Correspondence.

DYSPEPSIA.

SIR,—As methods of precision in diagnosis develop the treatment of dyspepsia is falling increasingly into the hands of the surgeon. That tendency may prove a permanent or a passing phase. The diagnosis also, in obscure cases at least, of dyspepsia is being more and more laid on him, and he feels the need of help from physician, physiologist, and pathologist. The columns of the JOURNAL have recently contained two important articles, the one entitled "Disappointments after gastro-enterostomy," from the pen of Sir Berkeley Moynihan (BRITISH MEDICAL JOURNAL, July 12th, 1919), and the other, "Dyspeptic and other referred symptoms associated with disease of the gall bladder and of the appendix," from that of Sir Humphry Rolleston (BRITISH MEDICAL JOURNAL, March 6th, 1920). Both articles indicate the somewhat widely separated sources of origin of the symptoms of dyspepsia—the biliary apparatus, the appendix, the duodenum, etc., in addition to the stomach itself.

In the JOURNAL of November 15th, 1919, in an article entitled "Remarks on the frequency, diagnosis, and treatment of chronic pancreatitis," I indicated the pancreas as an important source of dyspeptic symptoms, and in that article, and the correspondence that followed, pled for systematic examination of the pancreas as possibly the sole or the associated source of obscure dyspeptic symptoms. Such systematic examination, by means of microscopic examination of a *fresh* portion removed by the surgeon from the living gland, has, as I indicated, convinced me that chronic or subacute pancreatitis is the sole or the associated source of the symptoms in many cases of dyspeptic troubles, and, further, that certain supposed signs of pancreatitis on which we have hitherto relied are either unreliable or present so rarely as to be valueless, more especially in the early stages of the trouble.

The structure and the functions of the pancreas are apparently complex and at present ill understood. It stands, as I recently pointed out (BRITISH MEDICAL JOURNAL, January 24th, 1920), somewhat alone amongst the tissues in the rapidity and intensity of *post-mortem* changes. Autolysis is so rapid that examination of fresh specimens taken from the living gland, in the course of an abdominal operation, alone yields reliable results as to the presence or absence of morbid changes, and therefore of the value of the various alleged signs of, and tests for, the existence of pancreatitis.

I venture to renew my plea for the routine examination by the surgeon of the pancreas in all cases operated on for actual or supposed gastric, appendicitic, duodenal, and biliary symptoms, and to suggest certain of the questions awaiting answer:

1. What proportion of cases of pancreatitis is accompanied by cholelithiasis? My results give it as small.
2. What proportion of cases of pancreatitis is accompanied by catarrhal gastric, duodenal, and biliary apparatus affections? My results give it as large.
3. What proportion of cases of pancreatitis is accompanied by excess of dextrines in the urine? Except in cases of advanced pancreatitic cirrhosis, my results give it as negligible.
4. Is excess of dextrine confined to cases in which the islands of Langerhans are affected?
5. Does the pancreas, in addition to its digestive secretion, produce an "internal secretion," and, if so, of what nature and function?

—I am, etc.,

Glasgow, March 6th. JAS. H. NICOLL.

"INTRAVENOUS PROTEIN THERAPY."

SIR,—Dr. A. E. Gow rightly laments the confusion which has arisen in applying such a nomenclature as "protein" or "protein shock" therapy to the various cases under consideration. The latter term was first coined in America, and seems to have stuck more or less. The protein is used in two forms—(1) as bacterial emulsions (to which, indeed, the application of the term "protein" is utterly unscientific), and (2) as cleavage products of the simple protein molecule (proteoses). Each of these substances is used therapeutically in two distinct and opposing ways—namely, (*a*) in minute doses for immunizing purposes, and (*b*) in single (or more) massive doses for the purposes of the pyrogenic reaction ("shock" of American writers). From this it is evident that the expression "protein therapy" may mean anything, and the addition of the word "shock" conveys a false impression. It is therefore necessary to indicate so far as we can, by some nomenclature, which of these methods of treatment is specified.

Dr. Gow apparently seems to think the term "pyrogenic therapy," as applicable to the massive reaction, is defective for two reasons. The first is that certain non-protein substances may produce fever, and the second, that the clinical improvement cannot be ascribed to the pyrexia. I venture to think that these objections may be fairly met. If a non-protein substance can produce the desired reaction it may be just as good for use as a protein. The experiments with non-protein substances have hitherto only been made on animals, but the effects appear to differ in no way from those of proteins. Quite recently, however, the typical reaction seems to have followed the use of hydrogen peroxide in the human subject, as described in the *Lancet* (vol. i, 1920, p. 432) by Drs. Oliver and Murphy. Even if the non-protein substances were not suitable clinically, this does not affect the issue. We use only such pyrogens as are therapeutically appropriate, just as for purposes of purgation (to take an example) we use only certain purgatives, though many poisonous drugs are likewise capable of acting as purgatives.

As regards Dr. Gow's second objection—that the clinical improvement cannot be due to the fever—it is an instructive fact that all other workers at this subject are unanimous that the temperature production is the essential factor. Without the pyrexia no good can be effected. It also may be pointed out that the term "pyrogenic" does not refer to a possibly chance production of the symptom pyrexia, but refers to those substances themselves which act as pyrogens—a name given them by the late Sir J. Burdon-Sanderson. Nor is their capacity to produce fever a secondary or subsidiary one, caused by the liberation and mobilization of toxins in diseased conditions, as the fever is equally produced in the healthy subject, showing the substance to be a pyrogen in the true sense.—I am, etc.,

London, W., March 1st. A. G. AULD.

THE TRANSMISSION OF RELAPSING FEVER.

SIR,—With reference to the remarks by Dr. W. H. Willcox in your issue of February 14th, p. 222-3, on the possibility of there being methods of transmission of relapsing fever other than the louse, I quote some evidence to support his suggestion.

During the years 1906–7, and part of 1908, I carried on an extensive investigation of Indian relapsing fever, which resulted in the discovery of the body louse as the natural transmitter of the disease.

In the earlier part of the inquiry many possible sources of infection were studied, and in particular the blood, to see whether the disease required the interposition of an insect vector, or whether it could be transmitted by simpler methods, such as by contamination with one or other of the excretions.

It was shown in various ways that the blood during the febrile period (and less so during the apyrexial) was very infective to monkeys. Such blood, when brought into contact with the healthy uninjured skin, did not produce the disease, but if a few hairs were pulled out or minute abrasions made on the skin, the application of infective blood invariably resulted in the infection of the experimental animal.

Similarly, if an infected and a healthy monkey were laid side by side and several passages made from the one to the other by pricks with a grooved needle, the healthy monkey became infected on each occasion.

It was found also that when a few cubic centimetres of infected blood were introduced with a soft, well-greased rubber catheter into the oesophagus of a monkey, the disease was regularly transmitted by this means, even though the risk of abrasion of the surfaces was almost certainly excluded.

During the course of these investigations two of my assistants developed relapsing fever in the laboratory after doing autopsies on infected animals. They had no recent cuts or abrasions visible to the eye, so that the spirochaetes must have gained entry by means of microscopic abrasions such as may generally be found around the nails.

A case is on record from another laboratory in which an investigator received a spurt of blood into his eye from a cut vessel whilst operating on an infected animal. Notwithstanding the fact that the conjunctival sac was washed out, he developed relapsing fever after the usual incubation period.

These facts show that the spirochaete has no obligatory cycle of development before it becomes infective after leaving the mammalian host, though this does not disprove the possibility of an alternative cycle of development in its arthropod vector.

When my investigations had reached this stage and when a year's close study of the supposed potentialities of the bed-bug had yielded negative results, a series of cases of relapsing fever were observed to be occurring amongst the staff of a big lying-in hospital in Bombay, which I was directed by Government to investigate. It was shown that these cases were almost certainly being infected by contact with fresh blood. What happened was briefly as follows:

1. There was a small but widespread epidemic of relapsing fever in Bombay at the time.
2. This disease almost invariably produces abortion in pregnant women.
3. Women suffering from abortion and miscarriage were coming to the hospital in a highly infective state to be treated.
4. Spirochaetes could be found in the placental blood.
5. Doctors, students, and nurses who came into contact with fresh blood became infected. Cotton gloves (or none at all) were in use in the theatre.

None of the surgical staff of the big general hospital in the same compound contracted relapsing fever. This may have been due either to the fact that rubber gloves were worn at operations or more probably to the fact that operations would not be undertaken on patients who were suffering from relapsing fever or other pyrexial diseases.

It is true that at this time the real insect transmitter was not known, and it was not until some months later that I had the opportunity of investigating another epidemic with quite different epidemiological features, as the result of which I was able to point to the body louse as the true vector of relapsing fever. On reconsidering my evidence as to blood transmission in the light of these new facts, I was still able to adhere to my opinion that the series of cases in the lying-in hospital were due, not to insect transmission, but to contact with fresh blood.

Dr. Willcox's clinical acumen has led him to the same conclusion in his experience in Mesopotamia. His suggestion as to the transmission of relapsing fever through human faeces is not supported by experimental evidence, nor in my considerable experience by clinical evidence. I have repeatedly tried, but always failed, to infect monkeys by the subcutaneous injection of faeces, urine, and vomit taken from patients at the height of their infection. Microscopical evidence on this point would be valueless, as all these excretions contain spirochaetes under natural conditions. Experiments on monkeys, however, provide a very delicate test, as not one of my series of over a hundred monkeys failed to become infected even when the dose of virus was minute.

The practical point is that though the ordinary carrier of relapsing fever in Europe, Asia, and Northern Africa is assuredly the louse, a more direct transmission from person to person does take place under exceptional circumstances—namely, by the contact with, and subsequent absorption of the virus from, freshly shed infective blood.

Whilst on this subject I venture to refer to the question of priority of the discovery of the louse as the carrier of relapsing fever. This should fairly be placed to my credit (vide "The part played by *Pediculus corporis* in the transmission of relapsing fever," BRITISH MEDICAL JOURNAL, December 11th, 1907). This reference appears to have been overlooked by some writers on the subject, whilst some have referred to it merely as a "suggestion." The observations made in that paper were confirmed by Sergent, Graham Smith, and others, but particularly by Nicolle and his associates, who extended and amplified the original observations. The latter also corrected me as to the exact method of transmission by the insect, showing that it was brought about by the crushing of the body of the infected louse into an abraded skin surface rather than by the insect's bite. Other observations of mine showed that the buccal secretions of some infected lice swarm with spirochaetes, and the significance of this has yet to be explained.

It should be remembered that the transmission of relapsing fever was known before the louse hypothesis was applied to the transmission of typhus fever, and the close epidemiological similarity between the two diseases suggested their being carried by the same agency.—I am, etc.,

F. P. MACKIE,
Major, I.M.S.

Bristol, Feb. 29th.

THE TREATMENT OF MALARIA.

SIR,—In the expressions of opinion in your columns on the treatment of malarial fever in the tropics there seems to be little variance, but what concerns the general practitioner is the anti-relapse treatment in this country where reinfection is unlikely.

My experience of the treatment of malaria in tropical Africa from 1900 until recently is similar to that of Drs. Taylor, Law, and Collett, and, like the last-named, I have seldom had to resort to other than the oral method of administration. Cases do occur, but they are distinctly rare.

Since demobilization I have treated many hundreds of ex-soldiers suffering from recurrent malaria in this country. In the majority there has been little or no clinical evidence of the disease, but at the same time a considerable number show some splenic enlargement. The improvement in practically every genuine case is so marked that there can be no two opinions about the efficacy of the routine treatment which has been adopted by several of us working alongside each other. The treatment is simple. Every genuine case is given 10 grains of quinine sulphate in solution daily before breakfast; this is continued for a month. The patient is then seen again, when the same daily dose is repeated for another month or increased to 15 grains should the case require it. The treatment is completed at the end of three months should no relapse have occurred during that time. The patient is then given a tonic for as long as he requires it. Under this treatment I have only met with one case in which the spleen did not return to normal within three months. In the large majority of those who take their daily dose of quinine regularly no relapse occurs, but it has been found that in a small minority of the regular quinine takers slight relapses will occur, but that these will soon cease if the treatment is persevered in.—I am, etc.,

R. E. DRAKE-BROCKMAN, M.R.C.S.

London, S.W., Feb. 26th.

SIR,—As I find that the treatment of malaria by *intramuscular* injections of quinine salts is still recommended for certain classes of cases by some authorities, I wish to point out that this method was condemned by the medical service of the E.A.E.F. ("German" East Africa).

The intramuscular injection of quinine causes extensive necrosis of muscle, and its therapeutic value is negligible when compared with the other methods of administration, oral, intravenous, or subcutaneous.—I am, etc.,

AUGUSTUS R. BALMAIN, M.B., B.S.Lond.,
Captain R.A.M.C.(S.R.), late of E.A.E.F.

London, S.W., Feb. 28th.

MEASUREMENT OF EMOTION.

SIR,—The experiments of Professor A. D. Waller on the above subject (February 21st, p. 259) are very interesting to me as corroborative to a very small extent of the epoch making work of my friend Dr. Albert Abrams of San Francisco.

It is quite eight or ten years since Abrams showed that the electric discharge from the human body chiefly occurs at the tips of the fingers and toes; the discharge is greater in the light than in the dark, and, contrary to generally accepted opinions, greater in dry than in damp weather. As an absolutely dry atmosphere is a more or less perfect insulator, he legitimately concludes that human electric potential is high. He has also shown that the polarity in the normal male—I say normal because there are a good many asexual individuals about—is positive in the right hand and foot and negative in the left; in the normal female during the child-bearing age the polarity is