

be intensified. To the former action the author gave the name *piantication* and to the latter *anapiantication*. The experiments on which the above statements are based were described in the *Medical Journal of South Africa*, 1914, and in the publications of the South African Institute for Medical Research, 1915 and 1917. They were carried out first by the writer alone and later in conjunction with Sir F. S. Lister. Similar results were obtained with several species of bacteria.

To be able readily to alter the virulence of a bacterium outside the body without interfering with its capacity to grow on media may be of assistance in the production of antisera, in the prophylactic inoculation of individuals against certain diseases, and in the therapeutic inoculation of those already infected. An illustration may make clear what is suggested under the first and second heads. The cells of the body of an animal which has been inoculated may be likened to a company of men who are being trained to jump over hurdles. If the full height is presented straight away to the men many will be unable to clear the obstacles, and will injure themselves in trying to do so. If, however, the men are trained by being required on the first day to jump over a low obstacle and the height of this is increased on succeeding days, most if not all of them will soon succeed in clearing the hurdles without difficulty or injury. Similarly, if a highly virulent organism is injected straight away into an animal many cells may be injured. A gradation in the virulence of a vaccine in addition to an alteration in the quantity injected is desirable. The procedure known as *piantication* affords a ready means of diminishing the virulence.

With regard to the third point: when vaccines are injected for therapeutic purposes it is to be recollected that the organism infecting the patient is living and is capable of adjusting itself to the increasing immune properties of the patient's blood resulting from inoculation. It may be supposed that as a result of stimulation of the bacterium in the tissues an undesirably strong focal reaction of the patient's tissues might be induced. It might therefore be of distinct advantage to begin treatment with a vaccine whose most pronounced quality was not its virulence. The patient might be aided by stimulating some other quality such as his power to produce antitoxin in order to overcome the disease from which he is suffering. This may perhaps explain the beneficial results which have followed the use of sensitised vaccines.

I am, Sir, yours faithfully,

Devonshire-place, W., Jan. 20th, 1922. A. R. FRIEL.

DIRECT STIMULATION OF LEUCOPOIESIS IN INFLUENZA.

To the Editor of THE LANCET.

SIR,—The valuable paper in your columns by Dr. J. G. Willmore and Dr. F. M. Gardner-Medwin (Jan. 21st) on this subject is of such manifold interest that I should be glad if you would insert a few remarks by one whose main line of work for some 18 months has been the bone marrow. Injections of nucleinate of soda certainly produce an increase in the number of leucocytes in the circulation, but I know of no evidence that they are direct stimuli to the production of leucocytes in the marrow. Muir¹ and also Andrewes² have shown that during the rapid establishment of an infection there is no evidence that the leucocytosis is due to increased formation in the marrow but rather to the addition of a number of ripe cells to the blood. The bone marrow always carries a large stock of neutrophile polymorphonuclear leucocytes, and in most infections these come out in response to a "chemiotactic whistle," but in influenza they remain in the marrow. Is this latter phenomenon due to the absence of a "chemiotactic whistle" or is it due to a poisoning of the cells in the marrow?

¹ Muir, R.: Jour. of Path. and Bact., 1901, vii., 161.

² Andrewes, F. W.: THE LANCET, 1910, i., 1737, and ii., 8, 33, and 153.

Willmore and Gardner-Medwin seem to have shown that it is due to the absence of the chemiotactic call, as the cells are quite capable of emigration in response to another stimulus. It will be recalled that the establishment of a neutrophile leucoblastic reaction in the marrow requires at least 40 hours from the onset of the infection, and that therefore the injection of nucleinate of soda must be calling forth cells already present in the marrow. Examination of the marrow of cases succumbing to influenza does not demonstrate any definite depletion or exhaustion of the marrow—that is to say, one is not dealing with an acute aplasia of the marrow. I would suggest that this method of treatment does not depend upon direct stimulation of leucopoiesis, but upon the establishment of a leucocytosis at the expense of cells already ripe in the bone marrow.

Cinnamic acid is another of the substances capable of calling forth a leucocytosis, and J. C. Ross,³ who used cinnamon oil in the treatment of influenza, employed this fact successfully, even if unwittingly.

I am, Sir, yours faithfully,

ALFRED PINEX, M.R.C.P. Lond.

Pathological Department, The University,
Birmingham, Jan. 24th, 1922.

CHLOROPHYLL AND VITAMIN A.

To the Editor of THE LANCET.

SIR,—No answer having so far been forthcoming to Dr. E. Biddle's question in your issue of Jan. 21st (p. 154), as to whether it may be assumed that the vitamins as well as the chlorophyll are concentrated in "phyllosan," I venture to make a few observations, both particular and general.

First and foremost, it seems to me that although it is easy to conceive the concentration of chlorophyll, it is by no means obvious how we can theorise concerning the physical states or qualities (if any) of a substance so elusive and imponderable as a vitamin.

Another point raised by THE LANCET editorially is why concentrated chlorophyll should be required when abundant supplies of that material can be obtained cheaply from greenstuff such as spinach. In answer one may ask, Why use quinine instead of giving large doses of bark? Why use tinctures, or juices, or other preparations from seeds, roots, or leaves when one might administer instead corresponding parts of the untreated plant?

As to the actual presence of vitamin A in chlorophyll extracted by Prof. Emil Bürgi's process we have his high authority on that point. At the same time one may venture to hope that he will be induced to publish in THE LANCET a full statement of the facts of the case. The analogy between chlorophyll and hæmoglobin and the apparently established value of phyllosan in anæmia offer a fine stimulus to the scientific imagination, and it is to be hoped that the whole matter will be thoroughly sifted in the interests of medical science.

I am, Sir, yours faithfully,

DAVID WALSH, M.D.

Bentinck-street, W., Jan. 30th, 1922.

INFECTION OF UTERINE FIBROIDS.

To the Editor of THE LANCET.

SIR,—It is allowed that uterine fibroids may undergo degeneration without infection, the change causing little or no symptoms. It does not follow that a degenerative change, because acute and causing symptoms, is due to infection. Mr. W. B. Gabriel and Dr. A. N. Kingsbury, in your issue of Jan. 28th, suggest from the degenerating and infected (sub-peritoneal) fibroid they describe, that the red degeneration of fibroids is due to infection. The change in the latter state—they argue—cannot be due to a primary thrombosis—because thrombosis is often not demonstrable: it is unlikely to be due to an aseptic necrosis,

³ Ross, J. C.: THE LANCET, 1906, ii., 1240.