THE CAUSE OF ENLARGED TONSILS AND ADENOIDS IN CHILDREN AND THEIR TREATMENT WITH LYMPHATIC GLAND EXTRACT.

(PRELIMINARY REPORT.)

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MANY theories have been put forward as to the cause of enlarged tonsils and adenoids, but I think it is possible to rule all of them out conclusively. For this reason I am advancing a new theory, which may have some connexion with their enlargement.

By analysing some 3,000 cases in children suffering from enlarged tonsils and adenoids, it became apparent that by far the commonest ages for the children to suffer from enlarged tonsils and adenoids is from 2 to 5 years, but that many infants, even from birth, have enlarged adenoid growths in the naso-pharynx.

To find out at what age the adenoid tissue is developed I have obtained a number of fetuses of different ages and dissected away the posterior wall of their naso-pharynx, where the adenoids subsequently develop. The tissue thus obtained has been examined microscopically, and I have been able to find that adenoid tissue was present in every fetus after the age of 6 months. This being so, I think we can say that every child is born with adenoid tissue at the back of the naso-pharynx, but that it grows for some reason in some children more than in others.

The mere presence of adenoids does not constitute a disease, and they only become pathological when they hypertrophy and cause trouble. In infancy and childhood the lymphatic structures are at their maximum exactly at the time when enlarged tonsils and adenoids are commonest, and it is one of the functions of the lymphatic tissue to form the lymphocytes. Now it is a peculiar fact that children with enlarged tonsils and adenoids rather have a diminution of lymphocytes in the blood, and there is also an increase in the total number of leucocytes. For this reason it looks as though the enlargement of the tonsils and adenoids were an attempt on the part of Nature to supply the deficiency in the other lymphoid tissue of the body. At the age when enlarged tonsils and adenoids are commonest (2 to 5 years) the lymphocytes are normally beginning to decrease from the high percentage found in infants to the adult standard, and again, the thymus is also decreasing in size. I think these two facts support the view that the adenoids and tonsils are an attempt to augment the lymphoid tissue of the body. It is also a common fact that when the enlarged tonsils and adenoids are removed carefully by a competent surgeon before the age of about 5 years they are very liable to recur, so that Nature, as it were, makes a second attempt to raise the amount of lymphoid tissue, or at least supply the body with some secretion from the lymphoid tissue, which it needs at this age.

If we could supply these children with lymphoid tissue artificially, there would be no need for the enlarged tonsils and adenoids, and in consequence they would go down, and it is on these lines that I have been working. Messrs. Burroughs and Wellcome have made me some lymphatic gland extract, exactly on the same lines as they make thyroid extract. Up to the present I have treated about 30 cases of enlarged tonsils and adenoids with this lymphatic gland extract, gr. v, three times a day. No bad effects have been observed, and nearly all the children have improved in a very satisfactory way; the snoring and noises in breathing have disappeared, and the tonsils have diminished in size.

This being so, I think it is well worth while pursuing the treatment in a larger number of cases. In many instances of enlargement of the thyroid gland at puberty in girls, if thyroid extract be given the thyroid gland diminishes in size, showing that the gland enlarged to make up for some deficiency in its secretion, and that when the deficiency was made up artificially the gland went back to its normal size.

IODINE AS AN ANTISEPTIC IN TROPICAL COUNTRIES.

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In an article on strangulated epigastric hernia published in the BRITISH MEDICAL JOURNAL in December, 1910, I mentioned the beneficial use of a 2 per cent. solution of the tincture of iodine in skin preparation, as a preliminary to operation.

The following may be of interest, showing as it does the excellent results obtained by its use where preparatory antiseptic measures could not be adopted, and where the existing conditions were by no means favourable from an aseptic point of view.

Reclus, in *La presse médicale*, mentions the excellent results he obtained by its application in compound fractures, and quotes cases to substantiate his statement.

1. Early one morning a Bunyoro native came to my hut at Nimule to be treated for a crocodile bite. He had been down to the Nile close by to cut grass for his employer, an Arab trader. In stooping to do so, at the water's edge his right forearm was seized by a crocodile, but fortunately he was able to catch hold of a small tree with his left hand, and thus save himself from being immediately dragged under water. His call for help brought another native to his assistance, and he, by repeatedly belabouring the animal with a stout stick, succeeded in making the crocodile let go, and retire under water.

It was seen that he had sustained a compound fracture of both bones of the forearm in their middle third. The upper ends were protruding through the wound on the flexor side, the superficial muscles being extensively torn, but on the extensor side, beyond the skin and fascia being lacerated by teeth marks, the muscles escaped practically untouched. Embedded in the wound were small pieces of grass. From the nature of the wound I gathered that the forearm was seized by the front teeth of the crocodile, the hand, wrist, and lower third of the forearm being at the time inside the animal's mouth.

The wound and adjacent skin was thoroughly syringed, and washed with a 2 per cent. solution of iodine for about fifteen minutes. I then reduced the fracture and stitched up the wound, dressing it with plain sterilized lint and applying splints. The arm was bandaged to the side, and the forearm fixed across the chest, as it was impossible to get him to remain in a recumbent position. The wound was again dressed with the iodine solution in four days' time, and was to my satisfaction a healthy one, the same procedure of bandaging being carried out. The limb was left in this position until the twelfth day, when the stitches were removed. On examination of the forearm it was then found that union had taken place between the approximated ends. The same procedure as before was adopted in fixing the limb.

On the eighteenth day I again dressed him, and gave instructions to come to me again in five days.

On the twenty-fifth day, as he had not put in an appearance, my native servants were sent to look for him, and later brought him to me. He had removed the splints, but had applied the bandages over the lint covering the wound, which was then quite healed.

I again saw him on the thirty-seventh day. The radius and ulna had then completely united, he could grip firmly with his right hand, and was able to extend, pronate, and supinate the injured forearm, but had a limited degree of flexion.

Three months later he came to me complaining of symptoms resembling sleeping sickness, which unfortunately proved to be true.

2. On a punitive expedition which I accompanied as medical officer into the hinterland of the Nile against some hostile tribes, I was called upon to treat over a hundred cases, comprising gunshot, spear, arrow wounds, and cuts, incurred on both sides. In almost every case I treated them with a 2 per cent. solution of tincture of iodine, and was surprised at the excellent results obtained. The various injuries healed in a remarkably short space of time, and I am of opinion that the beneficial results obtained from the use of this drug compare most favourably with any of the other antiseptics now in use, and,

moreover, I venture to state that on active service of such a nature in tropical countries, it is to be preferred to other germicidal preparations.

THE BIONOMICS OF THE RAT-FLEA.

BY

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HAVING been in the "backwoods" for some time past, I have only lately had my attention drawn to a paper by Nicoll in the BRITISH MEDICAL JOURNAL for October 26th on the life-history of the rat-flea (*Ceratophyllus fasciatus*, Bosc).

The work which Nicoll describes and the facts which he has elucidated are essentially similar to my own in 1910-11, which are the object of a report to the Local Government Board now in course of publication. But I disagree with certain of Nicoll's hypotheses put forward to substantiate these facts, and I would like here to give the reasons for my disagreement.

The facts are these. Fleas are found to live in a certain medium of rubbish for apparently a great length of time without food—Nicoll found for fourteen months, while mine were alive after eighteen months, when they were accidentally thrown away. This fact is of great practical importance, of course, in itself, particularly if the fleas are able to remain disease-infective for the same length of time, and the explanation of the fact may be equally important to the fact itself. Nicoll's hypothesis is that the rubbish contains a large number of larvae, which do not all undergo metamorphosis at once. Some of them pupate and imagnate after one month, some after ten months, and so on, remaining in a resting state meanwhile. In this way a constant succession of new adults is produced which take the place of the old ones, which die off in about a month's time if kept without food, the net result being that the original fleas apparently live for a great length of time.

Now, I disagree with this hypothesis for the following reasons:

1. I have taken specimens of the rubbish from time to time and looked carefully in it for larvae, or pupa-cases which should contain either resting larvae or pupae, and I have never succeeded in finding a single one. Active larvae certainly could not escape detection. Pupa-cases are more difficult to find, but not so difficult that they cannot be seen on a careful search. I have therefore concluded that the rubbish does not contain either active larvae or any resting stages of the metamorphosis. (I admit, however, that for the first month or two resting larvae could probably be found.)

2. I have carefully taken away from a quantity of rubbish all the adult fleas present (a live bait is an efficient mode of procedure for this purpose), and I have then watched the material carefully from day to day for three to four months to see if more adult fleas would appear, but with negative result. Now, if resting stages of the larvae were living in the rubbish and constantly producing a batch of new imagines, it is difficult to see why during the time observed adult fleas did not appear.

3. It is postulated that metamorphosis is most active in summer, yet our actual experience was that the adult fleas to be found in the rubbish greatly decreased during the hot months. This is hard to explain, if the resting larvae imagnate more quickly than in winter.

The conclusion which my experiments led me to was that it was the adult fleas, and not any stages in the metamorphosis, which survived for such a great length of time as eighteen months. I imagined this was possible to the adult flea, because the rubbish in which it lived was hygroscopic and non-conductive to heat, so that heat and dryness inimical to the flea's life was escaped from in its depths.

The facts which I elucidated could only (on Nicoll's hypothesis) be explained on the assumption (1) that the pupa cases which I searched for and failed to find were actually there; (2) that although for some reason or other the resting stages failed to imagnate after three to four months, they might have done so later on; (3) that the resting stages are more prolonged in summer than in winter—a conjunction of events improbable enough to make me believe that the adult flea really survives for great lengths of time without food.

Memoranda:

MEDICAL, SURGICAL, OBSTETRICAL.

A CASE OF DERMATITIS ARTEFACTA.

In December, 1911, the patient, a domestic servant, aged 42, was admitted to a cottage hospital for convalescence from some skin trouble. The lesions on admission were merely several reddened patches fast clearing up.

In July, 1912, she returned for treatment in the initial stages of an attack. The lesions, which were distributed fairly generally all over the body, would start with painful erythematous patches followed shortly afterwards by the formation of little blebs. On several occasions there was vomiting and distaste for food, so that I wondered whether an intestinal toxin was the causal factor. This attack lasted one month.

In January, 1913, I received a letter from the patient, saying the trouble had recurred and asking to be re-admitted. The lesions then were much larger and coarser, chiefly on the right side of the neck near the angle of the jaw and on the right upper arm (flexor surface). The patch on the neck was some four to five square inches in extent. Furthermore, reddened patches would occur, which were not always followed by blebs. I very soon doubted my original diagnosis, but there was nothing obviously artefact about the shape of the lesions. One day I remarked to her that it was curious how on this occasion all the patches occurred on the right side. Two days later a patch occurred on the left elbow with bleb formation. At the end of a month the lesions were healed, leaving a little pigmentation, and the patient looked out for and obtained a situation. As it was near London, I advised her to see Dr. Sequeira at the London Hospital if there should be any return of the trouble.

A fortnight later I had a letter from Dr. Sequeira saying that the patient had been to see him, and that "the eruption as then seen was undoubtedly artefact." One spot had such a curious shape that he felt at once it was self-produced. "Its boundaries were all straight lines and sharp angles." There was a faintly acid reaction to litmus. The patient then wrote to me that she had been compelled to give up her situation owing to recurrence of the trouble, and asked me to sign a sick-pay certificate. I refused to sign without seeing her, and she returned to the cottage hospital. A glance at the lesions was sufficient to make me confident that the patches were artefact. I tested one of them with litmus and got a faintly acid reaction, but when, as a control, I tested normal skin I got a much more acid reaction. This proved absence of caustic as an agent, and probably acid too. I accused the patient point-blank of practising deception. She was obviously taken aback, but soon met the suggestion with repeated denials and later with tears. In the pocket of her other dress a screw of paper containing yellow powder was found, but as I had told the searcher to look out for some fluid no notice had been taken of it and no sample obtained.

Next day I asked the patient privately to turn out her pockets, telling her that she had every right to refuse. She was then wearing the dress in the pocket of which the screw of paper with the yellow powder was found. She had no objection, and the pockets contained a few postage stamps and an empty purse. I rather expected this; it did not enable me to know what the powder was, but I felt tolerably certain that it was mustard. She told me most emphatically that there had been nothing in the pocket the previous day which was not in it at the moment of turning it out, and this confirmed me in the belief that I was on the right track. Even when I confronted her with the finder of the yellow powder she still denied it. I called her attention to the penalty under Section 69 of the Insurance Act, and she eventually confessed to having used mustard. In three days the skin trouble had cleared up.

The cottage hospital is partly supported by a small fee charged to the patients (5s. to 10s.). The patient was entitled to sick pay.