Clinical Lectures

ON

LITHOTOMY.

DELIVERED IN

THE LEEDS GENERAL INFIRMARY.

By SAMUEL SMITH, Esq., Senior Surgeon to the Infirmary.

LECTURE III.

GENTLEMEN,—The last case that I related before we parted was that of an old gentleman who had a stone of considerable size in the bladder, and who, after taking mephitic water for several years, and expelling much fine sand, at length expelled also the nucleus of a calculus; after which he was cured of his symptoms of stone, and lived many years. In that case, the strong hereditary tendency to the disease is thus proved. His father died with a large stone in the bladder; and his only brother also died with several large urinary calculi in the bladder, which were removed after death. I think, from the circumstance of his having expelled four lithic acid calculi before such urgent symptoms of stone took place as induced me to sound him; from my finding a stone in his bladder; from the fact that the symptoms were so manifold that he requested an operation; from the fact that Mr. Costello saw him, injected the bladder, and in my presence felt and heard a stone;—from this combination of circumstances, I think, you will rather agree with my opinion than with that of the gentlemen who denied that there had been a stone in the bladder, because one was not found after death.

Case No. 21—John George, operated on September 16th, 1830, is a case worthy of remark, as proving the early period of life at which a stone can be formed in the bladder. This boy was $2\frac{3}{4}$ years of age, and made an excellent recovery. On the 4th of February, 1829, I removed from his urethra this small calculus, when, of course, he was only sixteen months old, and when he had probably partaken of no other food but his mother's milk.

You will see, then, that when I had operated twenty-three times, I had only had one fatal case, that being my first; and, therefore, that my average of deaths at this period was 1 in 23; but the death of No. 24, the case of the little boy from Knaresborough, reduced my average at once to 1 in 12.

I had now three cases in succession that did well; but my twenty-eighth case died—Samuel Kay, 59 years of age, from Wakefield. This was not a favourable case; for the man had had the operation of lithotomy attempted to be performed upon him by some one. I do not know by whom. If you look at this stone, which is of considerable magnitude (two inches and five-eighths in diameter) and of a very hard material, I think you will be convinced that the fracture which you see in it was not made during the operation of lithotomy, but by the attempt at lithotomy. The stone is heavy, and not of that material which breaks down under the pressure of the forceps. Here you see another stone, which was removed at the same time, with a surface evidently fitted by attrition with a concave surface to receive the convex extremity of the large oval calculus.

Case No. 29—David Sidebottom, aged 29, of Wakefield—did well; and the only circumstance worthy of remark is this. I see a memorandum on the box, "This patient was cut for stone by Mr. Hey seven years before". Here was a man 29 years of age, subjected to the operation at 22 years of age, and again at 29, doing well on both occasions.

Case No. 15—John Ratcliffe, of Leeds—had also been operated on before by Mr. Stansfield, my predecessor.

Case No. 30 was fatal. The patient was a man 50 years of age, of very gross habits, weighing about twenty-two stones, certainly the largest man I ever saw subjected to the operation of lithotomy. He was, therefore, not in a favourable state for recovery. By that fatal case my average was reduced to the rate of 1 death in 8; after which nine cases in succession did well, raising it to nearly 1 in 10. But, in consequence of two fatal cases taking place, both operated on in the same day, viz., October 7th, 1844, and both dying on the 11th, my average was reduced to its lowest point—1 in 7. After this, twelve cases in succession did well, the average rising to 1 in 9. But in June and November, 1851, I had again two fatal cases; since which I have only lost one patient—Wm. Scarborough, upon whom I operated May 5th, 1855. He died on the 26th of

May, three weeks afterwards, from the consequences of hæmorrhage occurring on three different occasions, under which he sank, although it was restrained by the usual means. Since this time I have not lost a single case; and my average now stands at 1 death in 8.

Now, gentlemen, it is unfair to make an average from a small number of cases; and it is unfair to make it from a selected number of patients. You must make it from all the cases you have ever had, to arrive at the truth. You are aware that in Norfolk, probably in consequence of the great quantities of cider drunk in that county, stone in the bladder prevails in a greater degree than in any other portion of the United Kingdom; and therefore the surgeons residing in that locality have more numerous opportunities of performing this operation than many others. I stated to you that Martineau, who was an eminent surgeon of the Norwich Infirmary, had performed eighty-four operations in succession, out of which number he had only lost two cases: there was an average, you see, of one death in forty-two. But these were not all the operations Mr. Martineau performed; for his success was not equal to this, either before those eighty-four cases or afterwards. Now, I will tell you the real average of Mr. Martineau in all his operations. He was considered the most celebrated and successful lithotomist of his day; but I will prove to you that his success was no better than mine, upon the whole. Here is a paper which I extracted from a memoir in a Norwich newspaper sent me by my friend Mr. Crosse, of the Norwich Hospital. Dr. Rigby and Mr. Martineau were surgeons there at the same time, and both very eminent for this operation. Here I give you the true statement of all their operations:—"Dr. Rigby lost three in the first fifty, or at the rate of one in sixteen; and one in four and three-fourths of the remainder, amounting to more than double, so as to reduce his average below one in seven. Martineau lost eleven in his first fifty (those were not included in his eighty-four cases), or at the rate of one in four and a half; and, in his remaining cases, one in thirteen and three-quarters; making the average in the end to be one in eight"—the same as mine.

Dr. Rigby's hospital operations amounted to one hundred and six, and to fifteen deaths. Martineau's hospital operations were one hundred and forty-seven, among which were seventeen deaths.

Now, gentlemen, I remember having an interesting conversation on this subject with the late Mr. Michael Thos. Sadler, of this town; and he informed me that the surgeon of a provincial hospital in a neighbouring county had told him that the average of deaths after his lithotomy operations only amounted to one in a hundred. When you hear such statements as that made, do as I did—smile, but say nothing. It would not be polite to give your opinion in plain English upon a statement of that kind. The house-surgeon of the Infirmary, a short time ago, said to me, "We have been very lucky in lithotomy lately, sir." Gentlemen, there is no such thing as luck in lithotomy at all; I have no belief in it. Where an operator has his average of deaths very low, it is a proof of skill; and where he has many deaths, it is a proof of want of skill. I believe that, if it were possible to subject a hundred sound healthy men, who had no stone in the bladder, to the incision made in the perinæum, the knife being carried forwards into the bladder, partially incising the prostate, then a pair of forceps passed, opened, shut, and withdrawn, my opinion is, that a given proportion of such men would die from such a wound. But if, in addition to these circumstances, you imagine each of these persons having a stone in the bladder, and having that bladder diseased to a certain extent by the presence and irritation of that stone, and probably also at the same time labouring under disease of the kidneys, the proportion of individuals who would die of the operation, under the latter circumstances, would be likely to be much greater.

It is singular, yet it is a well established fact, that a greater proportion of elderly men recover from this operation than men of middle age. Every man, without exception, who was turned 60 years of age when I operated upon him, recovered. Several of them were approaching 70 years of age, and one was 73. The case of Patrick Clarke is worthy of remark. Here was a man 66 years of age (not at all an unfavourable circumstance for operation). I remark upon his case, because I think I saved his life by a little ingenious manœuvre. He was operated on on the 27th of April, 1854; and these two large urinary calculi were removed from the bladder. He had been a patient of mine two years before. He came on my admission day—a big, powerful, loud voiced, broad chested, muscular Irishman. His symptoms were so manifestly those of

stone, that I sounded him at once, and detected two stones in the bladder, recommended him strongly to submit to the operation, and told him that every moment he delayed the case would be rendered worse. I did everything in my power, so far as argument was concerned, to induce him to submit; but he wilfully declined it, and disappeared. In the spring of 1854 he returned, a very altered man. He had lost his powerful voice; his shoulders were approaching towards his ears; he was bent; he had lost upwards of three stones of flesh; and it was clear that the irritation of the stone was doing its work—that it was destroying him. Still he would not hear a word of being cut for the stone, when a bright idea struck me. I said to him, "You are a Catholic?" "Yes, I am, sir." "Then go to your priest; consult him; tell him all I have said to you; and ask his advice." The priest was a sensible man, and advised him to submit at once to the operation, as giving him the only chance of saving his life. Thus he took the advice of the priest, when he paid no regard to that of the doctor. He submitted to the operation, made a most favourable recovery, regained his voice and his flesh, and went out of the Infirmary again a big broad chested Irishman.

Cases No. 39 and 35 are worthy of remark, from the circumstance that Thos. Waterhouse, of Horsforth, aged 66, was grandfather of Robert Waterhouse, of the same place, aged 4, shewing a strong hereditary tendency to the disease.

It is surprising with what facility little boys recover from this operation. I will prove this to you by the report of my last five operations on little boys under 7 years of age.

Benjamin Hustler, of Farsley, aged 5, was operated on April 14th, 1856; and went home perfectly well May 9th—that is, on the twenty-fifth day.

Michael Corney, 7 years of age, was operated on June 9th; and J. W. Roberts, 5 years of age, was operated on the same day. Both went home perfectly cured on the 27th of June, or eighteen days after the operation; and one of these little boys was actually bowling a hoop in the Infirmary garden on the fourteenth day after the operation.

William Hainsworth, aged 5 years, from Wakefield, operated on September 7th, 1857, went home cured on the twenty-fifth day after the operation.

Now, how could these wounds be healed in such a short space of time? It was because, in all these cases, it was ascertained that the stones were small; and consequently, although the incisions were deep, they were no longer than necessary. As soon as the urine ceased to flow from the wound, which would be somewhere between the seventh and tenth days, these wounds healed very rapidly; and in all these cases there was not a single untoward symptom.

The ages of those patients who did not survive the operation of lithotomy, as performed by me, were 5, 6, 21, 40, 46, 50, 51, 55, and 59. In some of these cases, the stones were very large. Here is one, removed from Benjamin Greenwood, weighing nearly four ounces, and of that horrible form, the mulberry calculus. You will see it has the appearance of a dozen or two mulberries placed in a glass, and cemented together by lime. There was not much difficulty in extracting it, proving the sufficiency of the incision; but I am sorry to say that the poor man died in a few days. In fact, a favourable result could hardly be expected with such a stone as that.

My average, gentlemen, would have been different, if I had selected my cases; but I took all comers. Where there seemed to be a chance of saving life by the operation, I performed it. Now, in Mr. Crosse's book, he states that Martineau was very particular in selecting his cases, and refusing to operate upon patients who did not appear likely to do well.

There is a case worthy of remark, upon which I operated on the 5th of June, 1850. He was a private patient, who had been for a long course of years subject to stricture in the urethra. He was under his own treatment, and was in the habit of continually wearing a gutta percha bougie night and day, excepting when he was emptying the bladder; and the extremity of the bougie was bent over the glans, to prevent it from slipping into the urethra. Several months before he consulted me, the bougie had slipped into the urethra beyond the glans. He went to a physician, who, instead of cutting boldly upon the bougie when it was in the membranous portion of the urethra, attempted to poke it out, and, in doing so, poked it in. Shortly afterwards, he had all the symptoms of stone in the bladder. When he consulted me, there were small fistulous openings in the perineum. He declared most emphatically that he could feel the presence of the bougie in the bladder every time he passed urine. I searched, by means of the probe, the fistulous openings in the perineum, and also

sounded the bladder; but I could detect no foreign body. His symptoms were so manifest, and he was so urgent himself for an operation, that I called Mr. Hey in consultation, and we decided to perform the operation of lithotomy, which was done with the smallest curved grooved staff I ever saw, and which was made for the occasion, being very little thicker than a knitting-needle, as nothing larger would pass into the bladder. I performed the operation in the usual way; but I incised the prostate with Sir Wm. Blizard's lithotomy-knife, which is the very thing for such a case. This knife was beautifully adapted, by the narrow curved point of the beak, for passing nicely along the narrow groove of a very small staff, and thus penetrating the bladder. Well, gentlemen, as soon as the prostate was incised, I introduced the forefinger of my left hand, and found immediately, to my great satisfaction, that the bougie was there, curled in a semicircular form, resting in that cavity behind the prostate, and the two ends coming up on each side of it, so that it was impossible that any instrument could touch it or detect its presence. As soon as my finger dipped beyond the prostate, it was felt at once, and was readily extracted by a pair of small forceps. This specimen is worthy of remark, as shewing you what a small amount of deposit took place upon this bougie, which had remained in the bladder a hundred and fifty-nine days. You must contrast it with these portions of elastic gum catheter I now shew you, and which I see labelled, "This portion of a catheter was broken, and left in the bladder, by a surgeon in the country; ten days after which it passed into the membranous part of the urethra, and was cut from thence at the Infirmary by Mr. Hey." Now look at the deposit upon that catheter in ten days, and look upon the deposit upon the bougie in a hundred and fifty-nine days: the one which had been in the bladder ten days has ten times more deposit of calcareous matter on it than the one which had been a hundred and fifty-nine days in the bladder.

Now you may say to me, "That catheter being removed by Mr. Hey, how came you in possession of it, Mr. Smith?" I will tell you. Mr. Hey called me in consultation, and requested my assistance in the operation, which was done in the ward while making our rounds. These portions were readily extracted (from a small incision in the line of the raphe of the perineum), and put in a little wooden dish. Mr. Hey washed his hands quickly, and marched away. I washed my hands very slowly, remained, took these portions, washed them carefully, wrapped them up, and put them in my pocket; and, as no inquiry ever afterwards was made for them, I thought them very honestly my own, and I kept them. That is the way, gentlemen, to make a museum.

Among my specimens of urinary calculi are many which have been removed from individuals who have refused to submit to the operation. I have generally kept my eye upon such patients, and have had an opportunity of removing the stones after death. I have also all the urinary calculi which were removed by the operation of lithotomy by my former pupil, John Clayton Cooper, of Bradford, who, I think, was the first lithotomist who ever performed several operations in that town. I have also all the calculi removed by operation by Mr. Lucas, who was Mr. Chorley's predecessor, and twenty years surgeon of this Infirmary, and who afterwards retired to Ripon. A few years ago, when attending in consultation in that town, a surgeon made me a present of all these specimens, formerly belonging to Mr. Lucas. This, you will see, weighs nearly five ounces.

Here is another very heavy urinary calculus (shewing a specimen as big as a cricket-ball, and weighing 6½ ounces), which was expelled from the urethra without laceration, without creating any sloughing of the parts or incontinence of urine. I will give you the case. A very excellent old mare, belonging to Mr. Morphet, of Camp Road Mills, in this town, was known to have symptoms of disease in the bladder, constantly suffering from strangury and bloody urine. One day, a dose of strong physic was given to her; and, the next morning, this calculus was found behind her heels in the stable. A little blood had been parted with, and a little abrasion of the mucous membrane of the meatus took place. Calling at the mill on business, a day or two afterwards, I was shewn this, and saw the mare. There is no doubt whatever that this is an urinary calculus, and that it was expelled in this manner. I begged it; and I consider it a very valuable specimen indeed.

There are also in this box a great number of urinary calculi, which have been removed by me from the urethra, most of them without incision, some of them by the aid of the knife. How are you to remove a calculus from the urethra? Here are little gimerack French instruments for that purpose. I

have had these instruments many years; I have never used them once for that purpose, and never intend to do so. They are very nice to look at; but, when you get a stone in the urethra of a patient of yours, this is the instrument to remove it with. Get the scoop of a small silver director passed into the urethra; and, when you once feel the stone, manœuvre to get the scoop beyond it: you then have it safe; alter the angle of the instrument, and it readily comes forward. Every stone I have removed from the urethra has been by this instrument. It is a far superior plan to the use of forceps of any kind. It is like what is seen in removing a pea from the ear of a child: if you attempt to do it with forceps, you will do what was done with the bougie; instead of poking it out, you will poke it in. But if you get a little scoop beyond it, you will soon have it out.

Transactions of Branches.

BATH AND BRISTOL BRANCH.

THE IMPORTANCE OF PRESENTATION OF THE BREECH OR INFERIOR EXTREMITY IN CASES OF INFANTICIDE.

By WILLIAM BIRD HERAPATH, M.D.Lond., F.R.S.Ed., Analytical Chemist and Chemical Toxicologist.

[Read December 16th, 1858.]

In introducing this subject to your attentive consideration, I do so with the more pleasure, as it appears to be one which, although it has hitherto escaped the attention of medical jurists, may, from the peculiar state of the English laws with respect to infanticide, yet be occasionally the means of bringing home the crime to the guilty party, and of awarding that punishment which it so richly deserves.

Professor Taylor justly says, that the great question on a trial for child-murder is, "whether the child has been born alive?" and in Chapter XLII, wherein he discusses the various proofs of a child having been born alive he adduces evidence derivable from the organs of respiration, from marks of violence, from changes in the umbilical vessels, foramen ovale, and ductus arteriosus—evidence from the discovery of food in the alimentary canal, and the detection of live birth by the application of chemical tests to the contents of the stomach; and he concludes this chapter, with a well founded complaint that our signs of live-birth are so few and defective, in these appropriate words: "The slightest consideration will show that the signs of live-birth above described are weak, and of purely accidental occurrence. If the child be destroyed during birth, or within a few minutes afterwards, there will be no medical evidence to indicate the period at which its destruction took place. The external and internal appearances presented by the body would be the same in the two cases. It is most probable that in the greater number of instances of child murder, the child is actually destroyed during birth, or immediately afterwards; and, therefore, the characters above described can rarely be available in practice. If any exception be made, it is with respect to the nature, situation, and extent of the marks of violence; but the presence of these depends on mere accident. Hence, then, we come to the conclusion, that although medical evidence can often show, from the state of the lungs, that a child has really lived, it can very rarely be in a condition to prove, in a case of infanticide, that its life certainly continued after its birth. We could only venture upon this inference when the signs of respiration were full and complete, or food was found in the stomach. Why the destruction of a child should be treated in the one case as a venial offence, and in the other as a capital crime, is one of those anomalies in our criminal jurisprudence for which it is impossible to account. The inference which we may draw from these observations is, that if positive proof of entire live-birth be in all cases rigorously demanded of medical witnesses on trials for child-murder, it is scarcely possible, when the prisoner is ably defended, that any conviction for the crime can ever take place. The only exception would be, when a confession was made by the accused, or the murder was actually perpetrated before eye-witnesses. The numerous acquittals that take place on trials for this crime, in the face of the strongest medical evidence, bear out the correctness of this opinion. The child is proved to have lived and breathed, but the medical evidence fails to show that the living and breathing took place or continued after entire delivery."

He also sums up the conclusions to be drawn from the facts contained in this important chapter, under the following seven heads:—

"1. That if the lungs be fully and perfectly distended with air by the act of respiration, this affords a strong presumption that the child has been *born alive*, since *respiration during birth* is, in general, only partial and imperfect.

"2. That the presence of marks of severe violence on various parts of the body, if possessing vital characters, renders it probable that the child was entirely born alive when the violence was inflicted.

"3. That certain changes in the umbilical vessels, and the separation by a vital process and cicatrisation of the umbilical cord, as well as a general peeling or scaling off of the cuticle, indicate live birth.

"4. That the absence of meconium from the intestines, and urine from the bladder, are not proofs that a child has been entirely born alive, since these liquids may be discharged during the act of birth.

"5. That the open or contracted state of the foramen ovale or ductus arteriosus, furnishes no evidence of a child having been born alive. These parts may become closed and contracted before birth, and therefore in a child born dead; or they may remain open after birth in a child born living, even after the establishment of respiration.

"6. That the presence of farinaceous food, sugar, milk, albumen, or other food in the stomach, proves that the child has been entirely born alive.

"7. That, irrespective of the above conclusions, there is no medical sign which indicates that a child which has died at, or about the time of birth, has been *entirely* born alive." (*Medical Jurisprudence*, Sixth Edition, p. 484.)

I have made the preceding extensive quotations from this usually excellent authority on subjects of this nature, in order that you might be fully in possession of the position of the question I am about to bring before you. With the majority of these conclusions of Professor Taylor we must all most fully concur; but with respect to the last, I hope, in the course of this paper, to adduce another source of evidence which we may possess under some circumstances, and which may enable us to prove that a given child has been *entirely* born alive. I here allude to the nature of the birth itself. It is well known that there are in most cases of parturition certain appearances left upon the infant's body, arising from the character of the birth and due to the nature of the presentation. Now, under such circumstances, it would be possible to adduce these as evidence, that the child had acquired a vitality independent of the mother, subsequent to its entire expulsion from the body of the parent. For it is not alone necessary to prove that the child had fully inflated its own lungs; it is also demanded of the medical witness to prove that the act of inspiration of the child was *not performed* whilst the body of the infant was still engaged in the pelvis and inferior outlets of the mother. Now when the presentation has been that of the breech, knees, or feet, or of only one of the inferior extremities, it follows that should the infant's body exhibit evidences of any one of these having been the presenting parts, there can be no reasonable doubt that the whole of the body of the child must have been expelled from the body of its parent before respiration could have possibly commenced; and consequently, if in the same case, we also find the lungs fully and perfectly inflated, it is presumptive evidence, not only that the child has been alive in a medical sense, but that it has equally acquired a legal right to protection from violence, as such a condition of the organs of respiration would indicate that the infant had also lived sufficiently long after its birth (which was necessarily entire,) to perfect the respiratory act, and thus overthrow the whole foetal circulation.

Without further prelude, I will now proceed to adduce the particulars of a case which, having recently occurred to me, has most forcibly brought the matter prominently before me.

POST MORTEM EXAMINATION, made on the body of the illegitimate child of Elizabeth Pyne; by order of the coroner, August 28, 1858, at 12-30 noon.

It was a mature female child, having talipes of both feet. The left leg at once arrested the attention from its swollen state and deeply purple colour; this discoloration reached as high as the inguinal region. It had evidently been the presenting part, and must have been some time in the vagina. The right leg was much smaller, and of the natural size and colour, with the exception of one purple swollen patch on the patella—this was probably from the same cause. The umbilical cord was about two and a half inches long—untied, and the extremity