

"GONORRHOEAL SALPINGITIS."

To the Editors of THE LANCET.

SIRS,—In THE LANCET of May 20th I am reported (p. 1367) as having said during the discussion on Gonorrhoeal Salpingitis at the British Gynaecological Society on May 11th that in cases of salpingitis the tubes were "sterile in 60 per cent. of cases, the gonococcus was found in 20 per cent., and other micro-organisms in the remaining 20 per cent. In the last group the infection had no doubt been primarily gonorrhoeal and there was a secondary infection by the bacillus coli or the tubercle bacillus." I cannot accept responsibility for this statement as to the tubercle bacillus. What I did say was that probably *some* of the cases included in the last group were primarily due to a gonorrhoeal infection but that there had occurred a secondary infection by some other micro-organism, notably by bacillus coli communis. I did not say anything as to tubes primarily infected by the gonococcus being specially liable to subsequently become infected by the tubercle bacillus.

I am, Sirs, yours faithfully,

May 20th, 1899. ALEXANDER G. R. FOULERTON.

"A CRITICISM OF THE MORE COMMONLY EMPLOYED TESTS FOR ALBUMIN IN URINE."

To the Editors of THE LANCET.

SIRS,—In leaving this subject pending further investigations I would like to point out definitely how the matter stands. In my paper I looked at the matter from the clinical point of view and did not push the tests to a chemical conclusion. I have repeated my experiments twice since and it may be taken as certain that, judging from egg albumen in aqueous solution and pushing the tests to a chemical conclusion, (1) taking the excretion of urine at 50 ounces per diem the heat test will not detect a loss of albumin (dried at 100° C.) of less than from three to three and a half grains per diem; (2) under the same conditions the nitric acid test will detect a loss of a quarter of a grain per diem; and (3) a solution of carbolic acid in absolute alcohol is almost as delicate as the nitric acid test.

The heat test was applied in the usual way—namely, by heating the upper part of the tube to ebullition and after observation heating again after the addition of a drop of acetic acid. Picric acid is much more delicate as a chemical test than I made it in my paper, but the fineness of the cloud, its colour, and the fact that at an early stage of dilution it is clear to transmitted light make its clinical value stop probably at a loss of about two grains of albumin per diem. The investigations will be continued with serum-albumin and in mixtures. Dr. Brodie, my fellow assistant to the Professor of Physiology in Glasgow University, has undertaken an independent investigation. We use different balances, pipettes, and weights and compare results of series of experiments, so that it is not likely that any error in observation will escape detection.

I am, Sirs, yours faithfully,

Glasgow, May 22nd, 1899. WALTER COLQUHOUN.

THE TREATMENT OF TETANUS.

To the Editors of THE LANCET.

SIRS,—I have read with considerable interest the cases of tetanus recently recorded in THE LANCET. It occurs to me that a great opportunity has been lost of harmonising practice with theory. No cultivation appears to have been made in any of the cases recorded. If the drum-stick bacillus of Nicolaier be the actual cause of the disease in question and, being an anaerobic organism, of necessity confined to the tissues in the immediate vicinity of the wound and from thence distributing toxin to the general system, surely the indication would be freely to excise the part, and if necessary to amputate, in either case taking special care not to inoculate the new wound. I must frankly confess to a feeling of disappointment at this not having been done, as two of the cases ended fatally.

I am, Sirs, yours faithfully,

STEPHEN TOWNESEND, F.R.C.S. Eng.

Inner Temple, May 23rd, 1899

PAROTITIS AND INFLUENZA.

To the Editors of THE LANCET.

SIRS,—In an annotation in THE LANCET of May 20th entitled "The Bacillus of Influenza," mention is made of the occurrence of parotitic symptoms in conjunction with the presence of the Canon-Pfeiffer bacillus in the bronchial mucus. This is the first time I have seen in print a suggestion that parotitis may be a complication of influenza and it will accord with the view I took of a case which came under my notice early in the year. In the early part of February I was in attendance for well-marked influenza on two children in a young ladies' school. The head mistress of the school, who, by the way, is a rheumatic subject, nursed them, and in a short while fell sick herself in the same way. On Feb. 11th she was convalescent and two days later, after being downstairs, she had a rise of temperature and pain in the parotid regions—in short, went through a mild attack of what appeared to be mumps. Arising in a school I was eager to trace the source of infection, although I was doubtful whether the complaint was true mumps. All I could ascertain was that a small child had had influenza and had thereafter a similar attack to that from which my patient was suffering, that it was considered an influenzal manifestation, and that she was allowed to mix with others—indeed, she attended at the school some three weeks or more prior to my patient contracting the parotid symptoms. I may say that no other cases of mumps arose in the school, although if the little girl was the source of infection she would be far more likely to infect some other child than to infect the headmistress, of whom she saw very little. I should add that my patient although in middle life had never had mumps, though of course I isolated her so far as was possible. I thought at the time that the complaint must be an influenzal parotitis and not true mumps, but I have not seen any other such cases and have not heard of any other than the little girl mentioned above. I could not get precise dates as to when this child came into the school. It would be interesting to know if many of your readers have met with such a complication or sequela of influenza. I should not have thought of mentioning the matter in print myself but for your annotation. I crave your indulgence to allow me to remain anonymous on this occasion and will subscribe myself,

Yours faithfully,

London, May 23rd, 1899.

M.D. CANTAB.

THE GERMAN SURGICAL CONGRESS.

(FROM OUR BERLIN CORRESPONDENT.)

(Concluded from p. 1190.)

Phosphorus Necrosis.

DR. VON STUBENRAUCH (Munich) said that in experiments made for the purpose of elucidating the action of phosphorus on the bones he trephined the mandibles in dogs and guinea-pigs and injected phosphorus through the alveolus, but necrosis did not ensue. He then exposed the animals to vapours saturated with phosphorus and obtained the same negative result. In order to imitate as much as possible the conditions which the workpeople encounter in match factories the animals were sent to a match factory to be left for a certain time in the drying room which is the most unhealthy department. Although the periosteum had been previously removed from the mandibles no necrosis took place. Dr. von Stubenrauch therefore concluded that the typical necrosis of workers in match factories is not caused by the vapours alone but that a specific predisposition must also exist.

Ileus caused by Gall-stones.

Professor REHN (Frankfort) reported the case of a man operated on for strangulated umbilical hernia. The intestine being gangrenous it was resected and a large gall-stone was found in it. The patient recovered. Professor Rehn said that gall-stones may produce the symptoms of ileus either by mechanical occlusion or by causing inflammation of the intestines. A reflex occlusion, as suggested by Professor Koerte, may sometimes occur; sometimes the stone by conveying virulent germs may irritate the mucous membrane of