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 dilatating, 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 vaso-motor 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 amenorrhœa; 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. Wyllys. 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, *ballottement*, and sounds of the