Correspondence.

"Audi alteram partem."

TIN IN CANNED FOODS.

To the Editor of THE LANCET.

SIR,—My attention has been drawn to a letter in THE LANCET of April 3rd, in which Dr. H. W. Wiley expresses his opinion that the results of certain experiments made by Dr. Schryver on this subject (taking known doses of non-irritant salts of tin with a view to determining the degree to which tin was absorbed from the alimentary tract) had better not have been stated because the statement may give encouragement to canners to continue improper practices.

Criticisms from Dr. Wiley on a matter of this sort deserve very careful consideration. He is chief of the Bureau of Chemical Industry in the United States, his efforts to secure the greater purity of foods are well known and appreciated in this country, while in the case of canned foods he has been endeavouring to secure the use of better grades of tin plate by American canners. Dr. Schryver and I have recently made reports to the Local Government Board—"On the Presence of Tin in Certain Canned Foods"—which include an account of the experiments referred to. The statement to which Dr. Wiley takes exception was—

They (these experiments) do not indicate much probability of serious risk of chronic poisoning by the absorption of non-irritant compounds of tin as a result of a diet which consists largely of canned foods and is continued over considerable periods of time.

If this is objected to, I can only say that it is not the practice in this country to withhold the results of experiments which have been carefully carried out even for such laudable objects as those indicated in Dr. Wiley's letter. I should not, however, have troubled you with any comment on this matter were it not for the consideration that, coupled with your own note on our reports in THE LANCET of Feb. 6th, Dr. Wiley's letter may convey the impression that the general effect of these reports is to suggest that no attention is required in the interests of public health to contamination of foods by tin.

To avoid any such misconception by your readers who have not seen the reports themselves, I would point out that they cover a considerably wider field than the individual experiments which Dr. Wiley discusses. The latter are contained in a section of Dr. Schryver's report which deals with the evidence available as to the accumulation of tin in the body as a result of continued ingestion of small quantities of tin such as are frequently present in properly canned foods of ordinary age. But Dr. Schryver's report also includes a collection of cases in which canned foods containing unusually large amounts of tin have been associated with definite irritant poisoning, and has further indicated the extent to which the degree of contamination of canned foods by tin is governed by the character of the food canned and the time which has elapsed since canning.

The need for discouraging the use by canners of unsatisfactory tin plate, together with the importance of avoiding contamination by tin through improper methods of soldering, is given prominence in my report, which also contains suggestions as to the control which sanitary authorities in this country may be able to exercise in the matter. It appears to be established that practically all canned foods contain some tin, and that at present no positive evidence can be ascertained of harm resulting from the small quantities of tin which may frequently be taken in this way. No suggestion has been made, however, that precautions should not be taken to prevent the introduction of exceptional or excessive amounts of tin into these foods. The basis of such precautions is the employment of satisfactory tin plate and methods of soldering, and also recognition of the fact that contamination by tin increases with the age of the can—a matter of practical importance where acid foods such as canned fruits and meat extractives are concerned. I cannot, however, admit that because this is the end to be attained we should be justified in suppressing the results of experiments because these results may possibly be misused by unscrupulous people.—I am, Sir, yours faithfully,

London, April 14th, 1909.

G. S. BUCHANAN.

SPONTANEOUS CURE OF THORACIC ANEURYSM.

To the Editor of THE LANCET.

SIR,—In THE LANCET of April 3rd and 10th accounts have appeared of the spontaneous cure of thoracic aneurysm. The following case bears upon these, not so much as a case of complete spontaneous cure, but one of nature's attempts at the same, where visible changes could be watched with the naked eye. About Michaelmas, 1906, a man, about 46 years of age, complained of pain in the chest and lower ribs. The pain in the chest extended through to the shoulder blades and the muscles of the back of the neck. This latter symptom had, he told me, led to a diagnosis of muscular rheumatism. When I saw him about 12 months later, he then told me that he had left off work just previously and was suffering from rheumatism in his neck. On placing the hand over the front of the chest a distinct heaving impulse was felt, and upon auscultation a double murmur. In due course a very troublesome cough set in with dyspnoea. There was no cedema or loss of voice, nor any hæmoptysis. About six weeks after I first saw him the heaving impulse distinctly pointed at the side of the sternum about the level of the third rib cartilage, and a pulsating swelling of about the size of a hen's egg made its appearance, occupying quite half of the breadth of the sternum. The man was now bad, and it seemed a question whether the aneurysm would burst through the skin or into the bronchus, and as the skin remained intact I fully expected the latter at any moment. I had him watched night and day and kept him absolutely quiet in bed and administered morphia regularly. After some weeks of expectation the man began to improve; the pulsating swelling which probably perforated the sternum without any spontaneous fracture lessened and finally disappeared, and at the present time, with the exception of a little tenderness in the third interspace to the left of the sternum, nothing remains to tell the tale. In the absence of any definite pressure symptoms or any alteration in either pulses or carotids or subclavians I considered the case to be one of aneurysm of the first part of the arch with a considerable sacculus giving the outward and visible signs. The man felt so much better that he wished to resume his ordinary occupation, but to this I would not consent.

In his present condition the patient presents a chest distinctly generally bulged and heaving on the left side, dull on percussion from the left clavicle to the right edge of the sternum. There is a soft systolic murmur all over both sides of the chest. There is a thrill over the left subclavian artery and a systolic murmur over the right subclavian and right carotid arteries. A systolic murmur is heard over the back. The right chest also heaves, but there is no tumour or thrill, and over the left side the breath-sounds are almost inaudible.—I am, Sir, yours faithfully,

JOHN J. WADDELOW, F.R.C.S. Eng.

Whittlesey, April 12th, 1909.

To the Editor of THE LANCET.

SIR,—About 30 years ago a chaplain to the forces who had been under treatment by the late Mr. Tufnell for aortic aneurysm was waiting for his train at the King's Bridge station, Dublin, on his way to the Curragh to resume duty. He suddenly leaped over the wall of the platform on to the foreshore of the Liffey, falling perhaps 20 feet. Although he fractured no bones he died from shock. He had been brought to Dr. Steevens' Hospital, and at the post-mortem examination it was seen that the solid clot which filled the aneurysm was in no way disturbed by the fall, and I very distinctly remember its smooth surface where it filled the entrance from the lumen of the artery.

I am, Sir, yours faithfully,

T. C. D.

April 12th, 1909.

THE CLINICAL VALUE OF HÆMOMANOMETER OBSERVATIONS.

To the Editor of THE LANCET.

SIR,—I regret to find that I have drawn an inaccurate inference from Professor Starling's experiment referred to in my letter in THE LANCET of April 3rd—the result of a hurried glance through his Mercers' Company lectures which had just come into my hands, and of not allowing enough for

the difference between mercury and sulphate of magnesia solution. The relations between the arterial and venous pressures were: At the beginning, 11 arterial (Hg) to 14 venous (MgSO_4 solution); during experiment, 5 arterial (Hg) to 17.5 venous (MgSO_4 solution). The experiment did not show the height to which venous engorgement pressure can be raised, as the venous outflow was not arrested, it was only impeded.

The controversy initiated by Dr. Leonard Hill seems to me to have narrowed itself down to the question of the height to which *venous engorgement* pressure can rise. I have quite "grasped the fact" that Dr. Hill believes "that the venous blood pressure cannot exceed the arterial." I have, however, also grasped the fact that Dr. Hill's observations do not prove what they were thought to prove, but seem to prove, if they prove anything, the unsoundness of the proposition that venous engorgement pressure cannot exceed arterial. I entirely agree with him, however, that it is useless to continue the discussion further, but I would venture to suggest that a point has been defined where experimental physiology might be of assistance in settling the question, which is of somewhat wide interest from its relations to various phenomena which I have avoided referring to in this correspondence.

In closing the correspondence as far as I am concerned, I desire to express my appreciation of Dr. Oliver Williamson's generous recognition of the accuracy of my clinical observations.

I am, Sir, yours faithfully,

Edinburgh, April 10th, 1909.

WILLIAM RUSSELL.

THE PROXIMATE FACTOR OF DELIRIUM TREMENS.

To the Editor of THE LANCET.

SIR,—I cannot altogether concur with the views expressed by Dr. F. Hare in his letter to THE LANCET of April 10th. I am not in agreement with him in his statement that "delirium tremens is a complication which mainly, if not solely, affects the chronic alcoholic as distinguished from the dipsomaniac and the numerous varieties of impulsive drunkards," &c.

In my experience, and I have referred to the records of several hundreds of cases, there are many more regular or continuous drunkards than periodic drunkards, but the latter are just as liable to suffer from delirium tremens as are the former. And while the cutting off of liquor without the substitution of suitable treatment may precipitate an attack of the malady, the disorder may and does occur among those who are at the time drinking heavily, and who are not troubled with vomiting or other symptoms which denote that the digestive tract refuses to retain or to absorb alcohol. Referring to the Norwood Sanatorium report of which notice has been taken in your issue of March 27th, that institution is described as being under the supervision of an influential advisory medical committee, and it is advertised that the object of the institution is to "place the therapeutics of inebriety upon a definite scientific basis," an advertisement, by the way, which seems to reflect on the labours of such men as Sir B. W. Richardson, Dr. Norman Kerr, and many others. The admission, therefore, that in such an institution there occurred during two years some 6 per cent. to 7 per cent. of such grave complications as delirium tremens and alcoholic epilepsy is a somewhat startling one.

Dr. Hare appears to desire further clinical evidence as to the advisability of a sudden discontinuance of alcoholic liquors in the treatment of inebriates. I will give him clinical evidence. At the Dalrymple Home retreat during the past 25 years there have been admitted some 900 inebriates. During the first 15 years of this period Dr. R. Welsh Branthwaite held the post of medical superintendent, and in this period there occurred three cases of after admission delirium tremens. One occurred a few weeks after Dr. Branthwaite commenced his duties, and this case he considered with the light of further experience was a preventable one. The second case was found on the morning after admission to be suffering from double pneumonia and delirium. The third occurred during his absence on a holiday.

During the past ten years I have been superintendent here, and not a single case of delirium tremens has developed among patients after admission. I have experienced three

"scares" in cases where the patient passed a restless first night and when, in the morning after admission, the condition appeared to be threatening, but in each case I have been fortunate enough to get the patient to sleep in the early afternoon and have had no further trouble.

I have had to treat only one case of alcoholic epilepsy, this occurred some four months after admission of the patient. He had indulged in beer when on parole, and in his case quite a small quantity of liquor produced this condition. For this absence of complications I am largely indebted to Dr. Branthwaite, for I profited by his experience. All of Dr. Branthwaite's cases and all of mine during my first eight years were treated by absolute withdrawal of liquor. During the past two years I have very occasionally given to patients some brandy or whisky during the first day or two if they particularly desired it. This I have done not with any idea of preventing complications but merely as a placebo. A certain amount of spirit has been given in the form of pharmaceutical preparations, in spiritus ammoniæ aromaticus, for instance, but to the person who is accustomed to take alcohol by the pint or more daily a few drachms of spirit during the 24 hours is a negligible quantity.

Judging by the experience gained at the Dalrymple Home Retreat I am of opinion that delirium tremens, in the absence of surgical injuries or a disease such as pneumonia, is an avoidable complication when the patient can be kept under observation, and that under the same conditions those patients recover more rapidly whose liquor is stopped at once. I have admitted a few cases where delirium tremens had already just commenced, but with the exhibition of bromides in large doses and of hypnotics, among which chloral and paraldehyde are the most useful, sleep has been induced within 12 hours of admission. So that I cannot agree with the view which Dr. Hare is inclined to favour, that the best treatment of this condition is to withhold practically all drugs. The condition, in my opinion, requires energetic drug treatment.

I am, Sir, yours faithfully,

F. S. D. HOGG.

Dalrymple Home Retreat, Rickmansworth, April 13th, 1909.

HILLYER v. THE GOVERNORS OF ST. BARTHOLOMEW'S HOSPITAL.

To the Editor of THE LANCET.

SIR,—In reference to your report and comments on the above case may I crave the hospitality of your columns to tell all who may be interested that my solicitors have entered an appeal against Sir William Grantham's decision? By the help of friends I hope to make this a test case and obtain a legal ruling as to the liability of hospitals in such situations, for it must be admitted that this question is of grave importance not only to the authorities of hospitals but also to the general public who support these charities. May I add that I went into court under the impression that a jury would hear *all* the evidence and would be permitted to express their opinion?

I am, Sir, yours faithfully,

London, April 12th, 1909.

WILLIAM H. HILLYER.

A CASE OF EXCISION OF THE SCAPULA.

To the Editor of THE LANCET.

SIR,—I was very pleased to read the letter of Mr. R. P. Rowlands in THE LANCET of Jan. 16th, calling my attention to a few discrepancies in my article on the above subject which was published in your journal of Jan. 9th.

In reply, I might state that my patient was able to abduct his arm voluntarily to an angle of 45 degrees, but on forced abduction to any degree beyond this limit he evinced much pain; but Mr. Rowlands's case was able to abduct his arm to a right angle only. It was originally the typist's error, when "90 degrees" were substituted for 45 degrees. Though the difference in the range of abduction in these two cases is only 45 degrees, yet I admit that every extra range of movement must certainly be of material advantage to any person. But the two cases under discussion are quite unlike each other: one was an enchondroma and the other a rapidly growing sarcoma. My subject was also able fully to flex, extend, and adduct his shoulder-joint, but internal and