Now I extend this theory to the cause of cholera, and employ either of the above means for its cure, and the uniform success of my treatment strengthens my confidence in the truth of my theory. I believe, then, the cause of cholera to be some material of an acid nature, or possessing acid properties, and that by virtue of this character, it acts so as to produce cholera, and destroy life. This acid can be neutralized by alkalies, or destroyed by stronger acids, even as uric acid can be neutralized by alkalies, or broken up by nitric acid. This theory I believe to be true, the facts upon which it is founded I know to be incontrovertible, and with regard to both, I would say, in the words of Horace, to those who hesitate to believe,

"Si quid novisti rectius istis,
Candidus imperti; si non, his utere necum."

The following list includes the principal modes of treatment hitherto recommended:—

Calomel, in large doses, and in smaller and frequent doses; calomel with opium; opium in large doses; alcoholic liquids; camphor and musk; acetate of lead; sulphate of copper; vegetable astringents; quinine; arsenic; iron; the saline plan, as recommended by Dr. Stevens; the tartarized-antimony plan, as recommended by Dr. Billing; emetics; the free administration of cold water and ice; the cold bath; the application of the wet sheet; injection of saline or other fluids into the veins; bleeding; electricity.

Calomel, according to my experience, both in large and small doses, has been totally worthless. Before the collapsed state other remedies are more efficient in arresting the progress of the disease, and in the collapsed state, when the process of absorption has ceased, calomel may be given in spoonfuls without any effect. I look upon the treatment of cholera in the early stages by calomel as only tampering with human life, whilst yet the disease can be arrested to a certainty by other means. Practitioners examine the alvine evacuations, and finding deficiency of bile, immediately throw in calomel, until the secretion of the liver is established, forgetting that the biliary secretion is dependent on the general state of health, and that the absence of bile is the consequence, and not the cause, of the disease. Calomel at the onset acts as a purgative, and relieves the stomach and bowels from any irritating matter which might tend to aggravate the diarrhoea; beyond that point it has no effect whatever on the disease. In London, calomel, given as a specific in all cases of diarrhoea, would, to the ill-fed and badly-clothed, and those residing in pestiferous localities, be a poison as destructive as the cholera itself. In the country its effects are not so deleterious because the people are stronger and better fed. A medical practitioner at Shoreditch told me that he had given calomel "until he was sick of it," without any perceptible effect. Children and young people are not so easily killed by calomel as the more advanced in age; nevertheless there is no reason to persevere in such a mode of treatment when there is no necessity for it.

Calomel and opium may be applicable in some cases in the early stages, but when the disease is bordering on the collapsed state, the remedy is useless.

Opium will allay the irritation of the bowels after they have been cleared by an antacid aperient draught.

Stimulants are serviceable according to circumstances; but there is no dependence to be placed on them as specifics in the disease, with the exception of ammonia.

The acetate of lead, the sulphate of copper, &c., are poisons, and totally useless in the cure of cholera. The same remark is applicable to quinine, iron, and arsenic.

The saline plan, as recommended by Dr. Stevens, is nothing more than an indirect application of a principle to destroy the acid products of the stomach, which can only be done by either alkaline or acid remedies.

I have seen the saline treatment tried in a favourable case without any effect. The young lady died in twelve hours.

Emetics act on the principle of clearing the *primæ viæ*; there is nothing in the plan beyond relieving the contents of the stomach, which might as well be done by an aperient draught.

The free administration of cold water and cold baths do more harm than good in nine cases out of ten. In the state of collapse, when the thirst is great, cold water may be given, but I have rarely seen any permanent good effects arise from it.

The injection of saline and other fluids into the veins only answers the purpose of those who aim at notoriety at any price. Bleeding is worse than useless.

Electricity has no influence over the disease. The case that was trumpeted in all the newspapers as having occurred at the Free Hospital I happened to witness. There was no collapse,

nor any urgent symptom requiring a peculiar mode of treatment.

In September, 1849, the Cholera Committee of the Royal College of Physicians sent a list of questions to the members. One of them was the following:—"Does it accord with your experience that cholera, in the stage of "serous" or watery diarrhoea, can with facility be checked? What means have you found most effectual in attaining this end?"

I have found no difficulty in arresting the serous or watery diarrhoea by either of the following plans of treatment. In the first place, I assume that the disease is dependent on acidities in the stomach, and treat it accordingly. According to the first method, a powder, containing a scruple of rhubarb, and the same quantity of the carbonate of magnesia, is invariably given by me before an attempt is made to arrest the diarrhoea. Without the above purgative I found I could not depend on the astringent mixture. I have been obliged to repeat the rhubarb and magnesia more than once in excessive purging before the diarrhoea could be arrested. Instead of hurrying the diarrhoea into cholera, as has been imagined, it is the only certain method of arresting the disease, since it clears the stomach and bowels from the morbid matter. After the powder has passed through the bowels I give the following mixture:—Powdered (prepared) chalk, two drachms and a half; sesquicarbonate of ammonia, sixteen grains; tincture of opium, half a drachm; compound tincture of cardamoms, three drachms; cinnamon-water, an ounce and a half; water, four ounces. Mix. Two tablespoonfuls after every motion.

It is important to give a dose of the mixture after every motion, otherwise the objects of arresting the diarrhoea by neutralizing acidities in the *primæ viæ*, and quieting the action of the bowels, will not succeed in equal ratio by giving the mixture every two or three hours. At the Islington Dispensary there were, in 1849, nine hundred registered cases of diarrhoea and incipient cholera, which were all treated on the above plan, without a single death. Not a grain of calomel was given. Within the last two months three thousand cases have been treated on the same principle at the Royal General Dispensary, without one failure. The success of this plan has been such that I am perfectly satisfied with it; and I have no hesitation in saying, that if the diarrhoea be treated as above, by antacids and astringents, or by mineral acids, there would be but few cases of death from cholera.

The second method of treatment is by the employment of the stronger acids in moderate doses. These, I believe, act by at once destroying the morbid material, and thus produce the same results as alkalies, which neutralize it; for this purpose two drachms of diluted sulphuric acid to six ounces of water, for a mixture, of which two teaspoonfuls to be given to children after every motion, or two tablespoonfuls in the same manner to adults, seldom fail of success.

Finally, I look upon the cause of cholera as a poison of an acid character, or capable of generating acids in the stomach, acting solely on the mucous membrane of the stomach and bowels, and if neutralized by carbon and carbonates, or destroyed by mineral acids, it will be rendered inert, just the same as any other poison by its antidote.

Keeping this theory in view, no one need be at a loss to treat cholera on a specific principle.

Finsbury-place, September, 1854.

ON A CASE OF POISONING BY CREWS'S DISINFECTING FLUID, ALIAS CHLORIDE OF ZINC.

By WILLIAM THORN, M.D. & L.S.A.

A LITTLE patient of mine, twenty months old, escaped from the nursery, and finding upon the stairs outside the door some fluid in a saucer, drank, as nearly as can be estimated, about a teaspoonful of the contents; within ten or fifteen minutes after he was most frightfully vomited and purged, became very cold, pale, and nearly pulseless, the pupils widely dilated, indeed life was ebbing fast. Being quickly summoned, I attended within twenty minutes, had him placed in a very hot bath, and having got him a little warmer, gave largely of dilute sulphuric acid and milk, until I supposed that I had converted the chloride into a sulphate, which is, I believe, decomposed and rendered inert by the milk. The purging ceased at once, but the vomiting continued for several hours, and my patient was in a very precarious state, until at length it was arrested by small repeated doses of cold sherry and water. Upon pressing the region of the stomach, next day, he winced very much.

This yielded to four grains of the chloride of mercury in divided doses, and the appetite and digestive powers recovered, by the use of soda, hydrocyanic acid, and tincture of cinchona, in three or four days. Had he not been a strong healthy child, I am sure he would have sank. I am not aware of the strength of Mr. Crews's fluid, but I should imagine it to be a concentrated solution from its great weight.

Harrow-road, 1854.

Reviews and Notices of Books.

Three Lectures on the Correlation of Psychology and Physiology. Delivered in the Chatham-street School of Medicine. By DANIEL NOBLE, M.D., &c. London. 1854. pp. 45.

THE first of Dr. Noble's Lectures is taken up with general remarks on the "Physiology of the Brain and Nervous System;" the second considers "Emotional Sensibility, and its Reactions;" and the third treats of "Ideas, and their Dynamic Influence." As these Lectures have already appeared in print, we need not dwell upon them further than to intimate their publication in a separate form, and to recommend their perusal by all engaged in the investigation of the relations of specific portions of the nervous centres to particular intellectual or emotional manifestations.

A Lecture on Respiration. Being the sixth of a series of plain and simple Lectures on the Education of Man. By THOMAS HOPLEY. London. 1854. pp. 86.

THIS seems generally well adapted to its purpose, though exception might be taken to particular and paradoxical views in it—*e. g.*: "It has yet to be solved whether towns may not be rendered even more salubrious than the country."—p. 63. We should like to see the solution in the affirmative, as it does not often fall to our own lot to be able to exclaim—

"To-morrow to fresh fields and pastures new."

Dissertatio Medica Inauguralis de Febre Flavâ a Contagione Vindicans, &c. &c., eruditorum examini subicit GULIELMUS MCGEE. Edinburgi. 1828.

THOUGH bearing the date of 1828, we have but lately received Dr. McGee's thesis, through the medium of the Secretary of the Royal Geographical Society of London. We presume it is wished to place on record at the present moment, —when so much argument is being carried on respecting the contagiousness of yellow fever—the early denial of its contagion by Dr. McGee, and also to point out where material will be found available to those who would support our author's view of the question. Knowing that useful information of an historic kind contained in inaugural dissertations is generally overlooked or forgotten, we will here place on record the author's opinion:—

"Ineunte jam anno 1820 ad Indiam occidentalem usque profectus sum: atque nullius theoriæ addictus observatione et experientiâ solis duci decreveram. Atque nunc post longam nec interruptam medicinæ inter tropicos exercitationem, palim profiteri nihil hæsito, nullam mihi unquam apparuisse causam cur febrim flavam contagiosam existimarem, nec eam sub quâlibet conditione contagiosam devenisse credo."—p. 16.

Contemporary Medical Literature.

FOREIGN BODY IN THE OESOPHAGUS; OPERATION; RECOVERY.

SINGHO NAIDE, a native of Colombo, aged forty, a fisherman by occupation, was taken into the pettah Hospital on the evening of the 12th of August, 1853, a fish, which he held between his teeth while baiting a hook, having slipped back into, and remained impacted in, the oesophagus. On examining the neck, it appeared swollen, with a feeling as if there were fluid in the cellular tissue about the muscles of the neck. In the fauces the tail of the fish was felt, and could be seen distinctly on depressing the tongue. The tail was inclined towards the left side of the throat, showing the direction the

fish had taken in its course down the oesophagus. Careful examination externally failed in discovering the situation of the fish, and it was found impracticable to withdraw it from the throat for reasons which will appear obvious when the fish is described. An incision was made between the anterior edge of the sterno-mastoid muscle and the trachea, commencing at the lower edge of the os hyoides, and extending down to the sternum. After a most diligent search, both by myself and my friends, nothing was discovered to indicate the spot where the gullet should be divided. The next step of the operation was conducted with great care. The passing of a male catheter was entrusted to Dr. Elliott, who, with no little difficulty, introduced it into the gullet, directed by his fingers, and turned the convex side of it towards the wound. This enabled the part to be seized with a pair of forceps, and a small opening to be made into the oesophagus. The finger introduced into this opening gave the feeling of something cartilaginous being lodged, which was soon found to be the edge of the fish. A polypus forceps was introduced, and attempts were made to extract it, but to no purpose, as the head of the fish was too smooth to be grasped by a polished instrument. A little manœuvre with the index-finger, however, soon dislodged the fish, which made its exit through the wound, head foremost. The fish was four inches and a half long from head to tail, and one inch and a half broad. It is named by Mr. Gray, in his "Illustrations of Indian Zoology," "Anabas Spinosus," and has long and sharp fins, both on the back and near the gills. About a week after the operation, a little nourishment was given through the mouth, but as some of it flowed out through the wound, it was deemed prudent not to repeat the attempt, but to continue nutriment through the rectum. In three or four days more the man was able to take nourishment by the mouth, from which time he began to gain flesh and strength. The wound healed gradually, and he was discharged quite cured on the 23rd of September, with merely a line of cicatrix on the side of the neck. The performance of the operation occupied more than an hour, and by lamplight.—Mr. P. D. ANTHONIESZ in *Ceylon Miscellany*, vol. i., No. 2.

POISON OF THE RATTLE-SNAKE.

There is good reason for the belief that its action is the same upon all living things, vegetables as well as animals. It is even just as fatal to the snake itself as to other animals, for Dr. Dearing informed me that one of his specimens, after being irritated and annoyed in its cage, in moving suddenly accidentally struck one of its fangs into its own body: it soon rolled over, and died as any other animal. Here then we have the remarkable, and perhaps unique fact, of a liquid secreted directly from the blood which proves deadly when introduced into the very source (the blood) from which it was derived.—Dr. BURKETT in *Pharmaceutical Journal*.

THE MEDICAL EXAMINERS OF THE EAST INDIA COMPANY.

To the Editor of THE LANCET.

SIR,—Having so ably and warmly advocated, as you have lately done, the cause of the Indian Medical Service at large, myself and all others, who either belong or have belonged to that service, are truly obliged for the independence and generosity of your conduct in this matter, as the class to which I and others here belong feel ourselves completely without a voice in the public press of this country, and altogether without sympathy for our cause, or a just appreciation of long and laborious services by those who compose the councils of the nation. It is time, I think, that we should bestir ourselves, in order to obtain a better and more just recognition by English authorities of our past laborious and zealous professional services in a most unhealthy country and overpowering climate. The valuable Medical and Topographical Reports of the Diseases and Stations of the Madras Army, published under the superintendence of the Medical Board of that Presidency; the no less valuable Transactions of the Medical Societies of Calcutta and Bombay; the elaborate Medical Reports of Cholera at the several Presidencies; and those, also, of the Systems of Medical Education carried out in the various Indian Colleges, are all evidences of the highest order of talent and zeal to be met with in the Indian Medical Service, but which are altogether lost sight of in distributing rewards for merit in this country. Sir Charles Wood's late appointments virtually deny any such title of Indian medical officers to reward.

Cheltenham, September, 1854. A RETIRED INDIAN SURGEON.