Correspondence.

"Audi alteram partem."

FIXATION ABSCESS.

To the Editor of THE LANCET.

SIR,—In an article on this subject in THE LANCET of Jan. 7th Major A. T. Todd suggests that there are probably factors, other than an increase in the formation of leucocytes, involved in the beneficial effect obtained from this mode of treatment. Certain considerations, although largely inferential in character, may be put forward in this connexion.

It is known that the polymorphonuclear leucocytes contain a proteolytic enzyme which may be readily extracted from them, and which is presumably set free into the surrounding medium whenever an accumulation, associated with a destruction, of leucocytes takes place in an area of the tissues. This enzyme is of the nature of a tryptase (i.e., it is most efficient in a slightly alkaline medium), and its action is inhibited or restrained by the antitryptic capacity of the blood. The accumulation and destruction of leucocytes in a focus of infection will lead to a liberation of the enzyme into the surrounding tissue-fluids and, should a concentration be reached adequate to overcome the inhibitory power of these fluids, to a certain degree of proteolysis. This is seen in its complete form in suppuration, but it is conceivable, and indeed probable, that in foci which do not suppurate, and also in the tissue round the fluid portion of a suppurating lesion, a certain amount of proteolysis occurs, and protein-degradation products are formed. These latter may pass into the more peripheral parts of the lesion, where the accumulation of leucocytes is not sufficiently great to cause active digestion. In the large majority of infective processes the antitryptic power of the blood is increased, and this increase will lead to a greater restriction in the amount of proteolysis occurring in the focus of infection. The extent of the inhibitory effect will, however, be dependent, not only upon the antitryptic power of the blood, but also upon the conditions prevailing in the lesion; it will be reduced practically to nothing in any part of this which has become completely cut off from the circulating blood, but will exert its full effect in the peripheral and more important part of the infected area. Here the degree of proteolysis and the concentration of protein-degradation products will be a function of the antitryptic capacity of the blood.

Now, it is a matter of common experience in bacteriology that bacteria prefer to obtain their nitrogenous pabulum from the proteoses and peptones rather than from undigested proteins, and indeed that, in the case of many pathogenic and potentially pathogenic micro-organisms, the former are, in vitro, essential to cultivation. It will not appear unreasonable to infer from this that the presence of the products of proteolysis will probably facilitate the multiplication of bacteria in vivo; and that, although the accumulation of leucocytes in a focus of infection is, on the whole, a beneficial and protective process, it may in this respect be detrimental to the host. The detrimental effect will, however, be partially restrained by the antitryptic power of the blood, and will become less detrimental as this inhibitory power is increased. If all this be so, it will be evident that anything which leads to a sudden increase in the antitryptic power of the blood will lead also to a sudden diminution in the supply of the more favourable pabulum of the infecting microbes. We cannot, of course, conceive this as being able to inhibit the further growth of the bacteria, but, assuming the issue of the combat to be trembling in the balance, we may conceive it as being able to turn this issue in favour of the host.

I have no actual knowledge as to whether a fixation abscess would cause an increase in the antitryptic

power of the blood, but having paid a certain amount of attention to the changes in this function under various conditions, I think that it would most probably do so, since such an increase is the rule whenever a localised destruction of tissue has occurred, whether through bacterial infection, through injury, or through the inoculation of substances giving rise to a local reaction. Most of the examples given by Major Todd are cases of pneumonia, and in this condition the antitryptic power is, in my experience, always markedly augmented, but it is probable that the supervention of a lesion such as he describes would lead to a still greater enhancement, and so produce on the infection the effect that I have suggested above.

The question as to whether the antitryptic factor has any effect also on the enzymes of the bacteria themselves is at present undecided, but Sir Almroth Wright has called attention to, and demonstrated experimentally, the great importance of this factor in the progress of infected wounds; and it should undoubtedly be regarded as a potential, and perhaps as a considerable, element in the antibacterial armament of the animal organism.

I am, Sir, yours faithfully,

E. BEATON.

Public Health Laboratories, Cairo, Feb. 10th, 1922.

AMAAS OR ALASTRIM?

To the Editor of THE LANCET.

SIR,—Dr. J. Fehrson, of Benoni, in his letter appearing in THE LANCET of Feb. 25th, claims that the term *alastrim* should not be employed to designate the form of small-pox hitherto known as *amaas*. Unless those who employ the former term can justify it by asserting that it identifies a recently discovered type, there certainly seems no object to be gained by its use. That this is not merely a matter of priority in nomenclature as employed by Drs. de Korte and Ribas, is evident from the fact that in your correspondence columns some time back there appeared a letter from a well-known medical officer of health, pointing out that the task of differentiation of small-pox, in its minor aspects, from vesicular diseases simulating it, was now further complicated by the necessity for diagnosis between *amaas* and *alastrim*. Drs. Aldo Castellani and A. Chalmers, in their book "Manual of Tropical Medicine," p. 1491, quote seven synonyms (including *amaas*) for *alastrim*. Dr. Bruce Low, in his report, "The Incidence of Small-pox Throughout the World" (1918), asserts that "strains of small-pox infection current in different parts of the world vary in their degree of virulence"; and draws attention to a "mild clinical type" found in North America, New Zealand, and Australia. Dr. J. C. McVail, in his work "Half a Century of Small-pox and Vaccination," insists on the importance of taking into account the type of small-pox in judging the influence of measures to combat epidemics; and holds that, in 1902-05, whilst in London the virulent "North African type" was dealt with, in the provinces the mild "American type" prevailed.

The origin and nature of types thus variously indicated seemed to me of interest; and relying largely upon the valuable statistics at disposal in Dr. Bruce Low's report, I compiled a map illustrating the incidence of current types and strains, and read a paper on the subject before the Royal Society of Tropical Medicine and Hygiene on Feb. 20th, 1920. I found whilst it was unnecessary to regard North Africa as the place of origin of a special type of small-pox, Dr. McVail had rightly called attention to its virulence, and I concluded that the term "American type" connoted *amaas*; seeing that the connexion of America with Africa and small-pox derived from that country is historically correct, and was insisted upon by Hirsch. America, therefore, may safely be given the benefit of the doubt of being the birthplace of its "mild type"; Dr. Fehrson has a better right to claim the onus for South Africa, and consequently the retention of its correct terminology.

Irrespective of the obvious advantage of not mixing terms where diagnosis is at stake, the practical point with which the practitioner in this country, as well as in America, is concerned is the distinguishing of amas from varicella. This necessity is especially the case when it is remembered (as contended by McVail in regard to the epidemic of 1902-05, and as has been frequently reported in America) amas may prevail side by side with a virulent type. The fact that amas refuses to wax materially in virulence whilst readily recognisable small-pox has this tendency, leads to the shaking of belief in its being in reality a type of small-pox.

Yet, Dr. W. G. Armstrong (Report of the Director-General of Public Health, New South Wales, 1913; published 1915) has placed on record the results of experiments conducted with the aid of Drs. Burton Cleland and E. W. Fergusson, which leave no doubt of the identity of amas with small-pox. Perhaps, when we understand under what conditions variola virus is deprived of infectivity as vaccine by passage through bovines, we shall also admit that a virus capable of thus behaving may, under other unknown conditions, lose its virulence for the internal organs whilst still exhibiting its influence on the skin.

I am, Sir, yours faithfully,

W. G. KING,
Colonel, I.M.S. (ret.)

Hendon, N.W., Feb. 25th, 1922.

* * A recent article on alastrim in the U.S. Public Health Reports, written by Dr. W. C. Rucker, chief quarantine officer on the Panama Canal, gives the following list of synonyms: Varioloid-varicella, amas, Kaffir milk-pox, Sanaga small-pox, West Indian modified small-pox, pseudo-small-pox, Weisse Pocken; and a note by the Editor of the Reports is worth quoting in full:—

"Leake and Force, of the United States Public Health Service, in their studies on the immunological relationship of alastrim (1921), inoculated monkeys and rabbits, using crusts and pustule contents from alastrim patients in Jamaica and Haiti. They summarised the results of their experiments as follows:—

"A vesico-papular eruption was produced in monkeys by inoculation both with crust and with vesicle contents from alastrim patients. The animals were protected against reinoculation with alastrim and vaccine virus. Rabbits inoculated with alastrim showed no eruption, but were almost completely immune to vaccine virus. Rabbits previously inoculated with vaccine virus gave positive intracutaneous reactions to small-pox crusts, alastrim material, and vaccine virus, but remained negative to chicken-pox crusts.

"The fact that definite immunity to vaccinia is produced by previous inoculations with alastrim is additional evidence of the essential identity of alastrim with small-pox. (Reprint No. 669 from the Public Health Reports, June 24th, 1921, pp. 1437-1443.)"

THE DEPARTMENTAL COMMITTEE ON THE CAUSES AND PREVENTION OF BLINDNESS.

To the Editor of THE LANCET.

SIR,—This Committee, which started investigations in the autumn of 1920, last year issued an interim report on the effect of lighting in the cinema studios upon the eyes of the artists. The next subject which they investigated was ophthalmia neonatorum. I believe that the material part of this report has been completed for some time, but not published, and has been held over until other injuries and diseases of the eye have been dealt with. It is a little difficult to see why a comparatively unimportant part of the report, such as lighting in cinema studios, should have been published as an interim report, whilst the most important and urgent subject of it has been held over for departmental reasons. In the meantime numbers of children are being allowed to go blind from ophthalmia neonatorum for want of the necessary precautions to ensure prevention, early advice, and proper treatment of this disease. If it is from motives of economy that the part of the report on this subject

is not issued, the cost of the care to the State of one blind child would more than repay for its publication.

It is to be hoped that the Ministry of Health will reconsider their decision and issue interim reports as the subjects are dealt with by the Committee.

I am, Sir, yours faithfully,

Harley-street, W., March 6th, 1922. M. S. MAYOU.

A DIPLOMA IN TUBERCULOSIS.

To the Editor of THE LANCET.

SIR,—Sir Robert Philip's letter to THE LANCET of March 4th is very much to the point; the "multiplication of Special Diplomas in Limited Departments of Medicine" is hardly advisable, while an additional certificate to the Diploma of Public Health might meet the case. This specialising in tuberculosis leads only a short way, numerous posts are offered for men who have had a minimum of six months experience in tubercle, whereas there is nothing offered for men who have had 15 years or more in this special work. The salaries offered are small, and in very few cases mention any increase. If the long experienced man dares to apply for such a post, he is not considered owing to his age, or to the fact that he has "encumbrances." Men accepting junior posts should bear in mind the limited number of senior posts in tuberculosis. Many tuberculosis medical officers have had to give up this work, go into private practice, or general public health. These facts show that it is hardly worth while to work for a special diploma in tuberculosis.

I am, Sir, yours faithfully,

G. BASIL D. ADAMS, M.D., D.P.H. Oxf.

Farm Colony, Kinson, March 6th, 1922.

To the Editor of THE LANCET.

SIR,—With reference to your annotation in THE LANCET of Feb. 25th I would call your attention to the advanced courses on tuberculosis which were instituted by the University of Manchester in 1912, in connexion with the course for the Diploma in Public Health. The course, which extended over six months, included laboratory practice, supplemented by demonstrations and lectures at the public health laboratory; instruction in administration at health offices recognised by the University; and clinical instruction at tuberculosis dispensaries, hospitals, and sanatoria recognised by the University. The main if not the only difference in principle between this post-graduate course and that outlined by the Welsh National School of Medicine is that six months' part-time clinical work is required for the Manchester certificate instead of three months; moreover, all medical men are eligible to take a course immediately after qualification should they so desire.

Owing to the war, these courses have been interrupted, but the certificate has been obtained by some 16 men, the majority of whom also hold the D.P.H. of this University—I am, Sir, yours faithfully,

E. J. SIDEBOTHAM,
Acting Director.

Public Health Laboratory, Manchester, March 1st, 1922.

MENTAL AFTER-CARE.

To the Editor of THE LANCET.

SIR,—The report of the annual meeting of the Mental After Care Association, which appears in your current issue, calls attention to the fact that 7206 persons were discharged last year from our public asylums as wholly recovered, and 3276 in whom recovery was partial. The work of the Association enables many of these ex-mental patients to resume their normal life and activities. The Association needs at the present time all the help that it can get, and while coöperation is welcomed from whatever direction I cannot find in the subscription list that any one member of the National Council for Lunacy Reform has contributed to the funds. A body that assumes to take a leading part in matters relating to mental diseases must surely know of the work that