bloated, alcoholic, and flabby men, of thick build, with an already somewhat restricted throat capacity. Certain of the patients in whom I have witnessed the phenomenon have been great smokers and have had "irritable throats" and some degree of nasal obstruction. The procedure most likely to induce tongue retraction is either dilatation of the sphincter ani or some rectal manipulation or operation. I have, however, known it occur as the immediate result of a simple cutaneous incision. I have met with it under ether and under chloroform. It is certainly more common *before* the cornea has become insensitive than after, although owing to the lateness of the rectal reflexes to disappear it may take place even when full anæsthesia has been produced. That tongue retraction actually occurs may be proved by observing the organ to be drawn back and thus to arrest breathing at the moment the particular stimulus is applied. The patients most liable to the intense form of laryngeal spasm which culminates in complete arrest of breathing are healthy, florid, young women. The operations most likely to induce this spasm are abdominal operations in general and especially those in which the peritoneum is dragged upon. Thus, it is common in oöphorectomy, abdominal hysterectomy, &c. It is, curiously enough, far more common under chloroform than under ether and is liable to arise during moderate anæsthesia when the cornea is just sensitive to touch. Rectal operations will also induce laryngeal spasm but the "crowing" is of a different pitch from that produced by peritoneal traction. In moderately intense spasm—i.e., when some air still passes through the larynx—the patient's colour may be but slightly altered, but in the most intense forms with high-pitched stridor the organ may completely close and the only remedy short of laryngotomy is the forcible tongue traction described by Lord Lister which, as he very properly insists, operates reflexly in relaxing the spasm.

I have not entered into the above details with the object of insisting upon the frequency of reflex tongue retraction and reflex laryngeal spasm as factors in chloroform accidents. As a matter of fact, each of these two conditions is often present in a minor degree during surgical operations, the former giving evidence of its presence by stertor, the latter by stridor. It is only when partial or complete occlusion of the air tract has taken place that the symptoms become urgent; and unless the administrator has been watching the case carefully he may readily attribute the symptoms to an overdose of chloroform, or, worse than this, he may overlook the asphyxial factor, and, putting his finger upon an indifferent pulse, he may quote the case as one of "cardiac syncope" or "chloroform collapse."

I am, Sirs, yours faithfully,

FREDERIC W. HEWITT.

Queen Anne-street, W., Dec. 20th, 1904.

To the Editors of THE LANCET.

SIRS,—At a recent meeting of the Royal Medical and Chirurgical Society I expressed my belief that the "importance of administering known proportions of chloroform was over-estimated," but as the exigencies of time did not allow of my giving my reasons for coming to this conclusion and as some very emphatic opinions in favour of "dosage" were expressed, I should like to be permitted to say a few more words upon the subject. In its ordinary medical sense the term "dose" is used to indicate the quantity of a drug which the patient actually gets, but this does not seem to be the way in which the term is understood by all the advocates of the new inhalers. For instance, it has been suggested that not to use a percentage-regulating machine was like giving a man a "dose" of liquor strychninæ without measuring it. This view is surely based upon a misconception of the possibilities of the new machines. These instruments, in their present forms at any rate, cannot, as far as I understand them, do more than attempt to standardise and to regulate the strength of the vapour which it is possible to supply; the exact volume of this standardised vapour—i.e., the dose—which the patient actually gets must surely depend upon his lung capacity, vigour of respiration, and other variable factors.

I am under the impression that the advocates of the exhaust or vacuum form of apparatus (Vernon Harcourt and Levy) maintain that by making the vigour of the respirations a controlling element in the working of their machines they have come within measurable distance of being able to regulate these variable factors, but this view seems to be

repudiated by those who are in favour of the plenum or propulsion system (Dubois, Waller, and others). Assuming, however, that one or other, or both, of these methods give correct results, it appears to me that if the knowledge of the percentage strength of the vapour inhaled is to be of any real practical value we must also assume that the effect of the drug is in all people in exact proportion to the strength presented for inhalation, that there is no such thing as personal idiosyncrasy, and that there are no extraneous influences which can modify this simple reaction. The arguments brought forward in favour of these assumptions seem to me to be inadequate. They are for the most part based upon laboratory experience under conditions which do not tally with the peculiar clinical conditions which the anæsthetist is called upon to face. I do not think that the available clinical evidence with the new instruments touches the question of "dosage"; the results obtained would have been equally good if the experienced men by whom the machines have hitherto been used had merely used them as regulating inhalers, and had been quite ignorant of the exact strength of the vapour which the movements of the index represented. Whether we should use an elaborate machine merely to regulate the strength of the vapour, or whether it may not be better, as I believe, to rely upon the skill and care of the administrator are, however, points quite apart from the question of "dosage."—I am, Sirs, yours faithfully,

Dec. 19th, 1904.

J. FREDK. W. SILK.

THE MEDICINE OF ASSAM.

To the Editors of THE LANCET.

SIRS,—The medical men in this district have recently been asked to record on forms provided by the Government authorities any cases of cancer that may come under notice. This has reminded me that during an experience of several years in Assam I have never once diagnosed a case of cancer of any description, neither have I ever heard of such a case. My practice has comprised the supervision of the health of many thousands of coolies from different parts of India and natives of Assam and adjoining countries. This complete absence of a disease so common in other parts of the world I think is worthy of note, though, of course, this fact must be well known to any medical man who has practised in Assam.

Another affection which I think one would certainly expect to be common in a tropical country is also extremely rare in Assam. I allude to hepatic abscess. The absence of this is the more curious since a very large percentage of the sickness and deaths occurring in the coolie population is the result of dysentery. This disease out here does certainly not run such a definite course as happens in other countries, neither is the temperature typical, but the stools are quite characteristic and have all the appearance of those of true dysentery. A medical man who has practised in Assam for 30 thirty years told me that he has seen only four or five cases of hepatic abscess. Enteric fever, too, is no doubt extremely rare. Apart from the absence or rarity of the diseases I have briefly mentioned we are afflicted with all the usual complaints of other countries with the addition of a few special diseases peculiar to this part of the world. Among them I may mention "sore-foot" and kala-azar. Malaria and ankylostomiasis are the two main causes of sickness.

I am, Sirs, yours faithfully,

D. J. DRAKE, M.R.C.S. Eng., &c..

Medical Officer, Teypur Medical Association.

Thakurbarrie P.O., Assam, Nov. 19th, 1904.

"FUNCTIONAL" DISEASES AND THE PATHOLOGY OF CHOREA.

To the Editors of THE LANCET.

SIRS,—Dr. W. Howship Dickinson's article in THE LANCET of Dec. 10th, p. 1629, on certain diseases of the nervous system sometimes regarded as functional, admirably fulfils the purpose indicated by the author in his short *postscriptum* in that it compels us to take stock of our pathological and physiological knowledge in regard to those diseases with which it is mainly concerned. Besides this, if it is not an impertinence to say so, it is a communication marked by a certain charm of style which is not always to be found in scientific literature.

If I may be allowed to offer a few comments on the paper

I should like to say something about the use of the word "functional," as to the propriety of which Dr. Dickinson leaves us in some doubt. He admits that "it has its uses" and would apply it "provisionally to disorders of a temporary sort which have not yet displayed their morbid anatomy," but considers it "most obviously out of place when attached to such as persist unto death and produce it" (p. 1630). A little later he appears to sanction the definition of a functional disease as one "without a pathology, disturbance of action unattended with change of structure." Now, in all deference I wish to submit that this use of the word is too exclusively that of the morbid anatomist. That change of function implies a physical change of some sort is an axiom to which no exceptions should be allowed. But there are physical changes which cannot be appreciated by morbid anatomy alone, which is concerned only with such changes as can be demonstrated to the eye by some process of "cutting up" dead and diseased organs. A "functional" disease to the morbid anatomist is a purely negative conception. Pathology, however, is a kingdom of many provinces, among which morbid anatomy is but one, and though a large and important area is hardly at present so large or so important as that of pathological chemistry. From the point of view of the chemical changes which take place in the cells of the body during disease the term "functional" has a very positive meaning and I venture to think that this meaning—namely, a change in chemical action or structure—is the one which should occur to the mind whenever the term is employed. Far from impeding the advance of knowledge such a use of the word enables the mind to carry on its inquiries at the point where morbid anatomy leaves off and suggests investigations on new lines and by other methods.

Chorea offers, to my mind, an excellent example of functional disease, but *not* of a disease without a pathology. The morbid anatomy does not tell us much, as the lesions described may well be secondary or symptomatic and not causative in character. Even allowing that all cases of chorea are not due to the same cause, it seems most probable that many, if not most of them, are like tetanus due to a specific micro-organism—namely, the micrococcus rheumaticus described by Paine and Poynton. Like tetanus it is a disease characterised by muscular contractions, and though these contractions do not appear in so definite an order or in such regular degrees of intensity, yet the occurrence of hemichorea and chorea limited to certain groups of muscles indicates the definite involvement of certain cells in the motor tract. Now in tetanus it is known that the toxin has a special affinity for certain motor cells and enters into so firm a combination with them that in some cases no recovery is possible. In rabies, too, the very elusive poison can be neutralised by an emulsion of brain cells and thus rendered harmless to susceptible animals. May it not be that in chorea certain motor nerve cells are similarly poisoned but that the chemical change is of a less stable character, so that dissociation and recovery are generally possible?

This is a view which has for some time been present in my mind and doubtless in the minds of many who have considered the question. It serves, I think, to illustrate the sense in which the term "functional" should be used in pathology, not as a pure negation but as expressing the positive idea of changes which are none the less physical in that they cannot be demonstrated by the ordinary methods of the dead-house or even by the more refined "cutting up" which is necessary for microscopical examination.

I am, Sirs, yours faithfully,

J. M. FORTESCUE-BRICKDALE, M.A., M.D. Oxon.

Clifton, Dec. 12th, 1904.

THE EVOLUTION OF MAN'S DIET.

To the Editors of THE LANCET.

SIRS,—In his kind reference to my papers on the Evolution of Man's Diet, recently published in THE LANCET, Dr. J. Sim Wallace (in THE LANCET of Dec. 17th, p. 1751) throws doubt on my contention that man "has lost the power of digesting in the unprepared state all but the very choicest varieties of wild vegetable food." He believes, on the contrary, "that man has it in his power to become able to digest or to cope with cellulose to all intents and purposes as efficiently as the anthropoid apes," and even hints that man and the apes differ but little in respect of digestive capacity. Without following Dr. Wallace in the *a priori* arguments he

advances in favour of this view, I will content myself with referring to the great biological fact that every species tends to alter in correspondence with alterations in its environment. Now, the diet of man and his homo-simian ancestors has been undergoing a steady change since anthropoid times—i.e., for many thousands of generations, and we may therefore safely conclude that the digestive capacities of man differ very decidedly from those of his anthropoid ancestors. Observation confirms this deduction. It would be quite impossible for man to subsist on the natural diet of any of the existing anthropoids. Much of the vegetable food which these latter consume is by reason of its natural density, to say nothing of its bitter and acrid taste, quite unfit for human consumption. This density is, of course, due to the presence of an abundance of tough cellulose, a fact which renders it highly probable that the anthropoid can cope with cellulose more efficiently than can man. Dr. Wallace appears to assume that the rôle of cellulose in the alimentary tract is a purely mechanical one, for he says, "if we consider the simple fact that cellulose is not digested chemically in either man or anthropoids practically all we have to consider is the ability to masticate food and to transmit it. Thus, all that is required is a more muscular intestine and especially a more efficient masticatory apparatus." I cannot, however, admit the proposition that "cellulose is not digested chemically in either man or anthropoids." It is true there is no evidence of the secretion either in man or the anthropoid, or indeed even in the herbivora, of a cellulose-dissolving ferment. There is, however, evidence that in all of them cellulose in its less dense forms is dissolved by bacterial agency, and it has been shown in the case of the herbivora that the solution is largely effected by means of a vegetable ferment developed from the vegetable food itself—a ferment identical with that which the living plant employs when a solution of cellulose is demanded—e.g., during the ripening of a fruit.

But while I am unable to accept all Dr. Wallace's deductions I am heartily with him in his main contention that man is capable of coping with much coarser vegetable foods than the pernicious paps and messes with which the stomach of the young human is nowadays so wantonly deluged. It is, as Dr. Stuart Tidey truly remarks,¹ largely in consequence of this system of dietary that the digestive tract of us moderns is so often "a pathological specimen from end to end," that "the buccal cavity exhibits a picture of ruin and decay the anal orifice is girt about with a ring of piles, while the intervening portion with its accessory organs affords a kaleidoscopic picture of pathological changes." If physicians could be got to realise the importance of providing the jaws, teeth, and the muscular coats of the digestive tract with adequate work an untold amount of disease and suffering would be averted.

I am, Sirs, yours faithfully,

Wimpole-street, W., Dec. 18th, 1904.

HARRY CAMPBELL.

TREATMENT OF ACUTE PNEUMONIA BY CONTINUOUS COUNTER-IRRITATION WITHOUT VESICATION.

To the Editors of THE LANCET.

SIRS,—The treatment of pneumonia by various forms of counter-irritation is of very old standing. Linseed-meal poultices with and without mustard, blistering fluid, iodine liniment, and similar preparations have been applied to the chest with success but with the great disadvantages of more or less severe vesication or some other drawback. The following simple method of applying counter-irritation without vesication has in my hands proved itself to be an excellent system of treatment. After making a thorough examination of the chest I sponge the affected side over with warm water, adding some alcohol (either rectified spirit of wine or whisky) which I always do to increase the irritation unless the skin be very tender or sensitive. Then I quickly apply thermogen or a capsicum pad which are much more easily applied and so do not exhaust the patient. The effect is to produce a dry glowing warmth and pain is relieved almost immediately. The application is never sticky or cold, it can easily be destroyed when required, and is perfectly cleanly. It is necessary only to replace it once or twice in the 24 hours, the medical attendant knowing that the patient's

¹ THE LANCET, Dec. 17th, p. 1749.