CASE OF LONG-STANDING DIARRHŒA CURED BY THE SURGICAL TREATMENT OF AN ULCER OF THE RECTUM.

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DR. —, aged 50, applied to me on the 28th of March, 1872, on account of persistent dysenteric diarrhœa. He had recently returned from India, where he had resided continuously since May 1847. For the last three years he had suffered from constant diarrhœa, which had resisted many remedies carefully tried. He also suffered from much irritation of the rectum, which was supposed to be due to piles. It was an usual thing for Dr. — to have four, five, or six motions daily, and he had had no "formed" motion for three years. Having made an examination of the rectum, I ascertained the presence of an ulcer, which was situated about one inch above the anal orifice. This ulcer was on the posterior wall of the rectum, was of the size of a fourpenny-piece, and had a depressed surface, with hard edges. It was only slightly painful when touched.

On April 22nd, I made a free incision through the sphincter and base of the ulcer, and this was followed by a decided improvement in the symptoms. On the 21st May, finding that the sore, although much smaller, still presented an irritable appearance, I again divided its base freely. Some inflammation and suppuration of the lymphatics passing up towards the groin followed this second operation; but on the 29th, eight days after its performance, a most marked improvement in the dysenteric symptoms took place. On June 1st, the patient had no motion at all; and on June 2nd, one "formed" motion, the first for three years. Since then, the patient's progress has been perfect towards recovery, and he writes me that he has gained one stone four and a half pounds in weight since the end of March, and is quite well. On examining the rectum a few days ago, I found that the ulcer had quite disappeared, and in its place was a small puckered cicatrix.

During the progress of the case, milk diet only was allowed, but the patient can now take animal food with benefit.

REMARKS.—This case is an illustrative example of a class of cases which are by no means uncommon, but the true nature of which is not always determined, for it is not the first one of the kind which has come under my surgical care after medicine has failed to afford relief. It is important to note that in such cases the operation is not always followed by immediate relief to the diarrhœa, but that some weeks may elapse before improvement occurs. In another case of the kind under my care, it was several months before an improvement in the symptoms was observed. I have also noticed that the first effect of the operation in these cases may be to cause a temporary cessation of the diarrhœa; but this result is not permanent, the final relief only taking place after some weeks or months.

It will not, I think, be doubted that the ulcer of the rectum in the case just reported was the cause of the dysenteric symptoms, for its removal gave the relief which careful medical treatment had failed to afford. The patient, a gentleman of high standing in the Indian medical service, has himself no doubt in regard to this point, and will, I feel sure, bear testimony as to the value of surgical treatment in cases such as his own.

ON MECHANICAL AND FORCIBLE DILATATION OF THE CERVIX UTERI.

By HENRY BENNET, M.D.,

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I HAVE read with interest the articles of Dr. Matthews Duncan on the mechanical dilatation of the cervix uteri, as also the subsequent remarks of Dr. Atthill and of Dr. Aveling. I quite agree with the latter writers, that Dr. Matthews Duncan has made a valuable contribution to the mechanics of the subject, and has added to our knowledge of what is actually done when *force* is used to introduce a sound or bougie into the cavity of the uterus, either for mere uterine examination or for dilatation of the os internum. I am surprised, however, that none of these gynecologists have noticed or discussed the doctrinal points which I brought before the British Medical Association in a paper read at Birmingham last August, which was published in this JOURNAL in October; yet these doctrinal points are at the base of the entire question of cervical dilatation. In this paper I merely reproduced the views brought before the profession in 1848, in the second edition of my work on uterine inflammation, and which I have repeated, with the autho-

riety of increasing experience, in each subsequent edition. I maintain, founding myself on anatomy and physiology, that the os uteri internum is normally closed during life, by a vital contraction of the circular fibres, which prevents, in healthy women, the easy entrance of the uterine sound *at all*, except in some women, not in all, a day or two before, during, and a day or two after, menstruation. In these healthy women force has to be used to pass it; and this force, Dr. Matthews Duncan tells us, is, in his experienced hands, equivalent to four pounds, an amount of force that would bend a metal sound if exercised at the handle.

Dr. Duncan does not speak of healthy sound women; but I believe his remark applies to them, as he appears entirely to ignore the fact that in healthy women the os is vitally closed physiologically, and evidently considers this healthy vital closure a morbid state, as did Sir James Simpson, from whom he has evidently taken this view.

The os uteri internum is actually open so as to admit the uterine sound or an equally large bougie, only, as a rule (to which, however, there may be exceptions), in morbid uterine conditions. It may be thus open in chronic inflammatory disease of the mucous membrane lining the cervical canal or the cavity of the uterus, in enlargement of the body of the uterus from whatever cause, from fibroid tumours, from chronic inflammation, or from non-involution after pregnancy. In any of these states the circular fibres at the os internum *may* relax (they do not always do so), and may allow a free passage to the sound or bougie. To these states I would add that of some women of very lax fibre, with relaxed vulva, vagina, and abdomen, and with general flabbiness of tissue.

In healthy women, I repeat, as a rule, the os internum during life is closed vitally by contraction of the circular fibre. To consider this condition a morbid one is an error—anatomical, physiological, pathological. Practically, it is a most pernicious error, for it renders nearly every healthy woman who is examined liable to be considered as suffering under morbid closure. Such an erroneous idea leads to unnecessary, unjustifiable surgical interference, to cutting, to forcible dilatation with four pounds' pressure, to dilatation of all sorts and kinds, and that without real necessity.

This is the question that we ought first to discuss—What is the natural state of the os uteri internum? What is the amount of contraction that requires dilatation at all? These points I endeavoured to decide in the paper alluded to, recently published, so I shall not here revert to them, but refer to the essay itself.

I would only add that it is indeed necessary that these questions should be settled when men in the position of my friends, Dr. Matthews Duncan of Edinburgh, and Dr. Greenhalgh of London, entirely ignore the physiological condition of the os internum—do not even allude to it. At a meeting of the Obstetric Society in 1866, at which I was present, in the discussion of a paper by Dr. Tilt on "Undue Surgical Interference in Uterine Disease", Dr. Greenhalgh told us that in less than two years he had divided the os internum, with his metrotome, in more than three hundred cases in his own study. I should have liked to know what proportion these cases of division bore to the cases of uterine disease thus seen in the course of the two years in which division was not practised.

CASES ILLUSTRATING THE USE OF THE PNEUMATIC ASPIRATOR.

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As Mr. Jessop has called attention to the use of the pneumatic aspirator—an instrument highly valued by my colleagues and myself at the Leeds Infirmary—the following additional illustrations of its employment may not be without interest.

Diagnosis of Multilocular Ovarian Tumour.—In the summer of 1871, I was requested by Dr. Bealy of Harrogate to examine a case of abdominal tumour of doubtful character. A large, apparently solid tumour, of uneven surface, in which fluctuation could not be detected, filled the lower part of the abdomen, and percussion of the hollow viscera did not give very decided aid in the diagnosis. Seeing that the tumour, if ovarian and non-malignant, was probably polycystic, and that, if it were polycystic, no two cysts would be likely to contain exactly similar fluid, I introduced the needle of the aspirator—first in one prominent part, and then in a second—and drew off in the first instance an ounce of turbid viscid fluid, and in the second clear limpid fluid. Thus was proved the existence of multiple cysts varying in their contents—a condition, I believe, exhibited by no other kind of abdominal tumour than ovarian.

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.

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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