of the circulation the effects of intrinsic vaso-activity are very likely to be obscured by other haemodynamic phenomena. Yet he seems unaware that it is therefore a necessary requirement of any experiment, designed to inquire into the presence or absence of vaso-activity in the lungs, that all these other factors should so far as possible be controlled. He may also be unaware that investigations which largely meet this requirement have been carried out in man by Witham and Fleming¹ using adrenaline and Fowler *et al.*² using noradrenaline.

Dr. Daley does refer to studies on the influence of acetylcholine for which I was partly responsible.³ In these studies a continuous infusion of 0.5 mg. acetylcholine/min. into the main pulmonary artery of man caused a slight fall in pulmonary arterial pressure when the subject breathed ambient air. The fall in pressure was associated with a constant cardiac output, wedge pressure, heart rate, brachial arterial pressure, arterial saturation, and ventilation. The recordings of pressure were taken at 2 and 4 minutes after the start of the infusion and the cardiac output measured at 5 minutes. During this time the infusion was maintained. I would be interested to know how he would explain such results if he does not think that acetylcholine has caused a pulmonary vasodilatation. Dr. Daley also tries the effect of acetylcholine in dogs after pulmonary hypertension has been caused by the injection of lycopodium spores. This is with the purpose of seeing whether the injection of spores has caused any pulmonary vasoconstriction relievable by a vasodilating agent. As in his previous experiment, the occurrence of cardiac arrest renders naïve any attempt to interpret the effect of the drug on the pulmonary vessels.

In the studies referred to above,³ pulmonary arterial hypertension was produced by hypoxia, under which condition Dr. Daley agrees there is substantial evidence that pulmonary vasoconstriction occurs. The effect of an infusion of acetylcholine in lowering the pulmonary arterial pressure was then found to be increased. Under these circumstances the cardiac output was slightly increased during the infusion, while the central blood volume, wedge pressure, brachial arterial pressure, heart rate, and arterial saturation remained substantially unaltered. Dr. Daley finds this "a most difficult experiment to understand." Yet the idea of measuring the effect of acetylcholine under conditions of pulmonary hypertension is common to both his and our experiments. Unfortunately the results do not seem to fit his present theories.—I am, etc.,

Birmingham, 15.

PETER HARRIS.

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Insufficiency of Internal Cervical Os

SIR,—The views of Hughesdon,¹ of Shaw,² and others, differing from those of Aschoff,³ and more recently of Danforth,⁴ serve to keep alive a stimulating controversy on the role of the cervix. Harvey bade us search out the secrets of nature by experiment; William Hunter told us to try rather than to think. The point we have laboured to make (*Journal*, July 20, p. 128) is that there is a hard core of abortion patients in whom certain clinical facts can be demonstrated, and for whom the management we suggest holds out a good chance of success. We ask others to confirm (or disprove) our findings. Up to date these are our results.

Is it only coincidence or good fortune?—We are, etc.,

V. B. GREEN-ARMYTAGE.

J. C. MCCLURE BROWNE.

London, S.W.3.

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Routine Ergometrine

SIR,—Mr. Terence Robinson (*Journal*, August 17, p. 408) refers once more to the routine use of ergometrine with the birth of the baby. We think that he misinterprets the idea in your leading article (*Journal*, August 3, p. 280) when he suggests that the implication is that its use should be enforced.

He states that it may be dangerous to mother and foetus if used by attendants who are not skilled, a far more misleading remark than that in your leading article. So far as the foetus is concerned, the only risk is that of giving ergometrine in a case of undiagnosed twins. That this danger has been exaggerated has been shown by Kurtz *et al.*,¹ who gave it in 64 such cases without ill effect. The only snag so far as the mother is concerned is a possible increased rate of manual removal of the placenta. In this connexion we agree with Mr. A. L. Deacon (*Journal*, August 24, p. 468) that many such operations can be avoided by traction on the cord, which is a perfectly safe manoeuvre if the placenta has separated. Against these can be put the real and undisputed advantages of a marked decrease in blood loss and a shortened third stage.

The important and, as yet, unanswered question is whether the routine use of ergometrine with the delivery of the baby reduces maternal mortality. It would be very interesting to know whether the authors of the *Report on Confidential Inquiries into Maternal Deaths*² can shed any light on this point.—We are, etc.,

J. G. DUMOULIN.

J. D. MARTIN.

Ivybridge, Devon.

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Acute Inversion of Uterus

SIR,—The case report by Drs. Bain and Ziolkowska under the above heading (*Journal*, May 11, p. 1102) is worthy of comment and criticism. The patient was a healthy young woman on her second pregnancy and was thrown into gross shock after the loss of only three pints (1.7 l.) of blood in the third stage of labour, which was "replaced" by a pint (0.6 l.) of dextran, two pints (1.1 l.) of blood after replacement of the inverted uterus (when her condition as a result of the manoeuvre was already dramatically improved and she was no longer in shock), and with an unstated amount of blood run in rapidly before the reduction.

I would refer the authors to the correspondence in the *Journal* of 1945 on this subject stimulated by a report by Dr. O'Sullivan¹ of a successful, though unexpected, reduction by hydrostatic pressure while washing the patient up with the old-fashioned vaginal douche routine, and especially the letter which closed the correspondence, by the late Dr. Gibbon FitzGibbon,² which completely sums up the rational approach to this problem.

The essential in the treatment of inversion of the uterus is to remove as early as possible the cause of the shock, which, in the complete case, is due to pressure of the uterine musculature upon the ovaries contained in the "inversion funnel." If replacement is attempted as soon as the condition occurs, then the cervix is still lax (as it is in early manual removal of the placenta even after the administration of oxytocic drugs) and the uterus turns right side out with surprising ease, as indeed the authors found in their case. Once the pressure on the ovaries is released the patient rapidly recovers from her state of shock, provided the process has not been too prolonged and there is no indication for replacement therapy, as shown in this case.

Where the inversion is incomplete the ovaries have not yet been trapped in the uterine pouch and shock does not occur, while with the passage of a few hours the cervix has closed down and thereafter halts the process, so that the inversion does not become complete. This was very

nicely illustrated by a case of Dr. Ninian Falkiner's (unpublished) which was seen in the Royal City of Dublin Hospital in 1938. The patient was admitted six days post-partum with the fundus inverted and appearing at the vulva; the cervix was in its usual position and firmly applied to the lower uterine segment. The patient had never been shocked and was in very good condition, the abnormality having been discovered accidentally. In this case the decision was to reduce by traction from above, and operation was delayed for ten days, during which the inversion reduced itself with the exception of the left cornu, where it was represented by a dimple in the peritoneum; the abdominal appearance of the uterus and adnexa was completely normal and there were no adhesions.

In their comment the authors state that the only factor which may have contributed to the inversion was traction on the foetal membranes, which was very gentle. I venture to suggest that this merely completed the inversion which had been initiated by the mother's expulsive efforts in expressing the placenta, for I doubt that the requirement of a firm contraction of the uterus, before using it as a piston, would be always fulfilled if the placenta is self-expressed in this manner. The mechanism suggested above is given by DeLee,³ who also mentions traction on the membranes as a cause.

To credit the exhibition of dextran as life-saving would appear far-fetched: if the attendant had replaced the uterus to its proper position as soon as the condition was recognized the recovery from the shocked state would have been just as dramatic, and it could then have been decided whether the patient required the further transfusion to replace the blood lost at the time when the uterus was congested, by interference with the venous return and while the maternal sinuses were still open in the atonic uterus. Whether or no the facilities of a well-equipped hospital are to hand at the hour of 3 a.m., it is well to remember that the first treatment of shock is to remove the cause where it is known, as it is in these cases, and to stop the bleeding in post-partum haemorrhage by simple measures while someone else worries about replacement therapy.

The prophylaxis of these cases lies in the careful conduct of the third stage of labour and knowledge of the mechanics of separation and expression of the placenta with restraint from interfering, either manually or by voluntary increased intra-abdominal pressure, with the uterus unless it is in a state of contraction.—I am, etc.,

Brantford, Ont., Canada.

HENRY FITZGIBBON.

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Listerial Meningitis

SIR,—With reference to the article by my colleagues and myself (*Journal*, July 27, p. 188) and to the letter by Drs. C. S. Shaw and D. C. Turk (*Journal*, August 10, p. 351), may I add the following information?

I have tested a culture sent to me by Dr. Turk isolated from the case of "diphtheroid" meningitis reported in 1954¹ and can confirm his later conclusion that the organism is in fact *Listeria monocytogenes*. It gave a positive Anton's test (rabbit conjunctival test), was (poorly) motile, grew in 6.5% NaCl broth, grew in the refrigerator in one week, showed the usual Listerial morphology, and the sugar reactions conformed with the results already obtained by Turk, these being compatible with the diagnosis of *Listeria monocytogenes*. Furthermore, H and O antigens reacted to titre with factor-specific sera, giving the result type 1. The only discrepancy found was its failure to produce lesions in either of two guinea-pigs injected intraperitoneally with cultures of the organism. This could be due to the prolonged cultivation since isolation of the strain.

To modify our statement that no type 2 strain had been isolated from animals, may I add that information has since reached me from Dr. J. Donker-Voet, of Utrecht, that

she has isolated two such strains from chickens in the Netherlands? Dr. Donker-Voet has sent me these strains as well as a further strain C1878, isolated from a child dying of meningitis at Rotterdam, aged four months. All these were confirmed here as type 2.—I am, etc.,

Edinburgh.

P. N. EDMUNDS.

REFERENCE

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Diet in Idiopathic Steatorrhoea

SIR,—Your readers may be interested in some further information on the patient (Case 1) to whom Dr. Alexander Brown referred in his recent paper (*Journal*, August 10, p. 337). She came under our care in 1956. For the previous eighteen months she had been eating an ordinary diet and her health had deteriorated. When first seen by us she was tired, unwell, and unable to work. She had had a series of painful ulcers in her mouth and had lost some 4 kg. in body weight during the previous year.

Investigation showed that she had steatorrhoea—mean value for daily faecal fat 8.1 g. (range 4.06–11.23). The stools were usually pale and semi-formed. The glucose absorption curve was flat (maximum increment 16 mg./100 ml.), xylose absorption depressed (8% excretion in 5 hours), although the chylomicrograph was relatively normal. Radiographic study of the small intestine showed a flocculation pattern and some dilatation. Examination of the blood revealed a moderately severe macrocytic anaemia (Hb 9.6 g.%; R.B.C. 2.55 million/c.mm.; W.B.C., 3,500 c.mm., with slight relative lymphocytosis; P.C.V. 29%; M.C.H.C. 33%; M.C.V. 113 cu. μ). These findings, together with Dr. Brown's previous studies, strongly indicated that she was suffering from gluten-induced enteropathy. This was confirmed by the results obtained with a gluten-free régime and also by a positive test on the effect of gluten.

On the gluten-free régime, with no other treatment, she rapidly improved. Reassessment six months later showed that she had regained her normal body weight, steatorrhoea was no longer present—faecal fat: mean 4.9 g. per day (range 4.19–5.96); the stools were normal in colour and consistency. Xylose absorption was normal (27.9% and 29.5% excreted in 5 hours). Blood examination showed marked improvement (Hb 12.2 g.%; R.B.C. 4.5 million/c.mm.; P.C.V. 40%; M.C.H.C. 30%; M.C.V. 88 cu. μ). There was now slight microcytosis and this was subsequently corrected by oral iron therapy. During this period of reassessment wheat gluten was introduced into her diet as an autoclaved peptide fraction that was known to be deleterious in patients with coeliac disease. The mean faecal fat value for the three three-day means prior to the administration of the gluten fraction was 4.8 g. per day, and for the three days following the introduction of gluten it was 11.2 g. per day.

It may therefore be concluded that this patient fulfils all the necessary criteria for a diagnosis of gluten-induced enteropathy. The condition appears to have been present since it was revealed in childhood, and there is every indication that she can only expect to maintain normal health so long as she adheres to a gluten-free régime.—We are, etc.,

A. C. FRAZER.

R. F. FLETCHER.

Birmingham.

Examination of Tuberculosis Contacts

SIR,—There seems to exist on the part of a few chest clinics a curious reluctance to examine contacts of patients with pulmonary tuberculosis. Three examples that have recently come my way have served to point to this fact. A young man was found on routine x-ray in the Service to have extensive involvement of one lobe which was almost certainly of long standing; six months previously one of his sisters had developed a pleural effusion, but no attempt was made to x-ray the other members of the family. Another young man was admitted with gross disease involving the whole of one lung and sputum loaded with tubercle bacilli; his family were notified as contacts, but