will be seen; that they have no relevance to the logical doctrine of inherited predisposition Professor Karl Pearson has pointed out.

Professor Cummins, in theorising about prevention, has, I think, been misled by an analogy. He is conscious of the successes achieved by antityphoid inoculation, and supposes that a similar triumph awaits the immuniser against tuberculosis. There is, I submit, no true analogy. Temporarily to confer upon a physically select group a greater than average power of resistance to a specific infection is an essentially different task from that of permanently elevating the resistance of a whole race. The difference is of kind as well as of degree. There is, I submit, a fundamental weakness in this philosophy of preventive medicine; it is like the difference between the examination coach and the real teacher. A skilful coach can inoculate a pupil up to a high grade of immunity against an ordinary "external" examiner; by so doing he often fulfils a useful purpose. But that more vigilant examiner whose questions are not printed but yet must be answered in the schools of life is not so easily baffled.

Shoemakers die of tuberculosis at $4\frac{1}{2}$ times and commercial clerks at $3\frac{1}{2}$ times the rate of clergy. I do not suppose Professor Cummins believes that the immunisation of the children of clergy through the milk-supply is so efficient as to explain these contrasts. As a bacteriologist, he will rightly appeal to the greater opportunities of massive infection enjoyed by factory operatives and clerks in crowded offices. May not even this be somewhat too simple an explanation? Vocational selection is after all a fact; I do not think my father could have persuaded a blacksmith to take me as apprentice. Nobody is concerned to explain away the fact that normal physical characters are inherited. In all parts of Austria, rural districts and the crowded capital alike, the mortality from phthisis has risen year after year since 1915. I suggest that the variations of natural resistance and the general lowering of the average of resisting powers due to an unphysiological way of life *must* be studied, that no ingenious short cuts to the solution of the problem of great plagues are available. Dr. Leonard Hill has shown how, quite apart from any factor of massive specific dosage, natural resistance not to one but to all infections may be reduced. Whether what Dr. Hill has shown *may* be the explanation of what we find, is indeed the explanation, can be ascertained by the methods of the "unintelligible" biometricians; not quickly nor easily, but securely.

I do not intend to be a newspaper pessimist, but the survivors of a great war, which has destroyed a considerable percentage of the physically select males, and of the second greatest pestilence in history, who read day by day of famine and sickness throughout large tracts of Europe, will do well to doubt whether security can be purchased on cheap terms. "Preventive medicine," says Sir George Newman in his first report, "is concerned with the physiology and psychology of man even more than with the physical form or end-results of disease, and to deal with it effectually involves dealing with all its aspects continuously."

I am, Sir, yours faithfully,

Loughton, Nov. 20th, 1920.

MAJOR GREENWOOD.

COELIAC DISEASE.

To the Editor of THE LANCET.

SIR,—In answer to Dr. R. Armstrong's letter, published by you on Nov. 13th, I must express my concern at having in my recent paper¹ appeared to have overlooked the important communication by Dr. F. J. Poynton, Dr. D. N. Nabarro, and himself, entitled "A Contribution to the Study of a Group of Cases of Chronic Recurrent Diarrhoea in Childhood."² I can assure him that I had studied it with care and made no mention of it because I understood that the whole point of his contribution lay in the supposition that his cases were not essentially instances of coeliac disease, but were cases of organic intestinal disease upon which a possible coeliac (acholic) element was superimposed.

The relationship of the cases in Dr. Armstrong's paper to true coeliac disease is a matter of great concern to those working at the subject. Setting aside the true but rare cases of pancreatic infantilism described by Bramwell,³ it would seem that we have three groups of cases showing pale fatty diarrhoea, enlargement of the abdomen, and, if the diarrhoea persists long enough, retardation of growth.

1. Cases in which the true coeliac (acholic) element is the primary disturbance. This I take to be a functional derangement for reasons which I will give later; and I suppose that bile-salt secretion or circulation is at fault. This group is the true "coeliac affection," as described by Gee,⁴ who noted that there were no obvious causative lesions to be found after death.

2. Cases in which as the result of the coeliac disorder there develops a secondary enteritis set up by the abnormal condition of the contents of the bowel. This group is at present hypothetical only, but should it exist it would be included under the true coeliac type.

3. Cases in which the primary disease is an organic disease of the bowel, which may by itself set up fatty diarrhoea, or to which the coeliac derangement may be added. To this group the first case in Dr. Armstrong's paper belongs; the symptoms, treatment, and pathology all prove this, and the reader is led to infer that in the authors' judgment the remaining cases described are essentially similar. But this, I think, is not coeliac disease, and in 1913 the authors went out of their way to avoid the use of that term, as witness the title of their paper. The question arises, What can we call such a case as their fatal case? I should prefer to call it "intestinal infantilism," using Herter's term⁵ without agreeing to his theory of the specific bacterial causation of the enteritis.

The crucial matter of the discussion is this: Was Gee correct when he stated that the coeliac affection might persist and produce stunting (for he described this vividly) without there being any recognisable causative organic disease? I think he was, and for this reason. I see no essential difference between temporary cases of coeliac stools, protracted cases, and persistent cases leading to infantilism. (a) Temporary cases. Acholic stools may be passed for a few days—e.g., with dentition (see Cheadle,⁶ Case 5). (b) Protracted cases. Cheadle's Case 3 had coeliac symptoms for two years, but Dr. Rudd kindly tells me (October, 1920) she got quite well and developed normally at puberty. Dr. H. J. van Praagh's case,⁷ after symptoms lasting over a year, got quite well, the author informs me. (c) Persistent cases with infantilism. In an unrecorded case of my own symptoms had been present for nearly five years, the child had been under observation for three years, was considerably stunted in growth, and had suffered from oedema of the extremities during acute attacks of diarrhoea; yet after death no chronic changes were found in the pancreas, liver, or intestine to account for the symptoms.

A further point in favour of the functional origin of the essential coeliac derangement is that it is at present impossible to correlate coeliac disease with any type of organic hepatic disease; nor do we find in cases of advanced hepatic disease the presence of coeliac symptoms.

I trust this letter will assure Dr. Armstrong that it was not without deliberation that I omitted reference to his cases; but because in dealing with coeliac disease from the points of view of its fat-digestion and treatment by bile-salts, I thought it safer not to refer to cases which appear to be, and were, as I thought, published as being, allied to, rather than true instances of, this disorder.—I am, Sir, yours faithfully,

Harley-street, W., Nov. 22nd, 1920.

REGINALD MILLER.

References.—1. THE LANCET, 1920, ii., 894. 2. Brit. Jour. Child. Dis., 1914, xi., 145 and 193. 3. Clin. Stud., Edin., 1902-3, i., 157. 4. St. Bart. Hosp. Rep., 1888, xxiv., 17. 5. Infantilism from Chronic Intestinal Infection, 1903, New York. 6. THE LANCET, 1903, ii., 1497. 7. THE LANCET, 1904, i., 224.

MALIGNANT GROWTHS OF THE ANTRUM.

To the Editor of THE LANCET.

SIR,—On one point my experience hardly agrees with that of Mr. E. D. D. Davis, who, in his very interesting article in your issue of Nov. 27th, says that in operating on the maxilla, the horizontal limb of Fergusson's classical incision did not give much more access to the operative area than was provided by the vertical limb alone. But my present object is to refer to a means by which the consequent occurrence of infra-orbital oedema may be circumvented—a small technical refinement

which was, I believe, first demonstrated to me by Mr. Wilfred Trotter.

In the modification to which I refer, the horizontal diverges from the vertical limb of the incision in the usual manner; but only for about half an inch. As soon as it has passed outwards in the skin to a point beyond the punctum lachrymale, the knife is carried through the lower tarsal plate into the lowest level of the conjunctival fornix, straight down to but not through the periosteum of the malar bone. The incision is continued outwards in this layer as far as desired, and the lower eyelid, except for its median quarter or third, is stripped outwards with the cheek. The cut surfaces of the tarsus are afterwards accurately apposed by three or four stitches of ophthalmologists' salmon-gut or horsehair; the conjunctival fornix may require a couple of fine catgut stitches, but usually falls into place without any. The usual stitches in the skin of the vertical and of the innermost half-inch of the horizontal limb keep the parts so well in place that there is no strain on the fine tarsal sutures; and in the end-result there is no cosmetic or functional damage. I have adopted this device not only for tumours of the antrum, but also on one occasion to obtain access by the naso-antral route for the removal of a pituitary tumour. The case was reported in THE LANCET of July 5th, 1919, and to-day the patient is in perfect health, with no facial disfigurement.

It is noteworthy that this manoeuvre eliminates the subsequent incidence not only of the cutaneous infra-orbital oedema, but also of the usual conjunctival chemosis. I am, Sir, yours faithfully,

Wimpole-street, W., Nov. 28th, 1920. H. LAWSON WHALE.

THE DANGER OF SELF-ADMINISTRATION OF NITROUS-OXIDE GAS.

To the Editor of THE LANCET.

SIR,—In view of a recent fatal accident resulting from the testing of a cylinder of nitrous-oxide gas by self-administration, we think it right to bring this danger to the knowledge of those interested in the administration of anæsthetics.

One of us (J. D. H.) called at the dentist's house to administer gas to a patient, and on entering the consulting-room found the dentist dead in his operating chair with the mask applied to his face. The head had fallen forwards and sideways over the arm of the chair and was resting upon the gas-stand while the cylinder was still supplying gas. At the inquest it was stated that the dentist at times tested the apparatus by inhaling the gas previous to its use for an operation.

We believe it to be a common practice among dental surgeons occasionally to take a so-called "whiff," sufficient only to cause partial unconsciousness. Several cases are known to us where this has been done by the surgeon while standing in the upright position, the result being a heavy fall on the floor. The mask being fortunately displaced, there followed a return to consciousness. But if the inhalation, as in this case, be taken while lying down there is a danger of complete unconsciousness supervening, when tonic muscular spasm may retain the mask in position until fatal asphyxia occurs.

The self-administration of nitrous-oxide gas is stated to afford a very pleasurable sensation, and this fatal case should be a warning that the practice is not devoid of grave danger. Sir Frederic Hewitt has reported a fatal instance.¹ We shall be glad to hear of any other such cases.—We are, Sir, yours faithfully,

JAMES DONALD HOLMES, M.B., C.M.

HEDLEY C. VISICK, M.R.C.S.,

Anæsthetist to the Hampstead General Hospital.

Nov. 24th, 1920.

THE LATE LEONARD GUTHRIE.

To the Editor of THE LANCET.

SIR,—Recently you published a letter asking for a photograph of the late Dr. Leonard Guthrie. I am glad to say that two copies have been obtained, one a

recent full-length snapshot, but unfortunately taken in strong sunlight, and the second a photograph taken many years ago. Both of these have been enlarged, and any of your readers may obtain copies from me, the price being 12s. 6d. each.

I am, Sir, yours faithfully,

H. W. BURLEIGH,

Secretary and General Superintendent, Hospital for Epilepsy and Paralysis.

Maida Vale, W., Nov. 25th, 1920.

ACIDOSIS AND BEE-STINGS.

To the Editor of THE LANCET.

SIR,—In his clinical note which appeared in your issue of Oct. 23rd Dr. J. O. Beven expressed the opinion that certain patients of his who were stung by the rock bee, *Apis dorsata*, developed an acidosis, and he treated them accordingly. In commenting upon Dr. Beven's observations you suggest in an annotation that the condition described was not an acidosis due to the formic acid of the bee-sting, but more likely was a severe toxæmia due to the specific toxalbumin in the bee-sting.

Crile, in his excellent book on "Surgical Shock," has thrown much light on the causation and pathology of shock. He has shown that shock is produced by an intensification of the everyday causes of fatigue, and that such shock is due to an excessive amount of nociceptive stimuli reaching the brain cells. These stimuli of a harmful nature to the animal organism are produced (1) in muscular exertion; (2) in emotional excitation, (3) in physical injury, (4) in anaphylaxis, (5) in injection of foreign protein, (6) in injection of various toxins, (7) in injection of acids, and in several other conditions.

It was also shown that any one of these factors, per se, if intense enough, produced shock of some degree or other, and that if any of the above factors occurred simultaneously the added result was a marked degree of shock. However, the interesting point is that in shock produced by any one of these causes there was a concomitant intracellular acidosis, which acidosis was measured and proved to be present by the diminution of the reserve alkalinity and an increase in the hydrogen-ion concentration of the blood.

Applying all this to the cases in point, the victims of the bee-stings were rock climbing, which induced a certain amount of muscular fatigue, the numerous bee-stings caused intense pain stimuli, no doubt there was an emotional element of fear, and there was an injection of acid, toxin, and foreign protein in each sting. Added together, these nociceptive stimuli were probably sufficient to cause profound shock, with an intracellular acidosis, and therefore Dr. Beven's treatment was sound and rational and his diagnosis correct.

I am, Sir, yours faithfully,

A. D. SYMONS, M.B., Ch.B., D.P.H.,

House Physician, Royal Hospital for Sick Children and Women, Bristol.

Nov. 23rd, 1920.

NATIONAL RELIEF FUND HOSPITAL GRANT.—The Executive Committee of the National Relief Fund have appointed a special committee to advise them as to the distribution of the English and Welsh share of the grant voted towards the reduction of the war deficits of the voluntary hospitals of the United Kingdom. The total grant is £700,000, and the amount applicable to England and Wales is £560,000. The Distribution Committee consists of Sir George Murray (chairman), Sir Napier Burnett, Rev. G. B. Cronshaw, Miss Mary Macarthur, Sir Arthur Robinson, and Viscount Sandhurst. Since the amount available will not be sufficient to allow of all deficits being paid off in full, the committee of the Fund have decided that assistance must in general be limited to hospitals having a resident medical staff and at least 50 beds. In special cases, however, the Distribution Committee have a discretion to recommend grants to hospitals with less than 50 beds. No grant will be made in cases where the deficit for the five years ending Dec. 31st, 1919, is less than £500, and cottage hospitals, convalescent homes, and sanatoriums are definitely excluded. Application forms have now been issued to all hospitals eligible for assistance.

¹ Anæsthetics, third edition, p. 284.