

he had now learnt to correct it by the position of his head. It is worthy of remark, in connexion with this symptom, that the double images were invariably parallel and of the same height, in whatever direction the object (a paper-knife) was held. Now, theoretically, the left hand image ought to have been slanting; because the left eye not being everted, owing to the paralysis of its abductor, its vertical meridian would remain nearly vertical, while that of its fellow would be inclined to the left; and thus the parallelism of the two meridians would be destroyed. I shall have occasion to revert to this anomaly in another case. As regards the range of the diplopia, it occupied not only the centre and left of the visual field, but extended more than two feet into the right half.

Feb. 19th. He had had counterirritation kept up by means of sinapisms in the cervical and upper dorsal regions of the spine, and had been taking a tonic mixture. To-day it was found that he could evert the cornea to three lines beyond the point at which it rested five weeks ago. Notwithstanding this gain, the diplopia still extended into the right field of vision. This phenomenon was due to the action of the adductor of the paralysed eye not being sufficiently controlled by its weakened antagonist. Thus the visual axis of that eye, instead of being in a line with the object, passed beyond it, as was proved by shading the sound eye, when the paralysed one immediately made a slight movement outwards.

This patient is now being treated by galvanism twice a week, according to the method recommended by Benedikt of Vienna; and, although not yet perfectly well, he is steadily advancing towards complete recovery.

[To be continued.]

## ON THE DISCOVERY OF TRICHINÆ IN THE HUMAN SUBJECT.

By THOMAS NUNNELEY, F.R.C.S.E., Leeds.

FROM the repeated references which from time to time have appeared in the BRITISH MEDICAL JOURNAL and other publications, as to who first discovered trichinæ in the human body, and the date of the discovery, it would appear that much interest attaches to the questions.

Some time ago, I observed that it was stated, in this JOURNAL and also in others, that Owen first noticed the parasite in 1835. Then it was asserted that it had been discovered by a German anatomist; and now I find in the JOURNAL of February 24th, a statement, made on the authority of the present curator of Guy's Museum, to the effect that Mr. Hilton was entitled to the honour of having first called attention to the subject in 1832. As the last date so nearly corresponds with the time when I dissected a subject in which I found trichinæ in enormous numbers, and it is hardly likely that two subjects affected with so uncommon a disease should have been seen so nearly together in the same dissecting-room, I cannot but suspect this latter statement refers to the subject in which I first noticed the existence of these entozoa, and that it may help to settle the question of priority of discovery if I relate what then took place; and put in such claim as may fairly attach to the person who first noticed the presence of the trichinæ. It is by no means improbable, that some who were then students at Guy's may, on reading this communication, have their memory recalled to an occurrence which, at the time, excited considerable interest. I made a full memorandum of the facts in my note-book; and have a firm conviction of the correctness of what I now state.

Towards the close of my student-life at Guy's Hospital, I think at the end of April or in May 1832, when subjects were not greatly in demand, I had the major part of a male subject to dissect and use for operations. On exposing the muscles of the neck, I at once observed that they were unusually pale, and presented a most peculiar spotted greyish appearance, which, on close inspection, was seen to be caused by innumerable minute semi-opaque, white ovoid-shaped cysts, deposited in rows in the cellular membrane connecting the muscular fasciculi. These cysts were so numerous as nearly to touch each other. They all lay in single rows with the long axis parallel with the muscular fibre. Though so plentiful in the connecting tissue, not one could be seen entering into the substance of the smaller fasciculi; and all lay, so far as could be noticed, with the long axis as stated—not a single one could be found lying in a transverse direction.

The whole body was carefully dissected, to ascertain the extent to which the muscles and various organs were pervaded by these cysts. There was not a single voluntary muscle but was thickly and apparently uniformly studded with them. They pervaded equally the small muscles of the glottis and orbit, as the large pectoral and abdominal muscles, the thin platysma myoides, and the entire substance of the thick deltoid; but I could not find a single cyst in any of the non-striated involuntary muscles. The muscles of respiration were full of them; but not a solitary individual could be detected in the heart, nor in the muscular walls of any of the hollow viscera. None were discovered in any of the solid viscera; and, if any existed, they must have been few in number, and were not observed.

So remarkable a morbid condition could not but excite much attention; and, doubtless, the matter would be brought under the notice of Mr. Cock and Mr. Hilton, who were the demonstrators of anatomy. Mr. Key, whose dresser I had for some time been, and others of the medical staff, amongst whom were Mr. Cooper, the anatomical lecturer, Dr. Bright, and Dr. Addison, I know, visited the dissecting-room and saw the muscles. I well remember that the attention of Dr. Hodgkin, who was then Teacher of Pathology and Curator of the Museum, was especially called to the matter. He had never seen anything of the kind, and was at a loss to account for it. The microscope was only just beginning to be used in pathological investigations, and very few persons knew much of its management; Dr. Hodgkin suggested the name of his friend, the late Mr. Lister, who was then perhaps the most expert microscopist in London, as a proper person to be referred to; and I removed a portion of muscle for his examination. For a similar reason, the name of Dr. Marshall Hall, who had lately come from Nottingham to reside in London, and who was known to have used the microscope, was mentioned. He had portions of muscles sent to him; and he afterwards came to the dissecting-room, where I shewed him the subject. After various speculations, as to the nature of these cysts—whether they should be regarded as ova, larvæ, or perfectly developed parasites—in the end, by one or other of the three gentlemen last named—I believe by Mr. Lister, though of this I am not certain (possibly Dr. Hodgkin may recollect)—these bodies were determined to be the *cysticercus cellulosa*, by which name for many years they were called. How they entered the body, or why they were located in the striated muscles of voluntary motion alone, no reasonable explanation was attempted. Indeed, in the then imperfect state of knowledge of the natural history of these entozoa, such would have been impossible.

I took a great deal of pains to ascertain if anything in the man's career before death would throw any light upon the subject; but could learn nothing that did so. Of the man himself little was known; he was of middle age, rather thin, and was supposed to have been an irregular liver, and to have suffered from long continued syphilis. He had not been long in the hospital. Shortly before his death, he had had feverish symptoms of no very marked character, or at least such as then had been recognised as peculiar. The body, being unclaimed, was sent to the dissecting-room.

I removed both sterno-cleido-mastoid muscles, which seemed to contain as many of the parasites as any of the muscles, to have them preserved as specimens—one for myself, the other for Guy's Museum. The one intended for the museum was so badly cared for, that I had the impression it was spoiled before being deposited in the museum; but if, as I gather from the paragraph in the JOURNAL, the curator declares that he has it in his custody, in this I must have been misinformed. The other specimen was well put up, and for many years shewed the pathological condition with great distinctness; and year by year for at least twenty years I used to exhibit it to my class at the Leeds School of Medicine, as what I supposed to be an unique specimen of a most unusual pathological condition of muscles. When I was transferred from the Lectureship on Anatomy and Pathology to that on Surgery, I lost sight of the preparation, which I believed to be safe in the museum, until, three or four years ago, when attention was so much directed to the affection from the discoveries in Germany, and the natural history of the parasite was so much elucidated, I looked for it, with the intention of relating what I now do, and, to my great vexation, could not find it. It had followed the fate of several other important preparations, which the carelessness of the curator had allowed to be stolen or destroyed. In confirmation of this part of my statement, I insert a note from my colleague, Mr. Wheelhouse, who, in 1845 and subsequent years, was a student at the Leeds School.

"My dear Mr. Nunneley,—I have a perfect recollection of the specimen of the trichinous muscle, of which you were speaking to me on Thursday last.

"It was in the museum of our school, to my knowledge, from the year 1846 to 1849; and I am exceedingly sorry to hear that it has been lost.

"It was a small piece of voluntary muscle thickly filled with what I now know to have been the *Trichina spiralis*; although it was at that time labelled, and always spoken of by you in your lectures, as a specimen of *Cysticercus cellulose*.

"Believe me to be, yours very sincerely,

"C. G. WHEELHOUSE.

"Leeds, Feb. 24th, 1866."

I should mention that, soon after the discovery of trichinae at Guy's, there was a rumour in our dissecting-room, of there being a body presenting a like condition at St. Bartholomew's Hospital. I at once went there; but could not learn much. No such subject was then in the dissecting-room; but there was some talk, though by no means a very definite assertion, of there having been, some time previously, a body dissected, which was afterwards thought to have been similarly affected, but which at the time of dissection had not excited more than some passing curiosity as to the peculiar condition of the muscles. No one appeared to be able to identify the appearances in that body with what had been found in the one at Guy's; and it seemed more probable than not, that the two conditions were not similar.

## Progress of Medical Science.

### SURGERY.

LARGE RENAL CYST: SUPPOSED OVARIAN TUMOUR: ATTEMPTED EXTIRPATION: DEATH. Dr. W. Krause, of Göttingen, relates the following interesting case as having occurred in the practice of Herr Baum. The patient, a young woman, aged 23, had always enjoyed good health; menstruation had been perfectly regular. In the beginning of 1863 she perceived that, without any known cause, her abdomen gradually enlarged. She consulted several medical men, and lastly a very skilful and well-known operator in Hanover, who diagnosed an ovarian cyst. The tumour was punctured in May 1863, with relief to the patient, who was enabled to undertake a journey at the end of ten days; soon, however, the fluid again collected. No information could be obtained as to the examination of the fluid removed.

On October 27, 1864, the patient came under Dr. Baum's care. She was a large strongly built woman, of healthy appearance. The abdomen was enormously distended, giving the following measurements: circumference from the upper end of the sacrum over the iliac crests and the umbilicus, 47.6 inches; horizontal circumference over the iliac crests, 44 inches; distance of umbilicus from symphysis pubis, 9 inches; height of swelling above symphysis, 21.25 inches; distance of xiphoid process from symphysis pubis, 28.75 inches. The swelling occupied the whole breadth of the abdomen from the left to right; it gave a dull percussion sound, fluctuated distinctly, and could not be pushed aside. Nothing peculiar could be detected by vaginal examination; no hardness could be felt, nor had the surgeon who previously punctured the tumour, been able to detect any tumour in the abdomen. Herr Baum considered the case to be one of ovarian cyst.

On November 28, the patient's health having been improved by appropriate treatment, she was placed under the influence of chloroform, and an incision between three and four inches long, ending about four inches and a third above the symphysis, was made in the linea alba. The abdominal parietes and peritoneum were successively divided, and the cyst was exposed; it was of a reddish blue colour, with many vessels on its surface. A hook was inserted into the cyst, and with a trocar there were removed more than twenty-five pints of fluid, at first clear, afterwards having a muddy purulent appearance. When nearly all the fluid had been removed, an attempt was made to draw the cyst forward by means of the hook; but this was found to be impossible, on account of numerous and very close adhesions of the cyst to the intestines, especially the transverse and descending colon. All thought of extirpation was therefore necessarily abandoned. The anterior wall of the cyst was divided, and fastened to the abdominal wall by six points of suture, including the peritoneum; the wound was covered with cotton-wadding, and the abdomen was bandaged. The patient was much exhausted by the operation, and had at noon a glass of wine, after which she was given milk and water. In the evening she felt tolerably well; the abdomen was not very tender, but pressure was rather disagreeable. The patient's temperature was 102° Fahr.; pulse 10. Ten drops of tincture of opium were given in the evening. On the 29th, she had slept but little, and was rather restless; the abdomen, however, was not tender. Towards noon she vomited. Two drops of tincture of opium were given, and afterwards five