quently no shortening, in fact sometimes slight lengthening. In excision, on the other hand, the results as regards shortening are very bad, especially in the knee-joint; hence in children excision of the knee is almost absolutely contraindicated. This also holds good as regards other joints, though no doubt to a less extent. Even after excision of the hip, more especially where the trochanter is interfered with very serious shortening sometimes occurs, and I see a case from time to time where excision of the hip was performed in a boy $4\frac{1}{2}$ years old, and where eleven years later there was 11 inches of shortening. It has been stated, as regards the knee, that if intraepiphyseal excision is performed the interference with growth is not great, and Hoffa advocates this operation strongly. His experience is, however, almost unique, and there seems no doubt that in many cases of intraepiphyseal excision serious interference with growth results. Thus Petersen showed a specimen where this operation was performed in a boy aged 11, and where six years later there were $5\frac{1}{2}$ inches of shortening although both epiphyseal lines could be seen with more than an inch of bone between them. Thus even though the epiphyseal cartilages are not actually interfered with, the condensation of the bone in connection with the healing process is apt to extend to them, and lead to their premature ossification. But after excision the limb below also often grows very imperfectly, more so than can in all cases be explained by deficient use of the limb. Neuritis has been suggested as the explanation; certainly disturbances of the nervous mechanism are not uncommon after excision of the elbow for example, the pulse on the affected side being often weaker than on the other, and there being increased growth of nails and hair, increased secretion of sweat, etc. Another cause of shortening after excision of the knee-joint is that unless care is taken that the posterior edges of the bones shall correspond, the larger upper end of the tibia projects backwards into the popliteal space, presses on the vessels, and thus interferes with the circulation in the lower limb.

The conclusion which seems to me most just is that the proper radical operation in children, and up to 15 or 16 years of age, is arthroectomy, and in adults is excision. Let us consider the individual joints.

In the hip a really complete arthroectomy is almost impossible, but in children I think that in many cases partial operations, such as clearing out abscesses, partial arthroectomy, etc., are often preferable to excision as regards the ultimate results, though no doubt there are cases where removal of the head of the bone is desirable. For example, in primary acetabular disease with or without abscess inside the pelvis satisfactory access can seldom be obtained without removing the head of the bone. Again, where the head of the bone is much diseased or the epiphyseal line destroyed, it is better to cut through the neck. And also in cases with septic sinuses where operation becomes necessary, slitting up the sinuses, removal of the head of the bone and subsequent stuffing of the cavity with iodoform gauze is probably the best practice. Apart from the question of shortening, which is not so very great, if only the head of the femur is removed, excision leaves a weaker and less stable joint than is obtained in other ways.

In the case of the knee-joint, arthroectomy possesses no advantage over excision as regards the immediate result; on the contrary, if a stiff joint is aimed at, a firmer result will be obtained by excision. But in patients who have not reached their full growth, the interference with growth after excision is so serious that it should not be undertaken. Certainly in young children excision is not to be thought of for a moment; in adults, however, a better ankylosis is obtained by it, and in my opinion it is the preferable operation.

In the case of the ankle-joint, arthroectomy with or without removal of the astragalus—I think in most cases with—is the operation for children, and gives most satisfactory results. I have not had the opportunity of trying it in adults, as the cases of ankle-joint disease which I have lately had have either not been limited to the ankle, or, if so limited and requiring operation, have necessitated amputation; but I should be very much inclined in a suitable case in an adult to perform arthroectomy with removal of the astragalus, instead of excision.

The remarks which I have made as regards the knee apply

also in the main to the elbow. Arthroectomy is the operation for children, and very satisfactory movement is thereby obtained. Excision is, however, I think, a better operation for adults; movement is more easily obtained, and the disease more readily eradicated.

As regards the wrist and tarsus, complete arthroectomy is, of course, impossible, and in children it is best to do what one can by partial operations. Fortunately, wrist-joint disease is not common in children, and in adults excision often gives a very good result. In the case of the tarsus in children, it is sometimes possible to get an excellent result by removal of the affected bone and synovial membrane, or of a healthy bone such as a cuneiform, so as to get at the synovial membrane. The shoulder stands, I think, on the same footing as the hip in childhood, but it is essentially a disease of adult life, and in adults excision is the radical treatment.

Time fails me to go into the operative treatment of tuberculous diseases elsewhere. The question, when removal is required, again lies between partial and complete removal; and I must say the more I work at the matter, the more I incline to complete removal where operation is necessary. Further, if complete removal can be done, clean cutting is much to be preferred to scraping. Similar partial operations, laying open sinuses, and stuffing with iodoform gauze, washing out and injecting abscesses, etc., come into play, and must be employed according to the circumstances of the case.

DEPRESSION OF THE EPIGASTRIC AND HYPOCHONDRIAC REGIONS ON DEEP INSPIRATION NOT ALWAYS INDICATIVE OF DIAPHRAGMATIC PARALYSIS.

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ABOUT twelve months ago I had under my care in the wards of the Manchester Infirmary a severe case of general diphtherial paralysis, and was carefully watching for signs of paralysis of the diaphragm, and of the other muscles of respiration. One day I observed that, on a deep inspiration, there was a marked sinking in of the epigastric and hypochondriac regions, and concluded that paralysis of the diaphragm had supervened. Only two days after the above observation I noticed that the intercostal muscles were not acting, but that the diaphragm was acting freely. As the patient gradually recovered, I looked upon the rapid recovery of the diaphragmatic paralysis as most opportune, since had it remained whilst the intercostal muscles became paralysed, death must of course have ensued. This case had been very much impressed upon my mind, and I had repeatedly spoken of it to students as an instance of a lucky coincidence in the recovery of an important muscle where a set of equally important muscles became paralysed.

About six months since I had another severe case of diphtherial paralysis under observation, and impressed upon the clinical clerk the importance of watching for signs of paralysis of the muscles of respiration. One day when examining the patient I told him to take a deep breath, and found very marked sinking in of the upper part of the abdomen when he did so. This repeatedly happened on the occasion when I was examining him, but I noticed that whilst on deep inspiration the epigastric and hypochondriac regions sank in, on quiet inspiration the same parts became protruded, and on expiration retracted, as one would expect in health. By explaining to this patient how I wanted him to breathe I found that in the course of that examination I could get him to protrude the epigastric and hypochondriac regions on a deep inspiration instead of allowing their retraction as he had done immediately before.

This second case made me very doubtful as to the correctness of my inference in the first case, and also very sceptical as to the value of sinking in of the upper part of the abdomen on a deep inspiration as a sign of diaphragmatic paralysis. My house physician (Dr. E. M. Brockbank) kindly undertook to examine a large number of patients who were in the hos-

pital for various diseases, but in whom there was no suspicion of diaphragmatic paralysis, and no cause for the same. He soon found that the behaviour of the abdominal wall on a deep inspiration varied very much in different persons and that in some few, but yet a considerable number of cases in which there was no diaphragmatic paralysis, the epigastric and hypochondriac regions sank in instead of protruding when a deep inspiration was taken.

Since then I have examined a large number of cases in my own beds, and, through the courtesy of my colleagues, elsewhere in the infirmary, and find that whilst in the majority of cases both during quiet and deep breathing the upper part of the abdomen is protruded on a deep inspiration; in other cases, which are not infrequently met with, that part of the abdomen becomes depressed, sinks in, on a deep inspiration being taken. In other instances we find that, on a deep inspiration, the abdominal movement is a very slight one; it protrudes to a small amount on a deep inspiration, but not very markedly, the chief intake of air being apparently produced by an exaggerated movement of the ribs. In all the cases there was no abdominal pain or other local cause for the abnormal type of breathing.

In some of the cases where the upper part of the abdomen does not protrude on a full inspiration, the sinking in is only an apparent one. The lower ribs are elevated, and especially so much expanded, that the upper part of the abdomen appears to sink in. In other cases the sinking in of the abdomen is not apparent but real on a deep inspiration. These latter cases are very interesting, because, in the majority of them, the upper part of the abdomen on quiet respiration protrudes on inspiration and becomes retracted on expiration exactly as we should expect it to do normally, but the moment the patient is told to take a full breath and does so, then retraction takes place during the inspiratory period. In some of such cases it is possible so to instruct the person that he will after a short time protrude the abdomen during a full inspiration, whereas only just previously retraction had been noticed. With children, however, and in nervous persons, in both of whom the abnormality of retraction of the abdomen on full inspiration has not infrequently been observed, by both Dr. Brockbank and myself, it may be difficult to bring about this result. In such cases, however, I have never noticed that on quiet respiration the upper part of the abdomen became retracted during the inspiratory period.

It is possible that this observation, that in some cases where there is no diaphragmatic paralysis the abdomen becomes retracted on the patient taking a full inspiration, has been noted before. I have, however, never seen any record of it, and as both myself and other medical men of my acquaintance have been misled by it, there will be no harm in calling attention to the fact, even though it may be well known to many physicians.

Vierordt, in his excellent work, *Diagnostik der inneren Krankheiten*, 1888, page 63, states that paralysis of the diaphragm may be met with not only in organic diseases of the nervous system, but also as a functional neurosis (hysteria). Is it probable that in such cases there is an actual paralysis of the diaphragm? For myself, I should think it much more probable that the cases of apparent paralysis of the diaphragm in hysterical subjects were instances of the condition I have referred to in this communication, and that the paralysis was only apparent and not real.

The practical result appears to me to be that it is important in all cases where we are examining for paralysis of the diaphragm not at once to tell the patient to take a deep breath, but to note the movements of the abdomen during quiet respiration first; and if the abdomen moves naturally on quiet respiration, whilst on full and forced inspiration the upper part becomes retracted, it is not to be considered on that indication alone that there is a paralysis of the diaphragm.

ON August 1st a new clinic for skin diseases, with accommodation for seventy-four patients, and the completest possible equipment for clinical and scientific research will be opened in the University of Breslau. A "poliklinik," or out-patient department, will be attached to it, the whole being under the direction of Professor Neisser.

MEMORANDA:

MEDICAL, SURGICAL, OBSTETRICAL, THERAPEUTICAL, PATHOLOGICAL, Etc.

VESICAL CALCULUS: PERFORATION OF BLADDER.

IN the BRITISH MEDICAL JOURNAL of April 2nd Dr. Farrar relates an interesting case in which a calculus ulcerated through the bladder wall and at the same time acted efficiently as a plug. He then refers to the question of lithotomy in such a case. The following notes may be of interest as bearing on that point:

A single woman, aged 23, was first seen three years ago, when she complained of pain in the abdomen, which was at times very acute; she stated that for some years there had been a discharge of "bloody matter" from the bowel. There was, she stated, no vaginal discharge or trouble with the water. No physical examination was allowed. The patient was not seen again until September, 1891, when she complained of pain across the middle of the abdomen and over the base of the sacrum, also of great frequency of micturition, and that the urine was thick and foul-smelling. These symptoms were said to have existed for six or eight weeks. The patient now for the first time mentioned that a long time ago she had passed a hairpin into her bladder. The urine was putrid and contained a copious deposit of triple phosphates and pus and about $\frac{1}{2}$ of albumen.

On October 1st, 1891, the patient was placed under chloroform by Dr. Dabbs, and, a sound being passed, a calculus was detected. I then introduced my little finger into the bladder and found that the stone was large and very fixed. The bladder was then emptied of urine and as much warm boracic lotion as possible—only a small quantity—administered. The bladder was opened above the pubes in the usual manner and the stone extracted. The bladder was washed out and a drainage tube left with one end projecting from the urethra and the other through the abdominal wound.

The calculus was phosphatic, and weighed $4\frac{1}{2}$ ounces. It measured $3+2+1\frac{1}{2}$ in. It was pyriform, and presented for the most part a roughened surface. The obtuse end of the stone was lying in the fundus of the bladder and was adherent to it; the smaller end was firmly embedded in the bladder wall near the neck. Both ends of the stone, but more especially the lower end, presented a ragged appearance, due to shreds of tissue still adhering to it. On making a section of the calculus, an ordinary hairpin was found in the centre, with its long axis corresponding to the same axis of the stone.

The patient was a good deal collapsed after the operation; but this passed off, and she continued to do well until the third day, when the drainage tube, though a large one, became temporarily blocked by thick mucus, and she complained of sudden pain in the abdomen, which was followed by vomiting. She died of peritonitis on the fifth day.

Post-mortem Examination.—The bladder was much thickened, contracted, and sacculated. The mucous membrane was deeply congested and in places almost black; it was thickened, rough, and granular. At the fundus of the bladder, posteriorly and slightly to the left, there was an ulcer the size of a shilling, with sharply defined edges, and having for its floor only the peritoneum, which latter had a minute perforation in it. Near the right side of the neck of the bladder there was another ulcer, smaller than the above, and perforating the entire bladder wall; it was, of course, below the level of the peritoneum, and, though it closely approached the rectum, no connection could be made out. These two ulcers corresponded respectively to the superior and inferior poles of the calculus. The ureters were much thickened, and there was advanced pyonephrosis on both sides, a greater degree on the left. The suprapubic wound had not apparently injured the peritoneum. There was much recent lymph over the coils of the ileum, and over the fundus of the bladder there were evidences of old peritonitis.

In the above case I am inclined to think, from the sudden onset of pain and collapse on the third evening, that there was no escape of urine into the peritoneal cavity until that time, when I believe a small perforation occurred through the