

his paper recommended psychoanalysis as a mode of treatment for this malady. Having had some psychoanalytic experience of paraphreniacs, I wish to utter a warning that the resistances, which are already very strong in these patients, become increased on psychoanalysis, that the ultimate result is unfavourable, and, therefore, that this mode of treatment is not to be recommended or attempted for such patients.

I am, Sir, yours faithfully,

W. H. B. STODDART.

Cavendish-square, W., March 23rd, 1920.

THE CULTIVATION OF THE MENINGOCOCCUS.

To the Editor of THE LANCET.

SIR,—Dr. H. Stanley Banks's article in THE LANCET of March 13th on the Serum Treatment of Cerebrospinal Fever is of considerable interest. The cultivation of the meningococcus is, however, not always such a simple process as Dr. Banks suggests. "Coagulated blood serum" is, of course, a very suitable culture-medium, but in many instances gives no growth until incubated for 48 hours, and occasionally fails altogether even when the usual precautions, as mentioned by Dr. Banks, are taken. Hence it is a frequent occurrence that one is unable to report on the type of organism for two-three days after first seeing the patient. Obviously this is a serious drawback if the clinician wishes to administer the homologous serum at once.

My experience *re* agglutination is that the macroscopic is the most reliable, and that Gordon's type sera are useless for the microscopic method. Theoretically, one should administer the homologous serum in any given case, but in practice I am not at all satisfied that it is a matter of great moment. I have examined many of the Lister sera for agglutinins to Gordon's four types of meningococci, and have found that each type serum agglutinates each type of coccus up to a varying titre—i.e., type 1 serum agglutinates not only type 1 coccus but also types 2, 3, and 4—similarly with type 2 serum, and so on. Type 4 coccus is extremely rare, so I always administer a mixed serum of type 1 and type 2. As type 3 coccus is so closely related to type 1 coccus, this mixed serum does well in type 3 infections. Before using this mixed serum one naturally must demonstrate to oneself that agglutinins for all three types are present.

I am, Sir, yours faithfully,

Wallasey, March 17th, 1920.

WILLIAM A. MUIR.

ANÆSTHETICS AND DIABETES MELLITUS.

To the Editor of THE LANCET.

SIR,—As a student I was taught that there was considerable risk in administering chloroform to patients suffering from diabetes mellitus. Fatal coma developed frequently within a day or two of the anæsthesia. This teaching was the result of experience and correct as far as it went, but since that time much has been added to our knowledge of diabetes mellitus. We have known for some time that when suitable precautions are taken the risk of post-operative coma is comparatively small. We know, too—but perhaps it is not known to all, hence the object of this letter—that the administration of any general anæsthetic, such as chloroform, ether, and nitrous oxide with oxygen, may lead to a comparatively slight case of diabetes mellitus developing into one of great severity.

It is only during the last five years that it has been possible to determine the gravity of any case of diabetes mellitus, because the mere percentage of sugar in the urine or the total quantity of dextrose excreted daily is no gauge to the extent of the alteration in the metabolism. Evidence seems to be accumulating that diabetes mellitus is due to a change in the interacinar cells in the pancreas; that the severity of the disease depends upon the number of cells destroyed; that the condition which has destroyed some cells has diminished the vitality of others, and these may be killed by various toxins brought to them in the blood, amongst them chloroform, ether, and nitrous oxide, and in all probability those elaborated by micro-organisms such as cause scarlet fever, pneumonia, measles, influenza, &c.

The observations upon which this conclusion is based are of the following type: A man suffering from diabetes mellitus, who excretes diacetic acid and β -oxybutyric acid along with a considerable quantity of sugar whilst on a diet extremely poor in carbohydrates, but rich in protein and fat, may be found to lose his glycosuria and all evidence of acidosis on a diet, say, of 50 g. carbohydrate, 60 g. protein, 180 g. fat. This diet possesses sufficient energy to allow the individual to maintain his weight and do as much work as is necessary to earn a living by manual labour. This man has an accident and breaks his arm. An anæsthetic is administered to permit the arm being placed in the best position. After the anæsthetic sugar is found in the urine, and persists. Upon reinvestigating the maximum diet which will control glycosuria it is found that it has fallen to 20 g. carbohydrate, 60 g. protein, and 100 g. fat. Often the deterioration in the metabolism is much greater than this. The patient can no longer remain on a diet which allows his urine to be free from sugar and permits him to retain his weight and live a useful life. He is, therefore, forced to disregard his disease, eat more or less what he likes, and he develops coma at a comparatively early date. The unnecessary anæsthetic may have shortened his life by years.

I quite realise that the presence or absence of sugar in the urine is not the best method of determining the optimum diet, but one which permits the majority of medical men making an attempt to do their best for the patient. In my opinion at the present day students should be taught that only operations that are absolutely essential should be performed upon diabetics; that spinal anæsthesia and local anæsthesia should be adopted when possible; that nitrous oxide with oxygen is less likely to damage the patient than ether, and ether is less likely to do harm than chloroform.

I am, Sir, yours faithfully,

Portland-place, W., March 19th, 1920.

O. LEYTON.

AFTER-EFFECTS OF MALARIA.

To the Editor of THE LANCET.

SIR,—I have been very much struck during the last 18 months while examining soldiers on their discharge, and after their discharge, when claiming for pensions, by the abnormal number of cases of thickened arteries occurring in men between the ages of 20 and 28, who have suffered from malaria during some period of their service. Many of these cases apparently have only been of mild type, some have been undergoing quinine treatment for prolonged periods, others have not.

I should be very glad to hear if this experience has been shared by other members of medical boards especially.

I may say that an examination of urine in these cases has been a routine, and the condition of arteries apparently exists in subjects which show no signs of, and in whose history there is no evidence of, albuminuria.

I am, Sir, yours faithfully,

A. HURRELL STYLE, M.D.

Ministry of Pensions, Headquarters, Suffolk Area,
Church House, Bolton-lane, Ipswich, March 17th, 1920.

THE GENESIS OF TWINS.

To the Editor of THE LANCET.

SIR,—In a recent number of *Genetics*¹ I have presented statistical evidence which shows, I believe conclusively, that it is far more probable that twins form a homogeneous group of uniform origin than that they are of two distinct types—"fraternal" and "identical." Dr. Thorndike's data are, I believe, the only extensive body of measurements of twins, and from his data it appears that (1) the variation in resemblance agrees closely with the variations due to sampling on the hypothesis of uniform origin, and is materially less than it would be for dual origin; (2) twins most alike in any one trait are not more alike than other twins in other traits.

In view of these facts the genesis of twins becomes a new problem, and I have ventured to put forward the

¹ Princeton University Press, September, 1919.