found in a progressive variation of the causative virus, itself determined by changes in its environment; and a tentative hypothesis was advanced with regard to the causes and character of such a variation.

In considering some of the possible deductions from such a theory, it was pointed out that—

"It might, perhaps, be supposed that the acceptance of such a view as this would involve the belief that any bacterial parasite, given the requisite environmental conditions, might give rise to epidemic disease. Neither clinical nor bacteriological experience would, however, lend any support to such a view. It would seem that we are dealing with a more or less specific capacity for variation, analogous to the acquired capacity for bringing about certain fermentations, which has been studied by Penfold and others. As the result of certain environmental changes the bacterial parasite will vary in a particular direction; but the ability to react in this way must be regarded as something inherent in the organism itself."

Thus an evolutionary hypothesis, in which especial emphasis is laid on changes in environment, does not exclude a belief in specificity, provided that this doctrine be not applied with an unintelligent rigidity. There is little evidence that we have fallen into error in believing that a given "disease" is caused by a specific organism, or sometimes by one or more of a definite group of organisms, and that in their absence the "disease" cannot occur. There seems reason, however, for thinking that we have not attributed sufficient importance to differences between various "strains" or "races" of a given bacterial species, or to the preponderating influence of environment on the evolution of those strains of the parasitic species which produce infection of the host. The future of preventive medicine would seem to be closely bound up with the acquirement of a fuller knowledge of the factors concerned in the production of such variations, and of the nature of the differences produced. An excellent example of the type of inquiry which will yield the data on which we must build is afforded by the admirable studies of Dr. A. Stanley Griffith on the types of *B. tuberculosis* isolated from various lesions in man.

The benefit that would most certainly accrue from the investigation of pathological problems on a wider and more comparative basis than is at present practised was pointed out by Sir Clifford Allbutt some months ago in a letter to the *Times*. If research and teaching could be established on the lines he indicates, there is little doubt that a decisive advance would be made along the path that will one day lead us to the conquest of infective disease.—I am, Sir, yours faithfully,

W. W. C. TOPLEY.

Institute of Pathology, Charing Cross Hospital, May 15th, 1920.

ANEURYSMAL VARIX IN THE NECK.

To the Editor of THE LANCET.

SIR,—A woman, aged 71, had a fall some years ago while cleaning a window. The frame broke and she fell backwards with frame and window on her. Nothing was noticed at the time in the neck, but she had glass removed from the face. Recently she developed an aneurysmal saccular swelling about the middle of the right transverse cervical artery, causing much pain and throbbing. The artery was tied by me near its origin on March 23rd last and the existence of a large expanded vena comes noted, which filled up with each beat of the artery. This sac has quite disappeared, but a fortnight ago aneurysmal swelling commenced in the inferior thyroid artery of the same side close to the clavicle and behind the sterno-mastoid muscle. As this aneurysm is so close to the origin of the artery to its parent trunk it would appear that further operation is impossible, especially in view of the patient's age. My friend, Mr. W. R. Williams, and I came to the conclusion that palliatives were now the only resource. The first part of the subclavian has, I believe, never been tied. If it were, what degree of collateral circulation would there be? I am writing this as I believe aneurysm of the thyroid axis and its branches are not common and aneurysmal varix very rare.

I am, Sir, yours faithfully,

E. C. B. IBOTSON.

May 17th, 1920.

PROFESSIONAL SECRECY AND VENEREAL CLINICS.

To the Editor of THE LANCET.

SIR,—As it must be most distasteful to the medical officers of venereal disease clinics to be compelled in the courts to reveal their patients' conditions, would not a simple procedure avoid the possibility of this? The name of the patient is of no consequence to the medical officer. I should suggest that the registrar, or clerk, or nurse at the clinic receive directions to give to each patient a number, which would be written on their attendance cards previous to their seeing the medical officer; which number would on no account be changed during the whole course of treatment. Then the question of the name of the patient need not arise in the clinic at all and the position of both medical officer and patient would be more satisfactory; and there would be less hesitancy on the patient's part of availing himself or herself of the help held out by the Ministry of Health.

I am, Sir, yours faithfully,

Dover, May 15th, 1920.

F. A. OSBORN.

* * The plan has been suggested by others. The course of public and individual justice may make it necessary that the venereal state of a particular person at a particular time should be medically certified.—ED. L.

THE CULTIVATION OF THE MENINGOCOCCUS.

To the Editor of THE LANCET.

SIR,—In THE LANCET of March 27th Dr. William A. Muir, in a letter bearing this title, commented upon my article on the Univalent Serum Treatment of Cerebro-spinal Fever which appeared in your issue of March 13th. Since I do not have the opportunity of seeing many cases of cerebro-spinal fever at present, I determined that I would not reply until I had treated at least another case. I have now done so. It confirms in all essential particulars the points made in my article.

Regarding the cultivation of the meningococcus, I have again been able to obtain a sufficient growth of the meningococcus on ordinary "coagulated blood serum" slopes within 20 hours of inoculation of the tubes. This was done both during the primary attack and during a relapse which occurred about three weeks after the primary attack had subsided. The growth was in each case sufficient for the purpose of carrying out the test for type by the microscopic method. It may be well to state that in each case about 1 c.cm. of cerebro-spinal fluid was allowed to run directly from the lumbar-puncture needle into the warmed culture-tubes, and the tubes were incubated within ten minutes. In control-tubes inoculated with only a few loopfuls of the fluid no growth occurred. In connexion with the relapse above quoted, direct microscopic examination of the fluid showed numerous pus cells, but no organisms, and yet, in culture, about a dozen colonies were available the next morning (20 hours).

Regarding agglutination, I again found the microscopic method to be sufficient to establish the diagnosis of Type I. of the disease. The only difference from my previous case was that the positive result was rather longer in appearing (six hours), and was very marked indeed in 24 hours. Comparatively, any tendency to agglutination which occurred with the other sera (and that affected Type II. only) was absolutely negligible. Any ordinary observer would have had no difficulty in pointing to Type I. as the reacting serum. I have no doubt that the macroscopic method is more accurate and more reliable. But here it is not necessary to determine the end-point of agglutination capacity of a serum, but merely to determine, given four high-titre sera, which of them is the chief or only reacting one. If low dilutions are put up, this, in my experience, can be done; and the test is an easier and more familiar one to most workers in the small laboratories attached to fever hospitals.

Regarding the administration of the homologous serum I have not found the delay in diagnosing the type of the disease to be sufficient to put the method out of use. In practice I have found Type I. to be the common type occurring in this area, and each case I have seen has presented almost identical and distinguishing clinical features. In such circumstances I have since my first case given the homologous serum first and diagnosed the type afterwards (within 24 to 48 hours—not a serious delay in this type of case). The response to the homologous serum treatment has been nothing short of marvellous. I have, of course, no