem never to enter a museum that the peculiar shape of the scoliotic spine is almost entirely the sum of changes in its component vertebræ—i.e., it is only in a small degree the result of changes in the ligaments and articulations. What are the changes in an individual vertebra? They are greatest in a certain very small number, chiefly those at or near the centre of each curve. They have been many times described; for example, by Bouvier, by Nicoladoni, and by Lorenz, the last two authorities having in their minds during the description each a special theory to account for the "rotation." Lorenz's description is very minute. If we look at the front of a vertebra chosen from the centre of a curve with convexity to the right, we should expect to find the outline of its body wedge-shaped, with the apex towards the concavity of the spinal curve, and more or less marked, with a horizontal groove on the left side, as if the force of growth, having been checked perpendicularly, had spread laterally, so as to form two lips. If the same vertebra be viewed from behind it will probably be found that, although not strictly symmetrical, especially as regards the articular processes, the wedge-shape is not at all

pronounced, and may even be not discoverable except by measurement. If, now, a third view be taken of the vertebra—viz., from above—it will, if at all a typical case, be seen to be strangely "slewed," as it were, in such a manner that the posterior segment of the vertebra, including the pedicles, the transverse processes, the laminae, and the spinous processes, appear to have struggled to retain their normal relations and positions in the particular human frame to which they belong, while the body of the vertebra has been equally determined to deviate to the right. "Struggle" is, in one respect at least, a good word, because it suggests a certain amount of confusion, such as might produce the minor varieties in the pathological anatomy of such bones as I am describing. But the chief peculiarities are both orderly and almost uniform. If it be granted that the prime cause of scoliosis is, in young children, ordinary rickets, and in older ones and in youths "rachitis adolescentium," it cannot be forgotten that in rickets it is at the so-called "epiphysial" cartilages that the greatest abnormality of growth, leading to the major part of each rachitic deformity, takes place. Now the epiphysial cartilages of the vertebral body are placed above and below the main bony masses, but the cartilages of growth of the transverse processes and the spinous process, and in fact of the whole segment of the vertebra posterior to its body, are placed horizontally with regard to one another. There is also the equivalent of an epiphysial cartilage in each vertebral body near the root of the pedicle. Hence rachitic deformity would have a much greater tendency to produce lateral inclination in a vertebral body than in the more posterior constituents of the vertebra. Further, the external projection of the transverse and articular processes gives them an increased mechanical power, like that of buttresses and of cross-trees, to check a tendency to lateral inclination. On the other hand, the comparative thinness from side to side of the pedicles, arches, and spinous processes offers diminished resistance to horizontal "slewing" of those parts. Therefore, the fact that the layers of cartilage of growth or epiphysial cartilages above and below each vertebral body do not extend backwards to the posterior parts of the vertebra is the prime cause of torsion, and the arrangements of the cartilage of growth of the pedicles, transverse and spinous processes, together with their shape, greatly contribute to the production of torsion by facilitating "slewing" of these parts in a horizontal direction. The only explanation of torsion which I have been able to find at all analogous to that just given is Lorenz's. He finds the cause of torsion in the existence of the cartilage of growth in each side of the posterior part of the vertebral body near the base of the corresponding pedicle. That this is a most important factor I grant, but I think it causes, not the torsion, but the great degree to which torsion may reach. It is a specially important fact that near the junction of the anterior with the posterior segment of each vertebra there is a cartilage of growth which is, like others of its kind, no doubt specially sensitive to rachitic influences. But I maintain that unless the vertebral bodies had their principal cartilages of growth placed above and below them, they would have no more tendency to lateral curvature than the rest of the vertebræ, and therefore the action of the pedicle cartilage would not come into play.

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## NOTE ON THE LIMERICK POISONING CASE.

By SIR CHARLES A. CAMERON, F.R.C.S. IREL., D.P.H.,  
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ON July 3rd, 1895, about seventy inmates of a convent and boarding-school in Limerick became very ill after dinner, and for several days suffered from vomiting and purging of a severe character. Three ladies died and many of the other inmates were for several days in a precarious condition. Poison being suspected, portions of the vomited material and dejecta were sent to me for examination on July 5th, together with a portion of corn-flour which it was suspected might contain poison, but none was detected in it or in the other matters sent to me. Suspecting the case to be one of ptomaine poisoning I telegraphed for portions of everything the

<sup>13</sup> In one case of which I have notes it was pustular on the third day.

<sup>1</sup> A paper read at the opening meeting of the British Orthopædic Society on Jan. 31st, 1895.

patients had had for dinner, but the only items available were sugar and sodium bicarbonate, both of which were pure. It has been clearly ascertained that the poison was not in the meat used, for some persons who had partaken of it were not ill, and the history of the carcass that furnished it was made out and negated the hypothesis of unsoundness or disease in it. It appears that a custard formed part of the dinner and that all who partook of it sickened. The cook (one of the sufferers) states that she prepared the custard as follows: "Tuesday, July 2nd: Took four quarts of skimmed milk of previous night, boiled it, added about half a pound of loaf sugar, added two tablespoonfuls of corn-flour and boiled again. This was done in a tin vessel. 3 or 4 P.M.: Resumed making of custard; beat up eight or ten eggs, all fresh except one which had a reddish-brown colour but no bad smell, poured them into milk which was then about the temperature of tea, and then poured into earthenware bowl; removed to a cold place during night. Wednesday, 3rd, 9 A.M.: Custard had become quite thin, of consistence of cream; poured it over seven or eight quarts of strawberries, about one and a half quarts of which had been gathered on previous night, whipped the whites of eggs used above, with one pound of sifted sugar and spread over strawberries and custard on glass dishes." That the strawberries were the cause of the illness is negated by the fact that three or four persons who took gooseberries and not strawberries with the custard were amongst the sufferers. The inquiry is therefore narrowed to the question, Was the *materies morbi* in the milk or eggs. It would seem that some surprise was manifested by the custard remaining thin. When I first heard of this fact I thought the fluidity might be due to the liquefying influence which certain bacilli have on gelatine and albuminous matters, though the rapidity of their action in this particular case seemed surprising; I therefore had a custard made similarly to that described by the cook, and found that it remained as thin as rather thin cream. This want of consistency was, however, due to the albumin of the eggs not having been coagulated by heat, for when my custard was heated to near the boiling point (as should always be done) it became on cooling quite thick. That a highly poisonous substance (tyrotoxin) is generated in milk (not necessarily very stale) is well known. In the Limerick case the milk was at least two days old when consumed, as at eleven o'clock on the previous day it was skim-milk of "the previous night." It was, however, boiled, and after the addition of cornflour and sugar again boiled. The question is, Could the milk in the presence of sugar and cornflour have generated tyrotoxin from the time it was boiled at eleven o'clock on Tuesday until it was produced at dinner at one o'clock on Wednesday? The time of the year was favourable to the fermentation of the unstable mixture. The temperature of the mixture was favourable to incubation of micro-organisms. Suspicion falls more strongly on the eggs. I have learned that they were market eggs, and had been purchased four or five days before they were used. Though not putrid, they were more or less stale, and one had a curious colour, described by one person as resembling that of claret. Now after the addition of the eggs to the luke-warm milk, sugar, and cornflour, there was no further heating, and it is clear that the egg albumin was not coagulated. There is reason to believe that the discoloured egg was cracked, and if so micro-organisms had access to its interior. The viscera of two of the patients who succumbed to the attack were examined for ordinary poisons, with negative results. A substance was extracted from them which gave all the characteristic reactions of ptomaines. The quantity available for examination was altogether insufficient to differentiate the ptomaine or ptomaines present from others of which there seems to be a very large number. I am not aware that in any case of ptomaine poisoning the ptomaine was extracted from the viscera and identified. In this case it is much to be regretted that neither the custard nor the matter ejected from the patients on the first or even second day of their illness was available for bacteriological and chemical examination. A fourth patient died yesterday, and there is only one now whose state causes anxiety. The inquest was held yesterday, and the verdict was in accordance with my evidence.

Dublin, July 17th.

ST. THOMAS'S HOSPITAL.—The Corporation of the City of London has granted a donation of 200 guineas to the special fund for the closed wards of this hospital.

## ON THE MECHANICAL TREATMENT AFTER PARTIAL EXCISION OF THE LOWER JAW.

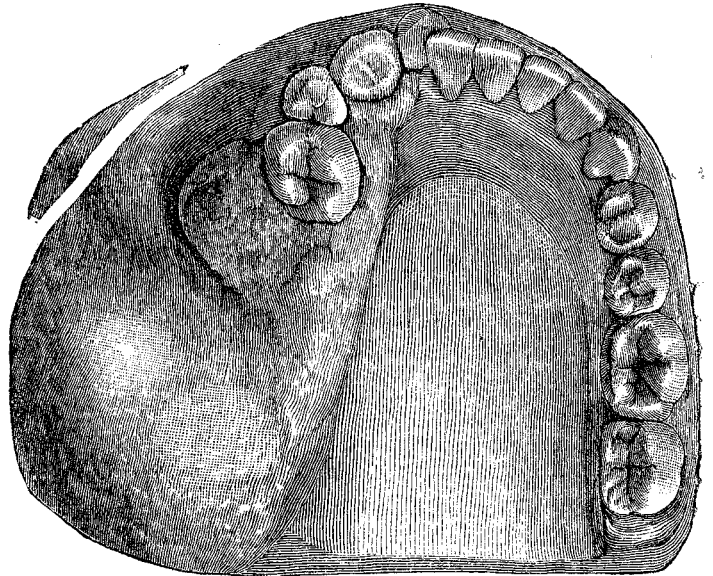
By F. NEWLAND-PEDLEY, F.R.C.S. ENG.,

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No great difficulty is experienced in artificially restoring portions of the jaw lost from disease or removed in surgical operations, provided that sufficient bone be left to maintain the dental arch and give support to a denture. Even when a large fragment of the jaw, including the base, is lost from necrosis, we may hope that in a young patient the bone will be reproduced, and the treatment consists in holding the fragments at rest by means of a splint whilst repair is progressing. The most difficult cases, however, are those in which it becomes necessary to remove the ascending ramus or half the jaw in order to arrest a malignant growth, for the remaining fragment quickly undergoes remarkable displacement, falling inwards until it rests upon the tongue, and occupying a position in the median line of the mouth. Here, as a rule, it becomes fixed by cicatricial tissue, rendering the jaw useless as a masticatory organ and occasioning great permanent deformity. Few of these cases have come under my treatment during fifteen years of hospital practice, and doubtless excision of large segments of the lower jaw for advanced malignant growths in elderly patients only prolongs life for a time, and mechanical treatment is not attempted. This, at least, has been my experience, for I cannot recall a single previous case in which treatment was commenced early enough to be successful; the rest, without exception, were failures, because the remaining segment of the jaw was firmly fixed, the muscles were shortened, and further surgical operation seemed contra-indicated. Unsatisfactory results could be averted if the dental surgeon was consulted before the bone became fixed, as may be proved by a case recently treated at Guy's Hospital.

A girl aged twelve years was admitted under Mr. L. A. Dunn for a large sarcoma of central origin, situated in the left side of the lower jaw, extending backwards to within half an inch of the sigmoid notch and forwards to the second bicuspid tooth. The annexed model of the growth (Fig. 1)

FIG. 1.



was taken by Mr. Parfitt, who was dresser to the case. The whole of the ascending ramus was removed and nearly half the body of the jaw, reducing the dental arch to the limit indicated in Fig. 2. The wound healed rapidly, and Mr. Dunn, foreseeing the deformity that would necessarily result from the operation, transferred the patient to my care for mechanical treatment some ten days after the operation. I then found the characteristic displacement, which, however, yielded to pressure; but it was clear to me that any contrivance to keep the lower jaw in place must take its fixed point from the upper jaw, for no continuous pressure should be applied over the recent wound. I therefore made a Gunning splint which covered