

The views of some of those who spoke at the recent British Medical Association meeting offer far better material for criticism. No greater condemnation of the suprapubic operation could be found than the progress of the case related by Dr. Burton, a case which died with a suprapubic sinus four months after operation. This stone, though it was 10 oz. 5 drachms, could easily have been dealt with by perineal lithotripsy. The giant lithotrite will easily crush a stone the size of a fetal head, and requires only an opening as big as the index finger.

Yours, &c.,

DELHI: J. A. CUNNINGHAM, M.D., M.Ch.,
November 1900. Major, I.M.S.

THE TREATMENT OF CHOLERA.

To the Editor of "THE INDIAN MEDICAL GAZETTE."

SIR,—I have read with great interest the leader in the *Statesman* of 24th October on Major Brown's article in your paper on the treatment of cholera, etc., by oil of eucalyptus. I am interested for two reasons: Firstly, because of the relief given in dealing with this dreaded disease; and secondly, because it has been confirmatory of a treatment conducted by Captain E. E. Waters, I.M.S., at the Military Hospital, Buxa Duars, and by myself in the tea gardens of the same neighbourhood.

History.—I came out in 1896 as medical officer for the Torsarkos Division of the Duars. On my arrival cholera was raging in several of the estates, and several hundreds of the coolies died, and many more fled in fear. On coming in contact with the disease for the first time, I was appalled with the terrible effects, and my thoughts were naturally led out to the bacillus which was the cause of all this mischief. This, I was sure, was of no avail in the present emergency, and I at once began to study the symptoms. I was almost from the first led to the use of turpentine c. ammonia which I administered myself, going from garden to garden and from coolie lines to coolie lines, I met with almost immediate and continued success. After the cholera epidemics were over (save a few sporadic cases) I was often troubled with cases of dysentery and diarrhoea, generally with bloody stools, but my unflinching remedy was my turpentine-ammonia emulsion, so that what hitherto had proved such a menace to the labour forces of the several gardens, was robbed of its dread and power. But owing to the nature of our free labour in this part, there is not the same opportunity for careful statistical observation as on agreement gardens, as in Assam. Consequently my work lost much of its effect as far as tabulating results was concerned. I only heard from the Sirdars how the sick were doing.

Towards the end of 1896 I had the pleasure of meeting Dr. Waters at Alipur Duars, and as a contemporary of mine at the Edinburgh University, we had much in common to talk about, and especially our work at that time. About a month or six weeks before there had been a change of the Garrison at Buxa, and a very bad epidemic of hill dysentery had broken out, and his hospital was full. I went over to him my experiences in the tea estates and gave him my formula for treatment. A fortnight after I had a line from him that the mixture was proving a great success, a week later that his hospital was empty.

Some time after I paid a visit to Buxa, and Dr. Waters kindly showed me over the "logbook" for that period, and also gave me extracts from daybook of some of the cases which quite accorded with my experience, although I was not able to get such definite observations. Since then I have always kept in my stores the abovementioned drugs in case of further outbreaks, but happily I have had none, but in cases of dysentery and diarrhoea it is my one stand by.

I believe Captain Waters at the time communicated with the P. M. O. regarding it, and also communicated his facts to one of the medical papers, but I cannot say how far it has been pushed in the Indian Medical Service, but with the observations and treatment of Major Brown, which have been entirely in sympathy with my own, I trust there will be a more definite and general recognition of this form of treatment by the service doctors, which I am sure will lead to an increased mitigation of the diseases connected with the bowels, which have caused such havoc in the numbers that have died.

What of Enteric? is there not something here that may come as a boon to the many who are battling with it? and you will be doing good service, Sir, by impressing this new fact brought forward, upon the minds of the professional men, again and again. We are so often so slow in the up-take that it needs the "summation of stimuli" to work its effect.

I hope you will not think me lengthy in my letter. I have only given you the barest facts and outline of what has been an important and pleasurable part of my medical career.

I am, etc.,

ALFRED J. COPPLESTONE, M.B., C.M.,
Edinburgh.

MR. MILTON'S PAPER ON STONE.

To the Editor of "THE INDIAN MEDICAL GAZETTE."

SIR,—With reference to Mr. Milton's article appearing in the current issue which you so kindly forwarded me, together with the samples of Mr. Milton's stones, I have some remarks to make which I shall be obliged if you insert. With reference to Keith's operation, as it has been up to the present practically only performed at Hyderabad, Sindh, I am sure the Surgeons of Hyderabad experience will be able to give Mr. Milton any details he may require concerning the necessity for that operation. One thing they have established, viz., that it is a safer operation than lateral lithotomy. Mr. Milton is mistaken if he thinks that Keith's operation is performed to any extent in the Punjab. One of the objects of my article in your "stone number" was to advocate its adoption in stones too large or too hard for litholapaxy by the urethral route in preference to any operation in which the bladder is opened by the surgeon's knife, and in case the stone would turn out impossible for a No. 20 lithotrite, to adopt an operation practically the same as the perineal operation advocated by Mr. Milton himself.

Both Major Baker and I were evidently under the impression that the formation of the vesical calculus in Egypt had some connection with chyluria.* My inference was drawn from Mr. Milton's article in the *Lancet*, May 2nd, 1896, which runs as follows:—"My personal experience of stone in children has been but small. In Egypt the very great majority of stones occur in subjects over ten years of age; the tables given with this paper do not demonstrate this point, but on reference to my records I find that out of the 200 cases given in Table I, nine only were in children under ten, whereas Cheselden in 213 cases met with 105. In India, on the other hand, the proportion of children operated on is exceptionally large. The difference is due to the fact that stone in Egypt is principally caused by the alterations produced in the urinary system by the presence of bilharzia embryos, and that the bilharzia parasite, while very common in adult males, is rare in women and children." These are the only facts I had to go on. I think my inference was fairly rational. In Mr. Milton's article in your current issue he now says, "chyluria is rare in Egypt, and I have never met with a case suffering from stone."

Mr. Milton states that my opinion as regards hard stones "should be based on knowledge." There was very little of the "Igo" in my article in your "stone number." I did not give my personal experience *in extenso*, as it did not seem necessary in the article I was writing. However, I have had experience of about 400 stones ranging in weight from 22 ounces down, and in patients from 100 years to eleven months, so that I think few would consider me an absolute novice in the art. I have never operated on an Egyptian stone, nor am I aware that Mr. Milton has operated on many Indian stones, so our power of gauging the hardness of stones is relative, except in one way, viz., the capacity of a standard lithotrite to deal with a stone of a given weight. Given a liberal amount of experience in litholapaxy, such as many of us have in India, I regard the operation as purely mechanical as the art of shoeing a horse, and I cannot understand one operator having what other experienced operators would consider magic power with a lithotrite. Mr. Milton lays down weights in children and adults above which he considers stones large. Weights are only one element in this matter. Hardness must also be taken into consideration, and I think that a better standard for this class of stones would be stones impossible from hardness or size or both for litholapaxy by the urethral route, the urethra being normal and there being no contraindications in bladder conditions. When Mr. Milton's dried fragments reached me, fragments of the 12, 14 and 16 ounce stones were invoiced as missing.† The specimens are interesting, and much resemble the stones I have personally come across as far as one can judge from dried fragments. However, I consider that dried fragments are a most erroneous guide to the actual hardness of the particular stone as it is found in the bladder. To illustrate this point. In this Province two stones, were dealt with by perineal lithotripsy with the same lithotrite, a No. 26 Weiss, the smaller one by a highly skilled operator not long ago, the other by myself quite recently. The two stones were of the same composition and structure, and closely resemble Mr. Milton's specimen No. 7. The smaller one was just under six ounces, the larger one was 22 ounces. In the case of the smaller one it took two strong men to exert their utmost power in screwing up the instrument, and when the stone was broken the instrument was strained in the shaft, which is No. 20 English scale. In the case of the large one the same instrument in my hands broke the stone with ease, and I am no more skilled in the use of the instrument than my brother officer. I defy any man to say from the dried fragments which was the hard and which the soft stone. Again, the dried fragments of stones of one district in this Province are just like

* Chyluria is not perhaps the best description of the urine in cases of bladder bilharzia disease. In his article, however, Captain Smith wrote "Chyluria or such urine," including therefore Bilharzia disease—ED., I. M. G.

† Lost en route in post to India.—ED., I. M. G.

the dried fragments of stones of another district, yet it is a well recognised fact that the stones of some districts are incomparably harder than the stones of others. From these facts I consider that the only way of gauging the relative hardness of stones other than soft phosphates, is by the capacity of a standard instrument to break them. If a standard instrument will break one stone of a given size and is not powerful enough to break another of the same size, it seems to me to follow that the latter is harder than the former. Mr. Milton's instrument, with which he crushed the 12 $\frac{1}{2}$ -ounce stone, "more than usually dense," referred to in my article, is No. 15 $\frac{1}{2}$ in the angle and No. 14 in the stem, $\frac{3}{8}$ " long in the jaw, with a 5" locking grip. Weiss' instruments, large and small, are made with a jaw about half the length of the locking grip. Thus Weiss' A. No. (No. 16) is just under an inch long in the jaw, and its maximum locking power is just under two inches. In this respect in practice I personally have found that they are made mechanically correct. Weiss' A. No. is actually longer in the jaw than Mr. Milton's under consideration, and I find that it and the smaller ones made on the same principle are not capable of grasping hard stones larger than those on which they will lock. Hard stones larger than those on which a Weiss' instrument will lock slip out of the instrument. These same instruments are capable of dealing with larger soft stones than they will grasp across the axis. They will cut into the side of a relatively soft stone and thus get a grip, and stones of medium or under medium hardness can be so dealt with by any one skilled in the use of the instrument. I have personally dealt with 7 oz. uric acid stone of just under medium hardness with a Weiss A No. without difficulty. If the same stone had been hard, it would have been impossible to deal with it with that instrument. Mr. Milton's instrument with a 5-in. grip should be 2 $\frac{1}{2}$ -in. long in the jaw. Of course, stones are not spheres, but large sized ones approach to spheres. I personally do not understand how it is mechanically possible for Mr. Milton's instrument, $\frac{3}{8}$ " long in the jaw, to catch what we in India call a hard stone of over 12 ounces. If it were what we in India call a soft stone, it is comprehensible.

We appear to not get as great a proportion of large stones in adults as Mr. Milton does in Egypt. Mr. Milton lays down weights over which he would consider stones in children large. If Mr. Milton had referred to the tables in my article relating to children, he would have seen that the standard I took excluded many cases which by his standard would be large, and yet there remains enough to show him that we have a liberal experience in dealing with large stones in children, and it is just as difficult to deal with a large stone in a child as it is to deal with a large one in an adult. Further, Mr. Milton's large stones were all operated on by himself, a skilled operator. The results in my tables do not pretend to show the results of the leading operators in this province, though they include their results. A large proportion of them were done by members of the Subordinate Medical Department and by officers in Military employ, whose experience was naturally limited. It would be as fair a comparison to compare the results of one of the leading Indian operators with the combined results of Mr. Milton and of the men in charge of the dispensaries all over Egypt, assuming that Egypt has a Civil Medical Department such as we have in India.

I am personally very interested in Mr. Milton's results in perineal lithotripsy, and consider that if he and the others who advocate the operation as an alternative to suprapubic lithotomy for large stones are able to keep up to his splendid record in the operation, we shall be able to render the suprapubic operation a thing of the past. With regard to Mr. Milton's remarks concerning what he calls my "sweeping condemnation" of perineal lithotripsy, I think he cannot have read my article with care. If he had, he would have seen that the very title of the article was a plea for this very operation, and that my second last paragraph placed it where I considered it should be. What I said only "required to be mentioned to be condemned," was performing perineal lithotripsy as an operation of election in cases in which there was no contra indication to litholapaxy. This I see no reason to depart from. Perineal lithotripsy as performed by Mr. Milton or by me is nothing less than a lateral lithotomy as performed for small and medium sized stones, in which is added much more instrumentation than is required in an ordinary lateral lithotomy, and it thus certainly cannot claim a smaller death-rate than Cheselden's operation for small and medium sized stones. I think at the present time it does not require to be argued that such a lateral lithotomy is a more formidable operation than litholapaxy in skilled hands in cases in which the latter operation is not contraindicated. I do not know whether Mr. Harrison does or does not do perineal lithotripsy as an operation of election. Mr. Dolbeau is of the past. Mr. Milton laid down in the *Lancet*, April 18th, 1896, that he then did perineal lithotripsy as an operation of election in cases in which urethral litholapaxy was feasible as a demonstration for his students. He says as follows:—"Bigelow's operation was performed in fifteen of the thirty-five cases, and the more experience I have in this operation the more I am convinced that there are very few cases indeed which it cannot cope with. It is true that in the larger portion of the thirty-five cases I have performed the perineal operation I have done so for one very sufficient reason. The operations are done before students, and practically their only experience of stone

operations is what they see me do. They will be called upon to operate on large stone cases; and for them and for the great majority of practitioners the perineal operation is by far the better. I therefore take frequent opportunities of demonstrating it to them. In the hands of a surgeon constantly operating, however, Bigelow's operation is practically capable of removing any stone however big."

If I am right in considering that perineal lithotripsy, as performed by Mr. Milton in cases in which litholapaxy is feasible, is a more formidable operation than litholapaxy as far as the patient is concerned, I think I am right in saying that such an operation is to be condemned, and that such demonstration should be done in the *post-mortem* room.

Thanking you for your courtesy.

Yours,

H. SMITH, M.D., CAPTAIN, I.M.S.,

Civil Surgeon.

JULLUNDUR :
1st November 1900. }

Service Notes.

WHILE we do not agree with all that is written in the Editorial in the *British Medical Journal* for September 22nd, the following is to be remembered:—

"This is the third time within five years that all leave has been stopped; and having regard to the frequent recurrence of such emergencies, and to the trying climatic conditions of service in India, the question whether the establishment of the Indian Medical Service ought not to be permanently augmented necessarily arises. It has frequently been discussed, and we have reason to believe that representations have been made to the Indian Government, not only unofficially but officially. The financial authorities are, of course, opposed to any augmentation, and are disposed to contend that the admitted pressure on the Service is of a temporary character and ought to be met by temporary expedients. But when pressure can be considered to cease to be temporary we do not know, for it has continued almost without intermission for five years. It has already contributed to diminish the popularity of the Service, and threatens ultimately to extinguish it altogether."

CAPTAIN G. G. GIFFORD, I.M.S., acts as Professor of Surgery, Madras Medical College, till the return of Lieutenant-Colonel J. Maitland, I.M.S., from sick leave. Major F. J. Crawford, I.M.S., is acting as Professor of Materia Medica, and Captain C. Donovan, I.M.S., M.D., as Professor of Hygiene in the same College.

CAPTAIN LEUMANN, I.M.S., who has been on plague duty in Natal, since before the war, has been invalided to England, and has arrived in London.

LIEUTENANT-COLONEL FRENCH-MULLEN, I.M.S., has returned to Rajshahye as Civil Surgeon from Patna.

WE congratulate Captain B. H. Deare, I.M.S., the Civil Surgeon of Midnapur, on the donation of Rs. 5,000 from the Raja of Moisdal, for the erection of an operating room in the Civil Hospital, Midnapur.

RECENT issues of the *British Medical Journal* have contained interesting letters signed I. M. S., on the question of the Professorships in the Indian Medical Colleges. He hits the right nail on the head when he points out the example of the R. A. M. C. Unless prizes such as the Professorships are held out to I. M. S. men does any one imagine the Service would continue to attract the men it does from the schools? It is these Professorships and the Civil Surgeoncies which make the I. M. S. attractive to the House Surgeons and other young qualified men who enter it. Take them away and we shall be reduced to the state of the R. A. M. C. It is not the military side of the I. M. S. which brings in the good men to the Service. Professor Ogston in his much criticised address last year saw this clearly. The statements made about the withdrawal of Professors to military duties are simply deliberate misstatements. Such officers are especially exempt from such transfers.

If the War Office are only able to offer "the pay of a civilian on contract rates" to retired Indian Medical Officers who may wish to act for R. A. M. C. men absent on War Service, they will not attract many candidates.