Lumbar Abscess.—The following two cases are almost identical in character.

CASE II.—Joseph Leach, of Bradford, was admitted into the Leeds Infirmary on January 5th, 1872, having suffered for some time from frequency of micturition, attended with pain and irritability of the bladder and discharge of pus in the urine. He also complained of pain in the back and left loin, which had commenced shortly before his admission. On examination, obscure fluctuation was detected in the left loin, which induced me on February 1st to introduce the needle of the pneumatic aspirator, by which half an ounce of pus was removed.

February 8th. Six ounces of pus were removed by the aspirator, and on February 15th five and a half ounces. The patient was relieved by these withdrawals of pus, the pain and vesical irritability being diminished. As the relief, however, was but temporary, the operation was not further repeated. The patient continued to lose strength and to suffer more pain; and, leaving the hospital on April 15th, died a month after his return home.

CASE III.—Mrs. —, aged 30, is at present in the hospital, complaining of pain and irritability of the bladder, and pain in the right loin; the urine containing large quantities of unaltered pus. Fluctuation can be detected on deep pressure, both in front and behind the right kidney. On December 5th, the needle of the aspirator was introduced into the back of the loin at the edge of the erector spinæ, and six ounces of fetid pus withdrawn.

December 9th. Since the operation, she had suffered rather more pain in the loin, and had had no diminution of irritability of the bladder; but for three or four days the quantity of pus in the urine was much diminished.

In both these cases, the question of the existence of fluid and of pus was settled by the aspirator; and in both instances the abscess in the loin appeared to communicate with the kidney or ureter; and the pus, entering the bladder, set up a most distressing form of vesical irritability. The relief, however, obtained by the withdrawal of the pus, was in the one case so doubtful, and in the other so transient, that I do not feel encouraged to continue the use of the aspirator as a means of further treatment in such cases.

ON MOLLUSCUM CONTAGIOSUM.

By DAVID FERRIER, M.D.,

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In the eighth volume of the *St. Bartholomew's Hospital Reports*, Dr. Dyce Duckworth gives a very careful analysis of the facts for and against the contagious nature of this affection, as well as of the various theories as to its production. That the disease is in some measure contagious, appears to be satisfactorily established. A case which has just come under my notice at the West London Hospital would confirm this view.

A. H., aged nine months, had numerous mollusca, twenty-two in all, situated on the lips, cheeks, and neck, some pedunculated, others sessile, varying in size from a pea to a millet-seed, the largest being on the lips. Two others were situated on the buttocks. They began to appear five months ago. Some had fallen off of their own accord, and had been succeeded by others. The mother of the child had two well marked mollusca on the left breast; they had existed for some time, and, as far as could be learned, came after the appearance of the tumours on the child's lips. The mother attributed the affection to contagion from a little girl, the daughter of a neighbour, who had "warts" on the face and different parts of the body, who used to frequent the house and kiss the baby. This girl I could not trace, as she had left the district. Whether the primary affection were strictly traceable to contagion or not, the case appears to establish the communication of the disease from the child to the mother, as has been recorded in other instances. The tumours speedily disappeared on being evacuated and touched with perchloride of iron.

The chief interest of the case to me was, that it afforded me an opportunity of carefully investigating the parasitic theory of the disease, propounded first by M. Hardy, and subsequently by Mr. Balmanno Squire. M. Hardy describes, among the well known elements of the follicular contents, spores and branched mycelium containing spores. Mr. Squire describes spores resembling those of the *achorion Schönleini*, large and faceted; but no mycelium. To the irritation caused by this fungus he attributes the hypertrophy of the follicles and their secretion. I have made repeated examination of the expressed follicular contents with a view to determine the question; at the same time

being favourably disposed to this theory, as most satisfactorily accounting for the contagiousness of the disease. When the secretion was treated with caustic potash, and examined under the microscope, it gradually became clear from saponification of the fat, and the outlines of the epithelial cellular contents became sharp and distinct. In the secretion so treated, especially the soft central part from the large tumours, I have seen one or two unmistakable torula-cells in the act of germination. They did not differ in size or otherwise from the torula-spores of penicillium. But, in addition, a great many other spore-like bodies were to be observed in the angles of the cells, apparently multiplying by germination, and even lengthening out into mycelium. They were of various sizes and forms, and had a peculiar vacuolate aspect. They may readily be mistaken for true spores. I have come to the conclusion that they are, however, artificial products of the action of the caustic potash on the fatty matters. I mention these appearances in order that the deceptive effect of the action of reagents may be guarded against. In the contents of the follicles, after several days' maceration in ether, I have failed to detect any spore-like bodies; nor have I been able to produce any appearance of such by treating them subsequently with caustic potash. In the small and still firm tumours, before they have undergone central fatty degeneration, I have not been able to satisfy myself of the existence of any true spores by any method of treatment. In order to determine whether they might exist in a form undiscoverable by the microscope, I adopted the method of cultivating the secretion in Pasteur's solution, consisting of materials favourable for the growth of fungi. In the soft secretion so treated, after a fortnight's cultivation in a closed tube guarded against external contamination, I have found, along with very beautiful specimens of *sarcina*—derived, no doubt, from the blood, in the manner which I have on a former occasion shown—a copious development of torula-cells in no respects differing from those of penicillium. Some of these may have had their origin in accidental introduction into the tube; but the mode in which they were developed in and around the fatty secretion indicated that there was considerable multiplication of elements existing in the secretion. This, however, I look upon merely as an accident, and do not regard these spores as having any connexion with the causation of the disease. It would be more remarkable not to find such fungi, considering their cosmopolitan nature and the possibility of their access to the contents of the follicle through its orifice.

The existence of a vegetable parasite as the cause of this disease appears to me to be entirely negatived by the result of my investigations. What the real cause of the disease is, remains yet to be discovered.

CASE OF POISONING BY ACONITE TREATED BY DIGITALIS: RECOVERY.

By WILLIAM DOBIE, L.R.C.S. & P.E., Keighley.

I WAS requested one morning, between 12 and 1 A.M., to visit a veterinary surgeon who was supposed to have taken poison. The place where he lodged was scarcely a hundred yards from my house, and only a few minutes elapsed before I saw the patient. He was stupidly drunk in bed, and unable satisfactorily to answer questions. His landlady, however, informed me that he returned home the worse for drink about midnight; that he went direct to his surgery, took out a bottle of medicine, and went up stairs to bed; that shortly afterwards he rang the bell, and said he had taken a large dose of poison, which was certain in a short time to prove fatal. There was a two-ounce bottle, with its label defaced, lying by the bedside. The bottle contained about half a drachm of a brown-coloured liquid, a portion of which I applied to my tongue, and was satisfied, by the characteristic tingling induced, that it was aconite. Up to this time there were no symptoms of the patient having taken poison. There had been no vomiting, the breathing was natural, the pulse of fair volume and strength, and the extremities were warm. An emetic was prescribed; and, in conjunction with my late partner, Dr. Ramsay, I visited the man again in less than half an hour. By this time he had vomited freely; a considerable discharge had also taken place from the bowels; there was evidence, too, of failing circulation; the pulse was rapid and feeble, and the feet and hands were getting cold. The use of stimulants was clearly indicated; and, in order to give ammonia and brandy, we raised the patient's head. This brought on alarming prostration; the breathing became laboured; the pulse, at the wrist, irregular, intermittent, and finally imperceptible; there was a quantity of frothy mucus discharged from the mouth and nostrils; the skin became dusky; a cold clammy sweat bedewed the face and forehead: in a word, the patient was dying. We quickly replaced his head upon the pillow, and, as he was unable