ANEURYSM OF BOTH POPLITEAL ARTERIES: CURE BY LIGATURE OF THE FEMORAL ARTERIES

AT THREE YEARS' INTERVAL: DEATH FROM MALIGNANT DISEASE OF THE STOMACH THIRTEEN YEARS LATER:
DISSECTION OF FEMORAL VESSELS.

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THE perusal of that most interesting Hunterian Lecture recently delivered by the President of the College of Surgeons of England, has recalled to my memory the particulars of a case which occurred many years ago in the Richmond Hospital, and which, as I believe, was never published. The following notes may possibly prove of some interest to the readers of the JOURNAL. Not the least interesting feature connected with it rests in the fact that the late Mr. Peile, who took great interest in the case, often told me, when examining it, that he was with the late John Hunter when he first tied the femoral artery for popliteal aneurysm; and, when he had secured the ligature and closed the wound, he turned round and said to Mr. Peile, "Now the absorbents are busy below, like mites in a cheese."

The patient's name was James Owen. In the earlier part of his life, he had been a herdsman and caretaker, and so exposed to cold and damp by day and night, and had a great deal of exercise on foot. At the age of 29, he applied to the late Mr. Todd, in the Richmond Hospital, for relief from an aneurysmal tumour in the left popliteal space. Mr. Todd, in the year 1817, tied the left femoral artery; and in a short time Owen returned home to the county Meath, able to resume his work as a herdsman, to which occupation he now added that of horsebreaker.

Three years after the first operation, he observed a similar swelling in the right popliteal space, and knew, from the similarity of the symptoms, that it was the same disease from which he had suffered in the left leg, and been cured. Accordingly, he returned to the Richmond Hospital, when Mr. Todd, in the year 1820, tied the right femoral artery, and he returned home, the second aneurysm being perfectly cured. He now enjoyed uninterrupted and perfectly good health for thirteen years, and continued at his work as herdsman, and occasionally breaking-in young horses. He had, in fact, recovered such perfect use of his limbs as qualified him for any species of labour. About the close of the year 1833, being then forty-five years of age, he was attacked with frequent vomiting and other very severe dyspeptic symptoms, and returned to the Richmond Hospital, convinced that another cure would now be effected for him. It was a great disappointment to him to find that Mr. Todd was dead; but he was admitted under the care of the late surgeon Adams, whose many clinical lectures on the various interesting points of the patient's history are still remembered by old Richmond Hospital students.

The dyspeptic symptoms steadily increased, and it was soon but too evident that he was suffering from malignant disease affecting the great curvature and pyloric end of the stomach. It was a very remarkable coincidence that, in the bed beside him, was placed a patient (Norton by name), who was suffering from malignant stricture of the cardiac orifice and the lesser curvature of the stomach; and to contrast the symptoms of these two patients was a matter of almost daily occurrence. The two stomachs are now placed beside each other in the museum of the Richmond Hospital, and the recent *post mortem* appearances are admirably represented in two drawings by Conolly. Owen gradually wasted, and became subject to uncontrollable diarrhoea; there was, at times, a distinct impulse conveyed to the cancerous tumour by the abdominal aorta, which was sufficient to impress the patient's mind with the idea that it was the same form of disease he was now afflicted with, as when he had the pulsating tumour behind his knees, and he, therefore, could not understand why he could not be again cured. He became frightfully emaciated, and died on April 3rd, 1836. Notwithstanding extreme vigilance on the part of his relatives, the indefatigable curator, the late Professor Smith, having abstracted the stomach and appendages, and secured, also, the lower half of the body, the abdominal aorta was injected, and the lower half of Owen's body was most skilfully dissected.

The following notes describe the condition of the ligatured vessels and the state of the collateral circulation.

On the right side, the femoral artery was tied, five inches from Poupart's ligaments, two inches and three-quarters from origin of profunda, down to which it was found obliterated. The profunda was much enlarged, and, besides its usual branches, gave one large

branch to the short adductors. The perforating arteries were also much dilated. One large branch from the middle perforating ran directly into a muscular branch of the femoral, which was given off in Hunter's canal, about one inch and a half above the origin of the anastomotica magna.

In the popliteal space, the artery was reduced to a cord, and contained no injection at the part where it lay on the posterior ligament of the joint. The inferior articulars and the recurrent tibial were larger than usual.

On the left side, the femoral artery was tied three and a half inches from Poupart's ligament, one and a half inches from the origin of the profunda, down to which it obliterated the descending and transverse branches of the external circumflex rose just above the ligature, from the superficial femoral; three-quarters of an inch below ligature, a large branch (of the size of the radial at its origin) ran into the adductor magnus; the termination of this seemed to anastomose with the perforating branches of the profunda, which were very large. The internal circumflex gave a large branch to the short adductor muscles.

In the popliteal spaces the injection stopped at the superior border of the ligamentum posticum Winslowii, and began against the inferior border of the same ligament. There were two superior external articular arteries, one of the size of the ulnar, anastomosing along the external lateral ligament with the inferior external articular, which was also enlarged.

There were masses of injection lying along both popliteal nerves. These came from a large branch of the first perforating artery.

The size of the branch from the profunda to the short adductors was rather remarkable on both sides. The terminal branch of the right and left internal circumflex was enlarged, and probably anastomosed with the sciatic. The anastomoses between the perforating arteries on both sides were very free. The left superficial femoral artery below the seat of ligature was larger than the right.

The preparation is now preserved, in good condition, in the museum of the Richmond Hospital, where the details given in the above description of the arteries of each thigh can be verified.

ON PICRIC ACID AS AN URINARY TEST.

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I HAVE just read Dr. Mahomed's letter which appeared in the JOURNAL of the 24th ultimo, relative to the question of the value of picric acid as a distinguishing test in the various forms of albuminuria, and Dr. Johnson's communication, which appeared last week, in reply. As the statement made by me at a recent meeting of the Pathological Society, with regard to the action of picric acid on peptones, has been introduced into the controversy, I should like briefly to offer a few comments on this interesting and important subject.

It appears to me that Dr. Johnson admits the point I brought forward at the Pathological Society, viz.: "Though these new tests—picric acid, etc.—are extremely delicate reagents for the detection of albuminuria, yet further tests are required in order to distinguish one variety of albumen from another; and that heat, and heat alone, can determine that the albumen present is serum-albumen, and not a modification or distinct variety." Since, in order to distinguish between the precipitate given with picric acid and sero-albumen from that of peptones, he says that, with the latter, the precipitate "was immediately and completely redissolved by heat below the boiling point," thus it is made clear that, in testing for albumen in urine, the test by heat must never be omitted. This also supports, I think, the contention raised by Dr. Mahomed at the Clinical Society.

With regard to the question of peptonuria, my observations do not coincide with those of Dr. Johnson that its occurrence is a "rare and exceptional phenomenon," since I have been able frequently to demonstrate the presence of peptones in many cases of temporary or intermittent albuminuria. The reason they are so often overlooked is due, I think, to the fact that they are usually associated with the presence of ordinary albumen; as is natural, since the presence of peptones in the renal vessels would render the blood-serum more diffusible. In testing these urines, it often happens then that serum-albumen being detected, no further search is made for peptones. It is, however, easy to demonstrate their presence, since they strike a peculiar rosy red with Fehling's solution in the cold, absolutely distinct from the mauve coloration the same reagent gives with any form of albumen. When peptones and albumen are mixed

in the same solution, then the rose colour predominates over the paler mauve. Another delicate test for their presence is the increased rapidity with which a peptonised urine will make a solution of albumen pass through a filter in a given time, as compared with a urine free from peptones. With respect to the nature of the peptones present in urine, I believe they consist chiefly of parapeptone, and that what may be called for convenience the "final" peptones, are only present in small proportion.

Dr. Johnson further infers that picric acid detects the modified albumen (alkali albumen) that occurs in highly alkaline urine. It undoubtedly does so, as it does with all forms and modifications of albumen; but still here the employment of additional tests, such as determining the nature of the alkaline reaction (fixed or volatile), and the reaction with heat, before and after neutralisation, will throw considerable light on the nature of the chemical changes in the urine which have brought about this modification, and thus prove of considerable service to the practitioner. Again, picric acid, I believe, precipitates mucin from urine (I have found experimentally that a solution of mucin, carefully prepared from ox-gall, gives an abundant precipitate with picric acid). Now here, again, unless a determining test is used, we are likely to fall into error, since the urine often contains a considerable quantity of mucus in solution; and, if we rely only on the picric acid test, it is possible that we may be misled into magnifying a slight vesical catarrh into renal disease.

The true position of picric acid as an urinary test, I believe, is this. It is the best possible substitute for nitric acid: since it is far more delicate, and it is far more convenient and cleanly for use. Further, picric acid has this advantage: that it can be made available for the detection of sugar in urine. The very best thanks of the profession are, therefore, due to Dr. Johnson for the introduction of this valuable and convenient reagent, which will enable the practitioner, at the bedside of the patient, with ease to detect the presence of these substances in the urine; and thus, on the spot, to gain an insight into the probable nature of the case. But, in making use of this convenient reagent, he should be careful not to employ it in a routine manner. In every instance, especially in testing for albumen, he should see that a sample of urine be sent to his house, where tests for determining accurately the nature of the albumen present can be employed. Of these, *heat* is the most important clinically; since the blood-albumens—sero-albumen and paraglobulin—are alone coagulable by heat, at temperatures ranging from 72° to 76° C.

CLINICAL MEMORANDA.

PERIOD OF INCUBATION OF MEASLES.

It gives me much pleasure to be able to corroborate the statements of Mr. Vacher with regard to the period of incubation of measles. Having taken careful notes in many cases, I have arrived at the conclusion that the period is fourteen days, but that it may vary a few hours either way.

The discrepancies with regard to the number of days, as laid down in our text-books, have probably arisen from one or other of the following causes. First, that measles may be communicated at least four days before the rash appears. Secondly, that, in a certain number of cases, a slight rash appears upon the face and chest from two to four days before the general rash comes out. My practice has shown me that, when a copious rash has appeared before the fourteenth day, the case has occurred in a house where a previous case existed, and where there has been a probability of the children having either slept or played together prior to the rash appearing in the first case. Measles appear to be most infectious during the days when the rash is most abundant, but, owing to the long incubation period, the disease is infectious for several days prior to the appearance of the rash, and, in some cases (varying according to the severity of the case), for quite three weeks afterwards.

During the past autumn and winter, a "measle wave" has passed over this neighbourhood, and, while watching its progress, I have been struck with the fact that the fresh cases usually appeared in batches, upon Saturday, Sunday, and Monday. In tracing the origin of the cases, I have come to the conclusion that they were, as a rule, caught in one of the following ways—1, in some place of worship; 2, and most common, in Sunday school (I say this because children are sent to Sunday school by parents, to get them out of the way, at an earlier date than they are generally admitted into the regular schools); 3, on Monday, at the ordinary school classes. I could note numerous instances in support of the fourteen-day period, but will only state the following.

I attended a choir boy on January 7th. On the 21st, he went into the choir against my orders to the contrary. On February 4th, the lady who sat next to him developed a copious rash. My own son had measles; fourteen days later, his sister had the rash well developed.

Fourteen months ago, I attended a single case of measles. My last visit was paid to the patient twelve days later, and, fourteen days afterwards, I was in bed with a good crop of measles. Both these cases were second attacks, and both patients had been frequently exposed to the contagion of measles with impunity.

A somewhat curious chain of cases occurred in my practice some years ago. I was attending the eldest daughter of a lady for measles of rather severe type. There were seven children, and the mother was anxious for them to have the disease and get it over; consequently, free intercourse was allowed. Fourteen days later, the second daughter had the rash. Fourteen days later again, the third daughter developed rash, and again fourteen days passed, and the remaining four children were in bed with a severe attack of measles.

While speaking of the period of incubation of disease, I may mention that I have had frequent opportunities of determining this in mumps, and have found it always twenty days, with some few exceptions, where second cases have occurred among members of the same family.

W. E. GREEN.

DEATH DURING THE ADMINISTRATION OF ANÆSTHETICS

The following case will, I think, support Mr. Patterson's opinion of Case IV, that, in "some cases, at least, shock" might be the cause of death. The patient, a young, healthy looking man, about nineteen years of age, was admitted into the Infirmary, suffering from a tumour of one orbit, by which the eye was considerably protruded. It was decided that both the eye and the tumour should be removed. Chloroform was administered, and the surgeon asked, "Is he under?" to which the reply was, "almost," the operation was commenced and the patient struggled somewhat until the optic nerve was cut, when his jaws closed with a distinct "click," his breathing ceased, and all efforts to bring him round failed. At the *post mortem* examination "all the organs were apparently healthy." This is the only case of a death under chloroform which I have seen, but in reading I have been struck with the frequency of its occurrence during the performance of trivial operations, and I think it is possible that sufficient care has not been taken to see that the patient is fully anæsthetised, and that the cause of death has not been too much chloroform, but "shock" from too little chloroform.

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CASE OF TRISMUS TREATED WITH CHLORAL AND BROMIDE OF POTASSIUM.

A. M., AGED 22 years, was working in a mine on October 5th, when two stones fell on him from a distance of ten or twelve feet; one struck him just behind the left hip-joint and knocked him down, the other struck his face. He walked to my surgery, about a mile from the mine. I was not at home, but called at his house about two hours after. He complained of being stiff, and could walk with difficulty; no visible injury on hip nor pain, only tenderness. He had a small wound on left side chin, about three quarters of an inch long, and about a sixteenth of an inch deep in the middle; it was rather dirty. His face was washed, the wound bathed with warm water, and soap liniment given him for his hip. Next day he was better. On the 7th, he sent in to say he was nearly all right again. I received this message at 5 P.M.; at 7 P.M. I received another message, saying he was much worse. I visited him at once; found he had gone up to bed at 6.30 feeling well; just after he got to bed, he was seized with severe pain in neck and at the angles of the jaw. His teeth were firmly closed; risus sardonius was well marked. He could hardly utter a sound when trying to speak. Pulse 108, temperature 102°, bowels moved twice during the day. I inserted a steel elevator between his teeth, and gave him twenty grains of chloral and twenty of bromide of potassium (in the form of a syrup). He swallowed this with great difficulty. I applied a very hot linseed poultice around his chin and neck from ear to ear, covering the wound and throat; it was changed every hour for six hours, after that every two hours for twenty-four hours; this gave him great relief.

At 12 P.M., he was still in much pain; I forced open his teeth and gave him twenty more grains of each drug. He looked very bad, and complained of severe pain at the pit of the stomach, going through to his back. At 8 A.M. next morning, still in great pain; slept a little; I forced