trates the ill effects which may occur from an accident, though giving no evidence of their existence at the time.

One peculiar feature in connection with this case is, that the child should have lived so long after the accident in fairly good health. He partook heartily of his meals, was cheerful, and fond of play.

ON A NEW METHOD OF DETERMINING THE PRESENCE OF, AND RECOVERY FROM, TRUE RINGWORM.*

By DYCE DUCKWORTH, M.D., F.R.C.P.,

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I THINK it will be conceded by all practical dermatologists, that there are cases of true ringworm, and also phases of this disorder, which are not immediately and distinctly recognisable, or which, at least, require some careful investigation before a decided opinion is expressed about them.

It is in such instances, and also for the determination of the amount of parasitic disease in any given patch, that I venture to recommend as an aid to the diagnosis, the application of chloroform to the affected part of the scalp.

A few drops are to be poured upon the head of the patient, who must be placed in a good light, between the operator and the window. On evaporation of the chloroform, the affected hairs, generally short, broken off, and twisted at their extremities, are seen to become of a yellowish-white colour, opaque, and like fine filaments of a vegetable lichen. The healthy hairs are quite uninfluenced by the chloroform. Not only upon the hairs is this change observable, but the skin in the immediate neighbourhood is commonly affected in a similar manner. Small whitish masses are seen upon the scalp, and especially at the point of emergence of the hairs. This effect is due, I believe, to *débris* of the parasite mixed up with the sebaceous-epithelial matter extruded from the hair-follicles. The parts look as if sprinkled with a film of highly divided sulphur powder, reminding one of vines that have been washed with sulphur lotion for grape-disease.

If ether be used, instead of chloroform, no such change takes place. And if the scalp and hairs be well rubbed and cleansed from sebaceous matters by ether, the specific effect is at once produced on the subsequent addition of chloroform.

As already stated, the opaque hairs become manifest immediately on the drying up of the patch, and thus the local application of chloroform becomes a perfectly accurate test of the infection, or the reverse, of the part. The change in appearance is generally very striking, and may be observed by an untried eye.

If these opaque hairs be examined microscopically, the spores can still be clearly seen in their interior. The shaft is desiccated, the oily matters are removed, and the fibre-cells are found somewhat split up at the margins. It would seem that the spaces which occur in the shaft, as the result of sporadic intrusion and development, and consequent disintegration of fibre-cells, become filled with air on the evaporation of the chloroform which had permeated the entire texture of the shaft. In this way I would in part account for the optical change which ensues, and I think it may be also partly due to some direct action of chloroform upon the mycelium and spores of the trichophyton. I believe that this action of chloroform upon parasitically affected tissues has not been observed before; at all events, the fact is not known to any vegetable histologists with whom I have communicated.

On adding dilute caustic potass to the hairs, the appearance is still maintained to some extent. Seen by reflected light the hairs seem white and frosted, while normal hairs, similarly regarded, appear transparent. I am not aware of any other chemical reagent that produces an effect upon these diseased hairs, in any way comparable to that following the application of chloroform. As already stated, ether has no such effect, nor has bisulphide of carbon. The latter is of some use as a reagent for showing the mycelium and spores in affected hairs. It renders them readily apparent without distending the hair-shaft; perhaps it may cause very slight shrinking of the fungus-elements.

Dr. McCall Anderson describes the hairs in ringworm as being naturally white. This is sometimes the case; but, even when this is so, the appearance is not always readily appreciable, and the action of chloroform intensifies it very markedly.

In *tinea* of the body (*Tinea circinata*) and in *Tinea versicolor*, the application of chloroform appears to indicate the presence of parasitic

elements, by the white powdery aspect which the patches assume on its evaporation. That this is not always a fallacious appearance, and due to frayed particles of epidermis which are frequently to be seen on these patches—hence the synonym *Pityriasis versicolor* for one of these affections—is proved by the fact that the surface, as shown by a lens, is sometimes perfectly smooth before the addition of chloroform. By this test, therefore, patches of pigmentary staining, melasma, and the dark portions of skin in leucoderma, may sometimes be distinguished from parasitic disorders, for chloroform has no action upon normal skin when thus momentarily applied.

The employment of this method is of least value, I believe, in such cases of ringworm as are attended with much heaping up of crusts or scales; and it is in these, too, that the broken off hairs are either few in number, very short, or entangled in the crusts. But the diagnosis in these instances does not usually present any difficulty, and the phase alluded to is probably due in great measure to over-stimulation or too irritating treatment. Even in these cases, after poulticing and liberation of the hairs, the test is quite effectual. I have had no opportunity of trying this method in cases of favus of the scalp, since these are rare in London; but I met lately with several cases of favus of the skin, in which I applied chloroform, but without any particular result. The diagnosis was sufficiently easy, and the microscope confirmed the first impressions formed. It is not, however, without interest to record that the action of chloroform upon the areæ and peripheral parts of porrigo decalvans is *nil*. If the latter affection be a parasitic one—a *tinea*—an opinion which I do not hold, and against which I have recently published my reasons at length (*St. Bartholomew's Hospital Reports*, vol. viii, 1872.), it might fairly be expected that chloroform would indicate the presence of the parasite; but such is not the case. No change of any kind occurs either on the patches, in the hair-stumps, or in the hairs immediately adjacent, and therefore presumably in the earliest stage of the disorder. This fact must, therefore, be taken as additional evidence against the parasitic theory of porrigo decalvans.

I believe the chief value of this test is to be found in the later stages of ringworm, when it becomes the duty of the practitioner to declare whether the disease is entirely removed or not. All who have practical knowledge of the difficulties which are in the way of forming an exact opinion upon this point, will welcome this method as a relief from a troublesome resort to the microscope, which has hitherto been indispensable. Not only is much time saved, but a more exact opinion may be pronounced upon the case; and there is the additional advantage that the attendants of the patients may always satisfy themselves as to the eradication of the disease, or the reverse, and may, therefore, withhold or continue the employment of suitable antiparasitic remedies.

ANELECTROTONUS OF THE DENTAL NERVES IN TOOTHACHE.

By JULIUS ALTHAUS, M.D.

Now that, thanks to the activity of instrument-makers in meeting the increased professional demand for constant batteries, fairly effective instruments of this kind are becoming household goods of the medical man, and are within the reach of even slender purses, I wish to suggest a routine recourse to the constant current for that very common affliction of mankind—toothache. I have never been consulted with the view of treating toothache by electricity, but have for many years past incidentally used the continuous current in a large number of such cases, and come to the conclusion that it is a ready and most effective means for the cure of odontalgia. Without wishing to enter here into the pathology of this "small misery," I may say that the mere decay of teeth appears, as a rule, only indirectly productive of toothache, which, indeed, is very frequently experienced in teeth that are perfectly healthy, and in decayed teeth not always, but only under certain contingencies. Chills and depressing emotions would seem to be the chief exciting causes of toothache. Now, the terminations of the dental nerves are more exposed to chills in decayed than in healthy teeth; and, under depressing emotional influences, the weak points of the system are chiefly apt to suffer. These considerations would explain why toothache is more frequently felt in bad than in good teeth.

What is the best mode of galvanisation for toothache? After numerous trials of various modes, I have come to the conclusion that the induction of anelectrotonus of the dental nerves, with complete avoidance of catelectrotonus in their neighbourhood, answers best. Pflüger has shown that a continuous current, which traverses a certain length of the nerve, divides this latter into two physiologically different sections or zones, one of which shows the condition of increased excitability or

* Read before the Medical Section at the Annual Meeting of the British Medical Association in London, August 1873.

catelectrotonus, while the other is in a condition of diminished excitability or anelectrotonus. The zone of increased excitability is in the neighbourhood of the cathode, while the zone of diminished excitability is in the neighbourhood of the anode. The condition of increased excitability is propagated from the cathode towards either side, and the condition of diminished excitability is propagated from the anode towards either side. This alteration of excitability in the extra-polar portions of the nerve diminishes in the same ratio as the distance at which they are from the electrodes increases, and at a certain distance it disappears altogether. Cyon has shown that Pflüger's researches, which were made on rheoscopic frogs' limbs, hold good for the nerves of the living man likewise; and the systematic production of catelectrotonus and anelectrotonus, for the purpose of increasing or diminishing the excitability of diseased portions of the nervous system, has thus been shown to be a therapeutical possibility.

These principles may be utilised in the case now under consideration by placing the large anode, armed with a moistened sponge, to the suffering cheek and jaw, and the cathode to the palm or the back of the hand. In this way, both the second and third branches of the fifth cerebral nerve are placed into the condition of anelectrotonus, while the influence of the cathode is neutralised by its great distance from the suffering parts. One application of a gentle, but plainly perceptible, current continued for five minutes is sufficient for curing almost every toothache; but, in very bad cases, a second application may be required for effecting the desired result. This may be had the same day, if practicable.

The same principles apply to the galvanic treatment of the different forms of neuralgia, which yield readily to the induction of anelectrotonus, if practised sufficiently early. In the later stages of neuralgia, the phenomena are not so simple as in the commencement of it, and the treatment then becomes more complicated, and less readily successful.

SURGICAL MEMORANDA.

M'INTYRE'S SPLINT: STARCH-BANDAGES.

IN using M'Intyre's splint, two difficulties present themselves—first, of fastening it so that it should not be easily displaced; and secondly, if so fastened, of moving the patient up or down in the bed. Nothing can be more distressing to a patient than to be compelled to lie for two or three weeks in exactly the same place; and in certain cases it might lead to sloughing of the sacrum, and even death. To obviate these drawbacks to the use of this ingenious instrument, I have contrived and used in hospital practice a very simple apparatus. It consists of two upright pieces of timber, about three feet long, two inches wide, and one inch thick, fastened by thumb-screws to the end of a deal bed without posts—one at each side—and connected at their upper ends by a cross-bar revolving on pivots. The thumb-screws are placed about midway, and also serve as pivots on which the upright bars may turn. To the cross-bar the end of M'Intyre's splint is securely fastened by screws, and if it be required to move the patient towards the head of the bed, the cross-bar is pushed in that direction, and *vice versa*. This movement will of course depress the foot; but if that be undesirable, it can be remedied by shifting the cross-bar, for the upright bars are provided with four corresponding holes, to any two of which the cross-bar can be removed by simply loosening one of the thumb-screws. By means of this apparatus, which could be made for a half-crown, not only can the patient be shifted several inches up or down in the bed, but the foot can readily be placed at many different elevations.

M'Intyre's splint could readily be converted into a "cradle" by suspending it from a cross-bar, like the one just described, and from another placed about twenty inches higher up in the bed; and a small bar of iron attached to the lower end of the splint, rising above it to the height of six or seven inches, and then bent at right angles, should terminate in a pivot which should be made to play securely in a hole in the lower cross-bar. In the upper cross-bar a pulley should be put, just opposite the hole in the lower one, and, by passing a strong cord over the pulley and under the splint, and properly adjusting it, the splint would be swung like a cradle.

In converting the splint into a cradle, the shorter the thigh-piece the better. By means of the contrivance just described, not only could a leg be swung, but the patient could be lifted up or down as easily as when only a single cross-bar is used, for the supporters of the cross-bars can always be placed at the same angle to the sides of the bed, and consequently be always parallel. In like manner the cross-bars

working on pivots would always take the position required. It is unnecessary to go more minutely into a description of the apparatus now suggested, for enough, I trust, has been said to make it intelligible to the instrument-makers.

In simple fractures of the leg I have recently used a starch-bandage, consisting of two cotton stockings, with very satisfactory results. To apply a starch-bandage of the ordinary kind, requires considerable experience, tact, and judgment; and it is scarcely an exaggeration to say that many surgeons who could perform an amputation of a limb, would be unable to put on a starch-bandage properly. When I want to apply a starch-bandage, I take two common cotton stockings; the one to be drawn over the leg first, fine and elastic, slips over the leg without the least difficulty; the other, coarse and strong, requires to be drawn on slowly and carefully. The size of the stockings must of course depend on the size of the leg; but it would be better and safer that they should be too large than too small, for no force should be used in drawing them on. To the first stocking, which serves merely to protect the leg from irritation, no starch is applied; but when the second stocking has been drawn over it, plenty of starch should be rubbed in from the toes to the knee. It will be found when the starch has dried, that an elegant starch-bandage has been formed, with no trouble to the surgeon, and with no danger or discomfort to the patient. I have found two stockings thus applied after the third week quite sufficient to support the bones; but if they were to be applied earlier, it might be necessary to draw on a third stocking, and to starch it also: this should be done when the starch on the second one had dried. Instead of drawing on a third stocking, however, I would recommend a few turns of a roller, starched, to be applied over the seat of the fracture, which might be done without the slightest fear of causing swelling of the foot, and would in cases of oblique fractures be particularly useful.

It appears to me that fractures of the fore-arm and arm could be treated in a similar manner—cotton gloves, of course, being used instead of stockings; but having as yet no opportunity of trying this plan of treatment, I must leave it to others to test its practicability. When the stockings are getting limp the starch should be rubbed in again, and when they are to be removed the starch should be washed out; and, when dry, they may readily be slipped off, or, if preferred, cut open with a scissors.

JEREMIAH DOWLING.

DISLOCATION OF THE RADIUS BACKWARDS.

A. B., aged 10, was brought to me on Saturday, September 12th. Twenty-four hours previously, he had met with the following accident. He had been playing with some other boys on a barley-mow, and was pushed off (no great height), and could not say on what part of his body he pitched. When I first saw him, the whole elbow was so swollen and tender that it was utterly impossible to feel the articulation, and the movements of the joint were impeded to some degree. From the commencement there was a look as if the elbow projected somewhat backwards, and the impression left on my mind was the possibility of a fracture through the condyles. There was no crepitus of any kind. Rest and spirit-lotions were continually applied, and the bowels well cleared out. The boy was seen in the course of the week by both Mr. Bleek and Mr. Hinton of Warminster, without any definite opinion being formed as to the state of affairs. The swelling was but slightly lessened, and the pain certainly as bad as ever. About ten days after the accident, he was again seen by us together. The swelling had subsided slightly; and, on a careful examination, we came to the conclusion that the radius was dislocated backwards. The deformity of the bones did not appear very marked; the anterior external portion of the humerus near the joint was more plainly felt than in its sound neighbour; and, on deep pressure, there was the sensation of being able to pass the finger a little under it posteriorly. The head of the radius was felt projecting, but certainly not to the extent that one would have imagined; yet the finger passed down the arm appeared to impinge on the head. The following morning we placed the boy under chloroform, and, on extension of the arm and flexion of the joint, the radius went home with a very audible noise.

The interest of the case turns upon the rarity of the accident. Sir A. Cooper, in his work on the joints, gives a drawing of the deformity, but says he had never seen it in the living person. Fergusson says (but my edition is not the most recent) that he once saw it in the dissecting-room, undetected during life, the motions of the joint being very little interfered with. Druitt says it is extremely rare. The two points that seemed to us to demonstrate the nature of the case were the nearness of the anterior and external portion of the humerus to the touch, and the slight projection of the radius posteriorly. This, I have said, was far less marked than one would have imagined.

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