

ago I was of opinion that tubercular mammitis was comparatively infrequent. Of late years, whether from more accurate observation or from the fact that they have really become more common I know not, I have frequently met with them both in the living and dead animal. Perhaps the recital of some facts which have recently come under my notice may serve to direct the attention of sanitarians to this all-important subject. A few weeks since, while prosecuting my duties as veterinary inspector to the local authority of the city of Edinburgh, I was asked by one of my assistants to proceed to a street at a little distance from the cattle market to look at a cow that had been refused admittance by the market superintendent. I found a very good looking young cow, in good muscular condition, standing at the side of a street in a state of great exhaustion, and showing unmistakable signs of the existence of pulmonary and mammary tuberculous lesions; but the udder was overcharged with milk to such an extent as to lead to a very active overflow from every teat. What could I do? I had no power to seize the animal, nor could I prevent her from entering the market for the purposes of sale. On inquiry I found that the cow had been purchased at a public auction three weeks previous for £5, and after some consideration I decided to accept all responsibility attaching to the proceeding and ordered the cow to be taken to the abattoir. Post-mortem examination revealed one of the worst cases of pulmonary and mammary tuberculosis I have ever seen. Observe the sequel. Four minutes after I had sent this animal to the slaughter-house I found another cow in the market in an advanced stage of tuberculosis, giving, however, no milk; she was sold subsequently for 30s., and doubtless her flesh was chopped up for sausage-meat. About the same time I was asked by the market inspector to examine a large quantity of beef which had been seized in a sausage manufactory. A number of the pieces were liberally besprinkled with fibrous tubercle. Shortly afterwards I was walking through a yard devoted to the doubled purpose of a live and dead meat market, when I incidentally walked up to the carcase of a sow for the purpose of showing one of my young pupils the method usually adopted by butchers of disguising the carcasses of nursing sows—viz., by stripping the teats with the fingers. On turning the abdominal section aside I discovered that every one of the mammæ was thickly bestudded with miliary tubercles. On meeting the market inspector shortly afterwards, I advised him to seize the carcase. This was done, and when it was cut up for the purpose of boiling it down, it was found that the whole of the dorsal vertebræ were tuberculous.

To-day I saw a cow sold by public auction with unmistakable signs of pulmonary and mammary tuberculosis. I saw two other cows standing in another yard waiting their turn to be sold; and on inspecting a sale byre I observed a short-horn cow in whose system the usual evidences of pulmonary and mammary tuberculosis existed. On inquiry I was informed that she was kept to supply milk for the family of the owner, as she was not considered fit for sale purposes. These are facts which require no further comment from me, and which, I think, justify me in saying that it is time steps were taken with the object of doing away with a great public scandal and a greater public danger.

My friend, Dr. Littlejohn, medical officer of health for the City, is conversant with the particulars of some of the cases I have here cited, and I am very pleased to be able to acknowledge that at all times he is ready to extend a helping hand to me in my researches in this direction.

Before concluding, I should like to direct attention to what I consider several misleading (unintentional doubtless on his part) statements made by Dr. Handford in an article published in your journal of November 28th and December 5th. 1. Mr. Handford says, "As they [domestic animals] produce no sputum, no tubercle bacilli are set free from the respiratory tract"; further on he says that the "presence of tubercle bacilli has been demonstrated in the breath of phthisical patients." His first observation assumes that sputa must be present to produce infection; his second, that the expired air is infective. But taken for granted that infection usually takes place through the medium of sputa, I would ask by what or on whose authority he bases his first statement? If cows do not spit, in the ordinary acceptation of the term, they very largely expectorate when suffering from bronchial or bronchopneumonic affections; and even if they did not do this, there is frequently a copious discharge of a catarrhal nature in pulmonary tuberculosis from the nostrils; and further,

the matter from the bronchial tubes, trachea, and larynx not so expelled is swallowed, and contaminates the grass through the medium of the fæces when animals are at pasture. But, putting all this aside, it is a well-known fact amongst veterinary surgeons that pulmonary tuberculosis is spread amongst cattle through the medium of the expired air from the lungs of phthisical patients. 2. In speaking of tubercular mammitis in cows, Dr. Handford states that it is of "exceptional occurrence." Unfortunately, he is very wide of the mark in making this statement, and I am afraid he has not read the very excellent paper on this subject recently published in some of the veterinary periodicals from the pen of Professor Bouy of the Copenhagen Veterinary School. 3. Dr. Handford refers to improperly cooked tuberculous flesh as if it were comparatively innocuous. Are sausages, I may ask, always properly cooked? I have already stated that I have seen beef besprinkled with miliary tubercles cut up for the purpose of being converted into these delicacies.

I am, Sir, yours obediently,

THOMAS WALLEY.

Royal Veterinary College, Edinburgh, Dec. 8th, 1885.

THE NATURE OF PERITYPHLITIS.

To the Editor of THE LANCET.

SIR,—At the last meeting of the Clinical Society, Mr. R. J. Godlee read a paper on a case of Suppuration around the Vermiform Appendix, under the care of Dr. Barlow and himself, in which he had performed the daring operation of opening the abdomen and draining the peritoneal cavity, with a very successful result. The discussion which followed turned almost entirely on the surgical aspects of the case. Had there been time, I should like to have alluded to the subject from a medical standpoint.

The case, if I mistake not, is one of a not uncommon class, generally described as perityphlitis (sometimes typhlitis, or inflammation of the cæcum), occurring most often in young adults of fairly good health, not usually phthisical or strumous. The affection is not specially preceded by constipation; it is acutely febrile in its nature; it commonly terminates in recovery; and it is very apt to recur more than once in the same individual. When it has caused death, suppuration is found around some part of the cæcum, especially, however, about the appendix; and this tube is sometimes found perforated. In some cases foreign bodies, concretions, or "hardened fæces" have been found in the appendix.

Thus far I have stated facts, all, I believe, readily substantiated. The experience of most practitioners will bear out many of them, and the records scattered through the medical periodicals (many of which I have gleaned from the invaluable "Digest") supply further proof. I may particularly refer to Dr. S. Fenwick's lecture on Perforation of the Appendix, which appeared in THE LANCET of last year.

What, then, is the cause of this perityphlitis? Some would say, the lodgment of foreign bodies in the appendix or the retention of fæces. But, according to Dr. Fenwick's statistics, in much less than one-half (55 out of 125 cases, including 14 of "hardened fæces") of the cases of fatal perforation is there any evidence of such lodgment found. Moreover, as he admits, in numerous instances in the post-mortem room the appendix is found after death to contain shot, seeds, lumbrici, &c., without any ill results having been apparent. I did not gather that in Mr. Godlee's case he was able to feel any solid body in the appendix, although he explored it with the finger. This cause, then, if it exists at all, is insufficient to account entirely for the disease. Nor am I prepared to offer any complete theory to explain its nature. But I wish to offer a suggestion. The appendix is thickly studded with lymphatic follicles, such as occur scattered over the cæcum, and in smaller numbers in the rest of the colon. They are identical with the solitary glands of the small intestine, and of the same structure as the agminated glands and the tonsils. The suggestion that I have to make is that perityphlitis is strictly analogous to quinsy. Quinsy is generally agreed to be a suppurative inflammation occurring in the connective tissue upon or beside one tonsil; perityphlitis is such an inflammation in the immediate neighbourhood of the lymphatic follicles, which form a dense lining to the appendix. Both are affections of adolescence,

or young adult life; both occur without obvious cause in the fairly healthy, not phthisical; both are attended with acute fever; both usually tend to recovery; both are very apt to recur. I submit that there is strong presumptive evidence of an analogy between these two affections.

I am of course aware that if this is granted, and perityphlitis is dubbed "abdominal quinsy," we are still far from discerning its true nature and cause, since quinsy itself is as yet largely an enigma.—I am, Sir, yours truly,

Finsbury-circus, Dec. 15th, 1885.

R. HINGSTON FOX.

"DANGERS OF COCAINE."

To the Editor of THE LANCET.

SIR,—Your correspondent, Mr. Hodges, need scarcely have gone so far as Professor Hirschberg's clinic to learn the use of small quantities of corrosive sublimate, boracic, salicylic, carbolic, and other acids in preventing the development of fungoid growths in an aqueous solution of cocaine. Although these growths are objectionable, and tend ultimately to deteriorate the solution, I do not believe that they constitute dangers as regards suppuration. Dr. Bell Taylor's letter in THE LANCET of the 5th inst. would lead to the inference that there was some pathological relation between these fungi and the suppurative process; but, if there is no better evidence of it than that which he adduces, the danger of such solutions must be more imaginary than real. Are there any facts in pathology which warrant us in connecting suppuration with the development of vegetable fungi, such as are seen in these solutions? And do not these fungi, indeed, find a natural habitat in some antiseptic solutions, notably boracic acid? The quotation from Graefe's statistics, as Dr. Taylor quotes them, can have no scientific value. He says, "in one series he lost 5 per cent., in another 6 per cent., in another 4 per cent., in another 1 per cent.," but these are surely high figures if they represent losses from suppuration alone after cataract extraction. If they do not, of what value are the statistics at all as bearing upon this point? Surely a loss of 6 per cent. of cases from suppuration when using carbolic acid, the most powerful of all antiseptics, is an argument rather against antiseptics than in favour of corrosive sublimate. Dr. Taylor quotes de Wecker, and seems to adopt his views, "Que toute suppuration de la cornée est due à une infection de place"; nevertheless, we frequently see different forms of intraocular suppuration, which it would be difficult to assign to "une infection de place." Suppurative iritis, choroiditis, and hyalitis follow injuries unattended by breach of surface; why, then, should we always look for local infection in cases of suppuration in the cornea? In my private practice I have for a long time used solutions of cocaine mixed with some antiseptic, usually boracic acid. In the infirmary, until lately, I have not done so; but I am certain that, as regards suppuration after cataract extraction, it has not been influenced by differences in the solution, although in the case of the infirmary it was sometimes three or four weeks old. I am therefore sceptical of its dangers, and it will take more evidence before I can say "J'en suis absolument persuadé." If it be a danger, it is one which is not peculiar to cocaine; it must be extended to belladonna, atropia, eserine, opium, and all other lotions having a vegetable basis.

I am, Sir, yours truly,

C. S. JEAFFRESON,

Surgeon to the Northumberland and Durham Eye Infirmary.

Dec. 5th, 1885.

UNUSUAL VESICAL CALCULI.

To the Editor of THE LANCET.

SIR,—A few notes on some cases which have occurred in my practice at the Mayo Hospital in Jeypore may be of interest in connexion with your observations in THE LANCET of October 3rd on two unusually large urethral calculi, which were reported by a Russian surgeon.

In 1878, I removed a phosphatic stone, weighing $4\frac{1}{2}$ ounces, less 30 grains, from the prostatic portion of the urethra of a gardener, a Hindoo, aged forty years. It measured $2\frac{1}{2}$ in. by 2 in. by $1\frac{1}{2}$ in., and a small staff could be passed over it into the bladder with some difficulty. It was only necessary to cut down upon the calculus through the perineum, which it distended, and to turn it out from its bed with the fingers. The rectum was slightly injured, but

complete recovery took place in forty-three days. The patient suffered from frequent and painful erections, and had great pain in the testicles also, with hæmaturia. He had had much pain in coition, and for seven years there was no seminal discharge. The total duration of symptoms was twenty years.

In 1881, I had a curious case, in a boy aged fifteen years, who had suffered from symptoms of stone for three years. Three calculi, of the aggregate weight of 560 grains, were removed. No. 1, which was wholly in the penile portion of the urethra, fitted into a concavity in No. 2, which was in the prostatic part of the canal. No. 3 revolved in the lower part of No. 2, and projected back into the bladder. When admitted, the boy was much emaciated, but he was well in thirty-five days after the operation. His penis was unusually long.

In 1882, a prostatic calculus weighing 120 grains, and furnished with a curious hook, which projected into the urethra, was taken from a boy aged seven. He was discharged in a healthy condition in twenty days. He could only pass urine when on his hands and knees at the time of admission. His troubles began, as in so many cases here, after an attack of small-pox.

Two cases of a different nature may be added to the above, as they are of interest. Five ounces and a half of fragments of calculi were extracted from the bladder of a boy who had been in the habit of eating plaster from walls. His recovery was very rapid. A large stone, measuring $1\frac{1}{8}$ in. by $\frac{7}{8}$ in., was removed from a female by dilatation of the urethra, without any inconvenience ensuing.

I am, Sir, yours truly,

T. H. HENDLEY, Surg.-Maj., I.M.D.

Jeypore, Rajputana, India, October 26th, 1885.

URETHAN AS A HYPNOTIC IN CARDIAC DISEASE.

To the Editor of THE LANCET.

SIR,—I have felt, in common with most practitioners, the want of a hypnotic which may be given safely in the insomnia of cardiac disease, or of acute maladies like pneumonia, where the tendency to heart failure is a pressing danger. The numerous instances of death after chloral have sufficiently indicated its danger. I hoped paraldehyde might have proved of use, but in my hands it was a failure. Within the last week I have treated two cases of cardiac insomnia successfully by two-grain doses of urethan, given at bed-time, in solution in water. One of these was a case of aortic and mitral incompetence, with congestion of the lungs, hæmoptysis, pleural effusion, and œdema of the legs. The patient complained that as soon as he fell asleep he awoke with a dreadful feeling of suffocation, and for three nights he had had very little rest; but the last five nights he had slept comfortably by the aid of urethan, and his condition has generally improved in consequence. The other case is one of cardiac dilatation, with mitral incompetence; the heart's action is very feeble and irregular. The patient had not slept for several nights, but urethan has obtained for her comfortable sleep ever since she began to take it, four days ago. Urethan is a substance described by Kobart; its formula is $\text{NH}_4\text{CO}_2\text{C}_2\text{H}_5$; it was first used medically by von Jaksch of Vienna. He experimented with it first on animals, and found that no toxic effect occurred even when the drug was administered in the proportion of .5 gramme for each kilogramme of the animal's weight. He relates two cases, one of endocarditis and mitral insufficiency, and the other of aneurysm, in which very troublesome insomnia was relieved by this drug. The dose he employed was gr. 7 (.5 gramme), but so far two grains have been quite sufficient for my patients.

I am, Sir, yours obediently,

Birmingham, Dec. 8th, 1885.

ROBERT SAUNDBY, M.D.

QUININE IN PNEUMONIA.

To the Editor of THE LANCET.

SIR,—The following case illustrates well the value of quinine as an antipyretic in pneumonia.

I was called to see a boy of fifteen, overgrown, thin, and very delicate in appearance. He had pain in the right side of the chest. The respiration was hurried and the pulse 20. His temperature when I first saw him was 104° ; at a later