whatever that an asylum of some sort is the best place for the patient, but he need not be certified as insane, at any rate until it is seen whether the insanity is likely to be permanent or not." Dr. Raw is not alive to the practical difficulties of dealing with insane persons without certification. The law which would render an insane person liable to be sent to, and detained in, an asylum, deprived of the control of his person and of the management of his property, without certification, would lay the same liability upon a sane person, and without certification every safeguard of the sacred liberty of the subject would be utterly swept away. I am unable to appreciate the distinction drawn by Dr. Raw between insanity on the one hand and "temporary mental aberration the result of bodily disease" on the other. It is but rarely that we can be quite sure that mental aberration will be of short duration, and even if we could usually do so, and even if we could agree upon the precise duration which is to be termed temporary, there remains the fact, or what I believe to be the fact, that, whether "temporary" or permanent, every manifestation of insanity is the result of bodily disease. With Dr. Raw's conviction that a large number of people are certified as lunatics who are simply suffering from insanity the result of, or associated with, some form of bodily disorder or of poisoning, I should be in complete accordance; but when he contends that such persons are not really insane, I should join issue with him, for I should contend that the "large number" includes every insane person who is certified as such. In short, I deny altogether the existence of Dr. Raw's distinction between mental aberration and insanity. All that I recognise is a difference in degree. Every case of mental aberration or insanity is, in my view, the result of, or associated with, some form of bodily disorder or of poisoning; and I protest with all the vehemence of which I am capable against the view that insanity is a thing apart, a specific entity, having no community of nature with trifling and temporary "aberrations of mind." This view is a lingering remnant of the belief in the demoniacal possession of the insane and is the most serious obstacle to the right understanding of insanity. That it should be held by a man of Dr. Raw's ability and eminence goes far to excuse the otherwise unpardonable length of this communication.

I am, Sirs, yours faithfully,

Catford, S.E., June 16th, 1902.

CHARLES MERCIER.

OBSERVATIONS ON DIET.

To the Editors of THE LANCET.

SIRS,—Probably Dr. Harry Campbell's shoulders are sufficiently broad to withstand the onslaught which Dr. F. W. Forbes Ross has thought proper to direct against him in THE LANCET of June 14th, 1902, p. 1724, on the subject of his "highly theoretic and erudite contribution" with reference to the feeding of old people. Nevertheless, as one whose practical experience bears out and confirms Dr. Campbell's views I may be permitted to enter the lists and shiver a lance in the cause of "physiological feeding for the aged"—an expression which, I gather from Dr. Ross's letter, is the same thing as the metabolism of starvation.

I find myself in complete accord with Dr. Ross's observation that "it is quite time we as a profession began to revise our practice and preconceived notions in certain directions where diet is concerned," as also with his clinical experiences, but I take exception to the deductions and generalisations which he draws from the latter. It is in the highest degree improbable that a man living on "the fat of the land" or even on a "generous, good, full diet" (*vide* Dr. Ross's dietetic canons), with lung, kidney, and heart perhaps as the sequelæ of this mass stimulation, would derive any immediate benefit from an ill-considered restriction of diet; indeed, deprived of the mass stimuli to which his metabolism (*pace* Dr. Ross) has been educated and accustomed I should expect the rapid dissolution which Dr. Ross so pathetically deplores. But what is this, after all, other than the phenomena which appear in the alcoholic or morphomaniac when his particular variety of poison stimulus is reduced to a physiological and pharmacopœial dose? I imagine it would not be seriously contended that a full, generous exhibition of either alcohol or morphia would be productive of longevity because when deprived of them suddenly their victims "sink rapidly to dissolution." *Mutatis*

mutandis, the argument is, I trust, a sufficient reply to Dr. Ross's *verb. sap.* I am, Sirs, yours faithfully,

London, June 13th, 1902.

ERIC PRITCHARD.

To the Editors of THE LANCET.

SIRS,—I have had very considerable experience in dieting the aged during the last 20 years. My experience gained is quite the opposite from Dr. F. W. Forbes Ross's views, expressed in THE LANCET of June 14th, and is more in accord with those of Dr. Harry Campbell if I read correctly what he intends to convey to the reader by the term "spare diet." The aged do not require quantity but quality in their foodstuffs and a full diet is generally harmful to the majority of aged patients. I always prescribe a diet rich in nutrient substance, easy of digestion and assimilation, but spare in quantity. The organs of aged people are more or less worn out, and this refers more particularly to the digestive organs, so that if you tax them with foods which require much digestion and contain a large quantity of waste material fermentation is sure to set in—the very bugbear of old age—which produces most dangerous and unpleasant symptoms.

How many aged gentlemen who dine well but not wisely at their clubs have gone home and died suddenly after a full meal and how many suffer from most painful gouty affections from living on a full diet. Of course, there are many exceptions. My old friend Dr. Pickett's father, who died at the age of 95 years, always used to eat a hearty meal of bread, cheese, and ale before going to bed. My great-grandmother, who lived to 93 years of age, had three full meals a day. We are generally called to diet the feeble and weak, who, I am afraid, form the majority, not the minority, of the aged. The less chance of auto-toxin there is by there being little waste material in the diet the greater will be the success of our treatment.

I am, Sirs, yours faithfully,

THOMAS DUTTON.

Manchester-square, W., June 14th, 1902.

SORE-THROAT AS A PREMONITORY SYMPTOM OF ACUTE RHEUMATISM.

To the Editors of THE LANCET.

SIRS,—The association of sore-throat with rheumatism is well known to the profession, but I desire to draw attention to a certain condition of the throat which in not a few cases signals the onset of the graver malady.

I have had under my care two cases which serve the purpose of illustrating my meaning. 1. I was called to attend a married man, aged 34 years, who complained of sore-throat and a marked feeling of general illness. On examination I found the throat red and relaxed but altogether clear of any ulceration or deposit—what might be quite properly and fully described as a catarrhal throat, no swelling of glands or pain about the neck and only little difficulty in swallowing; temperature slightly raised, barely 100° F.; a deposit of lithates in the urine which was otherwise normal, but no pain or discomfort whatever about the body or limbs. So far the case appeared simple enough with this exception—the man seemed to be ill out of all proportion to anything I could discover. The usual treatment was adopted and at the end of five days the patient was to all intents and purposes restored to his usual health and extremely anxious to return to work. I advised him strongly to the contrary; in fact, to remain at home another week as I was not certain of his complete recovery. However, he persisted and went, with the result that in four days' time he was back in bed with a sharp attack of rheumatic fever. 2. This case was very much on all fours with the one described, except that the patient's throat was slightly worse, and that at the end of 10 days he complained of a fixed pain in one heel which was undoubtedly rheumatic. He decided to follow the course I suggested, however, and remained at home, making a good recovery and resuming work at the end of three weeks well and strong and none the worse for his attack.

These two cases which I have described and some few others of a like nature which have come under my notice show at least the possibility of rheumatic fever supervening, the condition of the throat being its initial manifestation. The fact that there is no pain or swelling of joints at the

onset should not mislead us, for in neither of the cases cited did the patients suffer any pain whatever for the first few days; indeed, one actually went to work, the subsequent pain not yet in evidence. All such cases as I have described should be viewed with suspicion, the patient carefully watched and ordered rest and warmth for at least one week, and a further period of seven days in the house with appropriate medicinal treatment, until he is *quite* restored to health and we have no cause for further anxiety. In this way I am sure we might often ward off an attack of acute rheumatism with much subsequent advantage to our patients if with little credit to ourselves.

I am, Sirs, yours faithfully,
D. F. WHITELEY.

Fulham, S.W.

VARICOSE VEINS.

To the Editors of THE LANCET.

SIRS,—I read with interest Mr. Pearce Gould's lecture on Varicose Veins in THE LANCET of March 1st, p. 576, and your comment thereupon in your issue of March 22nd, p. 833, and I expected some critical correspondence to follow. As there has been none as yet perhaps you will admit a few unavoidably belated observations from me.

Mr. Gould says that "two views of the nature of varicose veins are commonly held to-day. One regards them as the result of the yielding of the veins to an increased intravenous pressure due to an obstruction to the flow of blood up to and through the heart. According to the other view, varicose veins are to be regarded rather as venous outgrowths, a superabundant development of venous tissue—venous angioma." He rejects the former and adopts the latter. But I would venture to suggest that another factor exists which he has overlooked, one so obvious that I have always thought it sufficient, though often combined with pressure, to account for the existence of varicose veins in nearly every case. I mean relaxation of the muscular coats of the veins and hence diminished resistance. Normally the veins are kept duly contracted by the tone of the muscular fibres that the walls possess. But under certain conditions of debility, and in some individuals much more than in others, these fibres become relaxed and the vessels dilated and weak. The chief of these predisposing conditions is temperature; trouble and distress from varicose veins is mainly a seasonal affection, occurring in hot weather. Just as attacks of certain diseases occur punctually and periodically in some patients at certain seasons year after year, so have I been accustomed to find suffering from distended varicose veins on the advent of the hot weather of summer. If one only looks at the back of one's own hand from time to time the conspicuous difference in the size of the veins according to the external temperature will be conspicuous; the veins may be small and almost invisible on a cold day, but large, full, and distended on a hot one. And this applies to all the superficial veins of the body; the deeper ones are better protected and supported. Thus the pressure of blood from gravitation in the lower limbs increases the distension, causing in time permanent dilatation and incompetence of the valves, and then the morbid structural changes of varix result. Is not this a true and sufficient cause? Heat and cold have this effect more or less on the muscular tissue of all superficial blood-vessels, arteries as well as veins. When the wind was in the east Mr. Jarndyce (if I recollect "Bleak House" aright) used to retire to his growlery, owing to the mental depression and irritability due to increased arterial tension brought about by exposure to cold. Some persons are made for warmth, others require a cool atmosphere. Varix is almost always aggravated in the summer. In that season of 1898 in London I saw a woman with extreme varicosity of the veins of both legs who was also advanced in pregnancy. One day a vein burst in each leg simultaneously. Pregnancy certainly has a considerable effect in inducing or increasing the condition. Ovarian and uterine tumours are less potent because their growth is so much slower. Thus I maintain that over-distension of a certain set of relaxed veins by a column of blood is the chief cause; obstruction is a secondary one. Symptoms of pure obstruction are absent because venous circulation is still carried on, especially through the deep veins. Mr. Gould, like others, has met with the disease much more frequently in men than in women, probably because men's occupations compel them to stand

about more than women, and on this account men are more liable to become incapacitated and seek relief.

If the angioma theory favoured by Mr. Gould were the correct one, why is it that varicose veins are always met with in the dependent parts of the body? Why, for instance, do they never occur in the neck or arms? The presence of adventitious muscular tissue in the varicose dilatations is no doubt due to a process of compensatory hypertrophy. The reappearance of veins near the scar after excision of a portion of a vein is doubtless due to the enlargement and growth of small pre-existent veins to take the place of that which has been removed, just as arterial branches enlarge after ligation of a trunk. Therefore the advantages of a surgical operation may be only temporary. The reason why varix does not ensue upon ordinary venous obstruction from cardiac disease is, I believe, that the veins have not become relaxed and thus the pressure falls upon the terminal veins and capillaries, causing cedema.

I am not aware that I am putting forward anything particularly novel in offering the above remarks—in fact I believe that the ideas are old-fashioned and common. They seem, however, to present a satisfactory and intelligible explanation of the condition and thus there seems no need to fly to a more mysterious theory. Mr. Gould will, I know, welcome any honest criticism.

I am, Sirs, yours faithfully,
F. LUCAS BENHAM, M.D., M.R.C.P. Lond.

Exeter, South Australia, May 9th, 1902.

NUTMEG POISONING.

To the Editors of THE LANCET.

SIRS,—In THE LANCET of April 12th, p. 1034, Mr. R. A. Pitter relates a case of nutmeg poisoning and asks for information as to other cases. Some years ago I was called to see a woman who had taken the whole of a ground nutmeg mixed with gin. The symptoms were exactly the same as those described by Mr. Pitter and the woman nearly died. The dose was taken to procure abortion; whether it had that effect I did not learn. The friends volunteered the remark that the woman had taken the nutmeg in spite of the fact that a neighbour had nearly died from the same cause shortly before. At the time I asked a good many friends, medical and lay, if they knew anything of the poisonous properties of nutmeg in large doses. None of them did except one old lady from the north of England, who stated that she and people in her part knew them well. Squire states that nutmeg in large doses is a narcotic poison.

I am, Sirs, yours faithfully,
Geelong, Victoria, May 13th, 1902. S. MABERLY SMITH.

CHLOROFORM FATALITIES.

To the Editors of THE LANCET.

SIRS,—It seems to me that M. Huchard's statement—as reported by your Paris Correspondent (*vide* THE LANCET, June 7th, p. 1649)—that among 100 deaths under chloroform not more than 10 are concomitant with organic heart disease, ought not to be allowed to pass unchallenged. The St. Bartholomew's Hospital statistical records for the 26 years 1875–1900 contain references to 31 fatalities of this kind, of which some particulars are given as to 30. In 26 of these cases death appeared to have resulted from cardiac failure and from respiratory failure in four. Of the former group of cases it is stated that the heart presented signs of extensive organic disease in nine instances (hypertrophy and dilatation, often with valvular disease, in six, fatty degeneration in three). In only six instances was the heart found to be normal. In 12 cases the condition of the heart is not stated (no post-mortem examination). Thus of 15 cases in which the condition of the heart was actually noted post mortem, in nine this organ was extensively diseased. Hence it may be concluded that morbus cordis adds greatly to the risks of chloroformisation. In five of the foregoing cases the cardiac disease was complicated by extensive pulmonary disease and nine of the patients were in an exceedingly debilitated condition. Most of these fatalities occur during the early stage of chloroformisation while anaesthesia is incomplete (in 11 cases before the operation had been commenced).

I am, Sirs, yours faithfully,
Clifton, Bristol, June 17th, 1902. W. ROGER WILLIAMS.