

Correspondence.

THE DECLINE OF LITHOLAPAXY.

SIR,—I have read with much interest the article in the *BRITISH MEDICAL JOURNAL* of April 11th (p. 690) by Major-General Hooton, C.I.E., Poona, India, formerly my house-surgeon at the Manchester Royal Infirmary, on the decline of litholapaxy. About six months ago my attention was drawn to this subject, for I found that in the previous ten years (1914-23), while only 6 operations of litholapaxy had been performed in the Manchester Royal Infirmary, the cases of suprapubic lithotomy were 65 in number.

On making inquiries at several of the London hospitals it was ascertained that litholapaxy, which during the war had temporarily fallen into disuse, was again coming into favour. Mr. Hamilton Bailey of the London Hospital informed me that in the ten years ending 1922, 34 cases had been treated by litholapaxy out of a total of 131 cases of vesical calculus. Mr. Geoffrey Keynes of St. Bartholomew's Hospital reports 19 cases of litholapaxy and 33 cases of suprapubic lithotomy during the same period.

In 1881 my first operation was performed for stone in the bladder. In 1910, on retiring from the active staff of the hospital, my last operation was undertaken, having in the interval operated upon 170 cases of calculus vesicae. In 62 cases, or more than one-third, litholapaxy was the operation selected. In the 62 cases of litholapaxy the operation proved fatal in only one case—namely, a male patient who was in his 73rd year.

Litholapaxy is an operation which may safely be performed at any period of life—in the very young as well as in those advanced in years. Six of the patients were between 70 and 80 years of age. Ten were under 10 years of age; in six the age did not exceed 5 years, the youngest being 3½ years of age.

My thanks are due to Mr. Geoffrey Keynes, Mr. Hamilton Bailey, and the surgical registrars at the Royal Infirmary (Messrs. Galloway and Scotson) for the above figures.—I am, etc.,

F. A. SOUTHAM,
Consulting Surgeon, Manchester Royal
Infirmary.

April 11th.

THE SEPARATION OF THE PLACENTA.

SIR,—I was very interested to read Dr. Vaughan's memorandum on the separation of the placenta (*BRITISH MEDICAL JOURNAL*, April 4th, p. 656).

For many years it has been my custom to leave untied the maternal end of the umbilical cord. The chief advantage lies in the better separation of placenta and membranes. It is difficult to say whether it lessens the risk of septic infection through the maternal sinuses.—I am, etc.,

Dublin, April 4th.

BETHEL SOLOMONS.

SIR,—When assistant in a rural district in the North of England nearly fifty years ago, I was much concerned with the large proportion of cases requiring manual removal of the placenta, and I began the practice of single ligature. I was so impressed with the advantages of it that it has been my invariable practice ever since, and it must be quite forty years ago that I explained its advantages to the class in the Rotunda, but it did not catch on. My original idea was that it lessened the bulk of the placenta, which it does, but it also allows it to fold up so that the edge presents, and it occupies less space in expulsion. I quite agree with Dr. Vaughan that it allows the uterus to contract more efficiently, and lessens haemorrhage, but also, if manual expression is necessary, it is much more likely to be successful.—I am, etc.,

Frome, April 5th.

W. G. EVANS.

SIR,—The late Dr. D. Berry Hart taught that the maternal end of the cut umbilical cord should not be tied, except in the case of twins. His recommendation was based on his theory of the method of the separation of the placenta. See his *Guide to Midwifery*.—I am, etc.,

C. J. HILL AITKEN, M.D.

Kilnhurst, nr. Rotherham, April 4th.

TREATMENT OF DIABETES BY RAW FRESH GLAND (PANCREAS).

SIR,—I was interested in the observations of Dr. Hollins and of Dr. Young in the *BRITISH MEDICAL JOURNAL* of March 14th (p. 503) and March 28th (p. 632), and decided to repeat their experiment under carefully controlled conditions.

The first case was a severe one of five years' duration—a woman aged 34, who has been treated with insulin for the last two years. For the last twenty-five weeks she has been on a constant intake of 60 g. carbohydrate, 70 g. protein, and 150 g. fat, together with 20 units of insulin, half an hour before breakfast, and 14 units just before tea. Under these conditions her blood-sugar curve has been much the same day after day (see columns 2 and 3 of table). From March 27th to 31st, both inclusive, she ate daily 2 oz. raw pancreas with her lettuce, the diet and dose of insulin remaining constant. The pancreas was obtained through the courtesy of Messrs. The British Drug Houses, Ltd. It was frozen immediately after removal from the body, and kept on ice right up to the time that it was minced by the patient, a few minutes before eating it with her meal.

Had the raw pancreas any influence on the diabetic state the blood-sugar curve on the fifth day (March 31st) would have been at a lower level, and there would probably have been symptoms of hypoglycaemia during the five days she was taking the raw pancreas. It will be seen from the table printed below that the blood-sugar curve was not appreciably affected: there were never any signs of hypoglycaemic reaction.

Hours after Insulin.	Blood Sugar, mg. per 100 c.cm.		
	March 17th, 1925: 20 units Insulin; no Raw Pancreas.	March 24th, 1925: 20 units Insulin; no Raw Pancreas.	March 31st, 1925: 20 units Insulin; fifth day on Raw Pancreas.
0	223	190	229
1	215	203	206
2	195	147	161
3	154	89	105
4	119	94	74
5	116	108	79
6	101	108	82

Note.—Breakfast 1/2 hour and lunch 3½ hours after the insulin. On March 31st, 1925, 5 grams of the protein of breakfast were taken in the form of 1 oz. of raw fresh pancreas.

A second patient, a little girl aged 4, who has been on a fixed intake of 40 g. carbohydrate, 50 g. protein, and 80 g. fat, with 35 units of insulin daily, was given fresh raw pancreas by her mother, who obtained it from her butcher herself. She arranged with the butcher that the pancreas should be delivered within a few hours of removal from the body. The child ate 6 oz. in a period of fourteen days, the diet and dose of insulin being unaltered. Neither the hyperglycaemia nor the glycosuria was in the least influenced.

It will be seen, then, that I am unable to confirm the opinion of Dr. Hollins and Dr. Young as to the value of raw fresh pancreas. From the many investigations on insulin reported in the literature, I did not expect that raw fresh gland taken by the mouth would act as a supply of insulin to these patients, but there was the possibility that while their clinical observations were correct, the interpretation of the results was a different one. I would ascribe the improvement obtained by Dr. Hollins and Dr. Young in their patients to alterations in the diet, and not to the action of raw fresh pancreas. At any rate, I maintain that it is unwise in diabetes to claim improvement as due to any particular preparation (including insulin) unless the diet be fixed rigidly and the effect of the diet alone be determined over a period of several weeks or months before trying the preparation. My reason for this is the well recognized fact that (quantitative) dieting alone may achieve such astounding results, particularly in diabetes of short duration.