quested to see the case on behalf of the "Accidental Insurance Company," the plaintiff having made a claim on them in consequence of the accident. Dr. Fletcher deposed on oath to the same general conditions as had been sworn to by Dr. MacLagan and Mr. Buckley—viz., the limbs of equal length; no want of parallelism; perfect mobility of the joint; absence of crepitus; excessive pain and swelling. (Dr. Fletcher's evidence is altogether ignored in your report.)

About *five weeks* after the accident Mr. Buckley became uneasy because he noticed, in addition to the unabated continuance of pain, that the left limb was beginning *slowly to shorten, and to become slightly inverted*. Now, Sir, what ought Mr. Buckley to have done under these circumstances? He should have remembered, you say, "that in the multitude of counsellors there is safety." This is exactly what he did, and, fearing lest some absorption might be taking place in the bruised head and neck of the femur, he expressed a desire for further advice. In response to this wish, Dr. Fletcher was again called in, this time in the character of "consultant." Once more the patient was most anxiously examined by these two gentlemen, and, according to their evidence, in all sorts of positions. Their attention was drawn by the patient to the following points: swelling over the hip-joint, continuance of pain, slight inversion, and apparent shortening. Dr. Fletcher considered all these symptoms to be probably the result of the original bruising, and recommended that the part should be freely painted with iodine, and that the limb should be brought into gentle use and exercise.

From this time the shortening of the limb *steadily progressed*; the patient dismissed Mr. Buckley, fell in with a bone-setter, and finally was seen by the late Mr. Beevor, who on March 4th, 1872, close upon three months after the accident, pronounced the case to be one of "dislocation on to the dorsum ilii."

The man afterwards went into the Manchester Infirmary, and was seen by the surgeons of that institution *for the first time* on July 1st, 1872. They unanimously confirmed Mr. Beevor's opinion, considered it too late to attempt reduction; and then it was that the patient began to talk of bringing an action against Mr. Buckley and Dr. Fletcher.

On the 19th of July, when the action was about to be tried, Mr. Favell of Sheffield and I were requested by Dr. Fletcher to examine and report upon the case. We made the examination in the presence of Mr. Southam, Mr. Heath, Mr. Bowring, and several other surgeons of eminence in Manchester. We found an undoubted dislocation of the joint backwards. The head of the femur could be felt behind the acetabulum, resting upon the upper margin of the sciatic notch; there was an inch and a half of shortening, slight inversion, and unusually free mobility of the part. The patient could plant both feet flat and parallel upon the ground, and could walk tolerably well with the aid of a stick and a high-heeled shoe. On grasping the head of the femur, and pressing it firmly down upon the pelvis, whilst the limb was rotated, I felt and pointed out to many of the others that a distinct sense of "grating" was produced.

The evidence I gave in the witness-box was this:—1. That the case was not, as it was said to be, a simple and uncomplicated one from the first of dislocation on to the dorsum ilii. 2. That the *lengthening* of the limb, observed during the first few days, was incompatible with such a view of the case. 3. That the accident was in its nature such an one as was exceedingly likely to produce deep-seated injury, and probably did produce great ligamentous disruption and fracture through the acetabulum. 4. That so long as the patient was kept recumbent, and the muscles were paralysed by crushing, the parts all held together; but that, on commencing to move about, and with returning muscular power, they slowly gave way. 5. That the case was not, as it was said to have been, a simple and uncomplicated one of dislocation on to the dorsum ilii from the first. 6. That Mr. Buckley had not been guilty of negligence, and that he had not treated the case either unsurgically or unskillfully.

As neither the Manchester surgeons nor I had seen the case until it was eight months old, I did not feel that we were at liberty to ignore the history of the earlier symptoms of the case; and I declined to believe that Dr. MacLagan, Mr. Buckley, and Dr. Fletcher had all three committed a

blunder from the first; and, accepting their account as a reliable one, I came to the conclusion that there must have been, in the first instance, a fractured acetabulum, and that the dislocation had been a secondary occurrence.

The jury returned a verdict, I regret to say, for the plaintiff; but, at the same time, they marked their sense of the obscurity and difficulty of the case by only awarding damages of forty *shillings* (not pounds, as is stated in your report).

I am, Sir, your obedient servant,

Hillary-place, Leeds, August 23rd, 1872. C. G. WHEELHOUSE.

GUARANA.

To the Editor of THE LANCET.

SIR,—The great interest your journal has always evinced in every matter connected with the medical profession, and its strict impartiality in discussing all questions that appear on the scientific horizon, embolden me to believe that you will favourably receive the communication I now have the honour to address to you. It relates to guarana, and has been suggested by the article which appeared in THE LANCET of the 15th of June last. I venture to hope that it will interest your readers, as containing accurate information respecting the seeds of the *Paullinia sorbilis*, mainly derived from a paper by Dr. Wiesner, recently published in the *Ausland*.

The first mention of guarana is found in a work by Martius, entitled "Travels in Brazil" (vol. iii., pp. 1061–1098).

Two kinds of guarana are to be found in the European markets, one being pure and well prepared, the other either adulterated or most imperfectly manufactured. For this product to possess its proper virtues, the seed must be used while quite fresh, otherwise fermentation ensues and greatly deteriorates the quality. It must also be made up without any mixture of cacao or tapioca (Silva Consinho's "Remarks on Guarana," Rio de Janeiro, 1866). The different manipulations to which the Manbés subject the paullinia seed sufficiently explain how the fraud in question may be practised.

The seed, after being slightly roasted, is reduced to powder, and then mixed with water and made into a stiff paste. With this paste a certain proportion of whole and broken seeds is blended; then the mass is moulded into cylindrical rolls, and in this state is ready for the market.

I have now before me the cases published by Dr. Wilks to which THE LANCET alludes. In one he declares that the guarana "acted like a charm"; in another, of which he himself was the subject, that he obtained a doubtful result; in a third he quotes a letter from Dr. Wood, of Montreal, who cured himself of sick headache with the guarana powders. "By taking one of these powders," says Dr. Wood, "when I felt premonitory symptoms, I have carried off the attack, and with the first box absolutely put it off for two months, a thing which had never occurred in my life before." Dr. Wilks earnestly advises his professional brethren to try this medicine, at the same time stating that it is prepared by Grimault, of No. 7, Rue de la Feuillade, Paris.

These cases, you will perceive, are altogether favourable to guarana, and I feel myself justified, as well by my own experience as by that of many physicians of my acquaintance, in affirming that this medicine never fails, except when improperly prepared, adulterated, or injudiciously administered.

The following is our process for preparing the powders mentioned by Dr. Wilks:—The rolls of guarana are first pulverised, and then treated with alcohol; the liquor is then brought to the consistence of an extract, dried, and powdered. Our product consequently contains in a small volume all the active properties of the seed, and is quite free from adulteration.

Administered in doses of two or four grammes, to be taken at twice, with an interval of two hours (in case of non-success), these powders are a certain remedy for the sick-headache.

Guarana owes its curative virtues to the great quantity of caffeine which it contains. Trommdorf makes the pro-

portion 4 per cent., Steinhous says 5 per cent., and Pockolt 4.28 per cent. (*Compte Rendu de l'Académie Impériale des Sciences*; Nenni, "Classe Mathem.," vol. liv., p. 473); whereas coffee contains only .02 to .08, and tea .06 to 2 per cent.

Guarana is considered as a kind of panacea by the natives, who take it in the form of an infusion whenever affected by diarrhoea or dysentery—the two scourges of Brazil.

Every Brazilian always has at hand his *magdalen* of guarana. As for the seed, it is difficult to procure; however, should you wish to have a sample, I shall be most happy to send you some.

In concluding, I pray you, Mr. Editor, to excuse the length of this letter, seeing the importance of the subject on which it treats, and on which I can furnish you with whatever information you may desire.

I am, Sir, your most obedient servant,

LECONTE, M.D.,

Prof. Faculty of Med., Paris, and Pharm. Chemist.
Paris, 23rd August, 1872.

IS MODIFIED SMALL-POX CONTAGIOUS?

To the Editor of THE LANCET.

SIR,—The case of small-pox on which you found some editorial remarks in your number of the 17th inst. brings prominently forward some questions of great interest both to the profession and the public—questions that I have recently made the subject of observation, and to which I am desirous of drawing the attention of the profession.

Mr. Wheeler, of Clapton, who treated the case on which you comment, describes it as "small-pox in a remarkably modified form, so much so that until she was nearly well I could not be sure if it was actual small-pox or chicken-pox." He further expresses an opinion that chicken-pox and small-pox are but phases or stages of one and the same disease. Now, Sir, it appears to me that the term "modified" small-pox is far too vaguely used by the profession; that, in fact, different men use it with different meanings. Nor, indeed, do I find that writers on small-pox define with sufficient accuracy the specific characters of the "modified" form of the disease. For I believe—and it is this to which I wish to draw attention—that the difference between true and modified small-pox is real and substantial, and, moreover, easy to recognise. It is, too, a difference of the highest practical importance, involving a question not merely of *relative* contagiousness, but of the absolute *non-contagiousness* of one form as opposed to the well-known contagiousness of the other.

In a paper I presented to the British Medical Association at Birmingham last week, I brought forward the cases on which I founded this opinion. I there stated that I had carefully watched as many as fifty cases of what I regard as modified small-pox, treated at their own homes, in one of the lowest, most crowded, and unhealthy districts of Marylebone, and that in no case was the disease communicated from the patient to any other person, either of those living in the house, or of those who, as friends or attendants, had been exposed to the chance of contagion. Now the distinguishing character of all these cases, on which *alone* I rely as diagnostic of the modified form of the disease, is the *conical form of the pustules without any terminal depression*, as opposed to the ordinary flattened pustule of true small-pox. My experience of these cases has led me to form a very decided opinion that this modified form of small-pox is absolutely free from all risk of contagion. On the other hand, of the cases of true small-pox that have come under my observation, in spite of all precautions against the propagation of the disease, no fewer than 40 per cent. communicated it, within three weeks of the attack, to someone within the focus of contagion.

And here I would most strongly insist on the great value of the one diagnostic character that I have pointed out—the appearance in any case of even the smallest number of *depressed* pustules being sufficient to characterise it as one of true small-pox. Could I even find but one such pustule among a mass of conical ones, I should consider it enough to mark the case as contagious.

I was unfortunately not present when my paper was read

at Birmingham, but I am aware that Dr. Stewart rejected my theory as founded, in his opinion, on imperfect observation, and referred to *one* case in his own experience, some time ago, which militated against it. It appears to me to be a point well worthy of the attention of those who have the necessary opportunities for investigation, and one that can only be determined by the observation of a large number of cases. I am myself, at present, so convinced of the truth of my conclusion, that I never hesitate to regulate my practice in accordance with it, and fearlessly advise the friends of any patient suffering from small-pox, with *simple conical pustules without depression*, that there is no fear of contagion, and I have not hitherto had reason to regret the advice.

I venture to hope that this subject will receive some share of the attention of the profession; and I shall be glad to draw forth any communication, either public or private, from any gentlemen who may be able to throw light on the matter.

I am, Sir, your obedient servant,

W. S. BRITTON.

Acacia-road, St. John's Wood, August 27th, 1872.

MR. PEMBERTON ON LITHOTOMY.

To the Editor of THE LANCET.

SIR,—Mr. Pemberton, in his address to the British Medical Association, published in your journal to-day, states in reference to the so-called dilatation of the prostate (p. 217): "Now, I know that many believe that this dilatation means laceration. It has, indeed, not very long since been described as 'complete rupture and laceration' (Mr. Teevan, THE LANCET, vol. ii. 1870, p. 237), and as 'unsurgical and dangerous.' I answer this statement, which I venture to characterise as eminently reckless, by a positive contradiction as to facts." Mr. Pemberton is inaccurate in his reference, for the words are not to be found at p. 237, as stated by him, but they can be seen at p. 891, where they appear in a very short abstract of a paper on lithotomy, which I read before the Medical Society in December, 1870, and which was afterwards published in full in THE LANCET for December 23rd, 1871 (see p. 882).

Now Mr. Pemberton either has or has not read my paper. If he has not read it he was not justified in characterising my statements as he has done, without troubling to make himself acquainted with the facts upon which they were founded. But if Mr. Pemberton has read my paper, he was in honour bound to refer your readers, not to the abstract, as he has done, but to the paper itself, where they could have obtained a knowledge of the facts upon which my statements were based, and have judged for themselves which was the more entitled to the expression "eminently reckless,"—Mr. Pemberton's assertion of what he does in the dark in lithotomy, unsupported as it is by one single jot of evidence, or my enunciation of facts which, so far as I know, admit of no contradiction.

I am, Sir, yours, &c.,

Portman-square, Aug. 17th, 1872.

W. F. TEEVAN.

LUNACY STATISTICS.

To the Editor of THE LANCET.

SIR,—About three years ago the Medico-Psychological Association suggested, and the Commissioners in Lunacy recommended, certain medical statistical tables to be adopted in the reports of lunatic asylums, and in consequence these tables have been generally adopted by medical superintendents, who base their calculations upon them.

Believing that one of these tables is calculated to lead to erroneous conclusions on the important item of the percentage of cures, I take leave to direct attention to it.

By Table 3 the percentage of cures is to be calculated on the admissions, and were it confined to the patients actually admitted in the year it would be correct. It is, however, not so calculated, but in this mode: Assume that 100 patients are admitted in 1871, and that 25 out of the whole body of resident patients are cured, credit is taken for 25 per cent. of "recoveries on admissions," whereas there is no relation between the cures and the admissions, as half, one-fourth, or perhaps none of the cures of 1871 occurred