

Correspondence.

"Audi alteram partem."

THE PENSIONS PROBLEM.

To the Editor of THE LANCET.

SIR,—In connexion with the assessment of the pensions to be allotted to partially disabled officers and men, the power of prognosis of the medical profession is in many cases being submitted to a test that it cannot sustain. It is possible to estimate with some measure of accuracy the degree of future disability of a man who after an injury has had a limb or portion of a limb amputated. For these and some other similar cases a partial pension can be more or less fairly assessed. It is quite impossible to forecast with any approach to certainty the future average employability and proportionate earning capacity of a man who has sustained a gunshot wound of a viscus or has suffered from tuberculous or malarial infection. For these and very many similar cases no fair individual assessment can be made. Such cases could be put in groups according to their history and present condition, and a rough calculation made as to the average incidence of future disability amongst all the members in each group; just in the manner in which impaired lives seeking life insurance are dealt with. Probably something of this sort is being done now in allotting partial pensions. But what will be the result? Those with partial pensions who do not subsequently break down will be heard of no more, but the minority who, according to the law of averages, will break down badly will find their small pensions altogether inadequate, their hardships will quite rightly come under public notice, and the profession will be blamed for not having made for them adequate provision.

It would be well, therefore, for the profession at once to make clear to the public that it is quite impossible for science to provide the data on which a fair pension can be assessed in the case of large numbers of partially disabled men. Since, however, the number of such men—some not yet demobilised, others demobilised but still on temporary pensions—is very great, some provision must be made to meet their special risk of disability. For a considerable number of cases the most suitable way of meeting the risk would be the issue of a sickness insurance policy guaranteeing in the event of a breakdown in the future the periodical payment by the Government of a sum to meet the then ascertained degree of disability so far as it is not met by any other national insurance. In some cases such a policy would take the place of a partial pension, in others it would be additional to it. The task of dealing with many cases, which now present an insoluble problem, would at once become simple. It would only be necessary at the outset to earmark certain cases as having been rendered by injury or disease resulting from war service specially liable to relapses of disability, and later on to determine the fact of disability should it occur. The Ministry of Pensions must naturally look to the medical profession for advice as to fair and proper ways of dealing with all the various types of cases amongst those whose health has suffered from war service. Those medical men who are in a position to offer such advice should lose no time in pointing out that there is a large proportion of cases whose special claim upon the nation cannot be met by the old-fashioned method of a pension assessed according to the demonstrable degree of disability, but can be quite satisfactorily dealt with by a well-considered scheme of sickness insurance. Such a scheme could be easily devised and at once put in force. Those who ought to come under it are, as things stand at present, either not having their claims met or are being dealt with in a manner which will ultimately prove unfair to them or to the State.

I am, Sir, yours faithfully,

August 4th, 1919.

LAURISTON E. SHAW.

INCIPIENT MENTAL DISEASES.

To the Editor of THE LANCET.

SIR,—In your issue of July 26th is published a letter by Dr. L. A. Weatherly on the treatment of cases of incipient mental disease, and while fully agreeing with most of his statements I do not think too strong a protest should be made against the proposed limitation of sojourn for such

cases to six months. I quite agree with Dr. Weatherly that under such a regulation a certain number of patients may be found nearly well at the time they have to leave the institution; but I would rather this happen than have in any way retarded the facilities for early treatment that are apparently rapidly materialising.

In dealing with many thousands of cases of acute mental disorder in the early stages during the war in a military hospital I found that three months was an average period of residence. Out of 1000 cases in hospital at the end of a two years' period of admissions only 200 were found to have been resident six months or over, and 70 per cent. of these were looked upon as unlikely to make an early recovery. At the time I refer to it was the custom to keep such cases for nine months prior to certification, but from my previous experience I consider that any retention of chronic cases in a hospital intended for treatment of early cases is to be condemned in the strongest possible terms.

An atmosphere of cure is what is wanted above all things in such an institution, and for this reason I would support the limitation to six months, but would suggest that the words "provided that the patient is not making obvious improvement" be added, as a means of overcoming the difficulty referred to.—I am, Sir, yours faithfully,

RICHARD EAGER, M.D.

Devon Mental Hospital, Exminster, July 30th, 1919.

THE COÖRDINATION OF CLINICAL RESEARCH AND PSYCHOLOGICAL MEDICINE.

To the Editor of THE LANCET.

SIR,—In THE LANCET of August 2nd there appeared an article by Dr. Bedford Pierce on "Psychiatry a Hundred Years Ago," also a letter from Dr. E. Goodall setting out what Cardiff is about to do in the present. I should like to draw attention to the fact that Birmingham already has a special hospital for the treatment of nervous diseases, the largest department of which is a "psychoneurosis clinic." The hospital was founded in 1913, but before beds could be provided in a suitable, quiet locality the war broke out. The governors, therefore, deemed it wise to postpone the opening of the in-patient department for mental cases until after the war. Immediately upon the cessation of hostilities they acquired a very suitable property with large grounds attached, and this will be ready for the reception of patients by the end of next month.

I wish to emphasise, in this connexion, two or three points which seem to me to have been passed over by the writers referred to above. The first point—mentioned in your leading article but not sufficiently stressed—is the necessity of separating early borderline and psychogenic from certifiable cases. If the public even suspect that they or their friends are being invited to attend an institution in any way associated with an asylum the early cases, in which treatment is most likely to be effectual, will not present themselves. For this reason it seems to me essential that the special clinics now springing up should be kept free from any taint of the asylum. The term "hospital for nervous diseases" or disorders seems acceptable to the general public, and I suggest that its adoption would avoid the danger under discussion. There is another advantage to be gained by working the new clinics from a special hospital for "nervous diseases"—viz., the well-recognised fact that even in cases in which the primary causal factor is indisputable of organic origin, it is nevertheless the super-added functional or psychogenic symptoms which cause most trouble, though they are also the most amenable to suitable treatment. Such cases willingly attend a hospital for nervous diseases where both elements of their trouble can be tackled, but they would merely be offended were they asked to attend a psychiatric clinic. Again, although borderline and psychogenic cases should be separated from advanced and hopeless cases of insanity, it will, from the research point of view, be a great advance if all diseases with a pronounced psychogenic element can be grouped and observed together with ordinary neurological cases instead of being dealt with by the more or less logic-tight-compartment methods hitherto in vogue.

My last point concerns the desirability, in large towns at any rate, of separating the out- from the in-patient department. The former must be in a central situation, which implies a small and noisy site, whereas the latter ought to

be in a quiet locality and have large grounds and workshops attached, so that occupation and recreation may be available for the patients. During the year 1918 the out-patient attendances at the Birmingham Nerve Hospital totalled 17,246, while the beds provided at present are for 30 patients only. It would be unwise to attempt to divert so many out-patients from a conveniently situated central institution, and impracticable to provide adequate accommodation for this special type of in-patient upon a centrally situated site. The governors, therefore, have located their new in-patient department for functional and borderline cases at some distance from the original institution, although this involves an increase in the expenses of management.

My plea, then, is that the basis of the new clinics be broadened and that at the same time everything reasonable be done to secure the confidence of those whom we are seeking to benefit.—I am, Sir, yours faithfully,

ALFRED CARVER.

Birmingham and Midland Hospital for Diseases of the Nervous System, Birmingham, August 4th, 1919.

THE ORIGIN OF LIFE: THE WORK OF THE LATE CHARLTON BASTIAN.

To the Editor of THE LANCET.

MONSIEUR,—Je n'aurais eu, pour le moment, que peu de chose à ajouter à ma lettre parue dans vos colonnes le 28 Juin dernier, si l'intervention de M. John Butler Burke (THE LANCET, 26 Juillet) n'était venue introduire de nouvelles hypothèses dans l'interprétation des expériences du Dr. Bastian et autres essais du même genre. Je dois pleinement reconnaître le très grand intérêt des *Radiobes* de M. Burke, dont les propriétés biotiques sont saisissantes; mais Raphaël Dubois (avant M. Burke) et Martin Kuckuck (après lui), ont obtenu des résultats exactement semblables en utilisant des sels non radioactifs de baryum, strontium, &c. Comme les radiobes de M. Burke, les *microbioides* de Dubois et les *baryumoytoden* de Kuckuck grossissent, se meuvent, se reproduisent par bipartition, semblent parfois se conjuguer à la manière de certaines algues monocellulaires: ce qui ne les empêche nullement de passer, en vieillissant, à l'état de cristaux polyédriques inertes. Il semble difficile de voir dans ces corpuscules autre chose que de très petits cristaux imparfaits dont le stade précristallin se trouve considérablement prolongé par l'ambiance colloïdale; d'ailleurs, tous les cristaux en voie de formation, surtout dans des milieux très visqueux, se comportent temporairement, au point de vue structural et dynamique, comme des êtres vivants. A l'appui de ce que j'avance, je mentionnerai les *Protobies* de A. L. Herrera (cristaux imparfaits en milieux siliciques) et nos propres expériences sur la formation des cristaux.¹ La radioactivité me semble, en toute sincérité absolument étrangère à de telles questions. Quant au rôle de la diffusion et de la pression osmotique dans l'apparition des bactéries minérales de Bastian et Mary, il doit être inexistant. Sans doute, mon éminent ami Stéphane Leduc a produit, par osmose, et aussi par diffusion de cristalloïdes dans les gels colloïdaux, une profusion de formes et de structures artificielles de nature à nous renseigner sur l'intervention des forces capillaires dans la détermination des caractères morphologiques et physiologiques généraux des organismes. Mais les croissances osmotiques, que nous avons aussi étudiées depuis 1908, sont des vésicules, et les figures de diffusion dans les gels n'ont pas d'existence en-dehors de leur substratum colloïdal. Les corpuscules synthétiques de Bastian sont formés par les colloïdes eux-mêmes, et ne sont pas vésiculaires. Pour expliquer le mécanisme physique de leur développement, c'est exclusivement à la physico-chimie colloïdale qu'il faut faire appel, et tout l'intérêt du problème tient précisément à ce fait qu'il n'y a qu'une physico-chimie colloïdale, embrassant dans des lois communes l'organique et le minéral.—Je suis, Monsieur, très sincèrement vôtre,

ALBERT MARY.

Institut de Biophysique, Paris, 30 Juillet, 1919.

To the Editor of THE LANCET.

SIR,—Mr. S. G. Paine, writing some time ago in your columns on the "Origin of Life," in particular connexion with the work of Charlton Bastian, mentioned that the sand of the Egyptian desert, which is subjected to a considerable heat, contains living protozoa. I do not know what temperature

these organisms can withstand, but it should be remembered that under those conditions the heat will be dry. The following facts, however, show that the limits of resistance of certain organisms, even to moist heat, are greater than was suspected. M. Paul Portier, professeur à l'Institut Océanographique, in his recent work entitled "*Les Symbiotes*" (Masson et Cie, 1918), describes certain symbiotic micro-organisms, isolated from both vertebrates and insects, which are extraordinarily resistant to physical and chemical agents. When freshly isolated they are killed by a temperature of 100° C., but after a few subcultivations the temperature must be raised to 115° C., moist heat. In a dry atmosphere they can resist a temperature of 140° C., and are only just killed by a temperature of 150° C., maintained for half an hour. Further, these organisms may be boiled in absolute alcohol, chloroform, or acetone, and yet remain capable of cultivation. In one set of experiments, indeed, they withstood heating in acetone in sealed tubes at a temperature of 100° C. to 120° C.

In connexion with this subject it may be mentioned that certain enzymes, as, for instance, ptyalin, which begin to decompose at a temperature of 60° C. and are completely destroyed by temperatures of less than 100° C., can, when dialysed free from all traces of electrolytes, be boiled without losing all activity, which returns on the readdition of a little salt. In view of such facts, experiments similar to Dr. Bastian's must be carried out with the greatest possible precautions as to technique and sterilisation. It is true that the interesting experiments of Dr. Mary were carried out at a temperature which should kill any organisms at present known. Dr. Mary, however, admits that the bodies found in his solutions do not contain any protein and are incapable of cultivation, even on the simple solutions which are supposed to generate them—that is to say, they are not living matter in any ordinarily accepted use of that term.

Since writing the above, I have seen Commander Bastian's letter of July 26th. I gather the objections he raises to my experiments¹ are two. (a) That the "yellow solution" contained ammonium phosphate and phosphoric acid in addition to the proper ingredients, and (b) that the tubes were kept for too long a period, during which the "organisms," which were supposed to have developed, died. In the first place, I must thank Commander Bastian for pointing out my error. I cannot excuse such carelessness, my only explanation is that when writing up the account of the experiments during the war and several years after the solutions were made up, I foolishly referred to Dr. Bastian's "Origin of Life," instead of to my own notes, in order to ascertain the composition of the solutions, which being quite arbitrary, I had not unnaturally forgotten. Though there is no doubt as to Dr. Bastian's meaning when carefully read, anyone who will take the trouble to look up the reference (p. 30) will see that the words, "the proportion of the other ingredients remaining always the same" might be misleading in the hurry of the moment. I have now looked up my original notes, and I beg leave to correct the error by making the following quotation from my note-book:—

"On Sunday, August 10th (1913) test-tubes of hard white German glass 5 in. × ½ in. were charged half full with Dr. Bastian's 'yellow solution,' consisting of 8 drops of liquor ferri pernitratis and 3 drops of dilute sodium silicate (from A and H's sample reserved for Dr. Bastian) to each oz. of distilled water, these proportions gave the port-wine colour recommended by Dr. Bastian with a minimum amount of sediment."

On the next page the correct formulæ for both solutions are written in a tabular form above the two series of tubes. I trust that Commander Bastian will accept this evidence. With regard to his second criticism, I think Commander Bastian makes a misrepresentation. He implies that the tubes of the "white solution" were kept for 38 months before being opened, whereas the truth is that the tubes of both series were opened at varying periods from 1½ months to 38 months (at intervals of about 3 months). The period of 38 months was only the maximum duration of the experiment. I believe the longest that Dr. Bastian kept his tubes was two years. When planning the experiments I therefore considered that if I kept some of the tubes for three years I could not be accused of impatience. I must apologise for taking up so much of your valuable space, and beg to remain,

Yours faithfully,

The Biochemical Laboratory, Cambridge,
August 2nd, 1919.

H. ONSLOW.

¹ Voir L'Actualité Scientifique, Paris, Mai, 1919.