stupor. 10. That the blood-pressure is not always altered in delusional insanity except in those cases where there is also some emotional disturbance. 11. That the blood-pressure in healthy, active, and excitable persons is low as compared with healthy but apathetic individuals. 12. That from this it would seem that the blood-pressure is chiefly affected in emotional or affective insanities in contra-distinction to the effective or ideational forms of mental disorder. 13. That the blood-pressure is raised in general paralysis of the insane when there is depression, whereas in the excited types of this disease the blood-pressure is low, as it is also in the later stages of all types. 14. That there is evidence to prove that the altered blood-pressure may in certain individuals induce mental aberration, but that it is so far not complete enough to enable me to state definitely that mental disease is usually caused by altered blood-pressure. 15. That the altered blood-pressure in different forms of insanity suggests the line of treatment which may be adopted in the various kinds of mental diseases. 16. That the feeling of weight and pressure upon the top of the head, so common a symptom in melancholia, is apparently vascular in origin and is lessened or disappears when the blood-pressure is lowered. 17. That certain depressed patients improve with nitro-glycerine but that there is difficulty in keeping the blood-pressure down with this drug as its action is so evanescent. 18. That the action of erythrol tetra-nitrate is more prolonged and reliable and is more powerful in lowering the blood-pressure in melancholia than is nitro-glycerine. 19. That the prolonged bath raises the blood-pressure and hence is of more value in the treatment of excited patients.

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NOTE ON LUMBAR COLOTOMY.

By LAWSON TAIT, F.R.C.S. ENG.,

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My letter on this subject in THE LANCET of May 21st has brought so much correspondence that I think it well to answer inquiries regarding it in a further note.

I have intended for a long time to write fully on this operation because it has been so needlessly complicated that at one time it seemed as if it would fall into disrepute. There came a "boom" on behalf of inguinal colotomy which was useless and has proved disappointing so far as I can learn. Personally I never gave any countenance to inguinal colotomy save in one case, where I had abundant reason to be dissatisfied with it. I have had every reason to be satisfied with the older operation so far as my own results were concerned, and there were arguments in favour of it as against the operation in the groin which seemed to me convincing. Thus there were of necessity many cases where the effects of the disease in the matter of obstruction were not overcome by inguinal colotomy or where the disease advanced into and above the opening which were not so affected in the event of the performance of lumbar colotomy. In fact, I have never seen a case of lumbar colotomy where the beneficial effects were destroyed by the progress of the disease. Again, it has been found, as might have been expected, that uncomfortable reversion of the gut after inguinal colotomy has been far more frequent and extensive than it ever is after the lumbar operation. Other and less important arguments also affect my choice in favour of the higher opening, amongst which is the inefficiency in most cases of the imitation sphincter to which I alluded in my letter of May 21st.

I have had a number of cases of the curious non-malignant stricture of the rectum found in women, but never yet described as having been seen in men, where I have relieved the patients by lumbar colotomy, the patients living in fair comfort with the new passage after the gut below has atrophied. One of them has lived now twenty-four years since the operation and I have seen her within the last few days. According to her account she has notice of the need of an evacuation and control over the aperture very much as she had some thirty years ago before the disease attacked her. Such an amount of relief is not of course obtained in the great mass of such operations performed, for the disease is malignant in probably 98 per cent. of the cases.

There is a new group of cases at present in existence where colotomy seems more urgently needed than in any

formerly amongst my experience—I mean those in which the uterus has been removed for malignant disease and the patients have recovered from the operation. I have not yet seen a case with a secondary success. It is an operation against which I was prejudiced from the first. I could not believe that any operation would be really successful in cancer of the uterus. My experience is certainly in favour of my prejudice and I can only explain any alleged success by an assertion that the diagnosis of cancer was a mistaken one. All the cases on which I have operated and now sufficiently advanced, some forty in number, have died within twelve months and most of them long before that time. Not only so, but their complications and sufferings have been more severe than in the cases left alone. Amongst these complications is a new one—one I never saw before in cancer of the uterus uninterfered with. I mean obstruction of the rectum. In the return of the disease in the pelvic cicatrix there seems an extraordinary tendency to contraction which soon pulls the bowel out of shape and afterwards completely obstructs it. Two such cases have happened to me this year already and the story of one of them will suffice. The patient was a woman, aged thirty-eight years, married three years and with one child born when I saw her first about fourteen months ago. She never got over her confinement but went on persistently losing, with menstrual exaggeration. For this she was twice treated by curettage, once in Calcutta and once in London. After the second operation she was much better for a few months and she returned to India. But all her former symptoms speedily returned and she was sent back to England and placed under my care. I diagnosed endometric cancer and advised removal of the uterus. This I carried out last November and found a polypoid patch of papilloma at what I presume had been the placental site. The body of the uterus was perfectly free from the disease apparently, the wound healed easily and in March last she was perfectly well and desirous of once more travelling eastwards. But I felt a nodule in the cicatrix and advised delay in the journey. The disease advanced with fearful rapidity and at the beginning of May I had to open the colon on account of symptoms of intestinal obstruction. In the second case I operated upon the patient for obstruction of the rectum ten days later, and she is still in bed recovering from the lumbar operation. Such experiences do not encourage me to advise removal of the uterus for cancer, for these two cases were such as ought to have had satisfactory results if ever they are to be obtained.

Now as to the method for operating on the colon for intestinal relief. I have not troubled to look up the history of the operation beyond consulting Portal and Neale and that assures me that Amussat was the inventor alike of the idea and the proceeding. According to a wit of the early part of this century the ancients have stolen most of our good ideas, but here they seem to have left us a good thing. The idea was good, and Amussat's method of carrying it out was good though complicated, but the professional anatomists have made it worse. It is very funny to hear a professional anatomist describe the structures to be seen and those to be avoided in the performance of a surgical operation. The worst operators I have ever seen have been those same people. They begin with their knives as if they meant to dissect and they proceed as if they were engaged in a demonstration. They want to see all the structures which are described in the books and to display them to the onlookers. The only thing they do not do is to recognise the fact that in the human body there are almost as many variations as there are subjects, and this is particularly true in myology. A man who attends to dissecting-room manuals in such operations as those involving incisions through the abdominal wall will come to woeful grief and may end, as I have seen happen more than once, in stripping the transversalis fascia off the peritoneum in the belief that he was undoing old adhesions. A good training in anatomical detail is all very well, but with ninety-nine out of every hundred of us it is a mere waste of time, and even with the hundredth of us the great bulk of it may be forgotten, and what is worth remembering for surgical purposes can be learnt best from frozen sections. A week with Cunningham of Dublin and his frozen sections undid all the mental mischief done to me by the careful scholastic training of Goodsir and Turner. For the first time I grasped the idea that structures were not separated, that there were no foramina, and that the abdominal organs in particular lay closely packed together in very varying relations.

So the muscles of the abdominal wall vary and so the operation of lumbar colotomy as performed in my presence by a professor was one of the most gruesome sights I ever saw in my life. He divided a lumbar artery with its accompanying nerve—he carefully identified them but could not tie the artery, for he lost it in the wealth of muscular tissue which he unravelled, and after her death his patient was found to have her peritoneal cavity full of blood. I do not recollect ever having a fatal colotomy—I am certain no case of mine ever died from hæmorrhage, for I disregard all anatomical curiosity during the operation. I put the patient on her right side with her knees bent up to steady her and I place a pillow under her right loin so as to put on the stretch that which is the guide to the whole thing—a strip of tendon which marks the union of the fascias encasing the external oblique muscle, the internal oblique, the transversalis on the one side and the latissimus dorsi (or its tendon) and the quadratus lumborum on the other. The real difficulty is that the relations of the edges of these muscles do not follow the directions of the text-books as of course they ought to do. Let the operator disregard the text-books and bear in mind that after he has divided the skin to a necessary extent over the tight band and absolutely in its direction he has only to open the glistening fascia in the same position and carefully look at the direction of the muscular fibres with which he meets. He must in his proceedings inwards bear only one route in mind and that is as nearly as possible to the umbilicus. Only one direction further is necessary so far as muscular fibres are concerned, that the surgeon must put his knife down and use only his blunt forefingers to separate the fibres. At first this would be in a direction parallel to the axis of the patient's trunk and afterwards at right angles to that axis—he need not trouble to identify even the muscles. When he has got through the two layers he will come on a layer of fascia of the greatest importance, concerning which he ought to be "well up"—the transversalis fascia. This he must slightly notch and after getting his forefingers through it he must search backward towards the body of the vertebra till he finds the base of the kidney. Having found this and remembering two arid facts of anatomy—that the descending colon is generally immediately in front of the kidney if placed on its usual side, in front of his fingers, is attached to the moveable peritoneum and separated from his finger by some loose fat, he will take two or three pairs of catch forceps and pull layer after layer of fat boldly and freely up into the wound, leaving the forceps on each layer as he lands it outside, and proceed in this way till he sees the unmistakable gut. If he keeps the points of his forceps well downwards and pointing well forwards towards the umbilicus he cannot miss the gut. When the gut is missed it is always by dissecting-room people who go over it instead of at it. Having found the gut, haul it freely and fearlessly up out of the wound, cut it nearly through and stitch with an all-round suture into the lower angle of the wound, carefully replacing all fringes of fat as you proceed, or removing them if much injured by the forceps, but remove them with your fingers and not with cutting instruments. You then close the rest of the wound, the upper part. The wound need never be more than three inches long and generally need not be so much, but it may look like five inches deep. A wound so made heals immediately, and as there is nothing but a temporary separation of muscular fibres they fall back at once into position and at once resume their function. The whole gut has been pulled through the crosswise-made opening in the undamaged muscle, so that the almost constant tension involved in all active positions of the body secures the closure of the aperture in the gut. In a badly done colotomy only a bit of the circumference of the gut is pulled up to or out of the muscular aperture, the bulk of it being as before left inside and the gut is emptied through the relief passage only by regurgitation from below and generally when the whole of it is distended by hardened fæces. This does not give full or indeed much relief, the retained fæces having from time to time to be dug out. After colotomy is done as I recommend it should be done all the motion comes through the wound at once and after some practice there comes marvellous control over the passage.

If anyone compares the proceedings described above with those of Amussat as described by himself and as copied or altered in text-book after text-book after him, it will be seen that there is very little in common between the two. The operation as I give it I got from the lips of Alexander

McKenzie Edwards, lecturer on surgery in the Extra-Mural School of Edinburgh, to whom I was apprentice, pupil, and assistant for about six years. I have no recollection of hearing from him that he got it from anybody else and I certainly have neither read nor heard of it elsewhere. He was a surgical genius of the highest order, as many can testify who are still alive who knew him, so that I have no doubt it was a conception of his own, and if any name is to be attached to it his name alone ought to be.

When the time came in the "seventies" that I ventured to operate upon and to remove kidneys I adopted the same incision save in such cases as those where I followed another rule as simple and imperative. Attack your tumour on its most prominent aspect and certainly avoid all dissecting-room rules about finding "oblique lines" or other untrustworthy directions.

Birmingham.

A NOTE ON THE ETIOLOGY OF BERI-BERI.

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IN THE LANCET of July 31st, 1897, I reported the results of a bacteriological examination of the blood in two cases of beri-beri. Since then I have had the opportunity of carrying out in two other cases a similar investigation, and this I now put on record in further support of the opinion already expressed—namely, that the white staphylococcus of Pekelharing and Winkler is in truth the specific micro-organism of beri-beri. The two patients, both seamen from the s.s. *City of London*, were admitted into Dr. Middleton's wards of the Glasgow Royal Infirmary on Oct. 29th, 1897. Neither of them spoke English with any degree of fluency and so the account of their illnesses is necessarily very brief.

CASE 1.—The patient was a native of Bombay and as far as could be made out his illness began fourteen days previously to admission with swelling and pain in the feet and legs. There was considerable difficulty in walking and the patient was sick and very drowsy. On admission to the wards the œdema of the feet and legs was well marked. The sensation to touch and pain seemed to be blunted over a considerable extent of the body, but examination on this point was not very satisfactory. The superficial reflexes were quite active. The left knee reflex was absent and the right was diminished. The patient walked fairly well except when he was told to go along a straight line and then he had a tendency to stagger. He could not stand with the feet close together and the eyes shut. No reaction of degeneration was found in any of the muscles. The right ventricle of the heart seemed to be somewhat dilated, but no murmur was to be heard over any part of the cardiac area. There was evidence of some fluid effusion into both pleural cavities. The urine was quite normal. Examination of the blood gave the following results: hæmoglobin, 70 per cent.; red corpuscles, 71.5 per cent.; and white corpuscles, 1 in 450. The patient remained in hospital for five weeks and during that time the only change noted was a return to activity of both knee reflexes. At no time was albumin to be found in the urine.

CASE 2.—The patient was a native of Zanzibar and the history of his illness was very similar to that in Case 1. He had swelling and pain in the feet and legs, but there was no sickness or difficulty in walking. He could stand with the eyes shut and the feet close together without any difficulty. The superficial reflexes were present, but both knee reflexes were abolished. The sensation to pain seemed to be practically absent all over the body and the sensation to touch was much impaired. There was no reaction of degeneration in any of the muscles. The lungs were quite healthy, but there was a suspicion of some mitral incompetence. The urine was perfectly normal. Examination of the blood gave the following results: hæmoglobin, 90 per cent.; red corpuscles, 85 per cent.; no increase of white corpuscles. The patient was dismissed from the hospital after a month's residence in much the same condition as on admission, the knee reflexes being still absent and sensation in no way improved.

In both these cases specimens of freshly-drawn blood were examined at various times and on each occasion micro-organisms were to be seen in rapid motion in the spaces