24. A medical officer, retiring after a full-pay service of twenty-five years and upwards, may, if recommended for the same by the head of his department, receive a step of honorary rank, but without any consequent increase of half pay.

25. Good service pensions shall be awarded to the most meritorious medical officers of our army under such regulations as shall be from time to time determined by us, with the advice of our Secretary of State for War.

26. Six of the most meritorious medical officers of the army shall be named My Honorary Physicians, and six, My Honorary Surgeons.

Given at our Court of St. James's, this first day of October, 1858, in the twenty-second year of our reign.

By her Majesty's command,

J. PEEL.

Correspondence.

"Audi alteram partem."

ON SUGAR-REFINING BY ACETATE OF LEAD.

[LETTER FROM DR. SCOFFERN.]

To the Editor of THE LANCET.

SIR,—Permit me, in behalf of science, to claim a small portion of your space for the exposition of a matter which, though associated with my name, I have now no direct interest in whatever.

Happening a few days ago to glance over one of the small books, the object of which was to popularize the facts relative to food adulterations, I felt not a little surprise to read the following abstract from a blue book, and to which my attention had not been called in the original.

"Dr. W. B. Carpenter says,—I was employed by the chairman of the Board of Inland Revenue a few years ago to make a report as to the ordinary process of sugar-refining. I mention the subject, because I believe that publicity did then answer the end. The question was, whether a process which involved the introduction of acetate of lead should be prevented, the result of chemical analysis showing that a minute quantity of sulphite of lead was found in the treacle. The question submitted to myself, along with the late Dr. Pereira and Dr. Alfred Taylor, was, whether we considered that that quantity of sulphite of lead was likely to be injurious to the consumers; the inventor of the process, Dr. Scoffern, maintaining that it was perfectly insoluble—that it was like so much chalk.

"We reported that there was the same likelihood of injury that there was from any other salt of lead. I may mention, in confirmation of our conclusion, that only last year a gentleman from Ceylon informed me that a detachment of soldiers in one of the up-country stations had shown characteristic symptoms of poisoning by lead. The medical officer made very careful inquiries, and could attribute the lead-poisoning to nothing but the sugar, and, carrying out these inquiries, he found that this sugar was made by Scoffern's process in Ceylon."

On these points let me offer the following remarks:—No person was ever authorized to use my process until he had come under a written engagement to pay £1 per ton on all sugar manufactured by it, and to accept a chemical superintendent of my nomination. No Cingalese manufacturer ever acceded to these terms, or, to the best of my knowledge, ever put himself in communication with me. This assurance, if accepted, is sufficient to dispose of Dr. Carpenter's very apocryphal accusation. Dr. Carpenter need not look far to discover lead, in soluble condition and poisonous quantity, mixed up with colonial sugar products. He need not associate my name with the discovery. I did not introduce sugar of lead as a purifying agent, but I introduced a plan for using it with safety.

As regards Dr. Carpenter's statement, quoted by me, I might well be surprised at the chemical ignorance displayed in it had Dr. Carpenter been known to the public as a chemist. "We reported that there was the same likelihood of injury as there was from any other salt of lead," says Dr. Carpenter; but if he refer to the published Report, he will find that his chemical associates saved him from the absurdity of proclaiming inferentially that *all* lead salts are equally poisonous—the sulphate and the acetate, for example.

The Report, Sir, perhaps you will remember, had hard judgments dealt out to it by more than one member of the daily

regimental guards as laid down in pages 29 and 30 of the Queen's Regulations for the Army.

press. Notwithstanding its palpable bias, which not all its ingenuity could cover, the Commissioners were obliged to admit that the chemistry of my process was irreproachable, and, if strictly carried out according to my published instructions, not a particle of lead, in any form, would remain.

I need not explain, Sir, to professional readers, that my operation, turning, as it does, on the conversion of *acetate* of lead into *sulphite* of lead, and involving separation of the latter by filtration, rendered it a matter of great consequence to determine whether the sulphite were poisonous, inasmuch as no one can aver, with confidence, that *every* insoluble particle, whatever its nature, shall be invariably filtered away from any solution.

On this question, the late Dr. Gregory, in connexion with myself and numerous other chemists, were enabled to pronounce, after repeated experiments, that it was even more insoluble than *sulphate* of lead, and, so far as we could perceive, innocuous.

To scientific men it will be evident that an unbiassed Report on the process should have been comparative. It should not only have been based upon comparative examinations between sugar and treacle made by my process, and sugar and treacle made by ordinary processes, but it should have taken cognizance *not* of lead in general terms, but of some particular combination of lead; at the very least it should have testified to the solubility or insolubility of the lead compound. *This was not done.* The Report, far from offering an opinion as to the combination of lead found occasionally, and I may say infinitesimally, in some of my specimens of treacle, simply dealt with it under the generalization of "*lead*." Had the Commissioners examined ordinary treacle, they would almost invariably have discovered more lead than in mine, and, what is more to the purpose, lead in a *soluble and recognisedly poisonous condition*.

Finally, Sir, I would beg to remark that chemical and toxicological propositions are more amenable to experiment than to disputation. I unhesitatingly affirm that *sulphite* of lead is no more poisonous than chalk is poisonous. Of course, your readers will see the necessity, in a case involving the use of the word "*poison*," to fix on some standard of comparison. I, therefore, fix on chalk. When Dr. Carpenter intimates that all salts of lead are equally poisonous, he either makes a great discovery or commits a great mistake. For politeness' sake, I will endeavour to assume the former.

I am, Sir, your obedient servant,

Barnard's Inn, Holborn, Oct. 1858.

JOHN SCOFFERN, M.B.

INTENSE THIRST A SYMPTOM OF DISPLACEMENT OF THE STOMACH IN DIAPHRAGMATIC HERNIA.

[LETTER FROM DR. WILKS.]

To the Editor of THE LANCET.

SIR,—In your number for the 16th inst., you publish an interesting case, from St. Mary's Hospital, under the care of Dr. Alderson, of a young man who had his stomach forced into his chest by a rupture of the diaphragm, and who, amongst other severe symptoms, had one which especially attracted my attention, and this was, *great thirst*. It was only incidentally mentioned in the report, but is one, I believe, of importance in diagnosis—at least I think its presence in three cases warrants such a conclusion.

CASE 1.—Henry S—, aged thirty-seven, was admitted into Guy's Hospital on July 7th, 1855. While working on the Crystal Palace Railway, about two tons of earth fell upon him, producing great injuries to his chest and pelvis. On admission, his breathing was so short that it was thought he would expire every moment; the left side of the chest was enlarged, tympanitic, and emphysematous, the ribs being also broken. He survived the accident twenty hours, and his most remarkable symptom was *intense thirst*. During the time he lived, he drank immense quantities of water, but he did not vomit. The post-mortem examination revealed several injuries, which I need not here detail, but the most remarkable appearance was the immensely-distended stomach, filling the whole left side of the chest. This, together with a portion of colon, had passed through a large rent in the diaphragm.

CASE 2.—In THE LANCET of Jan. 19th, 1856 (I quote from a note, not having the journal by me), Dr. Fraser, amongst a series of cases reported from the seat of war in the Crimea, relates the case of a man who received a wound of the diaphragm,

whereby the stomach was forced into the chest. He lived six days, but it was probably only on the last that the displacement occurred. The *thirst* on this day became excessive, and his plaintive moanings at not having as much cold water as he desired were painful to hear. He drank a large quantity a few minutes before death.

CASE 3.—This is the case you give in THE LANCET of the 16th inst., in the report of which I find, amongst other urgent symptoms from which the man suffered, on one day mentioned—a great deal of *thirst*; on another day—drunk a large quantity of water; on another day—drinks an *immense quantity of water*; and so on.

That this thirst is more than a coincidence is shown from the fact, that, having the two first cases in view, I read through the last one carefully, in expectation of meeting with this symptom, and was not surprised to find it present. To be able to predicate a symptom in any given case is sufficient to prove its intimate connexion with it. It is rather a question for the physiologist to solve, why an inordinate stretching or distension of the stomach should give rise to this intense thirst.

I am, Sir, your obedient servant,

St. Thomas's-street, Oct. 1858.

SAMUEL WILKS, M.D.

HEREDITARY SYPHILIS.

[NOTE FROM MR. DE MÉRIC.]

To the Editor of THE LANCET.

SIR,—In the third of the Lettsomian Lectures which you did me the favour of inserting (*ante*, p. 298), I submitted, basing my opinion upon some cases which have been under my care, that mercury, carefully administered to parents affected with systemic syphilis, might prevent the transmission of the disease to the offspring. The cases I have cited refer to children at the present time eighteen months and two years old respectively; but it is now in my power to quote one where the eldest child has reached her twelfth year. I am indebted for the case to the father himself, who, having read my lecture, was kind enough to send me the following short but significant sketch:—

"In August, 1843, I contracted the disease. A few days after intercourse, I discovered a chancre on the prepuce, which I at once burned with the caustic till I thought I had destroyed it. But at the end of October, I was covered with a papular eruption. I at once went to a surgeon of great repute, who put me under a course of mercury. The course was kept up until my mouth was sore, and pushed no further. About that period I was exposed to some wet, and have had rheumatic pains ever since.

"In 1845 I married, though still troubled with the pains, which could hardly be other than syphilitic osteocopic. My first child was born in 1846, and three others up to 1851; none, however, have shown, to this day (Sept. 1858), the slightest tendency to the syphilitic or any other disease. During a great part of that time, I suffered from pains in my bones, and very distressing swellings of the shins (nodes). I am obliged to take iodide of potassium occasionally."

This valuable case needs no comments; its importance, as showing that mercury *may* shield the children, is self-evident.

I remain, Sir, your obedient servant,

Brook-street, Oct. 1858.

VICTOR DE MÉRIC.

ON A CASE OF "HYSTERICAL PARALYSIS OF THE HAND."

To the Editor of THE LANCET.

SIR,—In the "Mirror" of last week, under the heading of Charing-cross Hospital, there is a case of so-called Hysterical Paralysis of the Hand in a man, under the care of Mr. Barwell; and the report goes on to give that gentleman's anatomical reasons for the correctness of his diagnosis. Mr. Barwell is said to have pointed out that the paralysis was not consistent with anatomy. "The median nerve supplies not only the superficial flexor of the fingers, but also the *muscles of the ball of the thumb* and its long flexor." And again: "The long flexor of the thumb is paralysed; but as the short one retains its powers, this cannot be attributed to interruption of nervous supply, *since both muscles are dominated by the same nerve*; the active condition of the abductor indicis, likewise supplied by the median, is another anomaly."

In the first place, I must beg leave to remind Mr. Barwell that the median nerve does *not* supply all the muscles of the ball of the thumb; for the adductor and half the flexor brevis are supplied

by the ulnar, which sufficiently accounts for the fact that the patient "can oppose the thumb to the little finger, and bend its first phalanx, but not the last." This same ulnar nerve also supplies the abductor indicis, which therefore presents no anomaly.

We are also informed that the man, "if told to flex the little and ring fingers, partially does so, and bends at the same time the middle finger to some considerable degree." This appears to me to be quite what was to be expected, seeing that the ulnar nerve supplies that portion of the flexor profundus which goes to the inner fingers, and that the middle would probably be affected at the same time.

All the anatomical symptoms, when properly understood, therefore, point without exception to paralysis of the median nerve; and this view is not unreasonable, I think, if we consider the nature of the injury—a bite just at the spot where the median nerve is most exposed, and which was accompanied by all the symptoms of injury to this very nerve; for not only did he turn sick and faint, but "at the same moment he felt pain shoot along the arm to the thumb and fore and middle fingers," the very ones which receive their cutaneous supply from the median nerve.

I am afraid, therefore, that this case must be considered to be simply one of paralysis, the result of a bruised nerve, which will probably recover itself in course of time without any special treatment.

In conclusion: as, in all cases of paralysis, it is of the greatest importance to determine what muscles and nerves are affected, I would recommend to the notice of your readers, who may, perhaps, have relinquished their anatomical studies for some years, a very useful little pamphlet, "The Motor Nerves of the Muscles," by R. Hughes (Walford, Strand), which will supply them with this important information in the most simple form I am acquainted with.

I am, Sir, your obedient servant,

CHRISTOPHER HEATH,

Demonstrator of Anatomy at the Westminster Hospital.

Gordon-square, October, 1858.

CALCULUS FROM THE URETHRA.

To the Editor of THE LANCET.

SIR,—I enclose a calculus which I extracted, by incision, from the urethra of a man, aged eighty years. He survived the operation three weeks. The patient informed me that he had suffered from difficult micturition for two or three years, but was not aware of the cause until the day of the operation, when he had complete retention. "It was situated at the bulb." The shape is peculiar.

I am, Sir, yours truly,

Workington, October, 1858.

W. L. DICKINSON, M.R.C.S.

* * The calculus which has been forwarded to us is of a pyriform shape, two inches in length, and three inches in circumference, and weighs about three quarters of an ounce. It appears to be of the uric acid kind.—SUB-ED. L.

NEW ARTIFICIAL LEG.

To the Editor of THE LANCET.

SIR,—I have just received THE LANCET of the 8th of May; it should have arrived about six weeks ago. In it is a letter from Mr. Cornish, commenting on my improvements to his "new artificial leg," which requires answering. Mr. Cornish says that any proposition, "that the artificial foot should be a little shorter than the other," may be taken in two senses, one of which is, that the words may mean that the *leg* should be shorter than the other. Now, when I say the foot I mean the *foot*, and not the *leg*. My reason for wishing the foot shortened is, that the wave-like curves of the spring foot, which were to answer the purpose of toes, were, when made sufficiently elastic to give with each motion of the foot, continually catching on the ground, and tripping up the wearer, and when made less elastic, they raised the wearer off the ground, so much so, as to make him lose his balance, and cause him to fall. After many trials, I found that making the "artificial foot about an inch and a half shorter than the natural foot overcame these difficulties. I disagree with Mr. Cornish, "that the greater the span of an arch (within certain limits) the greater will be the strength of that arch;" and, moreover, I do not see that it has anything to do with the matter in question, as by shortening the foot, the arch is in no way interfered with. I must also, again, disagree with Mr. Cornish, that "to