of low back and sciatic pain, as they are of similar pain at higher levels of the trunk and in the neck and upper limbs. The distinguishing character of sciatic and other radiating pain from these lesions is that it is purely "referred," not accompanied by objective neurological signs, though there may be subjective symptoms. Appropriate orthopaedic treatment, after determination and elimination when possible of aetiological factors such as focal sepsis, gives relief of pain in most cases.

Sciatic pain is far less often due to irritation of the roots or trunks of the lumbo-sacral plexus. Such irritation may arise from a rupture into the vertebral canal of the intervertebral disk between the fourth and fifth lumbar or the fifth lumbar and first sacral vertebrae, or from perineuritis, as well as from rarer lesions such as neoplasms and arachnoiditis. I believe that posterior disk rupture is not so common as some enthusiasts consider, and that there is a danger of many spines being operated upon unnecessarily if cases of persistent sciatic pain are thought to be only neurosurgical, and if reliance in diagnosis is placed only on neurological signs. Having made, as a B.M.A. Research grantee, between 1926 and 1931 a study of the intervertebral disks and of the work of the few others in the world interested in the subject at that time, I have since then been on the look-out for cases of posterior disk rupture. Yet between July, 1937, when I diagnosed and treated perhaps the first case operated upon in this country, and the present time I had only 20 certain cases of this lesion (including one in the cervical and one in the thoracic region) out of some 4,000 cases of backache with or without radiating pain—that is, 0.5%. Considering only cases with sciatic pain, the incidence of disk rupture would, of course, be higher, but probably not more than 2% of all such cases. Disk rupture is scarcely to be considered as a possible cause of pain of recent onset in patients over the age of 40 years, as by then the disks have become too fibrous to be ruptured. Between the ages of 20 and 30, when the disks are highly elastic and still have soft centres but the spine has not quite the same resilience to trauma as in earlier years, is the period when disk rupture and the pain it may cause are most likely to occur.

Perineuritis of the sciatic nerve roots—so-called true sciatica—may never have been demonstrated, but I believe that it does occur, though it is certainly one of the less common causes of sciatic pain. By sciatic perineuritis I understand a condition with an identical clinical picture to posterior disk rupture but distinguishable from it. Both show, in addition to the typical pain in the limb, impaired or absent sensibility to pain and touch in the same distribution to a greater or less extent, diminished or absent ankle-jerk, and deviation of the trunk to the affected side in all positions or at least in bending forward when standing. In my observation these are the only states of sciatic pain which give pain *in the leg* on straight leg raising: in sacro-iliac lesions the pain is localized to over the sacro-iliac joint; in fibrositis and sprain in the gluteal or lumbar region it is also localized and corresponds with the tender points; other causes of sciatic pain do not give this sign at all. Myelography with iodized oil is the only positive way of distinguishing between perineuritis and disk rupture, as it shows up the rupture, but it is not infallible and should probably in any case be followed by laminectomy, as the oil may cause arachnoiditis if left in. For this reason it should be the last means of diagnosis after all causes of pain other than disk rupture have been eliminated. Another way of distinguishing between these two states is by epidural injection of local anaesthetic in saline into the sacral canal. The symptoms of many cases of perineuritis will disappear spontaneously with the elimination of remote focal sepsis. If they persist, epidural injection is usually effective immediately and permanently, but it has no effect in cases of disk rupture. I have made this injection in many cases and am sure that the effect is not merely psychological. It is not certain how it acts any more than it is clear how other medical empirical treatments of proved efficacy act.

Of course there is often a psychological factor in sciatic pain, but it is no more significant in such cases than in other clinical states. To maintain that it is the chief or only factor is an attitude of despair—or of vain hope—towards an important cause of suffering and disablement. To put patients to bed without an exact diagnosis may by chance relieve the pain of a few, but for most wastes time and money and impairs the prospect of obtaining lasting relief by thorough investigation and treatment.—I am, etc.,

Bournemouth. N. ROSS SMITH.

Sciatica

SIR,—Sir Arthur Hurst, in his masterly survey of sciatica (Dec. 18, p. 773), appeared to establish the existence of a hysterical background—i.e., a psychical factor in the cause of the disorder—but in his remarks on treatment he entirely ignored any psychological approach to this factor. Physical trauma or exposure to the elements were assumed to be the cause of this admittedly semi-hysterical disease, and the psychological aspect was dealt with by forms of suggestion, more or less drastic, and directed towards the developed condition rather than towards a possible psychological cause.

The trauma complained of by the sciatica patient is reminiscent of the individual who contracted gonorrhoea by "straining himself," and the exposure, usually an imaginary draught, is similar to that blamed for the onset of facial neuralgia. An incontrovertible, severe, physical catastrophe—such as the kick of a horse, a fall from a two-story building, or a night spent lost on a mountain—rarely precedes either disease.

Those of us who have acquired a psychosomatic outlook believe that not only is the discovery of infidelity in a wife or the fear of impending bankruptcy more potent to lead us to becoming generally "run down" than excessive physical exercise, but that particular forms of mental strain and emotion appear to cause stress on specific parts of the organism, and result in definite types of disease or disorder. Treatment then indicates investigation with a view to unveiling: "What did he experience just before he took sciatica, and what emotions did he feel?" While it is true that a happening sufficiently severe to cause physical illness is invariably repressed and positive transference must be established to find it, the quest does not demand forty séances. It is often possible to reveal the upset and discuss the patient's future adjustment to it, all inside half an hour, especially if one has some knowledge of the emotion associated with the disorder under consideration.

A. B., an actor, comes home from tour to find his wife leaving the house with another man (on her way to take up residence with him) and taking her three children with her. Within a few days A. B. has pain extending from the left hip to the great toe. Two weeks later he is sent to hospital, where he receives treatment covering a wide range. He remains off work for five months, and in the course of the following six years has many recurrences, being off work in all nearly eighteen months. Superficial analysis of his thoughts and emotions: "The foundation of his life was no longer secure; deprivation of loved persons; loss of self-respect; desire to strike back." The experiences of patients just preceding the first attack of sciatica may be of infinite variety, but the emotions and feelings are of the same order. Permanent cure follows the regaining of solid ground under his feet with capacity for adjustment to his future.—I am, etc.,

Glasgow. G. GLADSTONE ROBERTSON.

The Cardiac Index

SIR,—Mr. Harold Dodd in his paper on the cardiac index (Dec. 25, p. 811) remarks that "it is seldom taught, and, because of this, when it is mentioned it meets with some scepticism." What this index represents is not clear; empirically it is stated that values above 100% and below 25% make the patient a poor surgical risk. "Abdominal distension, chestiness, pulmonary embolism, delirium, strangeness of manner, or sepsis" are given as post-operative sequelae of a too low or too high cardiac index.

To quote his example: "In elderly people the blood pressure is often around 140/70, giving a cardiac index of 100%" (i.e., abnormal and a contraindication to operation). Such a pressure is, of course, perfectly normal in an elderly patient; but he goes on to say that "pre-operative rest in bed for one, two, or three weeks, with a fluid intake of 6 to 8 pints daily and cardiac tonics, approximates these figures to, say, 130/80." It is well known that variations of ten points either

way may occur from day to day and from observer to observer; certainly such changes of themselves could not be taken to indicate any alteration in the efficiency of the cardiovascular system. What cardiac tonics are used? And what are "gentle" cardiac tonics? Why 6 to 8 pints of fluid a day? It is difficult to understand how such quantities of fluids are going to benefit a patient with fibrillation, as restriction of fluid is one of the standbys of treatment of the failing heart. Clinical details are not given, but if these patients were not in failure, then cardiac tonics and excess fluid do not seem to have been indicated for patients where fibrillation was apparently paroxysmal. In any case I imagine that different observers would arrive at widely varying cardiac indices when confronted with a fibrillating heart.

The statement that "the blood-pressure figures in themselves mean little" is most disturbing. A simple example will indicate the danger of such an assumption. A patient might be in marked left ventricular failure with a blood pressure of 200/140. This would give, according to definition, a cardiac index of 43%, which indicates a "fair surgical risk." Were the cardiac index to be generally adopted as an important criterion of operative risk—and, as Mr. Dodd makes little mention of other findings in his patients, one assumes that he attaches great importance to it—one hesitates to think of the large number of patients denied operation when urgently needed, and the equally large number who would be operated on when in no fit state.—I am, etc.,

Botleys Park, Surrey. G. R. FEARNLEY.

Blood Pressure in Surgery

SIR,—All anaesthetists will agree with Mr. Harold Dodd (Dec. 25, p. 811) in stressing the importance of continuous blood-pressure and pulse-rate recordings during major operations. It is hoped that the combined anaesthetic charts and record cards designed by Dr. M. D. Nosworthy and now being tried out in many hospitals will soon become universally used.

Mr. Dodd places considerable reliance on the "Moots-McKesson cardiac index," saying that if it is below 33% or over 85% the post-operative outlook is poor. Unfortunately anaesthetists generally have been unable to place much confidence in any of the usual cardiac efficiency tests, such as those of Barach, Crampton, Moots, and Sebrasez. The Crampton test would seem to be the most scientific and has been adopted by the Athletic Association of America. It is not, however, applicable to bedridden patients. Dr. W. S. Sykes made an investigation of Barach's and Moots's tests in patients dying from cardiac failure in the medical wards of a large hospital. In no less than 62% of tests the results were misleading, one patient being deemed "operable" by both formulae 15 minutes before death. Might I suggest, therefore, that it is unwise to put these so-called tests and indices before clinical observation and experience.

It is not always realized that there is no such thing as a patient who is "so ill that he cannot stand the anaesthetic." Any patient with cardiac failure who is not *in articulo mortis* will improve temporarily under an inhalation anaesthesia, which begins with 100% oxygen in a closed circuit to which the anaesthetic agent is gradually added in minimal dosage. The question which so often faces the modern anaesthetist is not "Can this patient survive my anaesthetic?" but "Can I improve his general condition sufficiently to enable him to withstand the surgical trauma and to live long enough to benefit by the effects of the operation?" If the answer is not a reasonably certain "Yes," surgical interference is surely unjustifiable.—I am, etc.,

St. Albans, Herts. C. LANGTON HEWER.

The "Surgical" Heart

SIR,—In his article in your issue of Dec. 25 Mr. Harold Dodd states that the Moots-McKesson "cardiac index" meets with some scepticism because it is seldom taught. It seems possible, however, that its unpopularity is based on a natural distrust of any formula which seeks to reduce a complicated problem to a simple rule-of-thumb. Measurement of the arterial blood pressure cannot reasonably be expected to replace the need for an accurate cardiac diagnosis, even for surgical purposes.

It is common to find a normal cardiac index although gross cardiovascular disease is present; this is so in many cases of coronary arterial disease (including not a few cases of recent coronary thrombosis), in most mitral lesions, in cardio-aortic syphilis without aortic incompetence but with or without aneurysmal dilatation (which may be on the point of rupture), and in the majority of cases of congenital heart disease (possibly with bacterial endocarditis). It would not be justifiable to consider all such cases fit for operation because the cardiac index was normal. On the other hand, a common cause of a high cardiac index is the rise of systolic pressure which so often occurs during examination, and this is, of course, not a contraindication to operation. On the basis of the cardiac index alone a large proportion of cases of thyrotoxicosis would be regarded as unsuitable for operation. Patients with aortic incompetence, aortic stenosis, patent ductus arteriosus, or peripheral arteriosclerosis should not be regarded as unfit for operation solely on account of the abnormal relationship of systolic and diastolic arterial pressures.

No one could take exception to the suggestion that the blood pressure should be taken as a routine before operation, but this should surely be regarded as one item in the routine pre-operative examination of *all* systems and not as replacing the "clinical examination of the heart and pulse."—I am, etc.,

A. MORGAN JONES.

Cardiographic Department, Manchester Royal Infirmary.

Digitalis a Cardiac Tonic?

SIR,—I have read with interest Mr. Harold Dodd's article on blood pressure and the cardiac index. I must protest at his inclusion of digitalis as a "cardiac tonic." It is well known that digitalis is a cardiac poison, and it is to the effect on the conductivity of the neuromuscular tissues that its therapeutic action in auricular fibrillation is due. Linnell and Thomson (Nov. 6, p. 573) have indicated its uselessness in many other forms of cardiovascular defect.—I am, etc.,

Seascale. L. GOLDMAN.

Treatment of Facial Palsy

SIR,—In his letter to the *Journal* of Dec. 25 Dr. Wilfred Harris has taken the statement, "The test for faradic excitability does not provide any useful information in forming a prognosis," apart from the context and has expressed his disagreement. The statement is not given as the opinion of the annotator but is the conclusion reached by Dr. Karsten Kettel from his investigation of the late results in 264 cases of facial palsy, and it is therefore fair to give his reason for reaching this conclusion. He says:

"In half my cases the paralysis was partial and the faradic test presumably positive, even if in a greatly lowered degree. Since the final result in half the cases of partial paralysis—that is, one-fourth of the total number in this group—was nevertheless bad, it is obvious that the faradic test cannot be a reliable indicator. It is possible that a completely negative test may provide a hint. The fact that the faradic excitability is only reduced does not permit any prognostic conclusion."

On the other hand, the long experience of so distinguished a neurologist as Dr. Wilfred Harris is conclusive. It seems, however, that Dr. Harris refers to cases of Bell's palsy without middle-ear disease, whereas the cases followed up by Dr. Kettel were all secondary either to middle-ear disease or to operative treatment for its relief, and consequently were cases with a greater or less degree of pathological change in the temporal bone as well as in the nerve itself. This may perhaps explain the discrepancy between the observations of Dr. Harris and of Dr. Kettel, and it would no doubt be of interest to your readers to know if this explanation commends itself to Dr. Harris.—I am, etc.,

YOUR ANNOTATOR.

Blast Perforation of Ear-drums

SIR,—In the *Journal* of Aug. 21, 1943 (p. 233), Capt. G. W. Palmer reports his findings in 60 cases showing blast perforation of the ear-drum, and concludes that the proportion of infection to be expected is 35 to 40%, "under the most ideal conditions." Some months ago I had the opportunity of examining a similar, though smaller, group of cases, when ten men were admitted to a military hospital following an