

human being (and after all, Mr. Peacock is very human) to furnish the information required. The vast majority of the thousand or more medical officers of the Second Army who were placed in medical charge of troops (and in whose capacity I wish Mr. Peacock could have higher faith) were accustomed to live in intimate contact with their brother combatant officers and shared alike in that intimate knowledge and sense of responsibility for their men which it has always been their pride to maintain.

I am, Sir, yours faithfully,

M. COPLANS.

The Jenner Institute Laboratory, Church-road,
Battersea, Nov. 30th, 1921.

FASTING.

To the Editor of THE LANCET.

SIR,—By his interesting and instructive article which appeared in THE LANCET of Dec. 3rd, Sir Henry Lunn has rendered a great service to the profession in redirecting attention to the therapeutic value of fasting. Pray permit me to support his thesis and offer a few comments. When the Allen starvation treatment for diabetes was first introduced, I tried it on several patients in my wards, and was more than satisfied with the results. The first patient, to whom I suggested it in private, was a semitic looking gentleman who claimed an ancient Scottish lineage. He irradiated a characteristic atmosphere of good temper and good living. When I had finished my recital of the benefits of a three days' fast, he became as if deflated; his face assumed a look of horror, and he growled at me, "That's all very fine, doctor, but have you ever tried it?" The retort that I was not a diabetic was obvious and prompt. It was, nevertheless, humiliating to have to confess the negative, and the unpleasant experience determined me never again to be obliged to make a similar reply to what might seem to a layman a perfectly legitimate question. For the next week-end, therefore, I ordained for myself a fast on Allen lines; that is, no food of any kind and only water to drink. Realising the probability and also the inadvisability of hoarding my faeces for the prescribed three days, I took a grain of grey powder on the Friday night and a dose of Epsom salts on the following morning. Then I fasted, secundem artem, until Tuesday morning. On Saturday I felt well, but I am told that my conversation was terse in matter and staccato in manner. On Sunday I felt very well, and endured my trial in the spirit of a Christian martyr. I was resigned. On Monday I felt very well indeed, and did my work at home and at the hospital with zest and vigour. I had arrived at the stage of the superior person who regards meals as mundane matters suitable only to the Philistine and the pork butcher. And truth to tell, I swallowed the cup of tea and the banana which broke my fast on the Tuesday with a certain indefinable regret, as though I had fallen from grace and was no longer worthy to be numbered among the elect.

Since that time, now many years ago, I have frequently repeated the experience, and always with the same result. The first day, craving; the second, resignation; the third, rejoicing and rejuvenescence. The result is that I can cordially recommend the discipline as an occasional exercise to anyone who is obliged to lead a sedentary life. The rationale of its beneficence is probably as follows. In the ordinary routine of town life we do not completely oxidise our intake; there remains a residue which is only partially oxidised. These "suboxides," if I may so term them, are mildly but cumulatively toxic, and give rise to many of the minor maladies and discomforts of everyday life. In the process of fasting these suboxides, which are relatively insoluble, become thoroughly oxidised, and thus fitted for normal and easy excretion.

It is almost certain that the energy which is set apart for our digestive processes takes the path of least resistance. It seizes upon the easily oxidisable, and when that has been disposed of, but not before, it attacks the tougher portion of the proposition. Thus

it comes about that the tired horse has the heavier load to pull. Is it any wonder that he falters by the way? The faltering in this case means suboxidation, which can only be corrected by giving an opportunity for an oxidation which is full and free. This is well and physiologically effected by fasting—and by no other means. Sir Henry Lunn says that he sometimes takes cocoa on his days of fast. He will, I am sure, forgive me for saying that, for the reasons given above, he is wrong. Cocoa is more readily oxidisable than, say, urates of soda, and the metabolic energy will naturally seize upon it rather than upon them. The result is a retardation of the curative process. If, instead of a cup of cocoa, Sir Henry Lunn will take half a grain of thyroid, he will expedite his cures and qualify himself to better his present most excellent and timely instruction.

I am, Sir, yours faithfully,

LEONARD WILLIAMS.

Harley-street, W., Dec. 4th, 1921.

THE QUESTION OF SELF-DISINFECTION.

To the Editor of THE LANCET.

SIR,—I have read the letter on this topic in your issue of Dec. 3rd with unusual interest, since I have never been quite able to understand why prophylactic measures against venereal disease amongst Canadian troops met with failure, complete and ignominious, whilst identical measures appear elsewhere to have attained considerable success, notably in the Portsmouth area. That "prevention is better than cure" is customarily regarded as axiomatic; and the Canadian military authorities in England attempted with characteristic vigour and thoroughness, through the medium of instructive lectures and the supply to soldiers proceeding on leave of packets and full instructions as to their use, to reduce the incidence of venereal disease. London, being comparatively accessible to troops in Canadian camps, a special treatment centre, suitably disguised, was established here in charge of a competent genito-urinary specialist, and soldiers urged to report immediately after exposure. As no upsetting questions were asked, no record kept of names and numbers, and they were greeted with neither head-wagging, curtain-lectures, nor sermons, the men made full use of the facilities afforded. After confirmation of the use of the materials which had been supplied, an irrigation with potassium permanganate was given, for which later on was substituted the injection into the anterior urethra of a little argyrol solution, the penis having been first clamped near the peno-scrotal junction and then cleansed with sterile water.

I thought very favourably of the scheme indeed, and as officer i/c treatment at the principal V.D. hospital, an institution with 1300 beds and a large out-patient attendance, I followed with great interest the results, supplementing the figures obtained in our admission-room by inquiries along similar lines in the various units through their medical officers. We finally concluded that the net results of the effort were nil; indeed, the advantage at times seemed with the men who had not availed themselves of the facilities; such are the vagaries of statistics, however painstakingly prepared. Our own hospital personnel, numbering 125, and many of them university undergraduates, were under no illusion as to the value of our prophylactic measures, and customarily declined with thanks both the packets and the treatment, preferring to rely on their own individual methods, usually prompt ablution with soap, and urination.

I hold no brief for either of the Venereal Councils competing for a place in the sun, and the literary weapon discharged by the N.C.C.V.D. will, no doubt, draw a return salvo from the N.C.P.V.D. It may be said, however, that the former base their whole campaign on two fundamental conceptions (or misconceptions): firstly, that the public, and especially the young, are ignorant and must be instructed; secondly, that if the possibilities of venereal disease are vividly brought to their notice they will remain

chaste. Some acquaintance with the psychology of youth, particularly in relation to sex matters, leads me to think that both these propositions are untrue. I am tempted to ask the N.C.C.V.D. the following pertinent questions:—

1. Are they quite satisfied that the statistics upon which they base their activities are accurate?

2. Is it not true that the number of "cases" automatically increases as the number of treatment centres is increased; the patients often visiting many centres in turn, so that "one" becomes in completed statistics "six," "ten," or more? We know that with dissatisfied patients this occurs.

3. Do they know that with venereal disease there is often associated a peculiarly characteristic mental depression and a tendency to introspection, due, perhaps, in the first instance to the exaggerated warnings of quacks, but also in some measure attributable to the deductions of the type of scientist who reasons from one to all?

4. Do they admit any connexion between their graphic descriptions of the "horrors" of venereal disease and the present increase in sexual hypochondriasis?

5. Do they not consider that the perusal of certain of their pamphlets may cause unnecessary suffering and anxiety to those unfortunately already infected? Or doesn't it matter?

6. Are they psychologically sound in regarding deviation from virtue as resulting from a calmly considered and rational decision? On the contrary, is it not usually an unreasoning act, the result of an emotional storm?

7. Do they believe their extensive, and expensive, campaign to be justified either by social conditions or, more important, any actual improvement attained?

8. Do they visualise the extent of the disservice their publications are doing and have done to this country abroad, and particularly in the United States?

9. Has the campaign greatly benefited anyone, excluding possibly the manufacturers of expensive "ethical" venereal remedies?

I am, Sir, yours faithfully,

HAROLD D. L. SPENCE, M.D.

Caroline-street, Bedford-square, W.C., Dec. 2nd, 1921.

THE ÆTIOLOGY OF ADENOIDS.

To the Editor of THE LANCET.

SIR,—I do not contend that the food factor is the only factor in the causation of adenoids. All causation is complex, and it is only for practical purposes that we single out one particular factor and speak of it as the cause. If the data I have enumerated in my letter in your issue of Nov. 19th are accurate, it follows as a logical conclusion that the great preponderance of adenoids among British children is related to the peculiar nature of the British diet. The only way Mr. W. Rushton (Dec. 3rd) can cast legitimate doubt on that conclusion is by disproving the accuracy of the data, and this does not seem to me possible. The citation of two or three special cases as proof that I am exaggerating the food factor in the ætiology of adenoids carries little weight. What is needed is to compare, as I have done, the relative frequency of this disease among large numbers of children brought up on British diet with large numbers of children brought up on a non-British diet.

But even the cases cited by Mr. Rushton are not convincing. They do not answer to my conception of properly fed children. The first child was breast-fed for six months only and developed rickets. The other two children were bottle-fed from birth. The fact that all three developed adenoids and tonsils suggests a pronounced catarrhal diathesis in each case, and this again suggests defective intestinal digestion, of which the most common cause in children is improper food. Dr. Sim Wallace, Mr. Rushton tells us, "attributes adenoids to weather conditions." While Dr. Wallace and I are in agreement on many questions, on this point I am reluctantly compelled to

differ from him. There is no room for compromise in regard to it. In so far as damp predisposes to catarrh (a bacterially induced inflammation) it may help to produce adenoids; but the average child, if properly fed and enjoying in consequence a sound intestinal digestion, has little tendency to suffer from those catarrhal infections which issue in adenoids. The climate of our country has been damp for thousands of years, but we have the evidence derived from disinterred skulls that it is only within comparatively recent years that adenoid disease has been common in this country. Consider also the fact that, although (strange to say) rickets is almost unknown in Australia among the white children, adenoids is rampant among them. I may add that this disease appears to be equally common among the white children in South Africa. One of the worst cases I have ever seen occurred in a British child who was brought up on the veldt!

Can Mr. Rushton escape from the conclusion to which these facts point?

I am, Sir, yours faithfully,

HARRY CAMPBELL.

Wimpole-street, W., Dec. 3rd, 1921.

PATHOLOGICAL RESEARCH: HUMAN AND COMPARATIVE.

To the Editor of THE LANCET.

SIR,—The science of pathology has made enormous advances during the last two decades, but great indeed as these are, where is the man who will deny that the more he studies and the more he learns, the less he seems to know, ever and anon emphasising the great home truths that a vast realm of unexplored territory awaits the enthusiast for investigation. Even in the field of routine cases which we see in practice daily, where clinical data appear to present themselves in almost standardised sequence, often leave us non-plussed when we take the trouble to verify our conclusions by an autopsy. This is an unsatisfactory state of affairs. In human medicine post-mortem verifications, largely for sentimental reasons, are tabooed, much valuable material being thereby lost to science. Such a plea, however, cannot be put forward by the practitioner of comparative medicine, for here sentiment counts less; nevertheless, the busy veterinary practitioner invariably loses active interest in his case the moment death steps in, simply because he cannot afford the time to make a special visit to the knacker's. What an enormous volume of valuable script is committed thus yearly to oblivion only those who come directly in touch with the subject-matter can realise. Has the time, therefore, not arrived when some scheme could be instituted whereby data and material might be collected at the animal "morgues" distributed throughout the country?—the veterinary surgeon attending selected cases supplying clinical history, &c. Knowledge as to the cause and course of disease in its most diverse form would be tapped to the economic benefit of the human and animal population. By such a scheme at first sight it may be urged comparative pathology would gain much and human pathology little, but in reality this would not be so, for the connecting link between animals and man, from a medical point of view, is very close indeed.

One need only cite experimental research work performed in animals to illustrate the benefit which man derives from comparative pathology. Further, a larger and broader outlook on the field of medicine is required from all of us to-day, and bearing on this point Sir Clifford Allbutt writes:—

"The address you enclose to me contains on every page illustrations of the cross lights which each branch of morbid biology would throw upon the rest. Many little points which to you seem of every-day interest are new to me in this sense, that they illuminate principles which in human medicine are less prominent, in your field they shine out with a new perspective. For instance, you can with every kindness and caution act at your own discretion unhampered by hesitations coming of the fear and reflections of more