

day, twenty hours having elapsed, I visited the incubator. Mr. Cantelo told me he had looked at the preparation early in the morning, and observed the heart "was beating very feverishly;" and, when I looked, I saw a quick, hurried, and, as it were, impatient action, very different from that seen the day before.

The area covered with blood-vessels was now nearly twice the diameter it was yesterday; they had undergone great changes, were more densely crowded, and their arrangement was strangely divergent from the natural one—all within twenty-four hours.

The view was a suggestive one: the heart in feverish action, arteries carrying blood of one hue and veins blood of another hue, and an action greatly extending the vascular area—a sort of inflammation—in full swing from the inappropriate admission of air to a creature not three days old, and with nothing but globules or cells in its composition. Mr. Cantelo's experience and my experiments show, in a very marked manner, the injurious effects of a "wrong heat," and the admission of air upon embryo growth. I apply the facts to reparation.

In the human embryo, blood-vessels commence and grow at a high temperature, quite excluded from air and light.

In subcutaneous surgical operations, ruptured tendons and fractured bones, injured parts are excluded from air and light; an equable high temperature is secured by neighbouring parts, and the wounds heal without suppuration: because the conditions of embryo growth are fulfilled. On the other hand, external wounds are exposed to variable low temperatures, to air and light, and they suppurate, the action denoting spoiled material—spoiled by conditions unfavourable to embryo growth.

Perhaps the confluence of pustules of small-pox and deep pitting of the face might be mitigated if, on approach of the eruption, the face could be protected from cold and air. New vascular tissue of healing sores—granulations—will often fade and die away, among other reasons, probably from exposure to air and a "wrong heat." It may be, the use of suppuration is not only to throw off spoiled material, but also to form a covering to shield young granulations from cold and air. Cold air is as injurious, as much an irritant, to serous surfaces as hot water to the skin; if it be admitted to them, the parts immediately proceed to protect themselves by suppuration. In these cases, a high temperature and total exclusion of air are necessary to healing. I remember to have read somewhere an account of a boy who stumbled into a vessel of boiling pitch, burning his arms to above the elbows. The surgeon took off all the clothes he could; but found it impossible, from the hardening of the pitch, to remove the sleeves of the coat from the injured parts. Subsequently, he expressed extreme surprise at finding the parts he could not bring under his observation healed long before those parts he had been daily dressing and cleansing. In the same person, and in the same injury, those parts, secluded from air and kept at an equable warm temperature, healed without suppuration, and sooner than those exposed to cold and air, and dressed by the surgeon. Healing, to keep parallel with the embryo growth, must have parallel conditions.

Sometimes, in extensive burns, large sloughs have to be separated; and, upon the principles we are now discussing, the dressings ought to be as few and far between as possible; what is called cleansing the wound is often unnecessary. I should have no fear for vigorous granulations from *vibrios* or *bacteria*; for, in confluent small-pox, the ugly suppurating parts heal under temporary shields, the scabs, notwithstanding the nidus in a hot summer for eggs and germs.

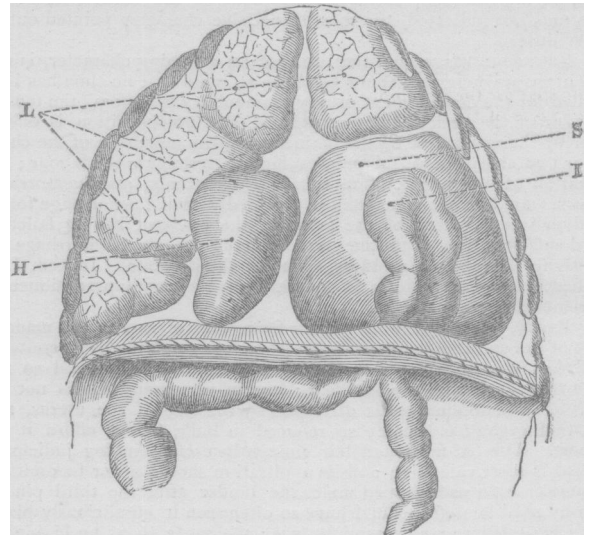
Healing in different parts corresponds in time with embryo growth; skin heals sooner than tendons, and tendons sooner than bones. *Inflammation is the requisite preliminary action, fitting existing blood-vessels for a renewal of embryo growth;* and, in correspondence with the heat necessary for embryo growth, parts in inflammation are of a higher temperature than surrounding parts.

DIAPHRAGMATIC HERNIA.*

By HENRY W. RANDOLPH, Esq., Milverton, Somerset.

I BEG to present to you for your inspection a specimen of hernia through the diaphragm on the left side, in a child 3½ years of age, produced by an accident which occurred when he was a year and three months old. The opening is of an oval shape, about two inches long by an inch wide, through which had passed the entire stomach and about eight inches of colon into the left pleural cavity. The stomach, though it had lost some of its contents, was much distended with fluid, occupying nearly the whole of the left side of the thorax. There was no strangu-

lation of the structures within the ring; and the stomach, with the fold of intestine, was readily returned within the abdominal cavity. The lung on that side was in a state of atrophy, perfectly hepatized by the pressure of the intruding viscus, except just at the apex of the upper



L. Lungs. H. Heart. S. Stomach. I. Intestine.

lobe, which contained some aerated cells; it was wasted to about one-fifth of its proper size, and flattened against the vertebral column. The heart was healthy, and pushed considerably towards the right side. The right lung was larger than is usually seen—probably from the over-work it had to sustain in the absence of its fellow-labourer. The liver was much congested and somewhat enlarged. The intestines through their whole extent were highly vascular. The remaining viscera were in a perfectly healthy state.

The following history of the case has been furnished by my son, who made the *post mortem* examination, and had the conduct of the case.

The accident alluded to was occasioned by the boy creeping under a steam-engine belonging to a threshing-machine. The horses being attached, the driver, unconscious of the child's presence, drove on, and the little urchin was crushed between the fire-box and the ground, a space of about six inches. The details of this case, and even the accident, had escaped my recollection; but I am informed that I saw the patient immediately after its occurrence; that the child was put under treatment, and confined to bed, and in the course of a week was apparently well and running about. We never heard more of the case until last November, when my son was sent for early in the morning, the child having been seized with urging (though nothing was vomited) pain and hæmorrhage from the bowels. He was pallid, faint, with venous congestion about the lips. Astringent medicines and stimulants were administered. In the middle of the day we were hastily summoned, but before we arrived the child was dead. This led to a *post mortem* examination, and this interesting revelation of the cause of death was the result.

Since the period of the accident, now more than two years ago, the child suffered from dyspnoea, greatly increased by violent exercise, and always worse after a full meal. His mother tells me that ever since the injury he had frequently been seized with violent vomiting, lasting from two to three hours: at all other times his digestive functions seemed to have been well performed.

It is evident that the rupture of the diaphragm was occasioned by the accident, and that the stomach, which at the time was probably full, passed through the aperture. After each meal, doubtless more stomach was protruded into the chest cavity; and the lung, as it gradually became collapsed, offered less resistance to the protruding viscus. Had vomiting been effected during the last attack, the child's life might have been prolonged for a short period; but this not being possible, the pressure exercised upon the heart and lungs was, I believe, the immediate cause of death.

It is but seldom in a country practice we are enabled to make examinations after death, or to present to the members of the profession interesting matter worthy their consideration; but I cannot but think the case submitted to your notice is worth preservation, since it illus-

* Read before the West Somerset Branch.

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.,

Assistant-Physician to St. Bartholomew's Hospital, and Demonstrator of Skin-Diseases.

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.