THE TREATMENT OF DYSENTERY.

To the Editors of THE LANCET.

SIRS,—I wish to bring before the profession the treatment of acute dysentery with full doses of ipecacuanha powder which has been deprived of emetine. I have used this powder in many cases of dysentery of late, and have been much pleased and gratified with the result, and, what is more, my patients have been saved the fearful nausea and vomiting and depression which have usually in my experience followed large doses of ipecacuanha given in the ordinary way, and which I have little doubt were caused by the emetine. The thought has been in my mind for the past ten or twelve years, but I have only now worked it out. Messrs. Symes and Co. of this town have fully carried out my idea, and I have no doubt that the same thing could be much more thoroughly accomplished on a large scale by the numerous manufacturing chemists at home. As far as I can ascertain it has been generally assumed that all the good effects of the powdered root were due to the large proportion of emetine which it contains (varying from 10 to 16 per cent.). This may be so, but nothing has as far as I have read been worked out about the therapeutic proportions of the other ingredients of the root. At all events, I have found by practical experience that the removal of the greater portion of the emetine does not take away from the power of ipecacuanha to cure dysentery. I have found that my powder causes little or no nausea, and that the patients are much more comfortable and not depressed at all, and that all the dysenteric symptoms rapidly disappear, the griping pain and tenesmus lessen rapidly, the stools become feculent and bilious, and the blood and mucus rapidly disappear. I would ask for an extended trial, and I hope that others will find it as successful as I have. Possibly some may say that the good effect may still be due to the minute quantities of emetine which have not been removed, and if it be found on further trial that ipecacuanha powder completely deprived of emetine is inert in dysentery, then it will be proper to give the pure emetia in fractional doses ($\frac{1}{16}$ to $\frac{1}{8}$ grain, according to Martindale), and combined in such a way as to obviate the nau eating effects without lessening its therapeutic effects. I would suggest a trial of the gallo-tannate of emetine in suitable cases of dysentery, as, according to Watts,¹ it is neither emetic nor poisonous. If, on the other hand, it is found by others that the curative principle *quâ* dysentery is not the emetine, then the sufferer from dysentery in India or elsewhere will not, as at present, have to undergo the misery of large doses of ipecacuanha in addition to the tortures of his disease. From personal experience I can say that there are few medicines which will produce so much misery and depression as forty- or sixty-grain doses of the ordinary ipecacuanha powder. I hope to communicate further details subsequently. At present I am sending as much of the prepared powder as I can get Messrs. Symes to make me to Calcutta to Surgeons-Major Birch, McConnell, and Crombie, to have it fully tried in the large European and native hospitals of that city.—I am, Sirs, yours obediently,

GEO. A. HARRIS, M.R.C.S., L.R.C.P. Lond.,

July 19th, 1890.

Civil Surgeon, Simla.

P.S.—I may add that the ipecacuanhic acid is at first abstracted, but subsequently remixed with the powder after the emetine has been removed. Is the sediment which is found in old samples of ipecacuanha wine an ipecacuanhate or gallo-tannate of emetine, and is that the reason why old specimens of vin. ipecac. are inert? I see that the writer of the article on Dysentery in Quain's "Dictionary of Medicine" throws doubt on the depression which large doses of ordinary pulv. ipecac. produce. I can only say that my experience, and that of others I have asked, is not in accordance with that of Dr. Ewart. I have found that cocaine will in some cases lessen the nausea produced by ordinary pulv. ipecac.

"INTERNATIONAL CONSUMPTION OF MEAT."

To the Editors of THE LANCET.

SIRS,—Your article on the amount of meat consumed by Australians has a wider interest than appears on the face of it. Speaking to an old Australian colonist the other day, he said they had "more hopefulness and go" in them

than the people of the old country, and he attributed this more to their eating more meat than to any other cause. The United States come next in the largeness of their consumption of meat, and we know the proverbial character for energy of the "Yankee." I believe you may almost gauge the character of a people by the amount of nitrogenous food it consumes. England stands well in this respect, as your article shows. Taking non-nitrogenous food, we in England consume 6 lb. of bread or its equivalent per week per head, France consumes 10 lb., Germany 11 lb., Spain 7 lb., Italy 8 lb., and Norway 6 lb. Take the physique of each nation, and I think we come out well. At least, this is my opinion after personal observation of them all. Those nations who consume large quantities of non-nitrogenous food are too fat—witness the French, Germans, and Italians. In treating great numbers of people for obesity, I have noticed how much lighter—in both senses of the word—and more active and energetic they become when non-nitrogenous diet is almost debarred. We need not go out of our own country to see the influence of food in daily life. Take the physique and energy of the meat-eating Yorkshire labourer and his Hampshire brother, who lives almost entirely on bread. They do not seem of the same nationality. To convince obese people what diet will do, I often instance the case of the horse out at grass—flabby, fat, and lazy, and the same animal put on corn, and in condition, strong, active, and high-spirited. This argument convinced an obese patient, but he was doubly convinced when he lost by dietetic means alone seventeen pounds in three weeks. I am writing to some medical friends in Australia to give me information as to the average duration of life there, and to inquire if the large amount of meat eaten there influences it or the health in general.

I am, Sirs, yours faithfully,

Aug 23rd, 1890.

N. E. YORKE-DAVIES.

To the Editors of THE LANCET.

SIRS,—In your annotation on the above interesting hygienic question, after mentioning that the consumption of meat per head in our Australasian colonies is at present about 276 lb., against 105 lb. in this country, and 48 lb. in Russia, you naturally inquire "whether the effect of this enormous consumption of meat affects in any degree the Australian mortality statistics." I have been accustomed to consider that the reason why the death-rate in New Zealand is so low as it is, is precisely because of the abundant supply of digestible animal albumens at the command of the population of that colony. Last year I find that the colony had a population of 650,000 and a death-rate of only 9.5 per 1000 inhabitants. Reducing this to a population similar in ages to those of our home population, the death-rate of New Zealand was still only 12 per 1000, against 19.5 in Great Britain. This I have hitherto attributed to the fact, not of the greater healthiness of New Zealand, for I do not believe any climate is more favourable to longevity than that of Great Britain, but to the fact that the 650,000 inhabitants of New Zealand possess 16,000,000 sheep, besides plenty of cattle and pigs, so that they are all well fed, which of course is not the case here or in any European country among the indigent classes. Indeed, I am surprised that so much time is taken up by inquirers on hygiene in getting rid of the *ledentia*, and so little thought expended in trying to secure the *adjuvantia* of human life—i.e., sufficient food for the different nations of Europe. As we cannot rapidly add to our food-supplies, it would seem rational to imitate the French, and not increase too fast.

I am, Sirs, your obedient servant,

London, Aug. 25th, 1890.

C. R. DRYSDALE, M.D.

"HERPES WITH TONSILLITIS."

To the Editors of THE LANCET.

SIRS,—The cases given by Dr. Collie under this head at p. 341 of THE LANCET for Aug. 16th point to a specific fever, and to a disease distinct from diphtheria, enteric or scarlet fever, excepting Case 4, which looks like a second attack of scarlet fever. The initial symptoms in these cases are those of most of the specific fevers, and the local lesion, not merely a follicular tonsillitis, increases as the fever declines. It is not directly, or at least not markedly, dependent upon personal contagion; and, as far as I have

¹ Dictionary of Chemistry, vol. ii., p. 485.

been able to follow cases of this kind, one attack is not protective against a recurrence; the herpetic eruption, though a frequent result of the attack, is not so constant or distinctive as to give its name to a disease that deserves recognition and independent study. It is now twenty-five years since the marked distinction of attacks of this kind from those of diphtheria struck me, and also their relationship both to febrile catarrh and to diarrhoea. How close may be their connexion with epidemic laryngitis and pneumonia has yet to be determined; so far the two latter seem to me rather allied to herpetic catarrh than to be identical with it. They may be seasonal forms of it, more prevalent in spring, as summer diarrhoea and winter colds are often the pulmonary and gastric forms of it. Cases of this herpetic fever, with stomatitis or follicular tonsillitis, were certainly frequent last autumn in London; one or two such, to my knowledge, were near being sent to the Asylums Board hospitals as cases of diphtheria, but remained at home without any ill result to other children in the same rooms. From Suffolk I have Mr. George E. Jeaffreson's account of cases of "herpetic fever" lately. In addition to eruption on the lips and throat, he mentions herpetic spots on the body and limbs, at first like the lenticular rash of enteric fever, though occurring earlier, and after the fever declines. At the Academy of Sciences at Paris, Drs. Verneuil and Germain Sée, on August 20th last, called attention to the increase of cases as above mentioned, and something like to diphtheria, since the epidemic of influenza. Even if these hold the position I have suggested in your columns and elsewhere, instead of their concurrence with cases of autumnal diarrhoea being a cause of alarm, they would be reassuring, as indicative that diarrhoea with us this season belongs to the milder form associated with Finkler's *Spirillum* and not to the Asiatic form, for which Koch claims his specific comma bacillus; and the two diseases are quite distinct and independent.

I am, Sirs, faithfully yours,

Harley-street, W., Aug. 25th, 1890. WILLIAM SQUIRE, M.D.

THE ALLEGED OUTBREAK OF ENTERIC FEVER NEAR SITTINGBOURNE.

To the Editors of THE LANCET.

SIRS,—In THE LANCET of the 23rd inst. you state: 1. "A serious outbreak of enteric fever has taken place at Murstow, near Sittingbourne." 2. "We assume that its cause has not been fully dealt with, since it is still spreading." 3. "The sanitary authority have a good isolation hospital, and a number of patients are stated to have already been received into it."

The following is the real state of things: 1. It is fallacious to suppose a serious outbreak (meaning an epidemic) has taken place, as only seven cases have been notified, and for the last fourteen days no fresh case has occurred. 2. Immediate action was taken as soon as the cause was discovered, which was not in the dwellings (which are in the most wholesome condition and receiving their drinking water from the Sittingbourne works), but away in the marsh where the men work, and so the increase of cases was stayed. 3. It is true we have a typical infectious hospital, and of the twenty-four beds, sixteen are full, but five or six patients are convalescent, and leave to-morrow. This hospital belongs, as it were, to the Milton rural, also the Sittingbourne and Milton several urban authorities, thus taking in the whole unions.

I hope you will correct your statement after my explanation.

I am, Sirs, yours faithfully,

HY. G. SUTTON, M.D.,

Sittingbourne, Aug. 27th, 1890.

Medical Officer of Health.

REVISION OF DIAGNOSIS IN NOTIFICATION CASES.

To the Editors of THE LANCET.

SIRS,—Referring to your article and correspondence respecting the Infectious Diseases Notification Act, 1889, it appears to me from the wording of the Act that the medical officer of health has no power to refer to the diagnosis of the medical practitioner notifying, nor the local authority to refuse payment of the fee on the plea of error of diagnosis, for the practitioner is bound under a penalty to notify that the patient is, in his opinion, suffering from the disease notified as soon as he forms that opinion, and certainly his opinion,

if honestly formed, is worth as much as, probably more than, that of the medical officer of health, or that of any other practitioner called into the case. Surely, if the diagnosis of the medical attendant notifying is to be challenged by the medical officer of health, there should be some impartial tribunal to whom doubtful cases should be referred. It is beside the mark for a medical officer of health to reject a diagnosis because he finds the patient on the day or the day after notification in the door of the dwelling-house, in the yard connected therewith, or in the playground without any symptoms to confirm the diagnosis, for it must be the experience of everyone in large general practice that cases of small-pox, diphtheria, scarlet fever, and typhoid are frequently brought to one's surgery while suffering from these diseases in all their stages, and that in many cases of scarlet fever there is nothing to show the existence of the disease after the fading of the rash until desquamation commences, which may be delayed for three weeks.

I am, Sirs, yours faithfully,

London, Aug. 23rd, 1890.

J. M. F.

HOMŒOPATHY.

To the Editors of THE LANCET.

SIRS,—The fact of your having admitted a letter from a homœopath into your columns may be taken as a proof that you do not utterly despise the claims of these practitioners to belong to our profession, and that you do not consider them otherwise than as honest, though irregular, professional men. In common with many others, I can record a broken friendship and losses of relatives, with others of my *clientèle*, because of a refusal to meet and to correspond with homœopaths. The case of the late Sir W. Fergusson was before me, and we can all remember that during and after the illness of the late Earl of Beaconsfield a well-known physician who had even ventured to be in the same room with an eminent homœopath, who has since recanted, received a series of underhand professional blows from which he perhaps suffers to this day. Should this be? The globule question has been strongly urged as a proof of dishonesty amongst these men, but were not their wholesale druggists answerable for these *placebos*? And can we all of us say we have never given a *placebo* in our lives? When a medical man becomes a homœopath is his name expunged from the Register, and is he expelled his College? I believe no such case is on record. Ought we to refuse to meet a practitioner whose name is upon the Register issued by the Council? Might not such a proceeding lead to an action at law? It seems that the time is coming when we shall meet them, and as they have abandoned, virtually, their small-dose theory there remains only the dogma of the *similia similibus* between us; we could write separate prescriptions, and leave our patient to choose his own remedial path. We can gather no light from the proceedings of our sister professions in their treatment of irregular practitioners, for in the case of the Church those who advance to the utmost extremity of religious belief are molested by a strong section of the public without; whilst those who descend to a denial of almost the first principles of their faith are persecuted from within. The lawyers seem to have no heresies, and although they have an etiquette at the Bar, yet it is washed out at home with all quietness. Should we cold shoulder homœopaths any longer? Let them be treated charitably, for they are a dying race, and their work has done us some good—they have been the pre-Raphaelites of medicine. That THE LANCET will let in more light is the hope of,

Mandeville-place, W., Aug. 18th, 1890.

FREDK. SIMMS.

THE REPUTED CHOLERA CASE IN LONDON.

To the Editors of THE LANCET.

SIRS,—In the reputed case of Asiatic cholera in THE LANCET of the 23rd inst., I observe it stated there was secretion of urine, also of bile "from the offensive evacuations." Now, in all the cases I have seen there was no secretion of urine or bile found in the rice-water evacuations. My observations are derived from being cholera district surgeon in 1842, and also being with the late Dr. Mackintosh in the Cholera Hospital in 1831, in Edinburgh.

I am, Sirs, yours truly,

Riversdale-road, N., Aug. 25th, 1890.

V. SINCLAIR, M.D.