ani; but latterly I introduced the rectal speculum first, and then passed the tube through it with the greatest ease to myself and the patient. During the month of August, I had the advice and assistance of several medical friends who were spending their holidays here. Dr. Kirk of Partick suggested that I should give extract of belladonna a fair trial, which I did, without any good effect. Opium had been given in large doses at an early stage of the disease, with a similar result.

I now sought the advice of Professor Gairdner of Glasgow, who came and saw the patient. He suggested my asking Dr. Cameron to see the case. Dr. Cameron and Dr. Grieve saw the case eight days after Dr. Gairdner. Dr. Cameron suggested puncturing the bowel; but, as I had been taught that that was a very dangerous operation, and one that should not be performed until all other things had failed, I did not consent to its being done. I was still able to partly relieve the distension with the long tube passed *per anum*.

In the BRITISH MEDICAL JOURNAL of September 27th, 1879, several cases of intestinal obstruction were published; and, as they had a direct bearing on my case, I read them very carefully. I was very much struck by what Dr. Goodhart said to his students in a lecture delivered at Guy's Hospital, when speaking about the operation of colopuncture. Here are his own words, taken from the JOURNAL just referred to. "You may, perhaps, think that some less hazardous means of relieving the distension than that of colotomy might be adopted; and another operation has been practised with that end in view, viz., paracentesis. The distension is due partly to gas and partly to fluid fæces; and it has been thought that, by withdrawing the former, the severity of the case might be relieved. One of the distended coils has, therefore, been tapped by a fine trocar and cannula. But there can be no doubt that this is an exceedingly dangerous thing to do; and I do not, from what I have seen and from what others have told, feel in the least inclined to recommend it to your notice. The danger is this: that the distension is, in the majority of cases, but little relieved—that alone is an objection fatal to its adoption; and, the bowel remaining full, and its walls tightly stretched, fæcal matter, which you remember I told you is always liquid, leaks out into the peritoneum after the withdrawal of the cannula from even the smallest puncture. I have myself seen the operation performed; and fæcal matter came out by the cannula at once; no relief followed; and the patient died not long afterwards of acute peritonitis. So do anything rather than this. You are taught, and quite correctly so, that small wounds of the intestine are comparatively dangerless, because the mucous membrane becomes everted, and so closes the aperture; but this only applies to a contracted intestine; but we are dealing with an overfull one. All the coats are in such a case distended probably to their utmost, the rugæ obliterated, and there is nothing to evert; and the smallest hole, under such circumstances, becomes a vent; and a vent, however small, in such a position, is fatal."

I called Dr. Cameron's attention to the articles published in this number of the JOURNAL. After he had read them, he told me he would come and see the patient, and do whatever might be considered best under the circumstances. Notwithstanding Dr. Goodhart's teaching, colopuncture was agreed upon. The patient was placed on his back in bed, the position in which he always lay since his illness. No chloroform was given—wisely, I think, as you will hear shortly. Dr. Cameron thrust the trocar and cannula into the middle of the transverse colon. The gas blew off for a few minutes, and the bowel was about half emptied of it, when the bowel was strongly drawn to the left side by its own peristaltic action. The cannula was laid down on the walls of the abdomen, and liquid fæces began to escape from the cannula. Dr. Cameron placed his thumb on the mouth of the cannula, and then injected a small quantity of tepid water through the cannula into the bowel. He now withdrew the cannula, and placed a small piece of adhesive plaster over the wound; and we left the patient in bed. We did not consider the operation very successful, as the gas was only partially liberated.

But now comes the wonderful part of the case. Three hours after the operation, the patient had a very copious discharge from the bowels of dark clayish liquid fæces, followed by a second one hour after the first. Seven hours after the operation, a large quantity of gas was passed *per anum;* and by next morning the abdomen was quite flat, and the distension completely gone. The kidneys now began to act vigorously. The ordinary chamber-utensil was filled three times in sixteen hours, and at the end of three days all the œdema in the limbs had disappeared. The heart's action was now greatly increased; the pulse rose from 48 to 70 per minute. The patient has improved every day since the operation, and is gaining flesh rapidly. Wherever the strangulation existed, I am of opinion that the bowel liberated itself at the time of the operation. Had chloroform been given, the peristaltic action of the bowel might have been reduced, and the operation might not have terminated so well. Had the operation been delayed much longer, the patient could not have lived many days. I look upon this case as another of the triumphs of surgery; and, should a similar case present itself to any of my medical brethren, I hope they will not hesitate to give their patient immediate relief and the chance of complete recovery by the operation of colopuncture.

This paper was read before the Glasgow Medico-Chirurgical Society on February 6th, 1880; and, from the discussion that followed, it was evident that no one suspected the true nature of the disease.

The history of this case since this paper was read is very interesting; and, for the benefit of the profession, I will briefly state the condition of the patient from that date till the present time. Colopuncture was performed on October 30th, 1879. He remained quite well till the month of September 1880. At that time, he took a rather long walk over a hill. The day following, I heard the click-clicking sound which was often heard in his previous illness. All the symptoms previously described began to show themselves. I did not allow the distension to become so great as it was at the time of the operation. I passed the long tube *per anum* at the end of eight days, and let off a large quantity of gas. From that date till the present, I have been under the necessity of letting off the gas every eight days, either by passing the long tube or performing colopuncture. I have punctured three times since the month of November 1880. At the beginning of the year 1881, I began to pass a rectum-bougie every second day. I had to use the smallest size (about the size of the ordinary stomach-pump tube). I had an idea that the cause of obstruction might be at the upper part of the rectum. It was almost impossible to get the finger introduced into the rectum, on account of the violent contraction of the sphincter ani. I introduced the rectal-speculum, and distended the sphincter ani as much as I could. I inserted my finger as far as I could, and thought I found something like a fleshy mass at the upper part of the rectum. I diagnosed the case as one of intussusception. I now adopted a somewhat novel plan to cure my patient, and the result was most successful. I got the ileo-cæcal portion of the bowel of a sheep. To a portion of it six inches long, I inserted a small ivory tube three inches long; to the one end I tied the intestine of the sheep; to the other I tied an elastic tube one foot long; to the end of the elastic tube I tied a small stopcock. I introduced the long tube, and let off the gas. I now put in the intestine of the sheep through the speculum. As soon as it was completely within the cavity of the rectum, I blew it full of air, and turned the stopcock. This at once pushed the prolapsed bowel up, and I could feel the distended bag taking the direction of the descending colon. I allowed the distended bag to remain in for half an hour; then opened the stopcock, and cautiously withdrew the bag from the rectum. Next morning, all the distension was gone, and the click-clicking sound was heard no more. The following day, the patient had a very natural motion of the bowels, the first of the kind since the month of September last. He is now quite well. It is now easy to understand what took place at the time of the first operation of colopuncture. The violent peristaltic action of the bowel withdrew the intussuscepted portion, and consequently got rid of the obstruction.

DUPUYTREN'S CONTRACTION OF THE FINGERS IN WOMEN.

By WILLIAM ADAMS, F.R.C.S.,

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DUPUYTREN'S contraction of the fingers is, according to the testimony of all surgical authorities, of extremely rare occurrence in the female; and the communication of Dr. Carter of Leamington, published in the BRITISH MEDICAL JOURNAL of December 24th, 1881, is therefore of great interest, as he records two cases of this affection occurring in the female; one, now under his own observation, in a lady aged 88; and the other a sister of this lady, said to have been similarly affected, who died at the age of 70.

From Dr. Carter's description of the cases now under his observation, more especially from his remark that "there is much puckering of the skin of the palm", I have no doubt that the case he records is a genuine example of Dupuytren's contraction; *i.e.*, a contraction described by Dupuytren as essentially depending upon a thickened condition of the palmar fascia and its digital prolongation, by which the fingers are drawn towards the palm of the hand, and contracted independently of the flexor tendons.

In all probability, the second case referred to by Dr. Carter may

also be accepted as a genuine case of Dupuytren's contraction; but the first case referred to is a valuable contribution to our knowledge.

When I published my little work on *Dupuytren's Finger-Contraction*, in 1879, I stated that "I have never seen it in women"; and since that time only one case has fallen under my observation, and that occurred in a lady aged 66, who consulted me in October 1880, in whom both hands were similarly affected, but the right in a more severe degree than the left. The little finger of the right hand was flexed towards the palm by a prominent fascial band. Contraction of the ring-finger was only just commencing. In this hand, also, a fascial band passing to the thumb was distinctly prominent, and the thumb was slightly drawn towards the palm. In the left hand, the first phalanx of the little finger was not flexed by a prominent band; and in this finger the chief contraction was between the first and second phalanges. In both hands, the skin over the entire palm was dimpled, depressed, and puckered in folds, showing the extent to which the palmar fascia was involved, and the close adhesions between the skin and fascia. In this case, of which models have been preserved, as some improvement in the right hand followed gradual mechanical extension by an instrument, the operation has been deferred. I may add that this lady had been the subject of gout, and belonged to an extremely gouty family. Her father and brother had both suffered from contracted fingers.

Another case, of a somewhat doubtful character, which might be described as a spurious Dupuytren's contraction, occurring in a lady aged 48 (a single lady), was brought under my notice by Dr. F. M. Mackenzie in September last. The ring-finger of the right hand was drawn nearly halfway towards the palm, as a result of a wound in the palm of the hand from a broken glass bottle fourteen years previously; small fragments of glass remained impacted, and were taken away three years afterwards, since which time the contraction had been gradually increasing. A prominently contracted band of fascia played a more important part than usual in this class of cases of traumatic origin, in which all the tissues are generally implicated; and, after its subcutaneous division, the finger was immediately straightened more than we could have anticipated.

My experience, therefore, agrees with the opinion generally expressed, that this affection very rarely occurs in women; and Dr. Myrtle's large practical experience coincides with this opinion. Dr. Myrtle's valuable paper on Dupuytren's Contraction of the Fingers, published in the BRITISH MEDICAL JOURNAL of December 3rd, 1881, is full of practical observations. His views as to the traumatic and idiopathic varieties, also as to the cause or causes and mode of production of the contraction, its connection with gout, etc., are of the greatest interest; but at the present time I wish to refer chiefly to the frequency or unfrequency of the occurrence of this affection in women.

Mr. Reeves, in his communication to the BRITISH MEDICAL JOURNAL of December 31st, 1881, differs from other authorities as to the rarity of the occurrence of Dupuytren's contraction of the finger in the female, and observes: "I can clearly recall five cases, and I am sure that I have seen at least seven or eight in females.....These cases prove not only that Dupuytren's contraction does occur in females, but that it may be regarded as not very uncommon in them."

Two of these cases are mentioned as occurring in young ladies, one aged 17, the other 25, devoted to piano-playing, the strain and irritation of which Mr. Reeves regards as the starting-point of the pathological change which produces the contraction. Mr. Reeves observes: "The younger patient has the ring-fingers of both hands affected, and the little finger of the right is also contracted. The fascial bands are not strongly marked as yet, although the disease has lasted two years. .....The second and older patient has only the ring-finger of the left hand affected." No allusion is made to prominence of fascial bands, nor to the condition of the skin in the palm of the hand, in this case; and no description whatever is given of the other cases referred to. Hence it may be asked, Were all these cases genuine examples of Dupuytren's contraction? Did they all present the clear evidence of fascial contraction, with dimpled depressions and puckering of the skin in the palm, and prominent fascial bands leading to the fingers?—characters essential to the class of cases described by Dupuytren. Such cases should, I think, at the present time, when professional attention is directed to the subject, be described accurately in detail, so as to avoid any possibility of error; as we know that contraction of the fingers may take place from a variety of causes, and the pathological conditions will be found to vary according to the nature of the producing cause.

Possibly the affection may be of more frequent occurrence in females than has been supposed, and an unusual number may have fallen under Mr. Reeves's observation; but many cases of supposed Dupuytren's contraction have been sent to me, which I have at once rejected from the class. Within the last fortnight, a young lady aged 16, having the ring and little fingers of both hands contracted, so as to interfere with her playing the piano-forte, was sent to me; but I at once observed that the fingers were bent at the phalangeal articulations, and that there was no evidence of contraction of palmar fascia. The first phalanx could easily be extended beyond a straight line with the metacarpal bone. The father of the young lady then showed me his hands, in both of which the ring and little fingers were contracted, but to a less extent than his daughter's. In him, there was no evidence of contraction of the palmar fascia; but the phalanges of the fingers were a little flexed upon themselves, and could not be straightened. Care must, therefore, be taken to detect undoubted evidence of fascial contraction, before the cases can be grouped in the class described by Dupuytren.

CLINICAL MEMORANDA.

ON CONJUGATE LATERAL DEVIATION OF THE EYES DIRECTLY AFTER EPILEPTIC FITS.

Conjugate lateral deviation of the eyes, away from the paralysed side and towards the side where the lesion occurs, is well known in the apoplectic coma preceding hemiplegia, but I have not found any record of this deviation occurring in epileptic fits.

Through the kindness of the physicans of this hospital, I have noticed that, supposing that a fit begins with rotation of the head and face to the right (and I believe the tonic stage of epilepsy nearly always begins with rotation of the head to one side or the other), and that this is followed by the clonic spasms affecting both sides apparently equally, instantly the clonic stage is over, and the limbs are relaxed, the eyes will be seen to roll slowly to the other side—in this case the left—and to remain conjugately deviated for half a minute to two minutes; and then they often roll slowly from side to side, but remaining parallel. I have noticed the conjugate deviation in eleven out of the last thirteen fits that I have witnessed; and I should think that this might be explained by the theory that the side on which there was the greater motor discharge—as shown by the initial rotation of the head—becomes at the end of the fit the more exhausted, and suffers more paralysis than the other side; and I look upon the slow parallel movement of the eyes from side to side, as probably due to the recovery from exhaustion in the two halves of the brain not being uniform, but one side preponderating over the other in turns. The head does not rotate with the deviation of the eyes, after bilateral fits, because there is not, I believe, sufficient difference of power to cause this; but I have seen rotation of the head accompany the deviation of the eyes in unilateral fits, when the paralysis was severer and lasted for some hours and affected the convulsed side.

The above symptoms, coupled with the knee-reaction (patellar tendon-reflex) and ankle-clonus being more marked on the side to which the head turns in the initial tonic stage, and with the temporary absence (in one case for 30 minutes) of the superficial reflexes (sole of the foot, etc.), on the convulsed side in unilateral fits, which I have observed, tend, I think, to show that the nervous system is temporarily reduced by violent motor discharges to the same condition which is more permanently produced in hemiplegia, viz.:—paralysis, excessive action of the (so-called) tendon-reflexes, diminution of the superficial reflexes, and conjugate deviation of the eyes.

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REMARKS ON PEMPHIGUS.

THE numerous readers of the BRITISH MEDICAL JOURNAL have, no doubt, read with much interest the graphic description of the cure of pemphigus by arsenic, as given by Mr. Jonathan Hutchinson in an abstract of a clinical lecture recently published. At the same time, if the treatment therein advocated be invariably adopted, it cannot but lead, I think, to dissatisfaction and discouragement.

The idea undoubtedly conveyed by Mr. Hutchinson is, that pemphigus is invariably amenable to a course of arsenic. This I most certainly dispute. Were the disease, in all cases, due to "simple" constitutional causes, then, no doubt, arsenic would be an infallible remedy; but, seeing this is not the case, the logical inference must follow that it is not. That pemphigus is frequently connected with the syphilitic taint, no one, I think, acquainted with syphilis will gainsay. In such a case, arsenic could be of no avail; and the only remedy would be to remove the cause, viz., the syphilitic taint. A case of this kind, treated by my much respected teacher Professor McCall Anderson, in the cutaneous disease wards of the Glasgow Western Infirmary, made upon