therefore be dangerous. Filters not being always trustworthy he recommended boiling the water. The lecturer did not conclude without a warning against the careless use of preserved foods.

THE ARMY JOURNAL OF THE BRITISH EMPIRE.

The proposal to issue a new military journal under War Office auspices, which has been set forth by direction of the Army Council in a special Army Order, has given rise to a good deal of comment but the idea strikes us as being a good one and well worthy of trial. Other nations—France and Germany, for example—possess official journals of the kind. The design of the publication is a bold and broad one—namely, to deal with subjects of a professional and scientific nature, to encourage the study of military science and history, and to circulate information likely to promote a knowledge of the principles of imperial defence amongst all ranks and all arms of the military forces of the Crown. Whilst upon this subject we may say that the numbers of the *Journal of the Royal Army Medical Corps* for March and April show that the journal is growing in scientific and general interest.

THE WAR IN THE FAR EAST.

Press reports state that many of the Japanese troops in Korea are suffering from symptoms resembling those of beri-beri. This disease has long been known to exist along the south-eastern coasts of Asia and in Japan. In THE LANCET of May 2nd, 1896, p. 1238, we gave some account of an outbreak which occurred in the year 1894 in the Fiji Islands, all the patients being Japanese labourers of whom about 300 were imported for the purpose of working on sugar plantations. The Japanese known to be attacked were 268 in number, of whom 65 died, and in February, 1895, the whole of the survivors were sent back to Japan. The disease was entirely confined to these immigrants, not spreading either to the native Fijians or to the imported Indian labourers. The medical officers of the Japanese army will no doubt do everything that may be possible to arrest the disease in the present circumstances.

THE ROYAL NAVY LIST.

The April issue of the "Royal Navy List," which is published by Messrs. Witherby and Co., 326, High Holborn, London, W.C., contains appointments, promotions, retirements, and such like details bearing date as late as March 26th and in the admirable "Current History of the Royal Navy," a comparatively new feature of the List, we notice that there is a reference to the lamentable accident to the submarine A 1. The war and meritorious services of officers of the Royal Navy, Royal Marines, and Royal Naval Reserve are arranged in a manner which admits of easy reference, not a small matter when it is remembered that the names of some 10,000 officers are dealt with. The book is full of information on all matters connected with our first line of defence.

MILITARY MEDICAL INTELLIGENCE.

While the Army Medical Service may not be disposed to regard with any favour the position recommended to be assigned to it in future by Lord Esher's Reconstruction Committee the proposal to attach an officer of the Royal Army Medical Corps, who will travel when necessary, to the section of the Director of Military Operations which deals with intelligence will be recognised by all as a necessary and valuable recommendation.

DEATHS IN THE SERVICES.

Surgeon-Colonel George Cochét Chesnaye, I M.S. (retired), at his residence at Bournemouth in his 67th year. He entered the service in 1859.

LITERARY INTELLIGENCE.—A treatise by Dr. R. R. Rentoul, entitled "Trachoma (Granular Conjunctivitis)" has just been issued by Messrs. Cornish and Sons, Lord-street, Liverpool.

THE ROYAL BRITISH NURSES' ASSOCIATION.—The fifteenth conversazione of this association will be held at the Portman Rooms, Baker-street, London, W., on Tuesday, April 26th, at 8 P.M. Princess Christian has signified her intention to be present. The band of the Grenadier Guards will play a selection of music. Tickets—members, 2s.; nurses (non-members), 2s. 6d.; guests, 5s.—can be obtained from the secretary, 10, Orchard-street, London, W.

Correspondence.

"Audi alteram partem."

THE USE OF HOT BOTTLES AFTER SURGICAL OPERATIONS.

To the Editors of THE LANCET.

SIRS,—I should like to call attention to a matter in connexion with the nursing of surgical cases which is of the highest importance. In directions to nurses, especially in laparotomies, a great point is made after the operation of packing the patient with hot bottles. No statement of the temperature of the hot water in the bottles is vouchsafed and in the absence of such grave risks are run. Bottles filled with water practically at the boiling point are placed in close contiguity to a semi-conscious patient and even the ordinary covering of a blanket may not suffice to protect the skin. The danger has been very forcibly brought home to me lately and inquiries amongst professional friends have resulted in my hearing of 17 cases of burns which have recently occurred from the practice. I thus feel sure that surgeons ought to be a great deal more explicit in their instructions regarding this detail of the treatment of their patients. I am, Sirs, yours faithfully,

Oxford, April 18th, 1904.

H. P. SYMONDS.

THE "N" RAYS AS A PROOF OF DEATH.

To the Editors of THE LANCET.

SIRS,—With reference to Dr. John Munro's letter suggesting that the absence of "n" rays might be used as a proof of death, allow me to point out that it seems advisable at present to draw a distinction between the rays under investigation by Blondlot and those discovered by Charpentier. The former are of interest to the physicist, being generated by the sun, Nernst electric lamps, steel under tension, &c.; while the latter, proceeding from living muscle and nerve under strain, are of physiological importance. I have experimented with muscle rays only and it is because their objective existence has lately been the subject of entirely negative experiments at Glasgow and elsewhere that I am asking you kindly to afford me space for this letter.

In order that the existence of muscle rays may be made evident in the most convincing manner the following conditions must be present:—1. A powerful generator of the rays. Presumably this will be a muscular man or animal. 2. A fluorescent screen the luminosity of which will increase and die away rapidly, so that the connexion between cause and effect can be easily noted. 3. A good receiver—i.e., vision naturally capable of detecting a small change of luminosity—used under conditions which render it as sensitive as possible. The common cause of bewildering results is the natural tendency of everyone to involuntarily open the eyes wider when expecting to see an increase which may not be forthcoming and therefore to interpolate false answers amongst real ones. I have by practice acquired some expertness in observation and shall be most pleased to give a demonstration of the objective existence of the rays to anyone who has failed in his experiments. I will ask him to bring up his hand and stretch the muscles of the wrist at uncertain intervals in a completely dark room behind a cardboard screen, made of two large sheets each one-sixteenth of an inch thick and separated by three-sixteenths of an inch air space. On the face of this the luminous disc will be fixed, and by observing it from a few feet in front I will undertake to tell him when he brings his hand up. If convinced there is no delusion about the matter I think the medical profession will take the investigation and application of "n" ray phenomena seriously in hand. I am, Sirs, yours faithfully,

Hatton-garden, E.C., April 18th, 1904.

LESLIE MILLER.

PS.—Since writing last night I have heard from someone who was present at Blondlot's demonstration of "n" rays before the French Physical Society on April 15th that he was able to show the effect of a coiled-up watch-spring and a stropped razor—i.e., metals under tension—as sources of the rays. I have just repeated this experiment with a powerful clock-spring, held in the jaws of a test-tube holder and waved behind my cardboard screen. The effect is more

evident than with strained muscles. The luminous strip unmistakeably brightens up even when the clock-spring is still some inches from it with the two thicknesses of card between. Professor Blondlot appears to have given a convincing demonstration.—L. M.

THE INTERNATIONAL CONGRESS OF SCHOOL HYGIENE.

To the Editors of THE LANCET.

SIRS,—In a note from your Berlin correspondent in THE LANCET of April 16th, p. 1086, I notice an announcement that the next International Congress of School Hygiene will be held in London under my presidency. Like all others interested in the subject, I am glad that the Congress has accepted the invitation to hold its next meeting in London in the first week of August, 1907, but although I have been appointed president of the International Committee the arrangements for the next meeting have been left very much in the hands of the English Committee and we hope to secure as president of the Congress at its next meeting some one of such position that even headmasters of public schools may be ready to work under him. The French delegates to the Congress, whilst expressing to me their regret that the next meeting should not have taken place in Paris, and whilst inviting the Congress to meet in Paris in 1910, have in the most courteous and kindly manner declared that they will do everything in their power to make the London meeting a success. The object of the Congress is one of vital importance, and I am sure that the English will collaborate not only with their French colleagues but with those from every country to render the next meeting in London a success, in forming bonds of friendship between the teachers of all lands, as well as forwarding the other objects for which the Congress has been established.

I am, Sirs, yours faithfully,

Stratford-place, W., April 20th, 1904.

LAUDER BRUNTON.

ON SYPHONAGE IN THE LARGE INTESTINE.

To the Editors of THE LANCET.

SIRS,—I am glad to find that Dr. A. Keith has examined my book and I must ask others interested in the subject to do the same before passing judgment on my views, for I cannot quite accept my critic's exposition of them and the space I can occupy here is insufficient to do them justice. 1. I think no one can deny that the large intestine with its short ascending limb and its long descending limb is (given the necessary rigidity of its walls) structurally a syphon and I proved it to be so with rubber tubing bent to reproduce its flexures. With glass I was hampered by the limitations of the glass bender and I had to be content with equivalent, instead of identical, curves. But I did more, I imitated the V colon and found, as I expected, that as long as its angle was higher than the outlet of the tube syphon action was unimpaired. As for the consistency of the contents, I do not claim syphon action unless these are "capable of flowing" (p. 41), but I succeeded perfectly well with thick treacle and other dense substances. 2. Dr. Keith is also mistaken in thinking that I claim that syphonage explains the peculiar course of the colon, for it is obvious that since syphonage cannot exist with the trunk horizontal, syphonic evolution could not have been a factor in producing this course. What I suggest is that man has only been able to maintain his existence in the erect posture because the colon happens to admit of syphonage. 3. I am not so foolish as to deny the existence of peristalsis in the human colon; all I affirm is that it is inadequate *per se* to effect propulsion of the contents of the ascending colon in the erect position. In animals it is not required to act in opposition to the force of gravitation. Further, I do not say that the arrangement of the muscular fibres is explained by syphonage but that it is consistent with it. 4. But the crux of the whole matter lies in the question, Is the resistance of the colon walls sufficient to counterbalance the intra abdominal pressure? It cannot be proved to be insufficient, because the human colon is never seen in its true taut condition. It is relaxed in the mortuary by death and on the operating table by anæsthetics, or, if these are not employed, by shock. On the other hand, although I

cannot prove the resistance to be sufficient I can bring forward much evidence in support of it. For instance, the daily morning evacuation is due to the resumption of the erect position in which alone syphonage is possible. If it were due to peristalsis it would follow the heaviest, or perhaps each, meal in the day. Morning diarrhoea is explained in the same way as is also the curious fact pointed out by Brunton that a morning saline will not act satisfactorily if the patient gets back into bed again. Syphonage is necessarily followed by a partial vacuum in the cæcum since the latter is emptied much more rapidly than it can be refilled. This explains the frequency with which the cæcum is found nearly empty (under peristalsis it would be fuller than any other part of the bowel) and it also explains the occurrence of ileo-cæcal intussusception.

Dr. Keith says that he cannot conceive how contraction of the tæniæ can increase rigidity except by shortening. It does so, as I explained, by the deepening and doubling in of the internal inter-saccular ridges. In the case of a transverse colon which measured *in situ* only ten inches, but which, when drawn out, measured 20 I found that each ridge projected inwards for half an inch, while its thickness, of course, was double that of the colon walls. The effect of this in preventing collapse must be very great and it is aided by the fulness of the circular blood-vessels and by the stiff tæniæ themselves which act like bridges from one full part of the bowel to another full part. Altogether I think it must be admitted that even the evidence given above points to the existence in life of sufficient rigidity to admit of syphonage. If more is required it will be found in the book.

I am, Sirs, yours faithfully,

April 16th, 1904.

R. W. LEFTWICH.

THE EVIDENCE FOR THE HETEROGENETIC ORIGIN OF BACTERIA.

To the Editors of THE LANCET

SIRS,—In what purports to be a review of my "Studies in Heterogenesis" in THE LANCET of April 16th, p. 1060, the writer tells your readers nothing as to the actual contents of my work, though he gives them very false notions as to its purport and as to the methods pursued in regard to the particular questions to which he refers. He attributes to me conclusions "which, if rightly established, must," he says, "of necessity cause the whole fabric of bacteriology, infection, and hygiene to fall upon, and to annihilate, the multitude of investigators who have helped to build upon the foundations laid by Pasteur and to cause the name of Pasteur himself to be erased from the roll of fame." This seems to me utter nonsense and as unjustifiable as it is nonsensical. Let your reviewer quote anything he can from my book which would in the least justify what he says. The above surprising statements are followed by this further remark: "To review this elaborate book at length would be but to attempt to fight over again the battle ably fought, and we believe won, by Darwin, Pasteur, and their disciples." I never knew that Darwin had ever written a line on bacteria or heterogenesis, and on consulting the very full index to the sixth edition of "The Origin of Species" my impression was confirmed. Neither of these words are to be found therein. Yet your reviewer says his work in this direction "is, or ought to be, a matter of common knowledge."

A little further on this very unscrupulous and careless writing is followed by a statement directly untrue in its bearing upon my Section XXIII. on "The Origin of Bacteria and their Allies by Heterogenesis." He says: "The antiquated methods which still find favour in attempts to re-establish the *de novo* origin and interchangeability of lowly forms of life find no place in modern bacteriological technique." But the work which I have recorded in the section indicated, dealing with the origin of these organisms, was conducted almost throughout under strict antiseptic precautions. I trust, therefore, that you will permit me to make two quotations from that section in order to show, what your reviewer has not done, the kind of evidence adduced, and to correct the very erroneous impression which he has disseminated.

As an illustration of the appearance of one of the bacterial allies within closed vegetable cells, which the doctrines of the day declare to be germless, I would cite (from p. 301) the following experiment. "A small new potato, after careful washing, was placed in a small tin with a very