

amputation as unlikely to do good." My case was that of a young woman aged 23, who had been bed-ridden for years with scrofula, and who also had the misfortune to be the subject of a severe attack of acute rheumatism early in life, which had caused mitral regurgitant disease of her heart. She was an unpromising patient for operation. Her left ankle-joint, which had been diseased about three years, became gangrenous (no doubt from embolic obstruction), and spreading with amazing rapidity, the gangrene soon involved all the soft tissues in front of the joint, laying bare the tendons as well as the upper parts of the tarsal and metatarsal bones, presenting a very ugly appearance indeed. The gangrene was extending up the leg in a most irregular fashion—the highest point being about a couple of inches above the internal malleolus—but no line of demarcation seemed to form, and the patient was beginning to sink. No cause, other than constitutional, could be discovered as producing the gangrene in this case: and as a *dernier resort*, with the assistance and concurrence of two medical neighbours, I amputated the leg immediately below the knee. The patient made a speedy and excellent recovery. She was in a fit condition to be brought into town a few weeks after the operation to have "herself and the stump" photographed.—Yours faithfully,

J. D. MCCAW, F.R.C.S.Ed.

Portglenone, Belfast, December, 1883.

CHRONIC INFLAMMATION IN BONE.

SIR,—Will you allow me to add to my paper the substance of the reply I made to the discussion, more particularly with regard to the specimens exhibited by Mr. Parker? These specimens, shown to counteract my statements, are of course more important than opinions or theories. Both Mr. Parker's bones, however, instead of being in antagonism to my conclusions, do, with a different reading in the one, and a clearer understanding of my point in the other, really support the view put forward. The femur shown was much thickened and very dense, in the centre was a sequestrum it is true, but, on examining it carefully, it proved to contain the original outer compact layers of the bone. This, I think, Mr. Parker afterwards admitted. Though the long duration of the case seemed at first opposed to the periosteal nature of all the new bone, viewed now by the light thrown upon the subject by "quiet necrosis," it can be understood. Another specimen was a beautiful example of a thickened femur, from an infant affected with congenital syphilis. It was shown to prove that other causes than osteitis deformans, could produce a uniformly enlarged bone. This is true of the bone when seen from without: but on section, the original compact layer, white and dense, still existed, and was completely ensheathed by a new periosteal deposit of considerable thickness. Between the two a slight interval existed. The point in my paper was, that, osteitis deformans will alone produce a uniformly enlarged bone, having a compact layer externally, but cancellous throughout the rest of its extent. The specimen shown does not disprove this point. Similar specimens I had studied at the College Museum, and Mr. Parker has been kind enough to present one to our museum at Guy's Hospital. I believe the difference at first seen on reading Dr. Coats's remarks is only an apparent one, and due to a different use of terms. That there is an absorption of the bone produced by the inflammation, there is, of course, no doubt; but instead of putting it, as Dr. Coats does, as "expansion entailing absorption," I would rather say, "enlargement entailed by absorption," for I think the use of the term expansion is productive of confusion.—I remain, faithfully yours,

St. Thomas's Street, S.E.

CHARTERS J. SYMONDS.

SUNDAY EARNINGS.

SIR,—I desire to suggest to the medical profession an easy mode of forming a fund, from which to draw when it is wished to contribute towards any subscription—British Medical Benevolent, a "Rogers" testimonial, a "Bower and Keates" persecution, an orphan's appeal, or allied object. This is by putting aside all Sunday receipts for the purpose. I find it easy to do this; while entering work in my ledger, a cross is placed over each Sunday's date; when the account is paid I deduct the amount received for the Sunday work, and place it in my "charity-box," kept with which is a debtor and creditor account, showing from what patients' payments the fund has been raised, and for what purposes help has been given. On principle, I do as little professional work as I can on Sundays, without neglect of my patients; but I find I always have something in my box when I wish to contribute towards the aid of my professional brethren and poor deserving neighbours.—Yours truly,

H. M.

A. DENIAL.

SIR,—In your summary of 1883 I find a repetition of the unfounded and libellous statement that an alleged death from vaccination in the Derby Union was caused by the use of "dirty appliances" by the vaccinator. I have from the first protested against this charge as false. I have challenged Dr. Barry to substantiate it, I have (backed by my board) demanded from the Local Government Board a proper investigation, but in vain. I should have brought an action for libel against Dr. Barry, but was advised that he could plead a confidential official statement; how reliable his report is may be judged by the one fact, that he stated therein the father of the child to be perfectly healthy, when he never even saw him, and when the contrary is a notorious fact. I have to request that you will give as prominent a place in the JOURNAL to this letter as to your repetition of this slander.—I am, sir, your obedient servant,

50, Friar Gate, Derby, January 1st, 1884.

WILLIAM LEGGE.

UNSWEETENED CONDENSED MILK.

SIR,—Seriously ill effects are, by Dr. Borchardt and other eminent authorities on the diseases of children, attributed to the use of the highly sugared condensed milk, now largely used as food for infants. Dr. Borchardt, in a published paper, which greatly impressed me, attributed a large amount of the prevalence of rickets among children to this cause. Since then I have been using unsweetened condensed milk, but I find among my friends that the subject has yet attracted but little attention. I should be glad to see some subjects of preventive hygiene taken up as matters for collective investigation by our committee; and I should like to suggest this as being one of vast importance to the rising generation, and one which might well be settled by collective investigation.—I am, sir, yours, etc.,

PHILOTEKNOS.

January 2nd, 1884.

SPECIAL CORRESPONDENCE.

PARIS.

[FROM OUR CORRESPONDENT.]

Action of Coffee—Pus and Micro-organisms—Pathogenetic Power of Bacilli in Dried Sputa of Phthisical Patients—Inoculation of Charbon—Case of Arrested Development of Olfactory Nerves—Cholera and "English Commercial Interests"—Destruction of Dead Bodies by Sulphuric Acid—The Influence of Chemical Salts on the Bacillus of Glanders.

ALLUSION was made in the JOURNAL of December 15th to M. Guimaraes' experiments on the action of coffee. In a recent publication (*Comptes Rendus de l'Académie des Sciences*, 1882) that observer demonstrated that a moderate quantity of coffee, given daily to a dog fed on meat, resulted in causing the animal, after a few days, to consume a larger quantity. M. Guimaraes ascertained, by weighing the urine and excrement, and likewise the animal, that the extra food served as nourishment; he, therefore, concluded that coffee acted as a stimulus to the nerve-centres, and increased the process of assimilation and elimination of proteids. M. Guimaraes then sought to determine if coffee had the same action on all nutritive substances; he therefore fed four dogs on the fat of pork and pap made with maize-flour; the quantity of food the animals swallowed was considerably lessened when he gave them coffee. When coffee was administered to a dog which was allowed to eat meat and pap made with maize-flour without restriction, it would eat the meat and leave the pap. A diet composed of this pap, fat, and 100 grammes of meat, caused the animal to grow thin; if coffee were administered, it lost still more flesh and consumed less food.

M. Strauss has made some interesting and valuable experiments in order to ascertain whether pus forms if micro-organisms be absent. He injected, beneath the skin and in the substance of muscles, water, five parts of almond-oil mixed with one of croton-oil, and turpentine. These irritating substances had been previously sterilised at 115° Cent. (239° Fahr.) The aperture was cauterised by the thermo-cautery before and after the introduction of the nozzle. When these antiseptic precautions prevented the development of micro-organisms, pus was absent at whatever epoch the animal was killed. Neither fragments of charcoal nor cloth introduced under the skin provoked the formation of pus. This result is the opposite to that obtained by M. Pasteur, 1878, when he adopted