In attempting to explain cases in which results obtained by dorsal percussion do not correspond with those of radioscopy, he seems to us to give insufficient prominence to the fact that in the former, information as to the immediately underlying lung is obtained, while in the latter the shadow thrown by fluid in front of, mingles with that of fluid behind, the lung, and the total result is a combination or average of the two.

C. L.

Correspondence.

IRRIGATION IN CATARACT EXTRACTION.

To the Editor of "THE INDIAN MEDICAL GAZETTE."

DEAR SIR,—I have read with deep interest Major Elliot's article in the June "Gazette" on "Irrigation Cataract Extraction" and his criticism of my article describing a new syringe for "Intra-Capsular Irrigation" which appeared in the April number.

I can endorse practically every thing Major Elliot says on the value and efficiency of irrigation in the removal of cortex, replacement of iris, etc., after extraction of the lens. Regarding irrigation in very nervous patients, I desire to say that the more I resort to irrigation the more I feel free to use it in such cases. Yet I would scarcely think it safe to advise those whose experience is comparatively limited in cataract extraction to make a free use of irrigation in this class of cases. For it has happened in my own hands, and I have seen it happen more than once in the hands of my associates, that with the syringe in the anterior chamber and a sudden upward motion of the eyeball has caused rupture of the posterior capsule and hyaloid membrane followed by escape of vitreous. If this accident occurs before all the cortex has been gotten rid of, iritis is practically certain to follow, especially if the cortex is of a stringy consistency and considerable has been left behind. My reference to "fiddling" of course could not apply to operators with the experience and skill of Major Elliot. My suggestion was merely to safeguard an excellent procedure in the hands of the comparatively inexperienced by suggesting by their doing too little rather than to attempt too much at first. Descemetitis, I am well aware, not infrequently results from too small an incision and forceful expression of the lens in cases where irrigation is not resorted to at all; that it also frequently occurs as a result of the syringing *per se* I am perfectly certain.

As I have said I personally had but one case in about four hundred (now over 600) in which the Descemititis after syringing failed to clear up, but I have two other cases in the hands of others where prolonged irrigation cause a Descemetitis which after several weeks showed no tendency to clear. I think, therefore, that it is better to leave a tag or two of stringy adherent cortex (for this is the kind most difficult to displace than to subject the anterior chamber to prolonged manipulation, especially if such cortex is eccentric and the major central portion has already been successfully removed.

With regard to the syringe itself, I think, it is largely a matter of what one is accustomed to. My syringe was suggested mainly to obviate the necessity of an extra assistant and to simplify irrigation generally. This I believe it does. Major Elliot speaks of the "double task" of watching the nozzle while squeezing the bulb in the use of my syringe. I would reply to this by saying that after one uses the syringe for a while he does not think of the bulb any more than he does of regulating the pressure by squeezing the clamp in the McKeown instrument, and the bulb has the advantage of serving as a very convenient handle for the instrument. The syringe has the additional advantage not mentioned in my former description of it, *viz.*, that it can be used as a suction apparatus as well as an irrigator. Often by releasing the pressure on the bulb a fragment of cortex can be dislocated by suction action, this proceeds and then washed out. Displaced iris too in the same way can often be reposited. I practically always use the syringe instead of spatula in the reposition of prolapsed iris and find it invariably satisfactory.

It saves the use of spatula or scoop for this purpose. Our plan of preparing the eye for operation has been practically the same for five years and differs but little from that employed by Major Elliot, excepting that we begin irrigation with bichlorid solution, 1 in 4000 swabbing the conjunctival sac with a cotton swab held in fingers (the hands having been sterilized as for major cutting operations) and complete the cleansing by washing out with sterile boric acid lotion. We use small rubber syringes immersed and kept in the bichlorid solution and used only for this purpose.

We seldom find it possible to keep patients with chronic conjunctival and lachrymal inflammation for the long periods mentioned by Major Elliot as in vogue in the Madras Ophthalmic Hospital.

I can heartily endorse what Major Elliot says about early examination of the eye following extraction. Our practice is to examine every case after 24 hours by raising the lid sufficiently to ascertain that all is going well.

Daily sterile dressings are employed for four days as rule, when an eye-shade is substituted. If there is any mucoid of mucopurulent discharge present at the first or subsequent dressings, drop of a twenty grain protargol solution is dropped into the inner angle of the eye at each dressing. Only dressings sterilized in an autoclave are used. The conjunctival sac is not irrigated excepted occasionally where the discharge is free.

At a later date we hope to present a series of cases in which irrigation has been employed, giving details and results. Only comparatively recently have we adopted a uniform method of tabulating operations and results in cataract operation with special reference to the irrigation.

We usually extract all hypermature and immature cataract in the capsule, and as we acquire additional experience our vitreous losses in such cases have considerably diminished, but as yet we do not see our way clear to adopt this method as a routine practice on all forms of cataract.

MIRAJ. W. J. WANLESS, M.D.

APPENDICITIS IN INDIANS.

To the Editor of "THE INDIAN MEDICAL GAZETTE."

SIR,—In the September number of the *Indian Medical Gazette*, Major Gabbett asks for the experience of Medical Officers on the question of appendicitis amongst natives during the last six months. I have operated on two such cases. Both were males, one being a sweeper, aged 24 years; the other, a military police Sepoy, aged 32 years, a Mahomedan. The first case was admitted with an abscess in the region of the appendix. The abscess was opened, and a few days later the sloughing remains of the appendix was irrigated out of the wound, the patient made a good recovery. The second case was one of chronic appendicitis, the man having been ill off and on with symptoms of mild appendicitis for two years. The appendix was cut down on, and found matted by old inflammatory adhesions behind the cæcum. The appendix itself was constricted near the cæcum, but dilated beyond and contained pus. After a little trouble the appendix was isolated and removed. The patient made a good recovery and was discharged, having lost all his former symptoms. Though I quite agree with Major Gabbett in thinking appendicitis a rare disease amongst natives, I fancy a good number of cases could be recorded from the experience of the large hospitals in India if an enquiry was made.

Yours, &c.,
MAYMYO. C. BARRY,
Major, I.M.S.

[We understand that a considerable number of operations for appendicitis are yearly done in Indian hospitals, but they are not always returned under this heading in the Hospital Annual Returns.—ED., *I. M. G.*]

PERMANGANATE TREATMENT OF HYDROPHOBIA.

To the Editor of "THE INDIAN MEDICAL GAZETTE."

SIR,—I have read with unabated interest Dr. McCabe Dallas's description of the successful treatment he had adopted to cure dogs bitten by other rabid dogs. I have no doubt that possibilities of the permanganate treatment are great, but there is one omission in Dr. Dallas's account which detracts much from the value of the treatment if it is meant to serve as a guide to the public; he omits the procedure he has adopted to secure bitten dogs, so as to render them perfectly harmless to handling them and thoroughly bathe them in the 1 per cent. solution, to search out all the wounds, and to rub in dry permanganate powder with a cloth button. If Dr. Dallas would be good enough to supply the above information through the medium of your Journal, he will not only confer a great boon on the public and help to solve a difficulty which now puzzles the medical and veterinary faculties.

I hope to write again on this subject for a further elucidation on certain points.

MADRAS: *22nd September* 1906. } Yours, &c.,
VITALIS LEWIS.

FISH, MOSQUITOES AND LIME.

To the Editor of "THE INDIAN MEDICAL GAZETTE."

SIR,—The more our knowledge is extended on the subject of the destruction of mosquitoes the better for all concerned, so I send you a few notes on an experiment I once made for the destruction of mosquitoes and their larvæ by fish and lime. If you should consider these notes of any value, I trust you

will give them publication in your Gazette for the benefit of the public, for I found my experiment to be thoroughly successful.

It is well known that in all tropical countries, stagnant water, wherever found, is a sure breeding place for mosquitoes. I once occupied a house in the Punjab which was infested by these pests. On searching for their breeding place, I found it in an open cistern which had been built to contain water for the garden. The cistern was about eight or nine feet long by five broad and five deep, and the water in it was quite brown from mosquito larvæ. To destroy these I placed in the cistern about fifty or sixty of the little silvery fish, named "Chilwa" (Cheela Argentea) which I netted in a neighbouring stream. In a week or so these fish had completely cleared the water of larvæ, and not only that, but every mosquito which sat on the water for the purpose of depositing its eggs was instantly devoured.

The Chilwa is a surface feeder, and is one of the most eager fly-takers in India. A mosquito hovering over the surface of the water even is jumped at and unerringly secured at a distance of an inch or so before it can settle.

This fish is easily procured all over India. In the Deccan, and in the south of India it is known by the name of "Roop-chál." If these little fish were introduced into patches of stagnant water which cannot easily be drained, and protected there would be an end to anopheles in that neighbourhood.

I made a further search in the same compound to discover, if possible, more breeding places. I soon found these in two rows of fifty water *gurrahs* which had been placed on each side of the house as fire buckets, for the house had a thatched roof.

A good handful of lime in each *gurrah*, well stirred up, not only immediately killed all of the mosquito larvæ, of which there were hundreds in each pot, but most effectually prevented the mosquitoes from using the *gurrahs* as breeding places again. After all this my house became quite free from mosquitoes.

I think the Chilwa fish should certainly be used as mosquito destroyers in the way I have described, but they should be protected from net men and anglers, for they are easily caught with the simplest tackle.

For the destruction of mosquito larvæ in *gurrahs*, fire buckets, and such like, a good handful of lime is more efficacious, instant in its action, and certainly cheaper than kerosene oil. The lime water would not readily evaporate, whereas kerosene oil is volatile, costly, and requires frequent renewing. In the case of fire buckets lime water would be harmless, but water mixed with even a small quantity of kerosene oil would probably increase the flames, instead of guarding them.

I was led to try the Chilwa experiment by having often seen these fish, which I know to be surface feeders, rising at, and taking mosquitoes, especially about sunset when these insects swarm, and with regard to the lime it is a matter of common knowledge that fish and minute animal life cannot exist for a moment in lime water.

Yours, &c.,
W. OSBORN,
Lieut.-General, I.A.

KULU (PUNJAB):
September 29th, 1906.

ABSCESS OF SCROTUM MISTAKEN FOR STRANGULATED HERNIA.

To the Editor of "THE INDIAN MEDICAL GAZETTE."

SIR,—Seeing Major Gabbett's record of such a case in the *Indian Medical Gazette* for August 1906, I thought the following similar case might be of interest:—

Patient, aged 48, admitted to Neyoor Mission Hospital, March 27th, 1906.

History.—He said that twelve years ago he noticed something coming down into his scrotum and going up into his abdomen. Native treatment failing to give relief, he came to this hospital three years ago, and refusing operation got a truss, which relieved him till it got out of order. Twenty days ago the swelling came down but never returned, and the pain has increased.

Examination.—He can only walk with the aid of a stick and with much pain. Right side of scrotum is swollen to about the size of a mango fruit extending up to inguinal canal, hard and tender to the touch. Tongue very furred, patient being ignorant and dull, history of bowels and vomit indefinite. Pulse 90 and small in volume. Temperature normal.

Operation.—Chloroformed and semi-circular incision made over neck of the swelling, the subcutaneous tissues, were dense and fibrous and parts not normal, incision was deepened cautiously till pus exuded. Finger inserted found an abscess cavity reaching down into scrotum and up to inguinal canal where the parts were firmly matted together and the opening closed so that no entrance could be gained to abdominal cavity; there was also a passage inward to the root of the penis. The cavity was scraped with the finger, flushed out, two drainage tubes inserted and the incision partly closed and gauze stuffed around the tubes. Dressed as usual.

After treatment.—Discharged in four weeks with a small sinus which was daily attended to in the out-patient department until healed.

Remarks.—The fibrous formation at the upper end of the abscess had performed a kind of radical cure of the hernia, tightly shutting off the abdominal cavity, and preventing the bowel from coming down. Careful questioning of patient and friends after the operation elicited a history of intermittent fever with rigors 20 days before he came. He was seen in August last with the hernia apparently beginning again, but refused operation.

Yours, &c.,
WM. C. BENTALL,
L.R.C.P. & S.E.

LONDON MISSION HOSPITAL,
NEYOOR, TRAVANCORE.

THE LAST PLAGUE EPIDEMIC IN THE GIRIDIH SUB-DIVISION.

To the Editor of "THE INDIAN MEDICAL GAZETTE."

SIR,—With reference to my letter on the subject of rats and plague at Giridih in the June number of the *Indian Medical Gazette*, the editor was pleased to remark "We are afraid that few will follow Assistant-Surgeon S. L. Sircar in attributing the disappearance of plague to anything so vague as climatic influences. Something more definite is wanted."

I am again tempted to write on the subject by the publication of the admirable paper of Major S. Browning-Smith, I.M.S., on the spread of Plague in the special plague number of the *Indian Medical Gazette*. This has not only thrown a flood of light on the subject, clearing up the points which were obscure in my mind; but by the light of this paper, I can see that what seemed doubts and difficulties to me are really corroborative evidences in support of the conclusions of Major S. Browning-Smith, I.M.S. The sequence of events in the Giridih plague epidemic appears to be this.

Towards the end of July, Pachamba, a town situated about three miles from Giridih, was infected by a man carrying the infection from Calcutta. The epidemic began to spread throughout the place and the rats of the locality became infected. Naturally there was a panic and a large number of people within a short time evacuated the town. It appears that a large number of rats left the town of Pachamba about the time and came to Giridih. The evidence of the fact is that rats began to die in the town of Giridih, while plague was raging at Pachamba, but up to that time not a single case of human infection had occurred in the town of Giridih. Now how did it happen that rats remained with plague germs without communicating the disease to men? Because it was the season when the fleas decrease in number, and the rat fleas are the principal medium of communicating the disease from rats to men. Then, as I have shown in my previous paper, this curious thing happened that though the rat-fleas did not communicate the disease to men, several parts of the town became infected by men carrying the infection from Pachamba. With the advent of the season during which the fleas increase the plague spread throughout the town of Giridih in an epidemic form. The people began to leave the town and with them the rats. Then the town of Giridih gradually became rat-free and the epidemic ceased.

Now as to the evidence that Giridih became actually rat-free. The evidence is this and I think it to be a convincing one. Dr. W. Saise, the Superintendent of the railway colliery, began the rat-killing operation within the colliery area towards the end of November and about that time the Giridih Municipality also began the operation of rat-killing within municipality area. As far as I can remember the municipality with their utmost efforts was able to kill only 50 rats. After a time Dr. W. Saise consented to take charge of the operation of rat-killing in the town of Giridih. Though Dr. W. Saise was able to catch some thousands of rats within the colliery area, he was able to catch only a few hundreds within the municipal area. Out of the number of rats paid for by the municipality as being caught within the municipal area I know for certain, some to be brought from outside, even from some distant villages.

In my first article on the plague at Giridih, I think I was able to show that the rat-killing within the colliery area had not any decided effect in checking the spread of the plague, as compared with what happened at Pachamba or at Giridih. I believe that the experence of the authorities at Rangoon was the same. Now how can this happen? Are not rats and rat-fleas the principal factors in the dessemination of plague? Does it not therefore follow theoretically that the epidemic should be cut short by the destruction of a large number of rats? Yes, the contention is perfectly true. But though no rats were destroyed at Pachamba, and only a few at Giridih, yet these places became free from rats and also from plague in due course.

In conclusion, I may be allowed to point out that the following points in the article of Major S. Browning-Smith are supported by facts noticed in connexion with the late plague epidemic at Giridih, which have been published by me before the publication of his paper. The pages referred to