

Correspondence.

"Audi alteram partem."

THE CAUSAL RELATION OF ALCOHOLISM TO INSANITY.

To the Editor of THE LANCET.

SIR,—In view of the frequent repetition, even in quite reputable text-books of psychological medicine, of the statement that alcohol is one of the chief factors in the causation of mental disease, it may be of interest to call attention to the statistics of insanity in this country during the recent period of decreased alcoholism. The accompanying table shows, for the years 1913 to 1918, the number of first admissions to asylums in comparison with the movement of drunkenness and of other manifestations of alcoholic excess. To avoid the obvious risk of fallacy arising from the absence of large numbers of the adult male population on military service, the figures refer to women only.

Alcoholism and Insanity (Women only), England and Wales, 1913-18.

Year.	Convictions for drunkenness.	Deaths from		Attempted suicide.	First admission to lunatic asylum.
		Alcoholism.	Cirrhosis of liver.		
1913	35,765	719	1665	988	9372
1914	37,311	680	1773	1049	9702
1915	33,211	584	1525	816	9078
1916	21,245	333	1163	436	8850
1917	12,307	222	808	452	8702
1918	7,222	74	579	400	9726

The fact that, despite a reduction of alcoholism amounting to about 80 per cent., there has been no decline, but, if anything, a slight upward tendency in the incidence of insanity, would be difficult to understand if it were true, as asserted by some writers on the subject, that "alcohol stands in the first rank as a factor in the production of insanity." It has already been shown conclusively by Sir Frederick Mott that the belief in the predominantly alcoholic ætiology of mental disease is a myth evolved by the application to unreliable data of a very crude process of inference from antecedence to causation, and these figures give further evidence of the soundness of his criticism.

The point is of more than merely theoretical interest at the present time, because, since alcoholism is often regarded as differing from other deleterious agencies by being in some sort a manifestation of wilful vice, it may happen that an insane person, whose insanity is erroneously imputed to alcoholic excess, will on that account be debarred from financial relief to which he might otherwise be entitled. There is, therefore, some need to emphasise the fact that, when insanity is really due to alcoholism, it bears characteristic clinical marks of its origin, and that, in the absence of such marks, a history, even if authentic, of antecedent drinking in a case of paranoia, or dementia præcox, or any other psychosis, gives no sort of ground for assuming that alcohol had anything to do with the causation of the disease.

I am, Sir, yours faithfully,

W. C. SULLIVAN, M.D.,

Medical Superintendent, Broadmoor State Asylum.
Sept. 14th, 1922.

DYSPIUITARISM, OBESITY, AND INFANTILISM.

To the Editor of THE LANCET.

SIR,—I am sure all will agree with the editorial remarks in your issue of Sept. 16th (p. 625) concerning the interest and value of Dr. Letheby Tidy's critical review of certain aspects of our knowledge of the pituitary body. Perhaps, however, I may be allowed to call attention to one or two points of experimental and clinical importance which are not made quite

clear, and to a suggestion that should be capable of confirmation or the reverse.

First, with regard to the experimental production of dystrophia adiposogenitalis, although the conflicting testimony as to the lesion necessary for the evolution of this syndrome is quite accurately discussed, and what I, personally, was unable to confirm is stated, my positive experiments have escaped notice. I found that dystrophia adiposogenitalis could only be caused by injury to the stalk—that is to say, when the stalk of the pituitary was divided or simply compressed with fine artery forceps the condition under discussion developed.¹ Obviously the blood-supply, which for the most part passes down the stalk, is seriously impaired by such procedures. Clinically, in this connexion it is interesting that suprasellar lesions, unconnected with the pituitary and which in no way distort the sella turcica, by pressure on the stalk may produce all the symptoms—except the ocular—associated with enlargement of the pituitary and deficient secretion. If ocular symptoms be present in any case of this kind, they are due, of course, to the suprasellar lesion, for the pituitary is compressed rather than enlarged in these circumstances. Such cases are often missed as a radiograph shows the sella turcica to be of normal size.

Secondly, I have long wondered whether the drowsiness present in encephalitis lethargica may not be due to compression of the infundibular stalk by the congested dura mater through a small aperture in which this process passes. I have often mentioned this supposition to those who have the opportunity of making post-mortem examinations, but I do not think the point has been elucidated.

I am, Sir, yours faithfully,

Liverpool, Sept. 16th, 1922.

W. BLAIR BELL.

THE SCOPE OF VACCINE THERAPY.

To the Editor of THE LANCET.

SIR,—I advocate eight injections because I find that the best results are obtained from a course of that length. I am not concerned to argue whether seven, eight, or nine doses are the most advantageous, for as Dr. A. K. Gordon remarks in your issue of Sept. 16th (p. 640), much depends upon the individual patient. But I had in mind such treatment as the administration of a vaccine continuously for two years. I have encountered this on more than one occasion, and I imagine that Dr. Gordon will agree that the best advice one can give in such circumstances is to allow the patient a rest and an opportunity to recover from the treatment. I note with much interest that Dr. Gordon believes in small doses, and that most of the improvement occurs in the latter half of the course. It is not an unfair inference that the improvement coincides with the period when the dosage becomes of appreciable magnitude, and I therefore suspect that there is small difference between his 16 doses and my eight. I also have had many cases sent to me which have not benefited by other vaccines, but my experience is precisely opposite to that of Dr. Gordon—it is the "old style" vaccine that has been found wanting. And I may add that it is not often that the residual (otherwise detoxicated) type fails to work a considerable change for the better.

But I do not think it fair to compare an autogenous vaccine with a detoxicated stock vaccine, as Dr. Gordon does. His success with gonococcal infections after the detoxicated vaccine has failed is interesting, but I fear that general opinion is against him. I have made a point of inquiring from clinicians in charge of V.D. centres as to the results obtained with residual vaccines prepared by myself and with detoxicated vaccines prepared under the direction of Dr. David Thomson. The general type of vaccine is the same, and the results are identical. I found that opinion is practically unanimous that they are of the greatest service in the treatment of gonorrhœa.

I am, Sir, yours faithfully,

Manchester, Sept. 16th, 1922.

C. E. JENKINS.

¹ Quart. Journ. Exper. Physiol., 1917, xi., 77.