blood pressure fell from 140/70 to 110/70. An intravenous blood transfusion was given throughout the operation, which lasted an hour, into a vein on the dorsum of the right hand which ran superficial to the space between the fourth and fifth metacarpals. At the end of the operation the fourth and fifth fingers were found to be cold and purple in colour, the tips of the fingers being dead white. Warmth was applied to the hand in the ward, and the drip continued for another hour, without improvement in the appearance of the two fingers. The drip was changed to a proximal vein in the other arm, with immediate return of circulation in the right hand.

In Case 1 the patient's peripheral circulation was minimal, and the diminution caused by the cold blood entering the limb was sufficient to precipitate gangrene. Case 2 might have ended in the same way if the transfusion had been continued long enough and if the patient's general condition had not improved.

These cases are in no way comparable to that of Dr. Sutton's, in which the stimulus causing spasm was an intravenous needle lying in close proximity to the arterial wall and acting as an irritating foreign body. In the above cases the stimulus was that of cold acting upon a circulation which was already impaired. But they serve to draw attention to the danger of using peripheral veins to transfuse blood, for long periods, in patients whose general condition is poor.—I am, etc.,

Isleworth, Middlesex. A. B. VAUGHAN.

Lumbar Intervertebral Disk Protrusion

SIR,—In view of recent correspondence about "painful back" I believe Dr. D. M. Graham-Service (October 18, p. 882) may be on the right track. I am with him in condemning back supports: it seems to me there can never be support of the back when the belt is partly enveloping the movable abdomen. All the same, I feel he has missed the point in endeavouring to replace bony structures by manipulation, and even when he has done so I fail to see how he can be certain that these have been replaced micro-accurately, if I may use the term. I have no doubt he is right to try to correct the posture, but I feel he is wrong in directing treatment to bones and joints rather than to muscle groups. If for any reason there is postural alteration of bones or of the joints related to the back, it seems quite obvious that muscle groups must then take on the permanent job of trying to hold the pre-injury posture.

It would seem, therefore, that we have a condition of chronic muscle spasm together with an associated ache. Local analgesia benefits most of these cases more than anything else except rest in bed, but with neither treatment do we get permanent relief. Certain surgeons seem to be showing some advance in their ideas of lengthening the tendo achillis, but they do not seem to realize that by so doing they allow the individual to take his weight more on the tarsus than on the heel. I have found great relief in most cases by a much simpler method—namely, by raising the heel of the man's shoes ¼ in. (0.6 cm.) or more.

For some unfathomable reason pitching the balance forward tends to relief of symptoms, and I think that here is a great opportunity for someone with ample time and facilities to investigate this approach. On the whole this gives more permanent relief than any other treatment I have investigated.—I am, etc.,

London, S.W.1. VINCENT HYSLOP.

SIR,—I cannot let Dr. D. M. Graham-Service's letter (October 18, p. 882) pass without comment, because it contains two statements which do disservice to our patients. In discussing diagnosis Dr. Graham-Service says that "apart from these two methods (i.e., operation or myelography) I do not think that an absolutely definite diagnosis of disk protrusion can be made." He goes on to suggest other causes for pain, and continues: "Here I agree with Mr. Henderson that because the condition is essentially subjective it is difficult to assess the results of conservative treatment." It would appear from these two statements that the criteria on which the diagnosis is made by these two gentlemen are inadequate.

I believe it is true to say that no surgeon with any experience would operate on, or treat by any other method, a patient thought to be suffering from a disk protrusion unless there were ample objective signs that this was the cause of the symptoms. The first set of signs are those relating to the spine, and concern its posture, its range of movement, and the presence or absence of tenderness. In their absence one seeks for some other cause of the sciatica. The second set of objective signs relate to the lumbo-sacral plexus of nerves. The most important are the alteration of a reflex or of skin sensation. I do not forget the importance of *x* rays and examination of the cerebrospinal fluid, or the great stress to be laid on a detailed history, or the assessment of the patient's emotional state and domestic, social, and industrial background. I take these for granted.

It is possible for a patient to have pain referred down a leg, but in the absence of signs indicating that one or more nerve roots are involved there must be some other cause than that of a protruded intervertebral disk, or any other lesion—such as an abscess or tumour—capable of involving the nerve root. It must, I think, be the experience of every surgeon that subjective sciatica has been caused by lesions of joints and in particular of the sacro-iliac joint; while it has recently been reported that osteoid osteoma, either of the vertebral body or of the neck of the femur, has been operated on mistakenly as cases of protruded intervertebral disks.

Finally, I have often been struck by the spelling of "disk" in your *Journal*. According to my anatomy book it should be spelt "disc," and according to my dictionary a "disk" is a flat circular surface, and a "disc" is a flat circular object. There is not the slightest shadow of doubt that the disks on which I have operated were objects and not surfaces.—I am, etc.,

Manchester, 3. W. SAYLE-CREER.

*** *The Shorter Oxford English Dictionary* remarks: "The better spelling is disk."—ED., *B.M.J.*

Gluten in Adult Coeliac Disease and Idiopathic Steatorrhoea

SIR,—The new light which has been cast upon the problem of coeliac disease by Dicke[1] and others in Holland, and later confirmed by the Birmingham workers, Anderson *et al.*,[2] has stimulated renewed interest in the problem of the malabsorption syndrome.

We have been interested to learn the effect of gluten on adults suffering from the malabsorption syndrome, for we have recently had in our wards two females, aged 42 and 30, who gave a history of diarrhoea in childhood, and are now of the petite size which may be found in those whose growth has been hampered by coeliac disease. They have been proved cases of fat malabsorption as the result of repeated fat balance studies. It seemed to us that the same improvement should occur in these patients when wheat flour was removed from the diet as occurs in children. They were therefore placed on a diet free of wheat flour. Neither of the patients was suffering from diarrhoea at the time, and this diet made no alteration to their bowel habit. Fifteen grammes per day of gluten, which would represent the content of about ½ lb. of bread, was then added to the same diet for a period of six days. The patients were unwilling to continue with the diet for longer. In neither was there any loosening of the bowels during this period. The fat absorption figures during six days on a gluten-free fat balance diet, and again during six days on the same diet with added gluten, were respectively 81.8%:81.3% for one patient and 84%:88.5% for the other. Both these patients had been on a maintenance dose of pteroylglutamic acid for some time, and this was continued during the experiment.

A third case of coeliac disease, 17 years of age, had had symptoms of the disease since the age of 11 months, and had been in hospital many times subsequently. We gave her gluten 15 g. to take every day at home, in addition to her usual food. After six weeks of this she reported that

she had in no way been upset by the gluten, and indeed had gained a little weight. Two adults suffering from idiopathic steatorrhoea showed no clinical change when given a wheat-gluten-free diet for 11 and for 21 days respectively, nor when gluten was added to the same diet for a similar period. Fat-balance studies showed absorption of 48.2% and 45% respectively by the two patients on gluten-free diet, and 60% and 53% respectively when gluten was added to the diet. Five other patients suffering from the malabsorption syndrome who were being maintained on the usual low fat, restricted carbohydrate, sprue diet were given gluten 15 g. daily, to be taken with their food for six days. In no cases did this addition cause any deterioration in the bowel habit, or discomfort.

If it is believed that wheat gluten has a deleterious effect in the adult or adolescent case of coeliac disease or in idiopathic steatorrhoea, and that this effect is due to an idiosyncrasy or allergy, it would be expected that some sign of improvement would be noted when the wheat gluten was removed from the diet, but this was not found to be the case. If the effects were due to a toxic action, further deterioration would be expected when an excess of gluten was given, but this did not appear to be so. Indeed, no evidence was obtained from this investigation to suggest that gluten was playing any part in the causation of the fat malabsorption syndrome. It must be noted, however, that the test periods during which gluten was deleted or added to the diet were usually of short duration (six days) except in three cases, and the possibility exists that different results might have been produced if all the experiments had been continued for several weeks.—I am, etc.,

Edinburgh. H. T. SWAN.

REFERENCES

1 "Coeliakie," Utrecht, 1950, M.D. thesis.
2 *Lancet*, 1952, 1, 836.

Asthma in Childhood

SIR,—I would like to enter into verbal combat with Dr. Cyril Josephs (October 18, p. 881) concerning bronchial asthma in children. The most convenient way of viewing this condition is to divide it into three groups; (1) true allergic asthma (I hope to demonstrate to your correspondent that such a condition does exist), (2) asthma associated with psychological disorders, and (3) asthma secondary to other lung conditions such as bronchitis and sometimes bronchiectasis.

From our practice, which is chiefly industrial, I took 10 children, all of whom suffered from severe recurrent attacks of bronchial asthma, and carried out dermal sensitization tests on them. Seven showed reaction to a specific allergen, and in each one of these seven cases a positive family history of asthma or an associated allergic disorder was elicited. In four of the subjects eczema, flexural in site, was also present. Of the remaining three cases, those not reacting to the tests, no positive family history was obtained. In two of these a strong psychological element was present, and in the remaining case the little boy was a sufferer from chronic bronchitis.

The cases showing a sensitivity were all cured with courses of desensitization with the particular allergen and have been followed up for three years. The associated eczema cleared up in all but one case. From the three non-allergic subjects I took a control and administered a course of normal saline injections without effect.

I found the provoking substances were all of the inhalation group, and I have not tried to eliminate them environmentally. Of course the series is small, which it really must be in general practice, but it has convinced me at least that allergic asthma does exist and may respond to such type of treatment as has been discussed. I quite agree with Dr. Josephs that a psychological element may be found in many sufferers from the condition, but these are surely distinct from the true allergic phenomena, though sometimes they may co-exist.—I am, etc.,

Derby. BERNARD JACOBSON.

Perseverance in Artificial Respiration

SIR,—Dr. F. C. Eve (October 18, p. 879) asks for details of cases of long-continued artificial respiration. We can supply at least one authenticated case. The late Mr. W. A. S. Calder, of Chance & Hunt, Ltd., was tireless in preaching the value of continued effort, especially in connexion with hydrogen sulphide, and in November, 1934, the Association of British Chemical Manufacturers issued a special Safety Circular No. 70, from which is taken the following:

. . . "particulars of evidence which has led him to conclude, from his extensive wartime experience of cases of gassing by hydrogen sulphide, that by continuing efforts of resuscitation, long after all reasonable hope might appear to have been abandoned, fatalities may almost invariably be avoided. He informs us that there was a very large number of men who were rendered completely unconscious by this gas, yet there was not a single fatality among them, although in some cases it required many hours of hard work to bring a man round."

An account is then given of a case on June 9, 1910, when a worker was overcome by hydrogen sulphide in a canal boat and lay unconscious for probably half an hour before he was discovered. After Schaefer artificial respiration for some three hours on the canal bank, this treatment was supplemented by oxygen. Mr. Calder went on to say:

"After several hours' further treatment, the man revived and, as soon as possible, hot coffee was given; he was hauled to his feet and made to walk about with a man on each side holding him up. He gradually recovered, regaining the use of his limbs, and was then wrapped in blankets and sent to hospital. He returned to work on July 16. Apparently the accident had no permanent ill effects, although he was described as a man who was particularly susceptible to asthma and bronchitis previous to his accident."

The concentration was considered to be at least 1% of hydrogen sulphide. The doctor in attendance reveals from his diary: "I was called at 7.30 a.m. and stayed until 8.30, then I had to leave, but came again at 10 a.m. and remained with him to 1 p.m., when he began to come round. I left then, but was back at 2.30, and at 3.15 he was removed to hospital. He was entirely unconscious for over six hours, and I am sure that it was only the continuous application of oxygen that saved him."

Mr. Calder also reported that on another occasion seven men became unconscious after an escape of gases containing 35% of hydrogen sulphide. In this case the men were all brought round without the use of oxygen. The worst of these cases took "several hours' treatment, yet the man some years afterwards went on active service in 1914."

The Safety Circular ends by remarking that, although the above experience is confined to the effects of hydrogen sulphide, it suggests that it would be worth while in cases of poisoning by other gases to continue artificial respiration long after it would seem to be hopeless. It is realized, of course, that these results are partly due to the peculiar action of hydrogen sulphide, but they do seem to support the value of long-continued artificial respiration.—I am, etc.,

Association of British Chemical Manufacturers, London, W.1. ALLAN J. HOLDEN.

The Harveian Oration

SIR,—While of course agreeing with Lord Moran in wanting "to purge the practice of medicine of all those elements of humbug," there is a very great danger indeed in making it unnecessary to win the favour of the patient. May not the pendulum swing the other way and the patient's confidence be lost? One hears so frequently: "I was told nothing," "I see a different doctor each time," "It means waiting so long, so many tests and examinations and then being told nothing," "I never see my own doctor, always an assistant," etc.

Indeed, the last sentence of the Oration, as reported, might have been in large block letters, for the human touch is fast vanishing from the Welfare State. The sentence was as follows: "The danger in this was that in the cultivation of medicine as a science the physician might neglect medicine as an art."—I am, etc.,

London, N.W.8. HENRY CHURCHILL.