a diphtheritic and a tuberculous, or one disease manifested in different ways clinically, must be left an open question; but of the widespread prevalence of tuberculosis in the feathered tribe there can be now little doubt. An admirable article on this subject appeared in the *American Journal of Comparative Medicine and Surgery* for October, 1886, by Dr. Bland Sutton; it was entitled "Avian Tuberculosis."

I am, Sirs, your obedient servant,
Edinburgh, August, 1887. THOMAS WALLLEY.

THE ALLEGED CASE OF ASIATIC CHOLERA AT NEWTON-POPPLEFORD.

To the Editors of THE LANCET.

SIRS,—You will probably think the following case of sufficient importance for insertion in an early issue of your valuable journal.

At 7 A.M. on Saturday the 23rd ult., I was called on to visit John F—, aged sixty-seven, a small farmer residing at Newton-Poppleford in the parish of Aylesbeare, distant about nine miles from Exeter. I was well acquainted with the man, who was strong, and enjoyed uninterrupted good health. Formerly he had been rather intemperate, but of late years had been what would be called a "moderate drinker." On my arrival at 9.30 A.M., I was informed that, after a light supper of bread and cheese and a single glass of ale, he retired to rest at 9.30 (his usual hour) on the previous evening, apparently perfectly well. At 1.30 A.M. on Sunday (eight hours before my arrival) he was seized with sickness, which was speedily followed by copious relaxation of the bowels, and in about an hour cramp in the upper and lower extremities set in, and increased with the purging and vomiting until my arrival. No stools or vomica had been preserved. The former were described as "at first like dirty water and then light yellow," and the vomited matter as "thick and brown," except when he took tea or milk, which was ejected as soon as swallowed. I found him prostrate and restless; features pinched; eyes sunk; the whole surface of his body bore a marked leaden hue, covered with a cold, clammy sweat. His voice (which I heard, and which forcibly struck me as I ascended the stairs to his room) was completely changed into a whine or loud whisper. His respiration was difficult, and the breath and tongue quite cold. The toes and fingers—the skin of which was much pinched—were severely contracted during the cramps, and the muscles of the calf "quivered" during the absence of cramp. He complained of no griping in the bowels, but of great uneasiness at the epigastrium, severe thirst, and craving for cold water, which was allowed *ad libitum*, but instantly thrown up. Pulse small, thready, and rapid. Temperature in the mouth (taken with great difficulty, and not to be relied on) 98°. At 10.30 P.M. I again visited him, in company with my son (Mr. Bingley G. Pullin). The colour of the body was very livid, the eyes more sunk, the face more pinched, and the hands sodden, as if long immersed in water. Pulse not perceptible. Body covered with sweat of icy coldness. There was at this time a slight return of heat in the breath and tongue, and the purging and sickness had been less severe with the medicine, and he had dozed for a few minutes at a time, but he was now very prostrate and restless, rolling from side to side with severe cramps. Three stools had been reserved, which closely resembled thin gruel or rice-water. The patient stated that no urine had passed during the attack, but about one ounce was taken, which in his struggles was spilt and the opportunity to analyse lost. My son remained to ensure the necessary remedies being carried out. All, however, was of no use, and the patient succumbed at 2.30 A.M. on Sunday (twenty-five hours from commencement of attack). It was not until an hour before he died that stupor set in and his mind gave way. The contraction of the muscles of the calf continued for two hours after death, and during this period the temperature of the body had risen, but in three hours rigor mortis had powerfully set in. The treatment consisted in the first instance of three grains of calomel placed on the tongue, with ten minims of dilute sulphuric acid and ten minims of tincture of opium, every hour, and subsequently diffusible stimulants of ammonia and ether, with friction and chloroform liniment.

The dwelling was isolated and elevated, and both it and its surroundings were sanitarily satisfactory and the water

pure. No diarrhoea prevailed in the family or village, and none has appeared since. It will be asked, What was the cause of the attack? Was it the result of a "specific poison"? I leave wiser heads to determine this point. There was no premonitory stage. It manifested itself suddenly in a most virulent manner, and, as is usual, as far as my experience goes, such cases invariably prove fatal. That the intensity of the attack was determined by the heat of the day, I feel no doubt. During his work on the previous day he came into his house three times to shelter himself from the effects of the sun—quite an exceptional circumstance; and the temperature taken in the neighbourhood that day by one of the most reliable meteorologists (Dr. Radford) was 139° in the sun—a heat never registered higher in the neighbourhood before, and only once so high. I consider it to have been a case of Asiatic or malignant cholera, and to guide me in this opinion I have the advantage of the experience of the severe cholera epidemic of 1849, when I acted professionally under the then Government General Board of Health at Portsmouth, Plymouth, and Cornwall. No symptom that I witnessed during that epidemic was wanting in this case. A post-mortem examination was refused. I may mention that the coffin was saturated with Burnett's fluid, and the corpse wrapped in cloths saturated with Calvert's strong carbolic acid, and interred in forty-eight hours. The bed, bedding, and everything used during the illness I insisted on being burnt at once, the house powerfully fumigated, and the entire premises thoroughly cleansed.

I am, Sirs, yours faithfully,

THOS. H. S. PULLIN, M.D.,
Sidmouth, Aug. 23rd, 1887. Medical Officer of Health for Sidmouth.

"PAIN AND ITS INTERPRETATION."

To the Editors of THE LANCET.

SIRS,—With your indulgence I would offer a few words in reply to Dr. Collins' letter in your last issue. I am not quite sure how that letter should best be answered, but certainly the worst way would be to imitate its method. No good purpose can be promoted in that way. The burden of this letter is in two remarks. The first is that my statements concerning pain "are so strangely at variance with universal experience and common sense that it is incumbent upon anyone who has read them to controvert them." Now, my answer is that I have the best proof possible that Dr. Collins' knowledge is at fault concerning universal experience, and I fear uncharitable as regards common sense; for I have from utter strangers and from all parts of the country the most unqualifying encouragement to continue my papers on the Interpretation of Disease, and this last paper on Pain has been received most favourably. I have had many private letters concerning it; it has been quoted *verbatim* by several provincial papers, with very pertinent and encouraging comments; and what I esteemed perhaps more than all this was your own annotation in the same issue, which very distinctly asserted the beneficence of pain.

I trust Dr. Collins will credit me that I am not anxious to entertain any theory of pain that is wrong or unreasonable, but I must say that there is nothing in his letter to convince me that this beneficent view of pain is wrong. The assumption of universal experience and a private property in common sense is scarcely safe, and any *ipse dixit* based on such disposition is sure to be largely discounted. I must be held excusable, then, if I say I still adhere to every word of my paper. When Dr. Collins charges me with not excepting malignant diseases he does me an injustice. I did make distinct reference to these "apparent exceptions"; no person at all thinking of the matter could escape so doing. But he takes no notice of my attempt to reconcile these with the beneficence of pain. This is unfair and unfortunate; for as long as the philosophy of disease is treated in that manner, so long will our profession rest under an *opprobrium medicinae* worse even than cancer. I have nothing to do at present with the "wider sense" in which pain may be viewed. I am only concerned with the individual, and I am not afraid that anything determined right for the individual shall be found at variance with the wider signification of pain when that is adequately declared and understood. As to Dr. Collins' own rendering of the matter, which I honestly confess I did not understand, the

Daily News seems to me to have touched it off quite neatly and gently.

Surely this most important question can be rationally discussed so that some clear understanding may be come to. There are to my own knowledge not a few able men quite clear on the purpose of pain. They may have something to say.

I am, Sirs, your obedient servant,
Brockley, S.E., Aug. 22nd, 1887. H. CAMERON GILLIES.

CASE OF HYSTERICAL SPASM SIMULATING TETANY.

To the Editors of THE LANCET.

SIRS,—In connexion with the above subject, reported by Dr. F. F. Caiger in your issue of Aug. 20th, I think the following will be read with interest by many neurologists.

M. P.—, female, aged twelve years, a provisional pauper, came under my notice about the end of last June with symptoms of gastro-intestinal disturbance, produced—as is so commonly the case, especially at this season of the year—by taking food of bad quality and in insufficient quantity. She lives in one of the lowest parts of a provincial town, amidst bad sanitary surroundings. The house she lives in is an ordinary cottage, and, besides the family, accommodates one or more Irish lodgers, and is in a very dirty state. My patient is a bright, intelligent little girl, with fair hair and blue eyes, small and slightly made, and with a pinched face and subdued manner. There is no evidence of syphilis or worms, nor has she ever had any catamenial discharge. During the winter months she nursed a younger brother through an attack of scarlatina, and when in her usual health she does most of the housework. She attended the surgery from time to time, complaining of occasional vomiting, pain behind the sternum and in the abdomen, and constipation, and was treated with a mild saline purgative and a few drops of opium. She came to the surgery for the last time on August 8th, and on the 9th, about 7 P.M., I received a message to go at once, as she was “in fits.” I attended almost immediately, and when I opened the door the following scene met my gaze. On a bed under the window of the living room into which I entered, and close to a great fire where culinary operations were going on, lay my patient, surrounded by six or seven dirty women and two or three dirty men, each of whom was holding some different part of her body. She was quite still when I entered, and watched me approach her; but as soon as I touched her the following convulsion occurred. The depressors of the lower jaw contracted, opening the mouth to its fullest extent, the facial muscles attached round the mouth contracted, fully exposing the teeth in both jaws. As soon as this action was completed the post-cervical muscles commenced to contract, and drew back the head until the tip of the nose rested on the pillow. The wave of contraction then passed slowly down the spinal muscles; the thorax bulged forward, then the abdomen, and then the legs became rigid. Although the child is not more than four feet six inches in height, I think I shall be well within the mark in saying that the lumbar region rose twelve inches from the bed, and she rested solely on her forehead and heels. The legs were now drawn up, and then violently extended. This had the effect of forcing the face upwards against the iron bed-head, and had not her shoulders been raised into a semi-erect position, the face would have been seriously injured against the iron work (as happened once or twice to a slight extent). The spasm seemed now to attack the remaining muscles, and the consequence was that the whole body became rigid and straightened out, with the head thrown back. The spasm seemed now to pass off, leaving one side sooner than the other, thus producing pleurothotonos, with the concavity towards the left. The whole attack lasted two or three minutes, and was repeated several times during my visit, all being very much alike by varying in slight details. It was impossible to take the temperature, but I came to the conclusion that the case was hysteria, and—my suggestion that the case would be better treated in the workhouse hospital not meeting with any approval—I enjoined strict quiet in a cool room, from which sightseers were to be rigorously excluded, and left without ordering any other treatment. The next day, Wednesday, the 10th, I found that the convulsions had continued through

the night, and were, if anything, more severe than when I last saw her. I injected a quarter of a grain of morphia hypodermically, but found later in the day that it had had no effect in checking the convulsions. On Thursday, the 11th, I found her still in convulsions, and much exhausted; the convulsions, I was informed, had continued at intervals since my last visit. I ordered her a mixture containing five minims of tincture of Calabar bean and ten grains of bromide of potassium every two hours. On the 12th I was told that my patient took the mixture with great difficulty, and complained of its being very salt. From her general expression and semi-somnolent manner, I began to fear that, after, all I had to do with commencing meningitis, and, there being some constipation, I gave her two powders at intervals of three hours, each containing a little calomel and half a minim of croton oil, which operated promptly and without causing any disturbance. When I saw her in the evening, she was not convulsed, but pressure on the ovarian region induced a pretty severe convulsion. On Saturday, the 13th, I heard her screaming as I came up the street—“Oh, mother! Cut my legs off. Oh, my legs and my back! Mother, be quick!” As soon as she ceased to cry out she went into a convulsion similar to the one I have described. She appeared quite unconscious of what went on around her, and after giving orders for the mixture to be pushed I left. On Sunday, the 14th, I found her singing, shouting, and beating time with her arms and legs, heedless of all that went on around. I was told that she had slept well during the night, and awakened seemingly all right, but that on an attempt being made to give her the mixture the present state of things commenced. The attack passed off during my visit, giving me an opportunity for the first time of taking her pulse (80) and temperature (98° 8'). I now became satisfied that I had to do with hysteria, and thereon determined to stop the mixture and watch the progress of the case. Later on in the day, at the urgent request of her friends that I should give her something, I sent a mixture of burnt sugar and water, and was informed on my next visit, when I found her perfectly quiet and sensible, that it had done her a deal of good, and that she had not had a single attack since taking the first dose. I took this opportunity of informing her friends in her hearing that I declined having anything more to do with the case, and that if she had any more convulsions I should insist on her going to the workhouse. Since that time I have heard nothing of her.

I am, Sirs, yours faithfully,
Barnsley, Aug. 24th, 1887. EDWARD KERSHAW.

EIGHTEEN MONTHS' TRIAL OF CANTANI'S TREATMENT OF CONSUMPTION.

To the Editors of THE LANCET.

SIRS,—Your admirable leader on Bacterio-therapy in your number for July 23rd last deserves not only the attention but the careful study of medical men. It is a garden of valuable suggestion, and contains the rich fruits of extended experience.

When discussing the erysipelas streptococcus injections of Fehleisen and the bacterium termo inhalations of Cantani, you write “Cantani's method has proved a failure,” and then you give Dr. Pawlowsky's explanation of the cause of that failure—viz., “the confusion of many species of bacteria under the head of bacterium termo,” and you add, “also the reliance solely upon the antagonism of these two organisms, the bacterium termo and the tubercular bacillus, as sufficient to cause the destruction of the latter.” Pawlowsky's explanation and your additions are instructive and reasonable, but, these exceptions being removed, experience leads me to disagree with the conclusion that “Cantani's method has proved a failure.” In the previous part of your article I read: “On all sides we hear of bacillicidal methods, but the results are not commensurate with the faith that inspires them or the plausibility of the grounds on which their employment is based . . . and yet we by no means despair of a way being found out of the morass.” And you continue: “That way lies, we think, in affording to the organism support sufficient to cope with the bacterial hordes—in other words, to strengthen the powers of vital resistance.” Did I try to describe to you the system which I have pursued in the treatment of consumption for the last eighteen months, I could not adopt more suitable language