of muscular sensation in his arms and legs. This was so marked that the power of writing was lost for the rest of the day after a ride of a few hours. He persevered for a while, but ultimately had to give up cycling altogether. The question how these symptoms are produced is one of much interest. Three factors are evidently of the greatest importance—viz. (1) pressure; (2) vibration; (3) auto-intoxication with waste products due to overwork.

1. *Pressure*.—The pressure of a bad saddle will undoubtedly produce nervous symptoms in the perineal region; any cyclist can verify this fact for himself without much difficulty. A really perfect saddle has yet to be invented, and when it is it will be impossible to press injuriously on the perineum, whatever position the rider assumes. Leaning forward on a cycle lessens the surface of the body exposed to the wind and decreases the effort necessary to propel the machine against a head wind. For this reason it is impossible to prevent large numbers of riders from adopting this attitude. Of late years the position of the saddle in relation to the pedals has been altered. In the modern cycle the saddle is placed much further forward than it used to be. The result of this is that with the ordinary form of saddle the position which is necessarily assumed leads to the weight being wholly borne by the perineal structures.

2. *Vibration*.—That this is of some importance will be readily granted by anyone who has ridden both solid and pneumatic tyred machines. Though pneumatic tyres have done away, to a very large extent, with vibration, the jolting on a rough road giving a succession of shocks to the body must be taken into account. The difference in comfort to the feet between rubber and rat-trap pedals is also well known. Numbness in the soles of the feet would be less likely to occur with rubber than with metal pedals. That vibration alone would produce these symptoms is not probable, for none of the twenty-three railway engine-drivers and firemen whom I have questioned on this point have ever noticed any numbness or loss of power after a day's work.

3. *Auto-intoxication with waste products due to overwork*.—During severe or prolonged exercise the untrained man rapidly dissipates his reserve tissues, and his excretory organs being unable to eliminate the waste products of metabolism he suffers from an auto-intoxication, shown either by a febrile condition, sometimes of great severity, or general nervous prostration or excitability, which often prevents appetite or sleep. It is probable that all these three factors, and possibly others, are concerned in the production of the nervous symptoms that have been observed.

I am, Sirs, yours faithfully,

PERCY FURNIVALL, F.R.C.S. Eng.,
Assistant Demonstrator of Anatomy, St. Bartholomew's
Hospital; Assistant Surgeon to the Metropolitan
Hospital.

May 24th, 1897.

FORCED REDUCTION OF THE DEFORMITY IN CARIES OF THE SPINE.

To the Editors of THE LANCET.

SIRS,—In your short report of a meeting of the British Orthopædic Society which appeared in THE LANCET of May 15th, it is stated that in speaking of Dr. Calot's treatment of Pott's disease, I "questioned if it were possible that such a procedure had been followed by the results described." As this statement by itself must give a different impression from that which would appear from my full remarks I beg you to allow me to add to it. It is now probably well known to your readers that Dr. Calot of Berck-sur-Mer reduces the deformity in caries of the spine by forcing the parts into a straight position under chloroform and then applies a fixation apparatus in which he keeps his patients from five to six months. At the end of this time he states that the patients are absolutely cured. It was the possibility of cure in this short time which I questioned.

The practice of straightening the spine in Pott's disease is, of course, not new, and it has been objected to by various surgical authorities. Samuel Hare, however, who published a work in 1838¹ and who was looked upon as a most reliable and skilful surgeon, having great experience with this disease, was a strong advocate for reduction of deformity, and in his work (p. 91) he gives two illustrations showing the position of the carious bones (1) in their angular position, and

(2) when straightened out. I think there is much to be said in support of this method, and although I have never attempted, like Dr. Calot, to separate the diseased parts by any great force I have aimed at improving the whole position of the back, chiefly by altering the curve above and below the seat of the disease. By this means in a great many cases the abnormal projection has become markedly less, as shown in many of the illustrations in my book, "Caries of the Spine." (I would especially refer to Figs. 80-81 and 87-88 in that publication.) Sometimes the projection has completely disappeared, and when this has occurred one cannot but suppose that the diseased bones have been separated at the seat of caries, and I have only seen good results from firm but gentle pressure upon the gibbosity by means of a fixation apparatus. The objection which has generally been raised against attempting to straighten out the spine has been founded upon the view that if the bones were separated the space would be so great that there would be little chance of re-formation of bone. This view I proved to be incorrect at a demonstration given at the annual meeting of the British Medical Association at Bournemouth in 1891.² A description of the case and specimen referred to are also given on pages 121 to 124 in "Caries of the Spine." The specimen and the history of the case show the following facts: (1) that a very large gap may occur from dissolution of several vertebræ without their usual spontaneous coalescence, the spinal cord being left exposed in the thorax and abdomen, unprotected by a bony covering; (2) that repair may take place while the undestroyed vertebræ remain separated from one another;³ (3) that repair of this kind can progress in spite of the most adverse circumstances in respect to the general health of the patient; and (4) that in such a severe case the new bony growth requires a much longer time to become thoroughly solid than in ordinary cases.

As regards the period of repair, in the case I allude to the patient so far recovered that on Sept. 3rd, 1882, eight months after I had first seen him, he was able to walk about comfortably and his health had improved very greatly. He continued to improve apparently in every way until September, 1884, when he died from tubercular meningitis—that is to say, two years and eight months from the time I had first seen him—and it may be assumed that repair had been in active progress during at least the latter two years. Upon examination after death it was found that the gap had been filled up by bony growth, but when the affected vertebræ were being macerated all this new bone crumbled away, leaving the vertebræ as they were before the repair had taken place; therefore this case shows that two years were not sufficient to thoroughly consolidate the new bone, and, although in some patients the progress of repair may be more rapid, yet I cannot imagine that six months, as stated by Dr. Calot, would be anything like sufficient to effect a cure in any case where a considerable gap had to be filled up.

As to danger from immediate and forced reduction it may be assumed that all the cases operated upon by Dr. Calot have been in a state of active disease before repair has commenced, but should new bone have been already thrown out there would be considerable danger of producing irritation by the loosened particles of bone from the new growth, which would act as foreign bodies. A case came under my notice some few years ago which bears evidence to this effect (Case 102, page 135 in "Caries of the Spine"). It seems probable that immediate reduction may prove to be a valuable method of treatment in suitable cases, and I congratulate Dr. Calot upon his success. I question, however, if it be possible to cure such cases in six months.

I am, Sirs, yours faithfully,

Queen Anne-street, W., May 24th, 1897.

NOBLE SMITH.

"RIGHT-HANDEDNESS."

To the Editors of THE LANCET.

SIRS,—In discussing the question whether the "faculty of bipedal progression in an erect posture" is an acquisition it is necessary to clearly define what we mean by this latter term. May I be allowed in this connexion to quote from my work, "The Causation of Disease"? "Scientifically it is

² Brit. Med. Jour., March 19th, 1892.

³ Mr. Jackson Clarke remarks upon this fact that when the anterior common ligament remains intact at the seat of disease the periosteum will probably be attached to it and is then capable of forming new bone.

¹ Practical Observations on the Prevention, Causes, and Treatment of Curvatures of the Spine. London, 1838.

perfectly justifiable to distinguish sharply between the hereditary and the acquired; practically, however, it is quite impossible. Scientifically we may distinguish as follows. If an individual is exposed to as negative an environment as is compatible with life it is evident that there will be little or no acquisition. We may, for purposes of argument, assume such a negative environment, one, namely, under which an individual will pass through a series of developmental changes, which are the outcome of heredity pure and simple—i.e., altogether independent of external influences. It is, of course, impossible really to exclude altogether the influence of environment, since life is due to the inter-action of the organism and its environment but we are justified in theoretically supposing an individual to pass through life under such a limited environment as that referred to, and we may speak of the individual such as he would then be as the strict and sole product of heredity. What he is capable of becoming under particular environments belongs to the region of acquisition" (pp. 140-1). Accepting this definition, it will be seen that the faculty of bipedal progression in an erect posture can scarcely be regarded as an acquisition. Certainly the view held by Dr. Reid, that the infant learns to walk by imitation, is erroneous—so erroneous, indeed, as to shake one's faith in Dr. Reid's other generalisations. No doubt the functional activity of the locomotor apparatus is needful for its full development and for the full development of the faculty of walking in man, but I imagine the majority of physiologists will agree with Dr. Bastian in regarding this faculty as essentially hereditary. Respecting Dr. Reid's lengthy argument in favour of language being a pure acquisition, I cannot think he can have properly interpreted Dr. Bastian's views on the subject.

I am, Sirs, yours faithfully,
Wimpole-street, May 17th, 1897. HARRY CAMPBELL.

"FRACTURES OF LONG BONES IN SCURVY-RICKETS."

To the Editors of THE LANCET.

SIRS,—The case brought by Dr. Guthrie before the Medical Society of London under the above title was one of extreme interest from a diagnostic point of view. He believed that the fractures were due to scurvy, and Mr. Morgan pointed out that in scurvy-rickets fractures of the long bones are usually near the epiphysis and not in the centre of the shaft. This term "scurvy-rickets" is confusing because of its loose application, and for clinical purposes it might be more precise to use the terms (1) scurvy, (2) rickets, and (3) scurvy associated with rickets. I think that this distinction has been definitely proved by Dr. Cheadle, Dr. Barlow, and others. My own experience has been that in scurvy in infants hæmorrhage may occur under any part of the periosteum, and when the shaft is completely separated from the periosteum by blood, complete transverse or oblique fracture may be caused merely by handling the limb. Such fractures may be at the epiphyses or in the centre of the shaft. In rickets the process is one of softening and bending of the long bones, with possibly greenstick fracture of the shaft. In scurvy associated with rickets the hæmorrhage is usually near the epiphysis, which becomes separated, as Mr. Morgan pointed out; and even if the effusion of blood extends along the shaft the bone does not appear to have the same brittleness as in uncomplicated scurvy, and fracture in the centre of the shaft is not common. I agree with Dr. Guthrie in his diagnosis of scurvy, and I should suggest that when these fractures occurred there was probably an absence of marked rachitic changes in the bones. As regards the other point he raises—whether the fracture leads to the hæmorrhage or the hæmorrhage to the fracture—I think the latter view is probably correct. One frequently meets with subperiosteal hæmorrhage without any fracture of the bone, but I have never heard of a case of fracture of a bone in scurvy which was not accompanied by subperiosteal hæmorrhage.

I am, Sirs, yours truly,
May 11th, 1897. G. A. SUTHERLAND.

"A QUESTION OF COURTESY."

To the Editors of THE LANCET.

SIRS,—I am not in the habit of seeing your valuable paper, nor did I ever imagine that I should be called on to ask you

to publish a letter from me; but since a copy of your issue of May 8th has been shown to me, and my attention has been drawn to a letter headed "A Question of Courtesy," which deals with a case in my parish, I will ask you, in accordance with the motto which heads your correspondence columns, "*Audi alteram partem*," to insert a few words from me as to a portion only of Dr. Miller's statements. I say nothing as to the action of the lady guardian therein mentioned nor to that of "the parson's [i.e., my] sister," nor to that of Miss Annie McCall, M.D. Bern, &c. The two former are away from home and the latter is engaged in practice at some distance from here. I would not pretend to give an opinion on a question of medical etiquette. But I have little doubt but that those ladies could give a somewhat different version of the circumstances from that given by Dr. Miller, and I may venture to say that I am somewhat surprised to read your editorial comment upon what is nothing more than an *ex parte* statement. What concerns me is this, Dr. Miller alleges that my parish nurse "persuaded" the patient "not to come into the Cottage Hospital, but to go to Euston road Hospital." Had she done so she would have made a grave departure from her instructions, which are to work under and with the medical men in the various cases which she visits. I therefore asked her the facts of the case and she positively declares to me (1) that she did *not* persuade the patient not to go into the Cottage Hospital, but, on the contrary, urged her to do so; (2) that she never said a word to her about the Euston-road Hospital at all, and (3) that on being accused by Dr. Miller on these two heads she denied to him that she had done what he accused her of. I should not have troubled you with this letter but for this one reason, that Dr. Miller's statement respecting the parish nurse, if uncontradicted, might greatly impair the very valuable work she is doing among my sick poor without cost or charge to them.

I beg to remain, your obedient servant,
H. R. COOPER SMITH, D.D.,
Vicar of Basingstoke.

May 19th, 1897.

SCOTLAND.

(FROM OUR OWN CORRESPONDENTS.)

Glasgow Medico-Chirurgical Society.

THE last meeting for the present session was held on the 14th inst., the President, Dr. W. L. Reid, being in the chair. Dr. Walker Downie showed a man, sixty-six years of age, on whom he had successfully operated for Septic Thrombosis of the Right Lateral Sinus. The condition had succeeded a catarrh of the middle ear and was regarded as a sequela of an attack of influenza from which the patient had suffered. A feature of the case which attracted some comment was the very slight disturbance of temperature manifested by the patient before operation. In the experience of the majority of the speakers septic sinus thrombosis was accompanied by considerable waves of febrile temperature.—Dr. T. K. Dalziel showed a woman who for seven years had been a victim to Severe Facial Neuralgia affecting all the branches of the fifth but always commencing with a burning sensation at the angle of the mouth, where also there was distinct and localised tenderness on pressure. These symptoms had been completely relieved by excision of the long buccal nerve. The operation performed was the external one, and though it had left a well-marked scar this result had been discounted before the operation, which had been deliberately selected in preference to the operation from the buccal cavity, with its attendant septic risks. Dr. Dalziel also showed a boy, aged three and a half years, who had been born with an Imperforate Anus, a condition easily relieved at the time by the passage of a blunt instrument, but in whom subsequently such an amount of cicatricial tissue developed as to cause stricture of the bowel. There had been considerable difficulty in dealing with the case, and in the end the stricture had been excised by Kraske's operation, and with very good results. Dr. Dalziel also showed a boy who had suffered from Gangrene of the Lung, believed to be due to the entrance of some particles of toast into the air passages, and who had been treated successfully by excision of the gangrenous tissue.—Mr. H. E. Clark showed the brain removed post mortem from the body of a man on whom he had operated for Cerebral Tumour. The case in its earlier developments had been seen by Dr. T. K. Monro and by Dr. Alexander Robertson, and these gentlemen contributed the clinical history. The earlier symptoms were very obscure, being