

organisms could be found in the air, as those coming in from outside fall to the bottom, while those in the sewage or the moist walls of the sewer cannot under ordinary conditions rise into the air. Only under two conditions—which were also investigated by laboratory experiments to remove all doubt—did we find that organisms were given off from the sewage: (1) where there was splashing; (2) when bubbles were allowed to rise through the sewage. To these conditions Major Horrocks has added a third, since he has shown that drain-pipes which have been dried are capable of giving off micro-organisms.

In our experiments we used quantitative, and not merely qualitative methods; and it still appears to me that the question of sewer air is one which ought to be regarded from the quantitative side. Infinitesimal risks of infection exist on all sides; and if, as I think, the risk of infection through escapes of sewer air is practically infinitesimal, we need not greatly concern ourselves about it. The unpleasant odours due to such escapes, and to stagnating sewage in badly laid sewers, are probably of much more practical importance, as many persons are very sensitive to them, just as they are sensitive to other unpleasant odours.

It is true that on account of the violent splashing in soil-pipes, etc., there are more possibilities of conveyance of infection through the drainage system of a building than through the air of a sewer; and this is clearly shown by the experiments of Major Horrocks and Dr. Andrewes. If, however, an infected person is present in a building, it is usually much less probable that infection will be conveyed from him to other persons through the drains than by numerous other more direct channels.

There appears to me to be little doubt that the great attention which has been given to the possible spread of infection through the air of sewers and drains has tended to divert attention from other and much more probable channels of infection; and my object in writing this letter is to assist in placing in its proper perspective the risk of infection through sewer air.—I am, etc.,

Oxford, Nov. 10th.

J. S. HALDANE, M.D., F.R.S.

THE TREATMENT OF ANAEMIA IN CHILDREN AND YOUNG ADULTS.

SIR,—With reference to Dr. Eustace Smith's interesting article on the use and misuse of iron remedies, in the BRITISH MEDICAL JOURNAL of October 17th, I would like to emphasize the advantage of medical exercises in cases of anaemia in young people of both sexes.

I have employed this method of treatment in a large number of cases during the past twenty-two years, with very satisfactory results. In some of the cases, iron in some form or another was prescribed while the physical treatment was being carried out; in others no drugs were exhibited. In each group the blood condition and general health were much improved. The haemoglobinometer indicated increased colouring matter, the appearance of the patient showed improvement, constipation was relieved, and in female patients the menstrual function regained a more normal measure of activity. Muscular movements stimulate the circulation, and cause the tissues to be better nourished, demanding, therefore, an increase of food and producing better appetite. Active physical exercises, if properly arranged and executed, imply mental activity and development, and are thus valuable as stimulants to the entire nervous apparatus. But perhaps the chief factor in benefiting anaemic patients is the fact that each exercise not only involves the movement of certain muscles, but also constitutes a breathing exercise. The blood in anaemic people is not only deficient in iron, it is also markedly in need of oxygen. Hence the importance of teaching patients to breathe properly and deeply. There is a physiological reason for each selected exercise, and during part of that exercise the chest is enlarging, during another it is decreasing in its capacity. It is here that the various school, so-called Swedish, exercises fail apart, from the fact that the important element in each exercise is often ignored or imperfectly carried out, owing to the inability of an instructor to attend to the many who are exercising at the same time, the proper respiratory effort is not put forth, because the instructor fails to grasp its importance or to insist upon its achievement. To obtain satisfactory results, medical exercises should be

under the care of medical men, and each patient should be dealt with separately.

The mere administration of iron may restore the iron content of the blood, but to secure the needed oxygen efficient pulmonary respiration is essential. Hence the need of combining respiratory exercises with the administration of chalybeates.

In addition to physical exercises I have found, during later years, that in the treatment of anaemia five to ten minutes of high-frequency treatment forms an important adjuvant, producing improvement in the nervous system, a better elimination of waste products and a more effective metabolism. These currents of high frequency improve circulation by causing peripheral hyperaemia, induce restful sleep in nervous people, and, as a rule, encourage increase of appetite.—I am, etc.,

Glasgow, Nov. 3rd.

W. F. SOMERVILLE, M.D.

THE MENTALLY DEFECTIVE IN PRISON.

SIR,—In the BRITISH MEDICAL JOURNAL for October 17th Mr. de Lisle, writing from New Zealand, traverses a sentence in a letter of mine, which appeared in the issue of July 4th, p. 55. The sentence is as follows:

The results of secular education in some of our colonies seem to show how dangerous education divorced from religion is, and the general experience is that morality is not successfully taught apart from religion.

Mr. de Lisle contradicts my statement, and adds:

It is unfortunate that so many persons holding positions that should entitle their opinions to weight argue on the logic of facts as they imagine them to be, instead of as they are.

I will not stay to point out that Mr. de Lisle contradicts more than I asserted, but will go straight to the point.

My statement was based, not on "facts as I imagined them to be," but on facts authoritatively published. His contradiction has obliged me to make further investigations, and has thus delayed my reply.

I am sorry that I cannot accept his denial; indeed, the result of my inquiries concerning some of our colonies, including New Zealand, more than ever confirms me in my opinion.

The Duke of Wellington was not far wrong when he described many of the products of godless education as "clever devils."—I am, etc.,

London, Nov. 10th.

F. H. CHAMPNEYS.

THE HOME TREATMENT OF SCARLET FEVER.

SIR,—The columns of the BRITISH MEDICAL JOURNAL form each week a veritable mine of information and suggestion to the busy practitioner. I venture, however, to think that the article, p. 1333, entitled the Home Treatment of Scarlet Fever, by Dr. Robert Milne, is one of the most important that has appeared for some time—provided that other observers corroborate the experience of the medical officer of Dr. Barnardo's Homes and Hospitals.

The mass of fact and observation published in the medical press is so great that much necessarily passes unheeded—as Matthew Arnold says, in another sense, "We forget because we must and not because we will." To those of us who have the responsibility of looking after large boarding schools in which epidemics of scarlet fever and measles are a disaster and a grave anxiety to all concerned, any effective prophylactic measure must be doubly welcome.

1. Dr. Milne implies that scarlet fever is chiefly contagious in the "peeling" stage. Thus he writes, p. 1334:

I have had children return, after seven weeks' absence in the fever hospital, with a little skin on the soles of the feet unpeeled. Within a few days the children who slept in a bed on either side of such a case were attacked.

And yet it is far more commonly believed that the tonsillitis is the initial and infective lesion of scarlet fever. "It is certain," says Comby (*Traité des Maladies de l'Enfance*, art. Scarlatine), "que la scarlatine est contagieuse au début, même avant l'éruption."

2. Dr. Milne maintains that, in measles, when eucalyptus oil is used at the earliest possible moment it has entirely stopped the epidemic spreading, his method being simply to "rub the oil most carefully from the crown of the head to the soles of the feet."

3. Dr. Milne states that with this treatment—rubbing with oil morning and evening for the first four days and then once daily until the tenth day, the throat to be swabbed with carbolic oil 10 per cent. every two hours for the first twenty-four hours—he has never known any nose, ear, or kidney trouble result.

While there may be minor aesthetic objections to complete anointing of the body of patients confined or not to bed, Dr. Milne's instructions are so definite and so evidently based on experience and results that I, for one, intend to carry them into effect, although I cannot yet share his confidence that "no isolation of the patient in the way now practised is necessary."—I am, etc.,

Paris, Nov. 5th.

A. A. WARDEN, M.D.

THE DURATION OF THE POTENCY OF VACCINE LYMPH.

SIR,—In view of the interest attaching to the storage of glycerinated calf lymph, may I venture to place before your readers a few experiments which I made in that direction with human lymph? I found the high temperature of summer in Chicago, where I then lived, far more destructive to lymph than the cold of winter.

On September 26th, 1878, in the primary vaccination of a child, I used lymph nine months old, and made three insertions; all three matured normally. On February 10th, 1880, in the primary vaccination of a child, I used lymph fourteen months old, making two insertions; both matured normally. On March 5th, 1882, in the secondary vaccination of two adults, I employed lymph thirteen months old, two insertions being made in each; both matured in each case. On September 11th, 1891, in the primary vaccination of a child 13 years old, I used lymph eighteen months old, and inserted it in two places; one failed, the other matured normally. Two days later, I used some of the same stock of lymph in the primary vaccination of a child 10 months old, and made three insertions; all three were perfectly satisfactory—were normal. On November 19th, 1893, in the primary vaccination of a child 4 months old, I used lymph thirteen months old, inserting it in three places; all matured most satisfactorily.

Two capillary tubes of lymph, taken by me during the winter of 1896, which I brought with me from Chicago, were active after being ten years stored. These were used on May 21st, 1906, on two children, of different families, by a public vaccinator who was not aware, at the time, of the age of the lymph. He was entirely satisfied with the result.

In all these cases the lymph was preserved by myself in capillary tubes filled from the vesicles on the eighth day after vaccination. When kept thirteen, fourteen, or eighteen months it had to withstand the trying effects of both summer and winter. The lymph used on September 11th and 13th, 1891, was taken March 18th, 1890. Therefore it passed safely through two summers and one winter.

My plan of preserving the lymph was this: Having filled the capillary tubes for half their length, I sealed them at once. Then I put them into a piece of small glass tubing, sealed at one end, and kept it in a boxwood case of a clinical thermometer with the head tightly screwed down.—I am, etc.,

Stillorgan, co. Dublin, Nov. 1st.

P. O'CONNELL, M.D.

"NEURORRHEUMA" OR "NEUROKYME."

SIR,—In her interesting little book, *What do we Know concerning Electricity?* Miss Antonia Zimmern has the following passage:

The modern conception of an ether is an invention due to Huyghens. I use the word invention advisedly, because we have no experimental evidence for its existence. . . . It must suffice for us that many phenomena take place as if there were such a medium.

It seems to me that by precisely the same argument we can justify the invention of a hypothetical "nervous energy," and the designation of it by a single title—either the "neurorrhœuma" of Dr. Hale White or, better

perhaps, the "neurokyme" of Dr. McDougall. The fact that Dr. Hale White's term has not met with acceptance may be partly due to its having been suggested before the need of some such expression had been generally recognized. I think myself, however, that Dr. McDougall's word is more likely to commend itself, and I hope that it may be adopted; for, whatever analogies and resemblances may exist between the manifestations of nervous energy and those of other species—for example, electricity—it can hardly be doubted that the former will prove in many respects unique. The adoption of a specific title, such as "neurokyme," would provide a nucleus for the crystallization of the observed characteristics of nervous energy into what might in time become a definite and serviceable conception.

In the *Annals of Psychological Science* for October, 1905, there is an interesting article by Dr. Paul Joire describing the results obtained by an instrument which he calls the othenometer, designed for observation of variations of the nervous force. It consists of a dial graduated into 360 degrees, in the centre of which, upon an upright glass support, is pivoted a pointer of straw. A glass shade protects the pointer from currents of air. Precautions are taken to eliminate the action of sound, heat, light, and electricity. When the hand is brought near to the side of the shade the pointer is attracted to an extent varying from 15 to 50 degrees. In health the deviation produced by the right hand exceeds that produced by the left in an approximately definite proportion. In cases of neurasthenia this relation is reversed, the deviation produced by the left hand exceeding that produced by the right; while in hysteria the deviation produced by the right hand exceeds that produced by the left in a greatly exaggerated degree. The hands of an epileptic, examined on the day after a sharp attack, produced no movement whatever. Six weeks later, when the health was greatly improved, the pointer moved 55 degrees for the right and 43 degrees for the left hand.

Here we have tentative measurements of a force presumably distinct from sound, light, heat, and electricity, as yet undignified by so much as a name. Whether Dr. Joire's results are substantiated or in some respects invalidated by further research, there can be no doubt that the hypothesis of a distinct form of "nervous" energy is both legitimate and likely to stimulate work along the same lines. And why should this energy lack an appropriate name?—I am, etc.,

Bath, Nov. 8th.

C. J. WHITBY.

THE LIMITATIONS OF A PURIN-FREE DIET.

SIR,—I am much obliged to Dr. Haig for his very kindly criticism of my article on "The Limitations of a Purin-Free Diet." I am rather surprised to note, however, that he quite fails to appreciate the conclusions at which I have arrived. He appears to think that I have levelled the very serious charge against the system of dieting associated with his name that it advocates a diminished proteid content, and he is inclined to attribute the bad results I have reported to this practice on my part. If he will carefully read my paper again, he will notice that whilst almost all the cases received some decided benefit on a purin-free diet so far as their maladies were concerned, this was at the expense of their health and vigour, but when they were put on a simple mixed diet they regained their physical strength, even though their maladies in some instances had a tendency to recur. I have argued from this that the purin-free diet is evidently an excellent method of treatment for chronic disease, but when its good therapeutic effects have been obtained, and its bad results have begun to manifest themselves, it is wise to return to the ordinary flesh proteids to which the patients were previously accustomed. As on this addition to the diet they regained their strength, and a large percentage maintained their health, the good effects were manifestly not due to absence of purin from their diet, but to a simpler mixed diet containing purins in lessened quantity, but also a lessened quantity of proteid and food in general, as well as greater attention to mastication and other such hygienic laws. I am inclined to think that many of the bad results obtained on a purin-free diet were due to