

whether the tumor were ovarian or not, if there seemed to be any probability of its being possible to remove it.

As to the mechanism by which this singular rotation is produced, we may at once dismiss any explanation which attributes it to the condition of the tumors themselves, for we find it occurring in tumors of all kinds, large, small, smooth, and globular, multicystic and irregular, parovarian, ovarian, dermoid, and solid fibrous tumors, the only intrinsic conditions of the tumors being that they should be free to move, and have pedicles capable of being twisted.

Unfortunately, in the majority, or at least in a very large number, of the cases, the direction of the twist is not clearly stated, or not given at all, nor is the side on which the tumor grew clearly given.

Of the cases narrated by Rokitansky, the great majority, about four-fifths, were tumors of the right side, and in a still larger proportion the twist was from the left to the right side—that is, taking the vertebral column as the starting-point, the twist travelled to the left side, and then forward and over to the right, that being what I read as his “und ebenso kommt die Drehung nach aussen weitans häufiger vor, als jene nach innen,” though it is by no means certain that my rendering is correct.

Certainly, in all of my own cases the tumor was on the right side, and the twisting in all those operated upon was as I have just described, and in the first case I have given my recollection as that it was in this direction also. It is not recorded so in my notes, however, and my memory may be in error, though I think it is likely to be correct, as the case made a more profound impression on my mind than, perhaps, any other incident in my surgical experience.

If we had exact statements on these points for a large number of cases, I think we might arrive at some conclusion as to the cause of the rotation.

In a few of the isolated cases explanations are given which seemed more or less possible to the narrators, but they do not bear the examination of extended experience. To two of these I have already alluded, and only a third requires to be mentioned. Dr. Barnes hazards the explanation that “the tumor being free from adhesions, and tolerably firm, may roll over on its axis. This may happen from the enlargement of the uterus tilting it over, or from over-exertion, when, one part of the tumor being more pressed upon than the opposite part, it rolls over.” The part of this explanation which applies to cases where the rotation occurs in association with a pregnant uterus applies

only to a small number of the cases, even if it were sufficient, which I do not think it is, and therefore may be dismissed. The rest of the explanation simply amounts to a repetition of the fact that this singular phenomenon does occur, and is no explanation at all.

The only reasonable effort to explain the incident has been made by Klob, who has made some experiments, from which he concludes that it is the alternate filling and evacuation of the bladder which rotates the tumor. I have not been able to find the original paper, and am, therefore, unable to criticise the basis of his opinion, but on *à priori* grounds I think there may be something in his idea. But before I knew of this explanation, and entirely from my own cases, I had come to the conclusion that it was the alternant filling and emptying of the rectum which caused the rotation, and it is possible enough that the bladder may help. That the bladder alone should do it is, I think, unlikely, for being central its influence would be, in all probability, neutral. If it were the rectum, then this force acting on the left side of the point of rest, the vertebral column, would inevitably push the tumor in the direction in which, in at least nine out of ten of my cases, the movement took place; and it would certainly act more readily on right-side tumors than on those of the left side, for the former are anchored so that the pushing force of the rectum will be in the requisite oblique direction, in the plane of a screw, and very nearly at right angles to the axis of movement.

If I might venture to apply a dynamical illustration to pathology, I would say that an ovarian tumor growing on the right side with a free pedicle, and resting, therefore, with its axis inclined toward the top of the ninth or tenth rib on the left side, would be in the condition of a body having freedom of the first order—that is, free to rotate about a fixed axis, but not to slide along it. To such a body a screw, in the form of a wedge, would be applied by the rectum in the most favorable of all directions, in a direction obliquely from above downward, across the axis of freedom and below the equator of the moving body. Every piece of feces which passed into the rectum, especially in the recumbent position of the patient, would act as a wedge to drive the tumor round. In obedience to the dynamic law, that by a successive repetition of the process an indefinite quantity of energy may be produced, however small the initial force may be, we have at once the explanation of the phenomena of many of these cases, notably of that published by Mr. Thornton. We have, in fact, this process of rotation going on slowly until the point of strangulation has arrived, when the sudden access of pain for

the first time indicates that something has gone wrong. For the bladder a similar wedge-like influence may be claimed, but from its want of obliquity, it is not likely to be so powerful an agent in the production of rotation. Both rectum and bladder would act, however, in the same direction, and if it be found on further investigation that the tumors are mostly those of the right side, and are generally twisted in the direction in which mine were, I think we may accept the rectum as the chief factor.

That this rotation may occur suddenly, that is to say, that an ovarian tumor may be twisted rapidly round two or three times in a few minutes or hours, is inconceivable.

Rokitansky publishes (1865, loc. cit.) the post-mortem accounts of fifty-eight cases of ovarian tumor in a period of four years, and in eight of these rotation of the tumor had occurred, but in four only did it seem to have given rise to strangulation and death. Rotation is, therefore, frequent, as Rokitansky says, occurring in about twelve per cent. of all cases, and in about six per cent. of all cases producing death. My own proportion is not nearly so high, my first hundred ovariectomies including only one of my cases, and in the practice of other ovariectomists we have not as yet heard much of gangrene from rotation.

The greater part of what I have just said on this interesting subject is taken from a paper which was read before the Obstetrical Society of London last year. In the discussion which followed, my theory of the cause of axial rotation received very material confirmation from Mr. Alban Doran, who said that it accorded with some of his own convictions grounded on experiments he had made in the post-mortem room of the Samaritan Hospital when examining cases of ovarian disease that had proved fatal before any operation could be performed. Supposing that a large tumor with an irregular surface lay to the right of the rectum, an accumulation of fæces might press upon the pelvic portion of the growth in such a manner as to push the whole tumor about a quarter of a turn round its vertical axis. Should the pedicle be very long, or short, yet inelastic, it would remain twisted after this pressure was removed, and might become still more twisted after it was reapplied. Should the pedicle be short and elastic, the tumor would slip back to its normal position every time that the pressure was removed; only this pressure might be applied so long that the temporary torsion might involve damage to the vessels of the pedicle, producing all the bad effects of permanent and complete torsion after the pedicle has become untwisted. In examining the body of a patient who died in Mr. Knowsley Thornton's ward last December he found a large ovarian tumor pressed upon to the left side, inferiorly, by the rectum, which

was slightly distended owing to a cancerous stricture. A little artificial distention of the intestine caused it to press against the tumor so as to push its left side backward, stretching and twisting the pedicle. In examining this pedicle he found that it was not twisted, but that its veins were partially plugged, in all probability from the effects of intermittent pressure through frequent extreme distention of the obstructed rectum.

Mr. Wells suggested that the rotation was little more than an accident, but an accident of such frequent occurrence must have some kind of uniform cause; and if it be true generally, as it certainly was in Rokitansky's observations and in my own, that the great majority of the tumors twisted were right-sided tumors, and that they were twisted in one specific direction, it is clear that some special mechanism must be concerned in the process of rotation. The facts given by Mr. Alban Doran are emphatically in favor of the theory I have advanced, and, doubtless, if Mr. Doran continues his observations, some valuable results will be arrived at. If a left-sided tumor could be found rotated from within outward and over to the left, in a case where the rectum was on the right side, I should regard my hypothesis as proved.

Rokitansky says that, as one of the results of this rotation and strangulation of ovarian tumors, we may get involution and wasting of the growth, so that in many cases they may diminish and disappear; and Mr. Wells, quoting Rokitansky, seems to agree with him.

There is reason to believe that sometimes this axial rotation succeeds in destroying the pedicle altogether, and separating the tumor from its connection, one might imagine a tumor possibly being quite cured in this way. Dr. Peaslee (*American Journal of Obstetrics*, 1878) mentions a case of an ovarian tumor detached from its pedicle, in which, from the history, I believe it likely that the detachment was effected by axial rotation. Dr. Peaslee says: "After the tumor had grown about two years, it ceased growing entirely for six or eight years, then it began to grow again. It was very singular that he could find no connection between the first cessation of growth and the second accession. It was thought by her friends that she had hernia, for there was great suffering, referable to the inguinal region. It kept her in bed some time. After she got up, the tumor grew no more. At the operation the omentum was found to be quite extensively adherent, and there was near it an artery of peculiar development, about the size of the brachial artery, which divided into a great many branches. Upon putting his hand into the cavity, he found that he could run it all around the tumor; he found no pedicle whatever; all the attachment it had

was that referred to above. He then proceeded to ligate the vessels and removed the tumor. He found it to present all the characteristics of an ovarian cyst; there could be no doubt as to what it was. There was a notch in the broad ligament, showing that, at the time of the attack, there had been a pedicle. It had become twisted around and around, so that the circulation had been cut off, and it ceased growing. It had been nourished, no doubt, from contact, from which circumstance this series of larger vessels had sprung up, the vessels being larger in proportion as necessity required."

Besides the acute symptoms which are induced by the strangulation of an ovarian or parovarian tumor, we may have a condition of equal severity arising from peritonitis or from suppuration of the cyst. I have on seven occasions been obliged to remove tumors at a very few hours' notice, when summoned to patients suffering from acute peritonitis. Of these seven, six made excellent recoveries, all the symptoms disappearing in a few hours after the operation. The fourth case was one which occurred in my early practice, when I used the clamp, and from this cause the patient died. From the results obtained by the immediate removal of tumors when there is peritonitis, in the hands of Dr. Keith, Mr. Spencer Wells, Mr. Pridgin Teale, of Leeds, and others, the rule has become quite established to operate without delay when there is this complication.

Suppuration of a tumor is not unfrequently met with. Usually it is the result of tapping, but I have seen it occur without any ascertainable cause. The symptoms are not generally so pronounced as they are in peritonitis, but they are always sufficiently severe to attract attention, and to give rise to a suspicion of the actual state of matters. When there is reason to believe that so grave an accident has occurred, immediate removal of the tumor is the correct practice, more particularly if the symptoms have occurred after tapping. In such cases, of course, the cleansing of the peritoneum must, if possible, be made with even greater care than in any others, and I think that probably in cases where there is pus in the peritoneum, drainage after the plan of Koeberle and Keith will conduce to the recovery of the patient.

Dr. Keith was the first to set the example of removing an ovarian tumor under these desperate circumstances. His first case was in December, 1864, and was successful. He tells us (*Edinburgh Medical Journal*, 1875): "Since then I have ten times met with cases of acute suppurating cysts, besides two chronic cases. In all of these, save one, the chance of ovariectomy was given, however hopeless-looking the case might be."

After the performance of an ovariectomy, and after the patient has recovered from the effects of the anæsthetic, the first symptom we have to deal with is, as a rule, sickness, and sometimes it is extremely distressing. It is due, in the first instance, to the anæsthetic. During my earlier experience I was under the belief that the use of ether was more seldom followed by sickness than was the use of any other anæsthetic, but I must now say I do not think ether possesses any great advantage in this respect; but as I do not find patients suffer so much from ether sickness after other operations as they do after ovariectomy, I am inclined to believe the constriction of the pedicle has something to do with the vomiting as well as the ether. Whatever be the cause, it is certain that in a good many of my operations there is a deal of trouble with sickness, sometimes lasting as long as twenty-four or twenty-five hours. I have tried a great many means of arresting this ugly symptom, and by far the most effectual I find to be the administration of tepid water, slightly flavored with brandy. I never now use ice for this purpose, as I have not found it very efficacious. It gives rise to intolerable thirst, and this is altogether avoided by the use of tepid water. Should there be any pain after the operation, I direct the use of a suppository containing one-fourth of a grain of morphia, but with this agent I am extremely cautious, for my patients never get a single dose of morphia or opium more than is absolutely necessary to relieve pain. Like other operators, I have long since discarded the routine use of opium, which was the fashion at one time, a practice brought into existence by the idea that it prevented the occurrence of peritonitis. With my patients as little as possible is done in the after treatment, each symptom being dealt with only as it arises, and it is quite exceptional for me to have to subject them to any active interference.

The ice cap seems to be in very constant use at the Samaritan Hospital, but I have never yet employed it. We hear of some thousands of pounds of ice being used every year for it, and I have been told that a large cistern has been erected at the top of the hospital, with pipes distributed all over the building, for the purpose of supplying iced water to the patients' heads. Dr. Bantock's explanation of this is to the effect that it is necessitated by the amount of carbolic acid used in some of the operations. In my own practice I have never seen cases of exaltation of temperature, except in cases where the Listerian details were fully carried out, or where the clamp was used. It never seemed to me that the use of the ice cap was at all a rational proceeding, but whether I am or am not correct on this point is

not a matter of much importance. I have never used it and my mortality is much smaller than the mortality where the ice cap is used.

When from some reason or another the patients begin to do badly, the first indication is an altered expression of the face. I am unable to describe this change of countenance, but I learned to recognize it only too well in the old days when, from the use of the clamp, my mortality ran high. Associated with this changed expression is a rapidly increasing abdominal distention, speedily followed by vomiting. At first the vomited matter is simply the fluid the patient has swallowed, but soon it becomes tinged with bile. Later on, should the patient grow worse, the vomited matter becomes entirely bilious, and toward the end it gets quite black and characterized by these features to which the name of "coffee ground" vomiting has been aptly given. In those instances where death followed the use of the clamp, the phenomena always began on the second or third day, the patient dying on the fourth or fifth; and when once these fatal symptoms had become fairly established, nothing I ever did could arrest them. To recapitulate here the therapeutic experiments based upon all kinds of hints, derived from reading and from the advice of friends, would be perfectly needless, for in not a single instance did I ever see beneficial effects from any of them.

On opening the abdomen after death the uniform appearances were found to be those of diffuse suppurative peritonitis. It was impossible to trace in every instance the exact cause of death, yet sufficient evidence was obtained to make me attribute the fatal result in such cases to the presence of a minute aperture in the wound, at the point where the pedicle was embraced, through which the discharges from the ulcerated surface under the clamp penetrated into the abdominal cavity. With the discontinuance of the clamp such cases as these have entirely disappeared, and now not only is it quite exceptional to have a fatal case, but it is quite unusual to have any anxiety concerning my patients' recovery. Sometimes bilious vomiting occurs, why I hardly know. I cannot think this a very dangerous symptom, for I can recall but one instance of its being so persistent as to give rise to anxiety. As soon as the vomited matter shows signs of bile, I give some mild laxative, generally a seidlitz powder or a teaspoonful of Epsom salts, and a small dose of calomel, and the symptom is speedily brought to an end.

With reference to movement of the bowels after an abdominal operation, I have entirely lost all the fears derived from tradition, and I never take any steps to prevent their ordinary

motion, indeed the administration of laxatives within a few hours after the operation is becoming quite a common practice with me, this innovation, in my opinion, being possibly conducive in some measure to my increased success.

Concerning fatal cases I am altogether of Dr. Keith's opinion, that the very first search to be made for an explanation should be in the details of the operation, and every fatal case occurring to me is subjected to most rigorous inquiry. Sometimes I have found reason to suspect some omission or commission on my own part; in others I thought the surroundings were at fault; while in other instances I have been totally unable to account for the catastrophe. Two of my fatal cases, where the ligature had been used, were due to intestinal obstruction, and this arose from a kind of paralysis of the intestines which was perfectly inexplicable. Dr. Battey tells me he has seen the same thing. In both instances the patients had made satisfactory progress until the sixth day after the operation, without the slightest interruption, when suddenly the abdomen became greatly distended, incessant vomiting occurred, and the patients rapidly sank. After death nothing could be found except enormous distention of the abdomen by fluid fæces and gases. In both cases temporary relief was produced by tapping the intestines. The only explanation I can offer of the fatality in these cases is that some mysterious influence, similar, perhaps, to that which causes tetanus, brought about this unexpected and inexplicable end.

One of my recent deaths arose from a cause which will serve to show how much care should be exercised in watching the actions of those who play less prominent parts in our operations, as well as in conducting our own proceedings. A death had occurred in the hospital from a distinctly septic cause, in a case under the charge of one of my colleagues, and after death the body was allowed to remain in the ward for an unnecessarily protracted time. My patient was placed in the same ward within a very few hours afterward, and though her operation was performed with complete Listerian precautions, within a very few hours she had all the indications of acute septic poisoning and died in less than eighty hours. Unfortunately, it was not until after the accident had occurred that I became aware that the directions which up to that time had always been carefully carried out, had in this instance been omitted. The lessons to be derived from that unhappy circumstance are twofold: that no precaution can be too great in guarding against such a catastrophe, and that the Listerian details cannot be relied upon to prevent septic poisoning.

Dr. Bantock has drawn attention to a distinct danger involved in the employment of carbolic acid after Lister's method, which I had not recognized until he pointed it out, but since that time I have seen three marked cases of it, and, looking back upon my own practice, I find record of what, I have very little doubt, was a fatal case of carbolic acid poisoning. What I allude to is the alteration of the urine, which is well known to be a result of the use of carbolic acid. Some months ago I had occasion to open the abdomen of a child, placed under my care by Dr. Totherick, of Wolverhampton, on account of a pelvic abscess communicating with the bladder. The operation was performed with complete Listerian details. Within twelve hours of the operation it was observed that the child's urine possessed the characteristic color given by indican, and that the small amount of albumen which had been recognized in the urine previous to the operation had increased to such an extent that the deposit after the urine was boiled occupied nearly half the tube. Twelve hours after convulsions occurred, and the child became comatose. My colleague, Dr. Heslop, whom I asked to see the patient, regarded the symptoms as perfectly characteristic of meningitis, and he gave a prognosis of an unfavorable kind. I was, however, strongly impressed with the belief that the child was suffering from carbolic acid poisoning. This opinion was, I think, sustained by the events, for the indican disappeared, the albumen diminished, the convulsions ceased, the child regained consciousness, and in about six days after the operation the whole train of symptoms had passed away. In another child upon whom I did abdominal section I saw similar, though by no means such severe symptoms, and in a full-grown woman of twenty-four years of age I again saw them repeated. One of the first operations I performed on the Listerian method died in thirty-six hours after the operation with complete suppression of urine; and my belief now is that she died of acute carbolic acid poisoning, though, as I did not recognize such a possibility at the time, I am not prepared to say absolutely that it was so, but I have never seen suppression of the urine, or any alteration of it, such as I witnessed in these cases, save where carbolic acid had been freely used. Years ago, when we used carbolic acid of the strength of one in four as the carbolic oil, it was a very common thing indeed to see serious urinary symptoms associated with its employment, and I am perfectly certain that a great many fatal cases of carbolic acid poisoning are caused by the too free and even indiscriminate use of this poisonous agent.

I have no very fixed rules concerning the diet of my patients

recovering from an ovariectomy, save that as long as there is any tendency to sickness they have no solid food. But I do not put them through a term of three or four days systematic starvation as I formerly did. Toward the end of the first or the beginning of the second day they are allowed to have some beef-tea or milk. The first solid food they get is either some custard pudding, some dry toast, or a little boiled fish, and these are cautiously followed by a little boiled chicken, or, indeed, whatever they may express a particular fancy for, if the preference be a reasonable one. If the bowels show no indication to move of their own accord by the third or fourth day, an enema is given and repeated at intervals until it has the desired effect.

The dressing of the wound is limited to absorbent wool or a little zinc ointment if stitch-hole abscesses should form, or some of the old red lotion if the wound should gape anywhere. The stitches are removed between the sixth and eighth days, and the patients are generally up between the fourteenth and twenty-first.

Before the patients are allowed to be up they are always provided with a well-fitting abdominal belt, and this they are earnestly enjoined never to be without, while in the erect position, for at least a year after the operation. If this injunction be obeyed, there is never any tendency to protrusion of the intestines; but with hospital patients it is almost impossible to get these orders carried out, and the consequence is that it is not unusual to find them return in about three or four months with a hernial protrusion, while in private practice this is almost unknown.

One of the many arguments used against the intra-peritoneal method of dealing with the pedicle was that the stump might contract adhesions to a loop of intestine and produce strangulation, or that an abscess might form round the ligature and give rise to much trouble. So far as I know the results of my practice, in not a single instance have these anticipations been fulfilled. I direct my patients to see me or write to me every two months after their operations, and I keep a correct record of their visits and letters, and therefore I think I may say that I watch the after-results of my operations with exceptional care.

In my earlier practice I laid great stress upon the practice of keeping the pelvis quiet, as I thought, by preventing the patient passing water for herself, and emptying her bladder every five or six hours regularly by the catheter. One source of trouble which arose from this practice was the frequent occurrence of cystitis, which in several instances assumed great severity and

gave rise to a very great deal of anxiety. I found that in most cases this arose from the carelessness of the nurses, for it was almost impossible to get them to understand what a clean catheter really meant, and I had to resort to the expedient of having the instrument, when not in use, always kept in a basin of water. If this simple precaution was properly attended to, my patients always escaped inflammation of their bladders; but even with the most scrupulous injunctions, a case of cystitis would occasionally occur. I therefore use the catheter now as little as possible, and my patients are always encouraged to pass water for themselves as soon as they can. Owing, however, to some reflex influence, arising doubtless from irritation of the nerves of the pedicle, they are sometimes quite unable to pass water for several days after the operation, and then the catheter must be employed with the precautions I have already indicated; and if it is to be used, let me urge the absolute necessity of these being attended to.

Another danger in this matter is to be avoided—one which is occasionally overlooked—over-distention of the bladder. I remember being told on two consecutive days, at each of my visits, by one of my most intelligent nurses, that a patient was passing her water all right. She was an extremely nervous patient, and the complaint she made of pain at first did not attract my attention; but at last I examined the bladder, and found it completely distended, although the nurse had informed me that she had passed a quantity of urine only a few minutes before my visit. The fact was that what she had passed was the mere overflow, and probably the patient's bladder had been distended for forty-eight hours. Thus, upon the slightest complaint on the part of the patient in the region of the bladder, a careful examination should be made. Fortunately in the case I have alluded to no harm ensued, but every consultant knows how frequently we are called in to see cases where the bladder has been allowed to remain over-distended for many days after a labor, and too frequently this is followed by terrible disasters.

For the treatment of cystitis arising from the use of a foul catheter, or from over-distention, I know of nothing equal to the injection into the bladder of a tepid solution (five per cent.) of hyposulphite of soda, and the administration every eight hours of a soluble pessary containing three or four grains of extract of belladonna.

The after-course of a case of ovariectomy is subject to many rude checks, which alter its history very much from the fortunate career supposed in the preceding pages. There are many dangers in the path of every patient submitted to this operation,

and there are many indications of their approach, but none so trustworthy as those derived from a close observation of the patient's temperature-curve. It should be the invariable practice of the surgeon to have temperature and pulse observations of his patient made night and morning for a few days before the operation, and afterward these ought to be repeated every four hours for at least ten days. Nothing has been to me more instructive than a comparison of a group of such charts; and I have repeatedly seen grounds for a prognosis in a case by the comparison of its temperature-range with those of former cases. It will almost invariably be found that, immediately after the operation, the temperature falls considerably. I have seen it do so as much as two degrees, indicating the risk the patient has to run in the form of shock. To obviate this being carried to a dangerous extent, it is always well to place hot water bottles to the sides and feet, and, if depression be severely marked, to administer a diffusible stimulant. By far the best is an enema of diluted champagne, with a little brandy. It is generally necessary to administer a small dose of morphia—one-third or one-fourth of a grain—immediately after the operation, by suppository, and by this I believe shock may be warded off in great measure, and the after-sickness prevented.

From the twelfth to the twentieth hour after the operation, the temperature slowly rises, unless the patient succumb to the shock; or, in the still rarer condition, where the operation has had to be undertaken on an emergency due to cyst-inflammation or an attack of peritonitis, in which case the temperature falls. In a case of the latter, where I operated with a temperature of nearly 40° Centigrade, it fell in twenty-four hours to 37°.

After the recovery from shock, the patient generally breaks out into a gentle perspiration, and this should be slightly encouraged, and the temperature may vary from 36.8° Centigrade to 38.5° without giving rise to any alarm. If it rise, however, above the latter point, especially if accompanied by an increased pulse frequency, dry tongue, pain, and inflation of the abdomen, green vomiting or hiccup, and anxious face, the access of peritonitis, in some form or other, may be taken for granted. The treatment of this must vary very much according to the circumstances of each case.

Vomiting and distention used to be, as I have said, an invariable indication of an ultimate fatal issue. In the old days of the clamp, when we saw these symptoms come on accompanied, as they always were, by the unmistakable expression of face, we knew only too well that our efforts had been in vain. Now it is extremely rare to see either of these symptoms of an extent