

evident than with strained muscles. The luminous strip unmistakably 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 fault 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