

## HOG'S STOMACH IN PERNICIOUS ANAEMIA

SIR.—In a note on the treatment of a case of pernicious anaemia with beef predigested with normal gastric juice, Dr. Astley Clarke (*Journal*, November 14th) remarks that some cases of pernicious anaemia relapse when treated with liver and hog's stomach, and "are not then benefited to the same extent—possibly not at all—" the second time. "Why is it so?"

The answer is that, assuming the diagnosis to be correct, the treatment has been incorrect or inadequate in one of the following ways: (1) inadequate dosage; (2) the use of an inactive preparation of hog's stomach; (3) the inactivation by heat of an adequate dose of an active preparation. All these points I have repeatedly stressed in the English medical literature. In two and a half years' observation of upwards of 240 cases of true pernicious anaemia I have never found a relapse after the institution of active and adequate hog's stomach therapy. The average maintenance dose is usually half an ounce of desiccated hog's stomach per day; in some cases the full ounce is required, and this should be insisted upon where postero-lateral sclerosis is present. I have already drawn attention to the risk of employing inactive preparations of hog's stomach (*British Medical Journal*, 1931, ii, 76), and have referred to the use of normal gastric juice, which is active when not predigested with beef, provided it is given at meal times (*British Medical Journal*, 1930, i, 236). This has been confirmed, as Dr. Clarke has observed, in the American press. In all the research work so far published (both from Manchester and abroad) on this aspect of the problem, doses of eight ounces or more of gastric juice have been used, and the two ounces given by Dr. Clarke would not be sufficient to produce a prolonged remission, if at all, in pernicious anaemia. There are many difficulties besetting the unwary: (1) incubation for more than two to three hours; (2) too high or too prolonged an acidity destroys the activity; and (3) the supply of unsuitable gastric juice—even a normal secretion may vary very considerably. The fresh gastric mucosa of a pig may be substituted for normal human gastric juice.

It is a common observation that a blood transfusion, small doses of liver, stomach, and at times even iron and arsenic, will initiate remissions in pernicious anaemia; these will sometimes proceed until nearly normal blood pictures are obtained before relapse. It appears possible that the "remission" in Dr. Clarke's case was due to the blood transfusion rather than to the two ounces each of beef and normal gastric juice—no blood investigations are reported.

Beef predigested with normal gastric juice is much less palatable than an active hog's stomach preparation given in milk, with sardines or tomato, etc.—I am, etc.,

Royal Infirmary, Manchester,  
Nov. 18th.

JOHN F. WILKINSON.

## THE CARDIAC CYCLE

SIR.—In the first place, I would beg the hospitality of your columns to thank the reviewer for his courteous and sympathetic criticism of my venture along lines of investigation unfrequented and leading to conclusions here and there certainly "unorthodox" (that is, not sanctioned by current teaching); however, he credits me with "sincerity, enthusiasm, and lucidity," and I wish for no more generous words to express my desire, motives, and endeavour. In the next place, I wish to meet as far as I am able certain comments, which seem to indicate that I have not quite succeeded in making myself clear.

Thus, dealing with the genesis of the pulse-wave, as the result of an impact upon the closed aortic valve by the ventricular blood, during the isometric phase of the ventri-

cular systole—in consequence of which a wave is "sent coursing through the aorta and its branchings down to the arterioles"—the case is admirably and succinctly stated. But now, immediately upon the above, there comes a sentence which puzzled me much—namely, "The stretching of the arterial wall is regarded as the expression of 'momentum' carried by the pulse-wave"—(I could recall no such isolated sentence). At this point, in direct sequence, came the definition of "momentum," as "the product of mass and rate of movement of the mass" (which had my full assent). This conceded, the sentence proceeds: "it is difficult to see how a vibration without any expulsion of blood into the arterial tree can add any momentum to that already possessed by the moving stream of blood before the impact strikes on the closed aortic valves." The comment ends here, and the puzzlement also, for at once it becomes clear that one form only of momentum is recognized within the circuit of the vessels—namely, that of the blood moving *en masse*. Is this so?

In my argument, from the beginning, I have laid stress on the fact that the circulation of the blood exhibits two forms of dynamism (the word should be justified since "dynamist" is recognized). The one, a movement of translation, which conveys the blood bodily to and from the heart; we shall all be agreed upon the fact and nature of this momentum. The other, a movement of vibration or undulation, which is confined to the branches of the arterial tree, and ceases in the arterioles. Clinically, it figures as the pulse-wave; what is its real nature? And has it momentum?

To answer these questions let us put the finger on the artery; it is lifted as the wave passes; substitute a lever, the lever is lifted in like manner; in either case the wave imparts *motion* to *mass*, and in so doing "does work" in the terms of the physicists. The lifted finger and lever are examples of momentum possessed and imparted by the pulse-wave. Can we put the Q.E.D. of the geometer here? No, because it has yet to be proved whether, if the impact of the isometric phase, as the rapidly rising tension of the intra-ventricular blood is brought to bear upon the aortic valve, is really the cause of the wave—whether, in this act some blood does or does not escape through the valve in spite of the adverse tension of the intra-aortic pressure, still in excess of that in the ventricle, for *ex hypothesi* we are still below the point *o* in the diagram.

At this stage we may ask: is it a canon in physics that an impact, which produces an undulation in a given liquid, must carry along with it a certain quantity of liquid, if the resultant wave is to possess momentum? *Fiat experimentum*. Let a stone be cast on to the surface of a river flowing smoothly; the immediate consequence is an undulation, starting from the point of impact and spreading in a series of widening circles to either bank. The actual form of the liquid movement constituting the wave is complicated, but it is *not* a movement of bodily translation, and it is *wholly* independent of the current of the river, to which it contributes nothing. Does *this* wave possess momentum? Let us see what happens as it meets with some floating object—for example, a child's sailing boat—and observe how the little craft is tossed about, up and down. Have we not here the manifestation of momentum? Yet the bare impact of the stone had no liquid accompaniment. All that happened as the stone sank was displacement, and perhaps in this movement we may find all that we need to account for the presence of momentum in the pulse-wave; for without doubt, as the tension-shock impinges upon the aortic valve there will be a bodily forward movement of the *unforced* valve, a displacement which carries with it a certain quantity of blood, this movement being permitted by the elasticity

of the walls of the bulbus aortae. Upon this *action* will follow the kick of the elastic recoil as the *reaction*, equal and opposite, according to Newton's third law, and the wave will then speed on its way.—I am, etc.,

London, N.W., Nov. 18th.

HARRINGTON SAINSBURY.

#### " FAILED FORCEPS " CASES

SIR,—The paper which I read on the obstetric forceps and its use was designed to remind my audience that unsuccessful attempts to deliver by forceps are still frequently made, and to give an accurate account of the results of misapplication of this instrument. For this purpose I investigated the last 100 cases admitted to the Liverpool Maternity Hospital after forceps had been applied without success. These cases have been spread, I am thankful to say, over no less than seven years. Their number, however, appears to be increasing. Twenty-two of them were admitted during the year 1930. I might have mentioned the period involved, and would certainly have done so had it occurred to me that anyone could think such a series of cases had been admitted within a month or a year. The figure is of no statistical value, because there are four other hospitals in this city with large obstetrical departments.

I therefore concentrated upon analysis of the causes of failure and its results. I can assure your correspondent (November 21st, p. 966) that my figures are of fact and not of fantasy. It is perhaps as well that we are not all gifted with his vivid imagination! If your correspondent had to deal with these cases, as I have to deal with many of them, I think he would be less ready to accept them without protest.—I am, etc.,

Liverpool, Nov. 23rd.

M. A. DOBBIN CRAWFORD.

#### ETHER CONVULSIONS

SIR,—On Friday last, during my usual work at St. Bartholomew's Hospital, I met my first case of ether convulsions, and as it presents certain points that may be of interest to other anaesthetists, I should like to record it.

The patient was a boy, aged about 9, who had been badly smashed in a motor accident some time ago, and had had several operations for the grafting of skin to parts of the body. He had been under the care of Sir Harold Gillies, and when I saw him it was to anaesthetize him for an appendicectomy for Mr. Girling Ball. The position was this—his right forearm and right thigh were connected by a pedicle graft, and his lower abdomen was devoid of skin, which had been used for the graft. Sir Harold divided the pedicle and Mr. Ball then had to remove the appendix through an incision parallel to Poupart's ligament, and about half an inch above it.

I gave the boy gas, oxygen, and ether endotracheally; the induction was normal, but after he had been under for about ten minutes, I noticed a slight twitching about the outer side of the left eye. I at once shut off the ether and continued with gas and oxygen, but the twitching continued and got severe, spreading to the face—in fact, it looked as though the facial nerve was being violently stimulated. The twitching continued, and now it appeared as though his diaphragm was in a state of spasm. The left arm was twitching, but the right arm and legs were not affected. These spasms increased to an alarming extent. I tried oxygen plus CO<sub>2</sub>, but this did not seem to have any effect. Next, as I thought that I must stop the convulsions, I disconnected the endotracheal tube and gave him about 8 to 10 drops of chloroform on lint. He then became slightly cyanosed, so I discontinued the chloroform, and resumed with endotracheal oxygen alone; he now began to breathe more regularly, and, after a few more minutes, to breathe automatically. His pupils, which had been three-quarters dilated during his spasms, now became quite small, not pin-point, but just a shade larger. Throughout the whole thing, with the exception of the few seconds when he had the chloroform, his colour was bright pink. As he was now

breathing automatically and well, the appendicectomy was proceeded with, and for this he was given endotracheal gas and oxygen, which was occasionally passed over the surface of the ether. On removal, the appendix was found to be dilated at its end and contained pus. At the conclusion of the operation his condition was fairly good, and he looked quite normal.

On considering the case, I think that in the early stage, just after induction, the patient may have had just a little too much ether, and this is the opinion that I have held with regard to some of the other cases that have been reported. I think that, in all probability, the reason why this boy recovered was that I started my efforts at remedy on the very first twitch, and did not wait for it to get pronounced. As it was, the twitches became very severe, and the patient's appearance was dreadful. From the twitchings it appeared as though the facial and phrenic nerves were both violently stimulated.

It will be noted that in this case the factor of sepsis is again present, as it has been in several other cases. I have never met with one of these cases before amongst my own administrations, and sincerely hope that I may never meet with another.—I am, etc.,

London, W.1, Nov. 23rd.

H. EDMUND G. BOYLE.

#### DEATHS ASSOCIATED WITH ANAESTHESIA

SIR,—Permit me to direct the attention of your readers to the following interesting statistics of deaths under or associated with anaesthesia in England and Wales, and in the United States. For England and Wales I have derived the figures from the annual reports of the medical superintendent of the Registrar-General's office, which give the data back to 1901. The results are as follows.

*Statistics of Deaths Under or Associated with Anaesthesia in England and Wales, 1901-29*

Year.	Total Deaths.	Rate per 100,000
1901-1905	740	0.44
1906-1910	1,010	0.58
1911-1915	1,415	0.78
1916-1920	1,535	0.87
1921-1925	1,990	1.04
1926	556	1.42
1927	596	1.52
1928	656	1.66
1929	730	1.84

The rate of frequency of deaths under or associated with anaesthesia has therefore persistently increased from a minimum of 0.44 in 1901-5 to 1.84 during 1929, the highest rate thus far recorded. These rates may be compared with the corresponding rates for the United States Registration Area, 1922-29, as follows.

*Statistics of Deaths Under or Associated with Anaesthesia in United States Registration Area, 1922-29*

Year.	Total Deaths.	Rate per 100,000
1922	513	0.55
1923	474	0.48
1924	598	0.60
1925	677	0.66
1926	654	0.60
1927	695	0.61
1928	569	0.49
1929	690	0.58
1922-1929	4,870	0.57

According to these comparative data the frequency rate of deaths under or associated with anaesthesia in England and Wales is three times the corresponding rate prevailing at the present time in the United States. Unquestionably there is some reason for this enormous difference, but no satisfactory explanation has been forthcoming.—I am, etc.,

FREDERICK L. HOFFMAN.

Wellesley Hills, Mass., Nov. 11th.