years. I passed a sound into his bladder, and verified my diagnosis. The urine contained no albumen.

On December 20th, I performed lateral lithotomy, and extracted an oblong mulberry calculus, which weighed 5.7 grains. Ether was the anæsthetic employed, and was administered by means of a sponge enclosed in a towel folded six or eight times, and held closely over the mouth and nose. Fully half an hour elapsed before complete anæsthesia supervened. The quantity of ether used was six ounces. There was no excitement, nor any noisiness on the part of the patient. The pulse and breathing were scarcely at all affected, but the return to consciousness was protracted considerably beyond the time usually required for recovery after chloroform inhalation. An hour and a half after the operation vomiting came on, and recurred frequently for twenty hours in spite of remedies. The patient complained of the vomited matter tasting strongly of ether. Only a little light food had been taken on the morning of the operation, and that three or four hours previously.

With the exception of the exhaustion produced by the persistent sickness, the patient had progressed favourably, and since December 30th has sat up for several hours daily. I am indebted to my friends, Mr. Jobson and Mr. Thwaites, of Bishop Auckland, for their kind assistance at the operation.

CLINICAL REMARKS ON STONE IN THE BLADDER.*

By W. F. TEEVAN, B.A., F.R.C.S.,
Surgeon to the West London Hospital; etc.

I HAVE now operated on thirty-five patients for stone in the bladder; of these twenty-three were adults, the remainder boys. I performed lithotomy in seventeen cases, and lithotripsy in eighteen. I lost two of my patients; one, a pauper aged 66, who had been bedridden for two years before the operation, died one month after I had removed from him, by lithotomy, a large oxalate of lime calculus; and the other patient, aged 70, died suddenly from heart-disease ten days after lithotripsy, whilst straining forcibly to pass a fragment of stone. It thus appears I have had two deaths in my practice, and I have published them both, for I think it more incumbent on a surgeon to make known his fatal cases than his successful ones: in the first place, it is only due to the profession; and, secondly, I think that more is to be gained from an investigation into the fatal cases. I would now briefly relate certain conclusions at which I have arrived from my own experiments, investigations, and experience. I will commence with the choice of an operation in a given case. Shall it be lithotomy or lithotripsy? I think the following rule is the safer one to act upon. We ought to cut every patient, unless we can clearly make out that lithotripsy is indicated. By adopting such a rule as this we shall, I am sure, obtain much better results than if we act on the converse and crush every case unless lithotomy be specially called for, as there will always be cases occurring, from time to time, in which there will be doubt whether they be better suited for lithotomy or for lithotripsy. I would say that every doubtful case ought to be cut. I have never regretted performing lithotomy, but I have some reason to think that in a particular case lithotomy would have been better than lithotripsy. I am of opinion that we ought never to crush a calculus unless the urethra be capacious, the stone small or of moderate size, and the kidneys fairly healthy; for if they be much diseased the patient will, in all probability, get a low form of chronic cystitis, and be carried off by irritative fever or blood-poisoning. However, every case must be judged by its own particular peculiarities and bearings, and not by any rules that may be or can be laid down. For instance, a contracted urethra does not preclude lithotripsy, because we can enlarge the canal, and if the meatus be abnormally small we can incise it. Neither does a large stone forbid crushing, for it may be safely done in a young man, provided it be phosphatic and not too large. Then, again, the fact of the kidneys being diseased does not put lithotripsy out of the question; for if the calculus were small, it could be crushed with comparative safety. If the urethra and bladder be extremely sensitive, I regard the case as unfit for lithotripsy, for the patient would be broken up by the want of rest and loss of appetite, attending the continued irritation set up by the passage of fragments. However, before relegating such a case as I have described to lithotomy, we ought to try if we cannot greatly abate the morbid sensitiveness of the urinary tract by the daily introduction of an olivary elastic bougie. I have known excessive irritability almost entirely removed in a few weeks by such method. Experience is everything in lithotripsy. A surgeon may cut his first case as brilliantly as his last, but it is otherwise with

lithotripsy—it requires practice, and a surgeon who has a gentle hand may impose on the tolerance of the bladder in a way that would be instantly resented if the surgeon were an inexperienced operator. The more I see of lithotripsy, the more I am convinced that the secret of success lies in making each crushing as short as possible. At the first and second sittings the male blade ought only to be screwed home once, but later on this procedure may be repeated twice or thrice. It would thus appear that I only allow my lithotrite to remain in the bladder from half a minute to two or three minutes, and it is to this that I attribute my success. I never give chloroform for lithotripsy, for I look upon the patient's consciousness as necessary for a successful result. If the patient take chloroform, the surgeon is usually tempted to do too much, and the result is that the patient gets incontinence of urine or cystitis. Indeed, I would say that, if lithotripsy cannot be performed without chloroform, the patient had better be cut. I have seen deplorable results ensue from chloroform being given in cases of lithotripsy—the patients being made chronic invalids for the rest of life, and actually put into a worse position than before they were rid of their calculi. Usually I keep my patients to bed only on the day of operation, and on that following it, for I find that confinement to the house injures a patient by lessening his strength; indeed, I have treated many cases most successfully as out-patients throughout. The after-treatment of my cases is as simple as possible; I do nothing unless there be a clear indication for it, and I never wash out a patient's bladder, unless imperatively needed, for it gives more pain than lithotripsy itself. No prudent surgeon would, I presume, ever think of withdrawing a fragment of stone from the urethra unless it were impacted; for any laceration of the canal is sure to be followed by traumatic stricture, and may be attended with fatal consequences through urethral fever or pyæmia.

I have cut seventeen patients for stone; twelve of them were boys, the remaining five adults, who had very large calculi. All my patients recovered, except the one already referred to. Before operating either for lithotomy or lithotripsy, I consider it advisable to keep the patient very quiet for some days, in order to reduce the general and local irritation to a minimum.* We ought to commence the external incision low down, for we then wound less vascular parts, which is all-important in feeble old men. The only disadvantage attached to it is that we may injure the rectum; but this is unimportant, as we can always close the fistula. Everything hinges upon the internal incision. The usual method, in England, is to make a very small hole in the bladder—merely piercing it; and the advocates of such practice found it upon grounds which are absolutely false. They say that if you exceed the margin of the prostate, and open up the deep fasciæ, you will get infiltration of urine, which will kill the patient. Now, infiltration of urine is a physical impossibility after lithotomy. How can you get infiltration when the urine is poured out as fast as it is secreted? Where is the urine to be infiltrated? All the tissues in connection with the wound are filled with coagulated blood a few minutes after the operation. They are, so to speak, varnished with blood, and no urine can affect them. Surgeons talk of infiltration of urine after lithotomy, but, so far as I know, there is not a single unequivocal case on record; the alleged cases of infiltration having been instances of cellulitis, occurring from the surgeon having bruised the prostate in extracting calculi from persons of bad constitution. The advocates of the limited incision are in this extraordinary position. Either two bodies can occupy the same space at the same time, or else the capsule of the prostate can be stretched like a piece of India-rubber, to permit the surgeon to extract the stone, and then immediately returning to its former circumference. Now, either of the above suppositions is absurd. Two bodies cannot occupy the same space at the same time; the prostate and the calculus cannot both be contained at the same time in a fibrous circle less than two inches in diameter. The fibrous jacket, in which the prostate is enclosed, is made of white cellular tissue; and if a student were to go up to the College of Surgeons, and tell the examiners that white fibrous tissue could be stretched like a piece of India-rubber, he would be deservedly plucked. It is impossible to extract a stone by dilatation. You may tear, but you cannot dilate. If there were no objection to the present method of operating, it would still, in my opinion, be open to condemnation, as it tends to make men timid or indifferent operators. No year passes in London without it being known to the profession that some surgeon has failed in the operation of lithotomy. Lithotomy may well boast that it has humiliated more surgeons than all the other operations in surgery put together. Many a surgeon operates brilliantly for years, and, at last, receives his *coup de grace* from lithotomy. At the present moment, London presents the melancholy fact of having a surgeon who has had much experience in cutting for stone, and who yet re-

* Read before the Surgical Section at the Annual Meeting of the British Medical Association in Birmingham, August 1872.

* I always perform the lateral operation. The median is not so successful method, and is admirably calculated to emasculate the patient. The medio-bilateral is founded on deductions from supposed facts which are fallacious.

fuses to allow the profession to learn his results. Can we come to any other conclusion but that his reticence is due to a want of success? The disasters that have occurred to surgeons, when operating for stone, have nearly always been caused through an insufficient opening being made into the bladder, the result being that the viscus receded before the pushing finger, and left the surgeon to grope about in the ischio-rectal fossa for that which he found not. Mr. Ellis has, long ago, proved that there is no such thing as dilatation of the prostate, and has very pertinently pointed out that the most successful cases of lithotomy occur in children, when the boundaries of the prostate *must* always be overstepped, on account of the very rudimentary condition of that organ. A surgeon will be heard to state that, in the case on which he has just operated, he has extracted the stone by a limited incision, and subsequent dilatation. But I would ask, what proof is there of such having been done? Not having examined the parts, how does he know whether he has, or has not, exceeded the bounds of the prostate. He cannot tell. On the contrary, all the specimens in the London museums which I have examined, prove incontestably that no surgeon has ever extracted a stone, unless a very small one, without exceeding the margin of the prostate. Some years ago, I carried out a series of investigations on dead subjects, and found that no ordinary sized stone could be extracted from the bladder without completely rupturing the prostate and its capsule. (*Path. Soc. Transactions*, vol. xvii, p. 186.) Three of the patients whom I cut for stone were troubled with incontinence of urine after the operation, and the reason is clear. When I first began to operate for lithotomy, I followed the plan taught me, making a small incision, and subsequently enlarging the wound with my finger. The result was that my first and second cases were troubled with incontinence. I then altered my method; the consequence was that all recovered without suffering from incontinence except one boy, from whom I was obliged to withdraw the stone without enlarging the wound, on account of the critical condition he was in from chloroform. I now come to that proof which will carry conviction, and I will, in the first place, explain what I mean by a free incision. No one would more strongly deprecate making a larger wound than is necessary than myself; at the same time, I believe that more injury has been caused to patients through making incisions too small than too large. I, first of all, make an aperture large enough to permit my finger to slip into the bladder without the slightest force being required. If, however, my finger should not slip in easily, I introduce the knife, and enlarge the wound. And in the bladder I grasp the stone with the forceps (in boys I extract the calculus with the forefinger only), and then commence to withdraw it. If there be the slightest resistance, I introduce a probe-pointed bistoury, and cut outwards and downwards till the stone glides out without the slightest traction being exerted. So far as I can ascertain, those surgeons who have adopted the cutting plan for extraction have attained a success wholly unapproachable by those who follow the so-called plan of dilating, which is lacerating. Mr. Gutteridge, Mr. Brett, Mr. Carr Jackson, Mr. Vincent Jackson, and myself have cut 349 patients for stone, and the sum total of all the cases that died after operation only amounts to eleven. These results carry with them the incontrovertible deduction that a method of operation which only loses 3 per cent. of its patients must have a better basis for its support than one which permits 15 per cent. to die. I will now conclude by giving one of the most important judgments ever uttered on the subject of lithotomy.

"It has certainly appeared to me that the very result so much apprehended from a free incision of the neck of the bladder seems to have followed in most of my unsuccessful cases from a want of a sufficiently free incision; whereas my unhesitatingly cutting all opposing textures has, especially in my last sixty-eight cases, been followed with the happiest results. Indeed I have almost felt conscious, whenever a case has terminated unfavourably, or the recovery has been slow, that my internal incision has not been sufficiently bold, and that the operation has been protracted thereby." (*On the Surgical Diseases of India*. By Mr. Brett, p. 206.) Better surgical principles than these no book contains.

THE PATHOLOGY OF SICK-HEADACHE.

By FRANCIS E. ANSTIE, M.D., Assistant-Physician to, and Lecturer on Medicine at, the Westminster Hospital.

SOME remarks of Dr. P. W. Latham, in a recent paper in the *BRITISH MEDICAL JOURNAL*, which advert to my late articles on migraine in the *Practitioner*, call for a reply from me.

Dr. Latham has misunderstood the nature of my claim, and he might, perhaps, have given me credit for familiarity with such hackneyed authors as Tissot, Romberg, and Lélut. I had no idea of

claiming the origination of the idea that migraine was a neuralgia: the statement for which I did venture to claim a certain merit of priority was much larger than that. It was to the effect that migraine is almost the only neuralgia of the period of bodily development, and depends on inherited defects in the nutrition of the medulla oblongata, and that it is intimately mixed up with, and frequently interchangeable with other and more formidable nervous diseases, which are also the results of similar defects in the nutrition of the medulla; and that it is, so to speak, a matter of chance whether a person born of a certain race will have migraine, or epilepsy, or asthma. Your readers will perceive that this, which was stated in outline five years ago, in my article on "Neuralgia" in Reynolds's *System*, and at much fuller length in my book on *Neuralgia* in 1871, is quite different—whether true or false—from what had been said by any previous writer. But I am exceedingly pleased to see that Dr. Edward Liveing has, quite independently, arrived at conclusions which come very near to mine. He thinks that migraine is an inherited disease, and that each paroxysm is a nerve-storm which may range over the whole tract between the optic thalami and the nucleus of the vagus. I am greatly satisfied to be so far supported by Dr. Liveing's opinion, which I esteem as highly as that of any pathologist living in regard to questions of this kind. In agreement with Dr. Liveing, I also am obliged to reject the idea of sympathetic nerve-disturbance as a primary phenomenon in migraine; I believe them to be only secondary. The papers of Dubois-Reymond and Möllendorff were long ago carefully studied by me; and the views which they take are rejected upon grounds which will be found stated at length in the chapters on "Pathology" and on "Complications" in my book on *Neuralgia*. I believe that the sympathetic phenomena in migraine are mere secondary matters, standing on a level with vasomotor secondary disturbances, which may occur in neuralgias of every situation and form.

[The above should have been inserted in last week's *BRITISH MEDICAL JOURNAL*, but the MS. was accidentally mislaid.—ED. B. M. J.]

A CASE OF UTERINE HYDATIDS.

By SPENCER T. SMYTH, M.D., F.R.C.S. Eng.,
Great Yarmouth.

THE subject of this history was S. J. T., aged 13 last April, of plethoric habit of body, her general configuration being equal, in many respects, to girls of more mature age. The catamenia made their first appearance just prior to the age of thirteen, and continued regularly to do so, until within three months of her present attack. No cause could be assigned for the amenorrhoea; the general state of health continued good, so that no medical aid was sought. Her mother noticed that she was more indolent than usual. The abdomen was swollen, as also were the mammary glands. She occasionally manifested symptoms of nausea. One morning in December 1872, I was requested to visit her, owing to severe flooding. Upon examining the abdomen externally, I found the uterus enlarged, forming a tumour equal in size to that of a three months' pregnancy. I stated to the mother that, if she were older and married, I should have regarded her case as one of threatened abortion. Much dark blood with coagula had been discharged, accompanied with violent pains. No examination was at this time made *per vaginam*. The areolæ were of a dark colour. Ergot was administered, which (after a few days) had the effect of restraining the hæmorrhage. Upon my second visit, I found her tolerably free from pain, with quiet pulse, without any febrile disturbance; the loss was entirely checked. Still, however, the abdomen was enlarged, and the line of the uterine parietes well defined. On the next day, she was visited by my partner, Mr. Wylls. The flooding had recurred with greater violence, accompanied with the throwing off of a mass of hydatids, attached to the lining membrane of the uterus, which was in a highly hypertrophied, indurated state, as if the result of inflammatory action. She is steadily recovering from the attack, although very anæmic in appearance. Upon interrogation, she stoutly denies having had sexual intercourse, although I strongly suspect that such had occurred.

REMARKS.—Both Sir C. Clarke and Dr. Kennedy mention that hydatids of the uterus may occur without previous sexual intercourse, although, in the majority of instances, they are the result of conception, being an enlarged condition of the villi of the chorion. Danger arises, at the time of expulsion, from hæmorrhage; frequently they are discharged piecemeal, the portion remaining *in utero* keeping up the flooding. If, in this case, conception had not occurred (from her own statement), the occurrence of hydatids must be viewed as the result of inflammatory action. Their symptoms simulate pregnancy very closely, but without the feeling of quickening, *ballotement*, and sounds of the