

the war between China and Japan, Brigade-Surgeon-Lieutenant-Colonel W. Taylor, now serving at headquarters in London, has been selected to join the Japanese Army, and Surgeon-Major James, already stationed at Hong-Kong, to join the Chinese Army.

The extraordinary rumours which have been afloat regarding extensions of age for the Army Medical Staff will shortly be disposed of by the appearance of some slight modifications of existing regulations.

Correspondence.

"Audi alteram partem."

"UNUSUAL CASE OF SCARLET FEVER."

To the Editors of THE LANCET.

SIRS,—If Mr. Kavanagh would refer to a paper "On the Features which Distinguish Epidemic Roseola from Measles and from Scarlet Fever,"¹ it would be seen that the "unusual case of scarlet fever" was a characteristic one of epidemic roseola.—I am, Sirs, your obedient servant,

Rugby, Sept. 15th, 1894.

CLEMENT DUKES.

"SCARLET FEVER AND RÖTHELN."

To the Editors of THE LANCET.

SIRS,—The interesting letters which followed the paper by Dr. Dukes on the above subject in THE LANCET of March 31st last will make this short account of an outbreak of an infectious fever, which occurred in a school, of interest to the readers of THE LANCET. The first case occurred two days after the reassembling of the school. The patient was seized with vomiting, headache, and much prostration, and the temperature rose on the first day to 104° F. On the second day the rash appeared; it was generally of the ordinary scarlet fever type, but on the inner side of the thighs it was morbilliform and of a brighter red; the glands were generally enlarged; the tongue was thickly coated, with bright red papillae appearing towards the sides; the throat was markedly inflamed and congested. The second case was precisely similar and was seen in consultation by Mr. Fairbank of Windsor, who without hesitation pronounced it to be scarlet fever. These cases were followed at intervals, which varied from ten to twelve days, by six other cases, all of which were of a milder type; the prodromal symptoms were mild, or almost absent; the throats were slightly congested; but in all cases the tongue was heavily coated, and the rashes were less scarlatinal in type, bearing greater resemblance in places to ordinary measles, but less papular. The desquamation was variable, lasting from four to five weeks, and being in some cases brawny and in others flaky. There were no sequelae, excepting that one or two of the boys had marked granular pharyngitis during convalescence. About three weeks after the occurrence of the last case the boys went home for their holiday, and amongst them was the son of Dr. Ridge Jones. This boy was noticed to be ailing a day or two after his arrival, and on the following day a rash appeared which his father thought to be septicaemic in character, but which other medical men, amongst whom was Dr. Cavafy of St. George's Hospital, pronounced to be that of scarlet fever. The boy was isolated and went through the ordinary peeling stage. It is obvious that all these cases were due to the same poison; the intermediate milder cases would never have been called scarlet fever by themselves, yet at the commencement and at the end of the epidemic there were cases to which no other name could be given.

I am, Sirs, yours truly,

Slough, Sept. 8th, 1894.

R. S. CHARSLEY.

THE TREATMENT OF DIPHTHERIA.

To the Editors of THE LANCET.

SIRS,—THE LANCET of Aug. 18th contained a very interesting article by Dr. Davison, recommending the treatment of diphtheria by the internal administration of soluble mercury salts, "as the sheet anchor." It is an interesting

subject especially in relation to the new antitoxine method. I have had a good deal of experience with this disease and would, therefore, beg a few lines of your columns. Diphtheria is one of the most uncertain diseases to speak about as the cases vary so much, and necessarily different men view cases differently. I am strongly of opinion that the only scientific treatment is by antiseptics. I doubt if Dr. Davison is right in advocating internal *versus* local treatment—as for five or six years I treated all cases locally with good results. (In fifteen years I have only had five deaths. The parish mortality during epidemics was forty a month or more.) Another argument against relying too much on internal treatment is, as Dr. Davison states, that if toxins are once formed in the blood the mercury is powerless, and yet he previously explains the virtue of the mercury by its chemical antidote action on the toxins, as demonstrated by Kitasato and Behring. Thus, by contradiction he shows that mercury is not an antidote. Then how does mercury act? Clearly as an antiseptic on the germs. But where are the germs? Not in the blood, according to present popular idea, for Löffler bacillus only grows on the surface of the membrane, does not penetrate it, and therefore cannot enter the blood. Now common sense at once dictates infallible methods if the surface-growing Löffler bacillus is the cause. Scrape off the superficial membrane, or apply strong caustic all over the membrane and over the whole throat, thus by removing the cause it stands to reason the case must recover. But is it so? Certainly not. Such methods are unquestionably wrong in treatment and result; hence the supposed cause is at fault—i.e., a surface-growing bacillus poisoning the blood with toxins prepared on the aerial surface of the membrane. Furthermore, no one has by inoculation of the Löffler bacillus at a remote part of the body produced membrane in the throat. Injection of the toxine of Löffler's bacillus often produces death from general wasting, and in some cases of rabbits leg weakness after diarrhoea, and in others paralysis. But we know so little of toxins that we may yet find the same results from other toxins. Grant that the Löffler antitoxine cures diphtheria, but it has recently been shown that the antitoxine of erysipelas does the same; so that is no argument. I infer that the action of internal antiseptics in diphtheria or any other disease is to destroy the causative organisms. But a well-recognised Löffler's bacillus is only a superficial fungus. Scientists now talk of mixed infection as an outlet of the difficulty, many having observed the continual presence of cocci in diphtherial membrane and in internal organs in fatal cases. Surely these cocci must have some play in the disease—they cannot be mere accidents. In early days I noted these along with the Löffler bacillus, and observed also that after local antiseptics the bacilli disappeared while the cocci persisted. Moreover, in making cultures I got the cocci even the first day by punctures to the base of the membrane. Thus I was led to make cultures of the cocci and inoculations under the wings of pigeons. I obtained the following results: In cases with death threatening the cocci were so virulent that the pigeons died in a few hours paralysed. At a later stage of the same cases or in rather milder types, the cocci produced after a day or so membrane in the throat, followed by paralysis and death. In still milder cases which recovered, paralysis afterwards, and in mild diphtheria no disturbance at all. These experiments (in which Dr. Enraght of Anerley helped me) were communicated to the Pathological Society, and they completely shook my faith in the "superficial" theory. I feel there can be no question that diphtheria is a constitutional disease of which the membrane is the local manifestation. If this be so, it is due to the diphthero-cocci, observed by so many, and not due to the surface bacilli of Löffler. But in treatment we have both cocci in the blood and cocci in the throat producing a membrane or slough such as only a caustic could produce. There is no question as to the local formation of diphtherial poison. Therefore it is wise and safe to treat the disease both constitutionally and locally. The principle which guides us in anthrax or malignant pustule should be pursued in diphtheria.

I am, Sirs, yours faithfully,

Leytonstone, Sept. 18th, 1894.

ALBERT WILSON, M.D.

"ANILINE IN PHTHISIS."

To the Editors of THE LANCET.

SIRS,—The letter of Dr. Seymour Taylor in THE LANCET of the 8th inst. on the administration of aniline in phthisis

¹ Churchill and Co.

induces me to ask you to be good enough to publish the following extract from a work by me which appeared in 1886:¹—"The affinity of the bacilli for certain aniline dyes, and the tenacity with which these are retained by them, even in the presence of an acid sufficient to drive the stain out of the other organisms and tissues, induced me to make the experiment of attempting to affect them in this manner in the tissues during life. I therefore administered fuchsin (rosanilin hydrochlorate) in pills to phthisical patients for prolonged periods, and in some with laryngeal phthisis added a spray of the same material, all without effect."

I am, Sirs, yours faithfully,

G. HUNTER MACKENZIE, M.D.

Edinburgh, Sept. 16th, 1894.

THE QUESTION OF DEATH CERTIFICATES.

To the Editors of THE LANCET.

SIRS,—I wish to elicit an opinion on the following case. I attended a Mrs. Hardt for fourteen days previous to and including the day of her death. For many years at short intervals I had attended herself and family. I certified the death as due to chronic alcoholism and cirrhosis of liver. The husband took the certificate to the registrar, who refused to give Mr. Hardt the burial certificate, but told him the case must be referred to the coroner. Mr. Hardt was of course very indignant and blamed me for not having written the certificate in such a form that the registrar could not receive it. I told him my certificate was the usual one in such cases and during the last sixteen years I had many times certified in the same way, and no objection had been raised. In spite of my protest, however, the certificate was sent on immediately to the coroner's officer. I saw the coroner's officer early the next morning, and it was arranged that Mr. Hardt should see the coroner himself with reference to the matter. The coroner, however, would not see him, or did not. The same day I saw the coroner myself and asked him as a personal favour if he would send on the certificate as there could be no necessity for an inquest. I understood him to say he would forward it on in the morning. He, however, summoned me to attend an inquest the next day. The statement I wished to make at the inquest was to the effect that having known Mr. Hardt for many years as a highly respectable man I regretted that the coroner had insisted upon holding an inquest on his wife, and thereby subjecting his family to a very painful ordeal and exposure. From the evidence of Mr. Hardt, as reported at the inquest, it would almost appear as if he desired an inquiry, but what he said really implied a pardonable resentment towards me, and a desire to clear his own reputation at all risks. The verdict was in accordance with the medical certificate, with the trifling exception that delirium tremens was tacked on, which my patient never had. What I wish to know is whether the registrar was justified in withholding the burial certificate and the coroner in ignoring the medical attendant and his certificate too.

I remain, yours faithfully,

FRED. W. BLACKWELL, L.R.C.P. EDIN., &C.

Commercial-road, E., Sept. 18th, 1894.

* * Without the facts supplied by both sides we do not feel in a position to make more than general comment. It is the usual practice undoubtedly for a death certificate given by a qualified practitioner to be registered without reference to the coroner. This official has very properly in his absolute control the decision as to whether an inquest shall be held or not. Speaking in broad terms, we would say that it is better that one inquiry too many rather than one too few should be held. Moreover, in a case of death from chronic alcoholism the annoyance to the friends caused by such procedure is preferable to being subjected to direct aspersion or innuendo implying possibly a question of responsibility as to the ultimate cause of the death.—ED. L.

"THE CREMATION OF INFECTED AIR."

To the Editors of THE LANCET.

SIRS,—In THE LANCET of Sept. 8th you have some valuable remarks on the cremation of air. Twenty-four years

ago, when a new wing was being built for the Geelong Hospital, I with difficulty persuaded the committee to include a male and female ward with bathrooms, attendants' rooms, and the usual annexes, for use as fever wards. These wards were immediately filled on completion. The ventilation arrangements were carried out by the architects under my direction in such a way that no air could escape without passing through fire. The wards filled with smoke would be cleared in a few minutes by the power. Though each ward was only calculated to receive six patients, according to the areas recommended by Parkes, I have treated as many as twenty cases in each ward during epidemics of the most malignant diphtheria, scarlet fever, measles, and typhoid fever, and in no case did the disease spread to any other ward in the institution. The idea was first suggested by the late D. B. Reid, M.D. F.R.S. Edin.

I am, Sirs, your obedient servant,

D. BOSWELL REID,

London, Sept. 18th, 1894. Late Senior Surgeon, Geelong Hospital.

"CHELSEA HOSPITAL FOR WOMEN."

To the Editors of THE LANCET.

SIRS,—May I be allowed to state that during the year 1893 I had two deaths in the Chelsea Hospital for Women after operations of all kinds. The two deaths resulted after curetting to remove placental débris. I have had no death in the hospital from any abdominal section since August, 1891.

I am, Sirs, your obedient servant,

Sept. 18th, 1894.

FANCOURT BARNES.

"THE HYDERABAD CHLOROFORM COMMISSION."

To the Editors of THE LANCET.

SIRS,—Surgeon-Lieutenant-Colonel Lawrie in his paper read before the Royal Medical and Chirurgical Society, as reported by THE LANCET,¹ said: "I think I have shown that, while it is possible by proper attention to the respiration to give chloroform with uniform safety, no man can do so who dreads heart failure and takes the pulse as a guide." In my brief paper I compressed this proposition into: "Chloroform is perfectly safe when the respiration alone is watched." I regret any essential discrepancy. I base my assertion that this proposition cannot be felt to be experimentally proved on the results of Drs. Gaskell and Shore, and on their criticisms of the tracings shown to the society. In view of these I think that the question of the direct action of chloroform on the heart must still be considered an open one, and consequently that heart failure may still have to be dreaded. Surgeon-Lieutenant-Colonel Lawrie challenges me to prove that a distinctly different proposition has not been confirmed by experiment. This highly technical negative is beyond my powers.

I am, Sirs, yours faithfully,

Hertford-street, W.

ARTHUR H. WARD.

THE ATMOSPHERIC CONDITIONS OF ELECTRICALLY LIGHTED LINERS.

To the Editors of THE LANCET.

SIRS,—Just at the completion of a nine days' voyage I wish to report that several passengers were severely affected with insomnia and an exhilaration to a very marked degree, and which were not relieved by the usual narcotics. These patients were of delicate, nervous temperament, and medical advisers and old sailor fellow-passengers assured them that their condition was caused by the exhilaration of the voyage salt water, &c; but to my mind the explanation is not satisfactory. I attribute it to an electrical condition generated, by the machinery itself or by the dynamos used in lighting the ship. I would suggest that a most thorough investigation be made in that field by competent specialists with a view to affording the nervous victims some alleviation.

I am, Sirs, yours faithfully,

St. Louis, Mo., U.S.A.

WM. N. MORRISON.

* * Continued proximity to the noise and vibration of machinery in motion may, no doubt, induce insomnia and similar conditions. It is necessary to speak guardedly of

¹ A Practical Treatise on the Sputum, p. 82. W. & A. K. Johnston, London and Edinburgh.

¹ THE LANCET, July 7th, 1894.