actually present. I was guided to the operation solely by the presence of the distant tubular breathing and bronchophony of the quality I have attempted to describe.

It would be vain to claim an absolutely invariable significance for any group of physical signs found in the chest, but as far as my observations go these signs possess as constant a significance as can be predicated of any that we know. I have never failed to find abundant fluid in performing paracentesis where they were present; I can scarcely recall an instance in which an exploratory paracentesis yielded more than a few ounces of fluid unless they were to be heard. If these facts alone should be found to be of general application I shall, I think, need no further excuse for having invited a more systematic attention to the practice of direct auscultation than it obtains at present.

I am, Sirs, yours faithfully,

ISAMBARO OWEN, M.D. Cantab., F.R.C.P. Lond.
Sept. 29th, 1903.

THE TRANSMISSION OF YELLOW FEVER.

To the Editors of THE LANCET.

SIRS,—In THE LANCET of August 1st, p. 345, in the letter of your special correspondent in New York, mention is made of me as one of the physicians who are not entirely in accord with the conclusion of the United States Army Yellow Fever Board of 1900 that "the female mosquito of the species *stegomyia fasciata* is the only and exclusive conveyor of yellow fever from one person to another." What I consider scientifically vulnerable points in the series of experiments carried on by the Yellow Fever Commission of 1900 in the island of Cuba are the following, some of which your correspondent has quoted. The house in which the experiments were made was an artificial house, under completely abnormal conditions never to be found in a living Cuban house. It was more in the nature of an autoclave than a dwelling-place. This house was not located in the city of Havana, as it should have been, but outside in the country, nine kilometres away to the south west, and at about four kilometres from the sea coast, where the recognised dangerous sea air could not reach it. There was not due care in the selection of the non-immune persons with whom the infection experiments were made, probably some already immunes to yellow fever being among them. These individuals experimented with did not remain all day in that house but only went in there to sleep after dark and early in the morning came out of the house and with changed clothes remained all the rest of the day in the open air in tents placed at a distance. I call this unsteadiness, this ebbing and flowing of the experiments, a sort of Penelope's work. Besides, it is a well-known physiological law that during sleep the human organism is at its minimum of energy and activity, the nearest to death. The time selected to try the infection experiments, therefore, was precisely when the organism of those experimented with was the least liable to contract the disease. In spite of all those favourable circumstances the statistics presented (44·4 per cent.) are not, in my opinion, sufficiently convincing.

A few facts which demonstrate that hygiene, sanitary reforms, isolation of the sick patients, the absence of the large number of poorly nourished, poorly clad, and generally neglected Spanish soldiers, and the immigration department and observation hospital in connexion with it established by the United States Government have been more powerful factors in the eradication of yellow fever from the island of Cuba than the mere protection of sick patients from the bites of mosquitoes, are the following. In the city of Havana alone mosquitoes have been killed and crude petroleum has been used in their breeding places to extirpate yellow fever. In no other city or town of the island has this measure been put into practice and yet yellow fever has disappeared not only from Havana but equally as well from Santiago, Matanzas, Cienfuegos, Cárdenas, Sagua, Manzanillo, Gibara, and all the other cities and towns of the island, no matter how great the distance from Havana. With the American occupation of Cuba from 140,000 to 160,000 Spanish soldiers went immediately out of it and were sent back to their native country. The large number of the Spanish Government civil employés who every month used to go to Cuba, all of them non-immunes to yellow fever, do not go now any more to that island. This immense amount of human flesh, which constituted before the fuel that kept burning the funeral pile of yellow fever victims, has completely been put out of the island. In consequence of the cleanliness, the sanitary reforms, the

considerably better hygiene public and private, and the prophylactic measures instituted by the United States Government in Cuba and continued by the present Cuban Government, not only yellow fever, but also small-pox, both of which diseases were there formerly epidemic, have been eradicated and the general death-rate in each and every city all over the island has considerably decreased. The newly established immigration department and its stringent laws have also greatly contributed to the eradication of yellow fever in Cuba.

I am, Sirs, yours faithfully,

A. M. FERNANDEZ DE YBARRA, B.A., M.D.,
Medical Biographer of Christopher Columbus.

New York, August 15th, 1903.

THE MEDICAL REGISTER.

To the Editors of THE LANCET.

SIRS,—In the interests of the profession may I ask you to be good enough to draw attention in your next issue to the importance to practitioners of keeping their addresses correct in the Medical Register. The chief points to which attention should be called are:—

1. The Register should not be confused with the Medical Directory as is so often done. Changes should be notified to this office by practitioners themselves and they should be careful to notice that a postcard is received in acknowledgment.

2. The present system is to send a circular of inquiry to one-third of the practitioners on the Register residing in the United Kingdom, from whom no communication has been received during the past 12 months, in each successive year during the period when there is no election of Direct Representatives. This year circulars were sent to practitioners from A to G inclusive and any person within those letters who has not received a circular should communicate with this office at once.

3. Anyone who is doubtful if his address is correct should inquire at the office without delay.

In past years you have kindly done your best to assist me in my efforts to save practitioners from the grave inconvenience of finding that their names do not appear on the Register because their whereabouts cannot be ascertained and I hope, therefore, that you will continue to give us the benefit of your valuable assistance in the matter.

I am, Sirs, yours faithfully,

H. E. ALLEN, Registrar.

229, Oxford-street, London, W., Oct. 2nd, 1903.

THE PREVENTION OF HORSE-DUNG IN THE STREETS.

To the Editors of THE LANCET.

SIRS,—With all the hygienic improvements hitherto effected no attention seems to have been paid to the prevention of the streets of towns from being contaminated by the dung of carriage horses. It is true that a great deal of money is expended in sweeping up the dung but it is never effectual, for part of the fluid and semi-fluid constituents adhere to the ground and by decomposing, especially in warm weather, diffuse an offensive smell through the whole atmosphere which people have to breathe, whether in the streets or in their own houses. Add to this the nuisance of clouds of dust, consisting to a great extent of fine particles of dried dung laden with an abundance of microbes of various kinds, blown about and taken into the mouth and lungs at every breath or eaten up with the provisions exposed for sale in the shops.

All this cannot be without injurious effects upon the health of the inhabitants of a town and it might even be the means of disseminating certain diseases in man as well as in animals. It is no less objectionable from an æsthetic point of view. It being therefore undeniable that the existing state of things is a great nuisance and is put up with simply because no effectual remedy had hitherto been proposed, I have tried for some time to solve this sanitary problem in a radical manner and think I have succeeded.

I have constructed a simple apparatus which I call "an automatic dung trap" for carriage horses which entirely prevents any of the droppings and even the urine from falling on the roads. I inclose a photograph of a horse and carriage to illustrate the apparatus as applied for actual use. On slightly magnifying the parts become very clear. It consists of a suitable receiver or dung trap fixed to some convenient

part of the vehicle, as the axle of the front wheels for example, and of a shoot or collector of some flexible waterproof material, attached at one end to the breech strap or its equivalent and at the other end to an outlet tube extending from the top of the receiver in the direction of the animal's breech. The receiver or dung trap contains an open-topped drawer or removable lining, which can be withdrawn and replaced by its handle at the back of the receiver. The shoot or collector has an expanded trough-like shape near the breech just large enough to catch the dung, but narrows down to the diameter of the inlet tube. The inlet tube is pivoted to the top of the receiver in such a manner that the shoot will always adjust itself to extend in a straight line from the breech strap, notwithstanding the variations in the heights of different horses.

Whether the animal is standing or running, its dung (and its urine in the case of a mare) is caught by the shoot or collector and travels by gravity along the same into the removable drawer, which can be removed, emptied, and cleaned periodically. With regard to the urine of a male animal, if desired, a funnel-shaped receptacle may be arranged in the proper position to catch the urine and convey it by means of a rubber tube into the dung trap or separate receptacle. There is also a valvular arrangement at the inlet tube of the receiver to allow the dung and urine to pass freely into the receiver, but to prevent any smell coming out. The shoot and receiver are permanently fastened together and to the carriage. When a horse is put to the carriage, all that is required is to attach the shoot by means of clamps or straps and buckles, which can be done in a couple of minutes. It will be seen that, with the exception of the receiver which is placed quite out of the way, everything is soft and flexible, and as no part of it touches the horse there is nothing in the least to frighten the most timid animal.

The only possible objection that I can see is its unusual or novel appearance. A glance at the photograph will show that it is not very obtrusive or ugly, and I think, considering its great utility, people will soon get used to its appearance as they have to the splashboard or any other necessary part of a carriage. I am convinced that if this apparatus were universally adopted the streets of towns would become almost as clean and as wholesome as a garden walk, disease and mortality would diminish, and the comforts of town life would considerably increase. I do not enter into the economical question for that will at once suggest itself to municipal authorities, the enormous expense of scavenging the streets being necessitated mainly by horse dung.

I am, Sirs, yours faithfully,

J. A. CALANTARIENTS, M.D. Edin.

Scarborough, Sept. 29th, 1903.

* * The apparatus as shown in the photograph is not at all obtrusively noticeable.—ED. L.

WHAT IS SCIATICA?

To the Editors of THE LANCET.

SIRS,—Dr. W. Bruce, in his letter in THE LANCET of Sept. 12th, p. 785, shows that he has not quite grasped either the substance or the spirit of my letter of August 29th, p. 633. I, like many other general practitioners, scan weekly the columns of THE LANCET to keep myself acquainted early with any new light thrown on disease or injury, and a paper like Dr. Bruce's professing to revolutionise our conception of sciatica is well calculated to attract the attention of the general practitioner. Sciatica is a common and frequently an intractable complaint, and if Dr. Bruce has given us a clue to its more effectual treatment he is worthy of our gratitude.

Having read Dr. Bruce's paper I was prompted to give details of a case on which his theory had direct bearing. A man with double arthritis deformans affecting the hip-joints is a martyr to sciatica; he sustains an intracapsular fracture of the left femoral neck, union is not good, but the sciatica on that side is cured. Three years later he gets an intracapsular fracture of the right femoral neck and comes into my hands. He is treated with extension; union is fair and the sciatica is cured this side also. I ask if the sciatica was cured by the long rest or the long stretching of the sciatic nerve? I cannot believe that an intercapsular fracture with a few weeks' rest will cure old-standing arthritis deformans of a joint and if it will not, surely the cure must be attributed to the stretching.

I believe it is an accepted pathological doctrine that the group of toxins which produce an arthritis deformans may also produce a neuritis and the facts of the case seem to me to warrant the opinion that while the sciatica was no doubt in the first place excited by the joint disease there was an actual neuritis of the sciatic nerves which was benefited, as other cases of sciatica have been benefited, by stretching.

Dr. Bruce in his letter confesses that he may have put his case too strongly and thereby relieves me of the onus of suggesting that he has fallen into the common temptation of the man who has a new truth to tell, the temptation of overstating his case for the sake of emphasis. I may be wrong, but it seems to me also that he has not kept quite clear of partisanship in the strife between consulting physician and surgeon which prompts the one to declare that surgeons are prepared to use the knife for every complaint under the sun and the other to refer to the post-mortem room as the "physician's operating theatre." Therefore, to return to Dr. Bruce's use of the term "barbarous" as applied to an operation, I can only repeat that unless he has an alternative to offer he has no right himself to apply the term but should leave it to futurity. Dr. Bruce assumes, without any ground for the assumption beyond my not mentioning the fact specifically, that the first fracture in my case was not treated by extension. I did not think it necessary to mention its treatment, but as a matter of fact it was not treated by a prescription but by extension, as is customary, the bad result being ascribed to the man's constant restlessness from the sciatica in his other leg.

In concluding let me once more assure Dr. Bruce that his paper has provided me with most interesting and suggestive reading which I have no doubt will be of great use to me practically. I am not presuming to pit my knowledge and experience against his but have written purely from the point of view of the general practitioner, to which class of the profession his paper mostly appeals. Few of us care to undertake such operations as stretching of the sciatic nerves or, *a fortiori*, removal of the Gasserian ganglion, but the responsibility of sending the patient to the surgeon or the physician is ours. I think Dr. Bruce has on the whole scored a success for the physicians.

I am, Sirs, yours faithfully,

Hampstead, Sept. 21st, 1903.

J. S. MACKINTOSH.

THE PROBABLE ACTION OF CANCER PARASITES ON THE HUMAN BODY.

To the Editors of THE LANCET.

SIRS,—Virchow's artificial histological classification of tumours into homologous and heterologous growths has obscured the intellects of medical men with regard to cancerous and other malignant disease for a considerable time. But of late years there has been a tearing away from dogma and there is at present a strongly growing belief that malignant growths are caused by protozoal parasites. I believe that most of the evidence we at present possess, without conclusively proving this to be the case, strongly points that way. Allowing for the moment that cancer is a parasitic disease how can its peculiarities be explained? The most valuable theories are those which connect all the known facts of a subject. When all known facts will not fit in with a theory then it must be wrong. I think in the following all the known facts of cancerous diseases fit. The cancer parasites attack only living epithelial cells. Under favourable conditions they enter some of these cells, live with them symbiotically, dominate them, and stimulate them to increase and to multiply abnormally and thus is formed the primary tumour. Under the malign influence of the parasites the epithelial cells turn arrant traitors to their commonwealth and, like all renegades, become the bitterest foes of that to which they owe allegiance. After a time the dual semi-alien settlement sends forth colonies through the lymph streams which form other settlements wherever they can succeed in getting a foothold—secondary growths. The renegade epithelial cells and their guests prey on the tissues of the body and poison the sources of life by their combined excreta and lead to the destruction of the commonwealth—the human organism. The stroma of cancer is the results of the efforts made by the connective-tissue cells to combat the invaders after the latter have gained a lodgment. The species of parasites which cause sarcoma attack only the connective-tissue cells which