ASSOCIATION MEETINGS.

BRITISH LARYNGOLOGICAL AND RHINOLOGICAL ASSOCIATION.

Dr. SANDFORD, Vice-President, in the Chair.

March 20, 1891.

AFTERNOON MEETING.

AFTER the minutes of the last meeting had been read and confirmed, the Secretary read the statement of accounts, which was formally adopted.

Other business of a formal nature was transacted, comprising the election of Fellows of the Association.

Dr. SANDFORD (Vice-President) read the notes of the following *Case of Œsophagotomy in a Lunatic.* The case which I have the honour of bringing before the Association to-day is one which occurred in the Cork District Asylum, when the operation of œsophagotomy had to be performed for the removal of a large stone from the gullet of an inmate. I am indebted to the courtesy of Dr. Oscar Woods, Chief Medical Superintendent, and of Dr. Horace Townsend, Visiting Surgeon, for the privilege of doing so.

The patient, Patrick Conner, aged twenty-nine, was admitted January, 1887, suffering from dementia, and was subject to extremely violent attacks of maniacal excitement.

On February 18, 1891, he was reported unable to swallow. On passing the œsophageal tube, obstruction was experienced nine inches from the teeth. A hard substance could just be reached, but this resisted all efforts to dislodge it with ordinary instruments. The patient himself was extremely cheerful, and appeared to take a keen and half-amused interest in the proceedings.

It was determined to operate, and chloroform having been administered, it was thought advisable to make the incision at the right side, as a hard substance could be distinctly felt about an inch above the right sterno-clavicular articulation. An incision about four inches long was made, and the œsophagus reached by dissection between the trachea and large vessels. No vessel of importance was injured, but the omohyoid muscle was divided between ligatures.

The œsophagus was opened in the usual manner, and a large stone extracted, weighing 620 grains—being triangular in shape—the sides of which measured about one and three-quarters of an inch, and the thickness about three-quarters of an inch. The œsophageal wound was not sutured. The external wound was sutured with silver wire—a drainage tube having been inserted.

The temperature, the first day, reached 101·4°. The patient was fed

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by nutrient suppositories and enemata. This continued for four days, the temperature sinking to 99° and 99·5°, and the patient appeared to be doing well in every way.

During the night of the fourth day the patient was seized with maniacal excitement, and became very violent, requiring three attendants constantly to restrain him. Hypodermics of morphia had little effect. Enemata and suppositories were injected.

During the fifth day maniacal excitement continued, and milk and brandy, beef-tea, etc., were administered by a tube, but were immediately vomited through the mouth and wound. Extreme exhaustion set in towards evening, though mental excitement continued. Patient was fed through the œsophageal wound to avoid faucial irritation, and food was retained.

Sixth day. Exhaustion increased in an unaccountable manner. Temp., 99·5°; pulse, 120; respiration, 28. Patient was kept under the influence of morphia as much as possible. It was difficult to restrain him when conscious, but he was evidently sinking from prostration.

Seventh day. Patient died.

Post-mortem. The œsophageal wound showed slight tendency to heal. In the intestines eight stones were found of various sizes.

In reply to Mr. Lennox Browne, Dr. Sandford said he believed that this was the forty-seventh case on record since 1735. The patient evidently died of exhaustion due to the maniacal excitement.

Mr. MAYO COLLIER related a case of *Post-Diphtheritic Paralysis of Palate.* The patient was a coachman at Highgate, aged thirty-five, who two months before had had a slight sore throat, not sufficient to call for treatment. He subsequently developed symptoms of bronchial catarrh, for which he was admitted into the North-West London Hospital. A month later some peculiarity was remarked in his speech, and it was noticed that fluids regurgitated through the nose. He examined the man, and found there was complete paralysis of all the muscles of the soft palate. In addition to this, there was a discharge from the right ear, associated with absolute deafness on that side, and diminished power of hearing on the left side. Mr. Collier pointed out that the tensor palati muscle, according to Ross, was supplied by the sphenoid ganglion, but he had several cases on record in which disease of the intra-cranial portion of the facial nerve had been associated with complete paralysis of the levator palati. Other cases were on record in which an affection of the intra-cranial portion of the sphenoid ganglion had also caused complete paralysis of the levator palati, and a case recently operated upon by Mr. Victor Horsley, involving the removal of Meckel's ganglion, was unassociated with any affection of the palate, showing that this could not be the only nervous supply, and suggesting that the nerve supply might be derived from two sources. He insisted upon the fact that the muscles surrounding hollow tubes were not as a rule governed by intra-cranial centres, but derived their nerve supply from ganglia in their neighbourhood, and it was therefore probable that this was the case there. This would explain the state of the parts, complete paralysis due to an affection of the tube.

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Mr. LENNOX BROWNE observed that, independently of the anatomical considerations, they readily acquiesced in Mr. Collier's views. The clinical aspects of the case were full of interest, and the case was an instance of those cases of diphtheria in which the diagnosis was only arrived at in consequence of post-paralysis. He pointed out that in the last edition of his work on diseases of the throat, he had quoted a case in which he had diagnosed diphtheria of the anus to have occurred in the adult by the paralytic sequelæ, and from the fact that one of the patient's children had subsequently contracted the disease, presumably in the water-closet, and had died. That was a case in which there was grave doubt as to the nature of the trouble. The interesting point was the fact that this man was occupied in stables, and that Mr. Collier's patient was also employed in stables. It was a curious thing that so many cases of diphtheria occurred in persons whose occupations took them a good deal into stables. The same information had been given him by Hermann, of Capetown, that, although the sanitary conditions of that town were indifferent, yet that diphtheria was most frequently seen in the outlying districts where the cattle were close to the dwellings. In one case down at Hartproosher, in a school, the origin was clearly traced to an adjoining pigstye. There seemed to be a concensus of opinion in favour of Renshaw's opinion, that diphtheria was due to a combination of decomposing animal and vegetable refuse giving rise to the specific germ.

Dr. DUNDAS GRANT observed that, when the subject of diphtheria was brought forward, it was difficult not to take it beyond the narrow limits of the paper, particularly in view of the intense interest that the subject excited among medical men, and even more so among specialists in throat diseases. It was difficult to make up one's mind which of the suggestions which the case afforded was the best one to take up. There was at any rate one moral. They had been told that the disease had expressed itself so slightly that its serious nature was not recognised at the outset, and that justified the assumption that the local treatment usual in such cases was not carried out. The one case was not perhaps sufficient to prove anything, but they all knew that the type of case in which post-diphtheritic paralysis was most marked was precisely the type in which the disease had not expressed itself definitely in the earlier stages, and in which consequently no treatment was resorted to. It behoved them, therefore, to accept the treatment of even a case of ordinary sore throat with some sense of responsibility. He had formerly passed some years in general practice, and he attributed his success in the treatment of cases of diphtheria to his practice of careful examination of every case of sore throat, a practice which he had always insisted upon even at the risk of ridicule at the hands of those who were less expert with the laryngoscopic mirror. The early attention to local treatment in these cases was, in his opinion, the chief agent in enabling him to obtain such good results in diphtheria. He could not say that he had not lost a case or two, but he would affirm that the cases in which the sequelæ were most marked were those in which he had made a wrong diagnosis, cases in which he had committed the error, common to every man in practice, of taking a case

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of diphtheria for a simple case of tonsillitis. The moral of that was that local treatment ought to occupy the very first place in dealing with cases of diphtheria. There was of course a tendency to overvalue local treatment, but there were many who regarded diphtheria as a constitutional disease, to be treated exclusively by constitutional means. The truth, no doubt, lay between the two extremes, but he pointed out that, after all, the general condition was not one peculiar to this disease, but was common to all diseases of a suppurative kind, viz., septicæmia. Since his experience in this department had begun, the whole science of clinical bacteriology had sprung up, and this taught them beyond a doubt that there was a specific bacillus, which was to be found in the mucous membrane and subjacent structures in animals that had been inoculated for some time, and even upon the most superficial layers of the false membranes. Dr. Ruffer, in the "British Medical Journal," had spoken very definitely as to this, and that confirmed what he (Dr. Grant) had said as to the necessity for devoting themselves to the destruction of these bacilli, which were found on the surface of the mucous membrane. To go further back, if they read Donder's "Accommodation and Refraction," upon post-diphtheritic paralyses of the ciliary muscles, they would see, notwithstanding the rough and ready observations made with regard to the frequency of this lesion, how as soon as an energetic treatment of the throat was adopted, this sequela disappeared. He referred to the Milroy Lectures, by Dr. Thorne Thorne, and observed that it was evident from what had fallen from the speaker, that they were labouring under several delusions in respect of the etiology of diphtheria. It was astonishing how little it had to do with insanitary conditions. Statistics had been brought forward by Longstaff, in one of the most interesting books which it had been his lot to look through of late, the "Studies in Statistics," throwing a most extraordinary light upon this disease. It was made clear that the disease was not most frequent in the most populous districts, but on the contrary, in those only sparsely populated, where the conditions were least likely to enable insanitary conditions to affect the general health. Further, Dr. Thorne Thorne had quoted several outbreaks in which it was pretty clearly proved that there had been no origination of the disease *de novo*, but that the disease was due to infection by conditions which were called into activity from time to time. They would remember how this was gradually demonstrated in the case of typhoid fever; how cases which were supposed to be sporadic, proved to have been due to infection, of which he mentioned several instances. It was not always possible to trace the infection; no doubt the stables were to be blamed, but a great many of these cases appeared to be due to milk derived from certain cows suffering from what was apparently slight disease of the udders. It had been shown that the milk from these cows was capable of giving diphtheria to those who consumed it. Some investigations of Klein, to be shortly published, would show that the lymph from the udders of the cows would produce diphtheria in cats. These observations concerning cows seemed to him to give additional weight to the observations that had been made by Mr. Browne in reference to the outbreak at Capetown.

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Mr. MAYO COLLIER brought forward a case of ***Symmetrical Growths (syphilitic) on the Vocal Cords.*** The patient was a man, aged thirty, who had been a porter at the North-West London Hospital. He gave a well-marked history of syphilis, and several attacks of laryngeal catarrh. Within the last twelve months his voice had become somewhat hoarse, and on examining his throat he had found a well-marked fringe of warty growths on the superior part of one vocal cord, papillomatous excrescences due to syphilis, and to repeated attacks of catarrh. He subsequently removed them by the aid of forceps. He pointed out that on the first occasion of his examining the throat it was so extremely irritable that it was quite impossible to introduce a forceps even into the mouth, much less into the larynx, and cocaine remained absolutely without effect in diminishing the hyperæsthesia. Indeed that had invariably been his experience with cocaine in relieving spasm of the throat. It was vastly inferior to simple gargling with cold water, or the use of ice, or, still better, systematic training. He had day after day passed the forceps into his throat without attempting to touch any part of the larynx, and the ultimate result of the training had been that now one might, without exaggeration, allow his epiglottis to dangle in his trachea without his feeling it. Then he gave him large doses of iodide of potassium, and for some time he saw nothing more of him—for about three months, by which time he had quite lost his voice. He looked down his throat, and found that he had a mucous tubercle on the right vocal cord, and this he watched very carefully, ceasing the treatment in the meantime, and he found that the opposite aspect of the vocal cord became infected, and a similar growth presented itself upon it. The growth was at present absolutely symmetrical, and he had no doubt that this was due to infection by contact. He adopted no local treatment, but gave him perchloride of mercury and iodide of potassium, and these growths diminished almost to disappearance. The patient then discontinued his attendance, and since then they had begun to grow again. He was anxious to retain this case in ***statu quo*** in order to try an instrument which he had designed for the purpose of removing growths from the larynx. It was of course sometimes necessary to operate without any preliminary training, and his instrument, which he proposed to call the intubating cage, was like a rectal speculum, with three springs shutting up to the size of an ordinary lead pencil. When the instrument was perfected he intended to bring him before the Society, and show how the instrument was used. It enabled them when introduced to do whatever they pleased, and he thought it might be applicable to many other cases to prevent closure of the larynx during operation.

Dr. GEORGE STOKER pointed out that the year before last he had read a paper before the Society with regard to the administration of anæsthetics in operations on the larynx, and he had proposed the head-down position. On that occasion he had brought before the Society an instrument exactly like the one which Mr. Collier had just described, consisting of two lateral and a posterior blades. It was designed so as to permit of operation on the larynx while the patient was under the influence of an anæsthetic. In justice to Mr. Collier, he would admit that, so far, he had not met with a case in which he could employ this

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instrument. It was eighteen times before he had ventured to operate, but some men would of course operate sooner than others.

Mr. LENNOX BROWNE asked what was the date of infection in Mr. Collier's case ?

Mr. COLLIER said it was about ten years before, while he was in the army.

Dr. STOKER, in reference to the spread of the growth from one cord to the other merely by apposition, suggested that it was Mr. Collier's intention to lead them to suppose that the effect was due rather to mechanical irritation, even though occurring in a person the subject of syphilis. When he had met with this it had been explained on purely mechanical grounds, just as ulceration at the junction of the arytenoid cartilages was liable to be communicated.

Mr. LENNOX BROWNE said he had asked the date of infection, because it was unusual for mucous patches to come on ten years after the primary attack, especially in the larynx. He asked whether there was any evidence on the fauces of the disease.

Mr. COLLIER said there was.

Mr. BROWNE observed that this showed that the best of their theories might be bowled over. As a rule, tertiary evidences of syphilis were more ravaging than that. He was unable to concede that it was a case of infection from one cord to the other, because it was a recognised peculiarity that two vocal cords rubbing against each other would bring about a similar condition merely by irritation, and, moreover, as regarded mucous patches, symmetry was a characteristic of the affection. Mr. Hutchinson had insisted upon this tendency to symmetry, to "Dutch garden precision of distribution." It was a diagnostic point in a syphilitic patient also that he was not an irritable patient to examine, and this was a point of importance. In reference to the diagnosis between tubercle and syphilis, he pointed out that if they got a growth at a particular spot, the mere reiteration of contact would suffice to produce irritation, and in this case there was an affection of the under-surface of the vocal cords. He agreed with Mr. Collier, that the effect of cocaine was very disagreeable to certain patients, and was productive of intense spasm, lasting much longer than the anæsthetic effect. Besides, they had ice or cold water with a whiff of chloroform when there was hyper-sensitiveness—just to the point of the patient feeling a singing in the ears without going on to general narcosis. He was then enabled to use his instrument with the greatest success. He mentioned that he possessed an instrument which had been presented to him by Dr. O'Dwyer in 1887, with his first set of instruments for intubation in the adult. It was exactly the same kind of instrument as that described by Mr. Collier. It did not often occur that they invented an instrument which they did not use, but it did happen that they overlooked instruments invented by others. That particular instrument had been shown at the International Medical Congress in 1887.

Mr. COLLIER discarded any idea of having forestalled Dr. Stoker. He hoped to have an opportunity of demonstrating the use of his instrument at their next meeting. He differed from Mr. Browne when he

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stated that they seldom met with instances in which one portion of tissue was infected by an adjacent portion. In his own experience, nothing was more common than for one mucous membrane to be infected in that way. So common was it in fact that he was enabled to diagnose syphilis from the fact. He pointed out that it often took place as between the gums or the tongue and the cheeks, and the infection of a tonsil by the tonsil on the opposite side was frequent enough. Mr. Browne's argument was one that cut both ways, and it would not do to carry it too far. It was true that they might get affections on both elbows, but he was not referring to manifestations like that, but to affections of tissues in immediate apposition.

Mr. BROWNE said he did not dispute the possibility of infection by contiguity, but it was characteristic of syphilis in the throat that it was symmetrical independently of the apposition of parts. This could be seen in the illustrations in his work. A man, who detected a sore on one side of the tongue, would be negligent if he did not look for another on the opposite side, and that came to much the same thing as the elbows.

Dr. STOKER said it was one of the difficulties of the question that cases in which there was symmetry had nothing to do with the point under discussion, seeing that the parts were not in apposition to each other. The case of a mucous tubercle on one cord giving rise to a mucous patch on the other was quite a different matter, for symmetry was due to a constitutional disease.

Mr. BROWNE observed that this was not quite what he had said. He urged that as symmetry was a characteristic feature of syphilis in the larynx, therefore it must not be assumed that the second patch was necessarily due to infection from the opposite side.

Dr. STOKER said that in such case they would be contemporaneous, whereas in this instance the second was long subsequent to the first.

Mr. COLLIER said it was six months later.

EVENING MEETING.

Dr. DUNDAS GRANT, Vice-President, in the chair.

The Hon. Secretary (Dr. STOKER) announced that the discussions and papers would in future be published in the JOURNAL OF LARYNGOLOGY, and would be bound up separately at the end of the year. He also announced that the subscriptions for 1891 would become due in the month of June.

Dr. MACINTYRE read a paper entitled *Study of Bacteriology in Affections of the Nose and Throat*, with special reference to its influence in diagnosis and treatment. [This paper will be published in full in this Journal subsequently.]

DISCUSSION ON DR. MACINTYRE'S PAPER.

Dr. DUNDAS GRANT said that the first thing they had to consider was the relationship between these micro-organisms and the diseases with which they were associated. So long as Koch's four requirements were observed, this connection was clear enough. There was the further question of the

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antagonism of different kinds, which was illustrated by the antagonism between the microbes of erysipelas and of lupus. What the nature of this antagonism might be it was impossible to say. Much still remained to be done in the way of identifying these organisms, either by studying their natural history or their reaction to stains. The next thing would be to classify and mark out the diagnostic table of these appearances, and if the author could tabulate them in this way they would be under a further debt to him. Passing on to the pathological micro-organisms he observed, in reference to the existence of an organism of lupus, that those who had heard of it were far more numerous than those who professed to have seen it. He himself had never been able to identify it, and when he asked several experienced bacteriologists they too had confessed not to have done so, though they knew someone who had. At the same time he admitted that the evidence of inoculations in the lower animals seemed to point to a close relationship between lupus and tuberculosis. He alluded to the distinction between epiblastic and hypoblastic blastoderm in relation to the various micro-organisms, which he thought was stretching the point rather far. There was, at any rate, no immunity as concerned the larynx. Again, it could not be stated absolutely that the mouth and nose enjoyed any immunity from tubercle, for it had been met with often enough on the tongue and palate, and in the nose, sufficiently at any rate to deprive the analogy of a great deal of its force. No doubt the respective areas offered different facilities for the growth of micro-organisms. Passing on to discuss the dwelling of the microbe he observed that it had not been found in the blood of infected animals, and the author's observations pointed to their being found in the deeper layers of the membrane in diphtheria, and according to Ruffer in the most superficial layer of the membranes. The moral to be drawn from these observations was of the necessity for early treatment of cases of diphtheria. It was only right to say that Ruffer's observations were carried out on sections taken from bodies after death, and, as Dr. Macintyre had pointed out, that might make a difference. The apparent want of accord between the observations of Ruffer and of the author might thus be explained. The bacillus of syphilis could not yet be said to be within their ken. That of pneumonia was interesting, but had still not been made out. The coccus had turned up in the middle ear and might possibly be found elsewhere. He referred to a paragraph which had recently appeared in the "Lancet," giving a simple plan for detecting the presence of the bacillus of tuberculosis in the sputum, by mixing the sputum with a weak solution of caustic potash boiling and precipitating. The formed products of the bacilli were precipitated along with the rest, and by conducting the process in a conical glass, it was possible to make out its presence even if in very small numbers. He pointed out that a negative result, when searching for the bacillus, did not absolutely show that none were present, for in certain cases which clinically had been recognised to be tuberculous none had been found. In conclusion he urged that the time had now come for them, as far as possible, to arrive at a definite opinion in respect of the value of Koch's fluid, and it would be an opportune moment to elucidate the matter and say definitely what

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its value was in the treatment of tuberculosis. As the subject was formally before them in reference to some cases to be shown by Mr. Browne, he suggested that special attention should be devoted to this point.

Mr. LENNOX BROWNE said they had before them a much larger subject than that of Koch's treatment, though he doubted whether there were any present who were in a position to discuss the paper; he himself, at any rate, did not feel equal to it. Within the limited time at his disposal, he had not been able to digest the facts which the author had brought before them. He expressed his admiration for the essentially clinical character of the paper, which smacked of the ward rather than of the laboratory. Dr. Macintyre had said that he (Mr. Browne) would make some remarks as to the early recognition of tubercle in cases in which no bacilli could be found, though admittedly of a tuberculous nature. No doubt the method of precipitating the solid matter from the sputum by caustic potash and boiling would facilitate the search, but certainly the ordinary quick methods were eminently unsatisfactory, so that when they did not find them it did not negative their diagnosis, though conclusive enough when the bacillus was found. The important thing to remember in pursuing a treatment for tubercle was his remark that one possible reason for the failure of Koch's treatment was the neglect of the accessory measures. Touching the identity of the bacilli of tuberculosis and lupus, it was like the old story of the German flute. He had heard of someone who could play it, but he himself was in the position of someone who could not talk German. To describe lupus as a continued form of tuberculosis was, he thought, an obvious mistake, and he was disappointed to find that Liebreich had taken lupus as a test to try the effect of his cantharadine treatment in arresting tuberculosis. A certain amount of caution was, therefore, desirable. He agreed that tubercle was developed much more frequently in the upper respiratory passages. It was, of course, rare for them to find it there in cases of pulmonary disease. It was met with, but in the vast majority of cases it was not met with there. That fact might be due to variations in the anatomical structure. It was an important point, the discovery of the bacilli of diphtheria in the superficial layers of the false membranes; but as they had to deal with living patients, it was desirable that they should carry out their observations upon them, and not upon bodies in the *post-mortem* room; in fact, he thought that it would be safer to ignore researches made on the dead. He agreed as to the attention to the local treatment, but he asked what that local treatment ought to consist of. If only the superficial layers contained the bacilli, no doubt rubbing the throat might be of use. If the deeper layers were infected it was another matter. They used to be told never to remove the deeper layers, because membrane would certainly form again, and doubtless if they kept tearing them away they could only do harm. He thought that Watson Cheyne was the first person in this country to inculcate the tearing away of the deeper layers and treating the raw surface, and that plan was well worthy of their consideration. He thought that a change of views was desirable concerning the undesirability of removing the

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deeper layers of the false membranes, for it might be of service to treat the raw surface. That was to be deduced from the fact that in the living subject the bacillus was to be met with in the deeper layers as well. Then came up the question of the constitutional treatment—a very important point. They all made at present for the mercurial treatment of diphtheria, and he preferred the biniodide to the perchloride, because it did not precipitate albumen, and had a better effect on the membranes than the latter. Dr. Macintyre said that in an acute disease like this there was not time to attack the disease, and that remark should be borne in mind ; also his remark that it was possible to do things with pure carbolic acid which it would not be safe to do with the impure product. The changes which had taken place in regard to the use of carbolic acid in Lister's methods might perhaps be due to his having used the impure acid, so that his carbolic spray was perhaps, after all, not so wrong as those had asserted who now derived the benefits of his discoveries. He hoped that at an early meeting of the Council of the Association some suggestion would be made to appoint a committee to carry the subject further. He looked upon the work of the evening as among the best they had done. They wanted more work in this direction and of this kind. Records of cases were interesting, but work such as that which Dr. Macintyre had done was what was wanted to justify the existence of the Association. Dr. Macintyre, in coming so many hundred miles to read his paper, had put the Association under a debt of gratitude.

Dr. CAMPBELL said he was very pleased to find that, working quite independently, he had arrived at the same conclusions as the author, in respect of the relationship of lupus and tubercle. With all deference to Koch, he felt bound to say that their identity was, in Scotch legal phraseology, not proven. Kaposi had urged that in this matter the clinical aspects ought always to override the purely speculative.

Lupus was a disease of long growth, and was long in developing itself. Had it been tuberculosis he presumed the patient would have been dead long ago. He pointed out that lupus attacked the orifices of the mouth, nose, &c., but so did tuberculosis, but if the two diseases were identical, why did they not run a similar course? They did not get implication of the lungs in lupus, nor dysphagia or discomfort. None who had tuberculosis of the throat survived five years, to put it at the outside, yet he could show two cases of lupus of the throat which had existed, so to speak, from time immemorial. Then again, in lupus, after many researches, he had found a bacillus which resembled that of tubercle, but how many others were there that resembled the bacillus of tubercle? The dog and the wolf were both of the genus *canis*, but they were very different animals, and he thought that the experiments to prove the identity of these two bacilli were failures. Some years ago he had read a series of experiments conducted in Paris. The series was a long one, but there were only two positive results out of fifteen. Considering the conditions under which the guinea pigs were kept during the carrying out of the researches, he thought that there were plenty of other ways in which they might have contracted tuberculosis. He had bought dead

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animals at Leadenhall Market, and found that they had died of tuberculosis, without having been inoculated at all, and he asked what was to prove that the tubercle in these test animals was not of fortuitous origin. There was a far deeper question as to why pathogenic organisms sometimes took root in the organism, and sometimes failed to do so. The fundamental principle was that the perfectly healthy body was proof against any infection whatever. He mentioned that years ago in Edinburgh, in the fever wards, there was a nurse, a strong healthy woman, who had faced every variety of infectious diseases, and rather prided herself on having an immunity against such diseases. Some years later, however, she got a little out of sorts, and she contracted typhus fever, and died. That meant that the patient must be in a particular condition of body for the poison to strike home. The same reasoning applied in the case of doctors in general practice. He himself had seen 150 cases of scarlet fever in a month without ever having contracted the disease. Their duty as physicians was to get their patients back to normal health as promptly as possible, hence the advantage of sea air, country air, and the like. It was quite a different matter when it came to giving the patient something to kill the bacilli. They might give the patient mercury, and they might kill the bacilli. That was possible, but in so doing they would be going a long way towards killing the patient himself. Strengthen his tissues, and they would throw off the morbid influences for themselves. That was the alpha and the omega of treatment.

Dr. STOKER agreed in the analogy of the dog and the wolf, but he pointed out that though closely allied, dogs did not breed wolves, and *vice versa*. The only proof by which the theory was upheld was that the tubercle bacillus was a poison, which they injected into the healthy body of the lower animals, thus producing a disease. Then, again, a cure was arrived at by the fact that, when the second injection took place, under the same circumstances, a cure resulted. Of course there was nobody who could challenge the statements which that gentleman made. No one doubted that their immunity from contagion was due to the actual condition of their tissues at the moment of exposure. That was exactly the theory that was held by Koch. He said, if you find a soil upon which these bacilli can grow and flourish, there you can set up tuberculosis, while in a healthy body no effect followed. He mentioned a case of pulmonary phthisis in a patient who had not been steady in his youth, and who developed the undoubted symptoms of the disease at the age of thirty-eight, bacilli being present in the sputum in any quantity. He had now been three months under Koch's treatment, and the improvement was in every way satisfactory. During the first two weeks his weight had increased 6lbs., the moist sounds had disappeared, the evening temperature had diminished, together with marked amelioration in the character and the quantity of the sputum. He thought it was only right to record their actual experience. He had nothing but horror for those who used such a remedy in advanced cases, but it was quite otherwise in such a case as he had narrated. The study of bacteriology was a large subject. He referred to the investigations of Mosetig, of Vienna, who had observed that when cancer cells were stained with aniline the nuclei were destroyed.

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He had, therefore, injected interstitially a 1·500 solution of picrotoxin. One of his colleagues had tried this in a case of epithelioma of the face, and so far the results had been most surprising and most gratifying. The growth was gradually disappearing.

Dr. MACINTYRE, in reply, thanked the Fellows for their kindly appreciation of his work, which he admitted had taken up a great deal of his time. In reference to his remarks as to the epiblastic and hypoblastic structures, he said they only amounted to this, that owing to anatomical differences of structure, certain tissues might not be so liable as others to take on particular forms of growth. He simply meant to convey that it was comparatively rare to find tubercle in the nose and mouth, and it was admitted that tubercle was much more common below the epiglottis than above it. With reference to local applications, he recalled a discussion at the meeting of the British Medical Association, at Glasgow, at which a gentleman had said that after many years' study he had come to the conclusion that diphtheria was a local disease manufactured near the tonsil, and so much impressed was he with this view that he had decided the very next case he came across to stick a red-hot poker in the mouth and rub the tonsils. If they believed that diphtheria would be cured by removal of the parts, then this was a very efficient form of applying the actual cautery. With regard to the question of the identity of lupus and tubercle, he agreed with Dr. Campbell. As the result of long clinical experience, he was convinced that there was a difference between the diseases. They occasionally got tubercular ulceration of the tongue, but it did not resemble in the slightest the lupoid ulceration. He had under his observation a man with a tubercular cavity in the lungs, who, some time ago, bit his tongue, and that proved the starting-point of a tubercular ulceration. It had a hard irregular surface, and gave one the impression of tubercle of the lung. It was distinctly different from lupus. It was painless, at least the patient did not complain of pain, but the man's general condition was so bad that he had not thought it worth while to pay any attention to it. He admitted the difficulty of finding the bacillus in lupus, but lupus had not entered for a great share of his work at all. A dispensary was not the place to get hold of such cases, because they went to a skin hospital. He was not, therefore, prepared to give the same opinion as Dr. Campbell, but so far he agreed with him. Recognising the lack of specimens of lupus-bacillus in his collection, he had applied to a gentleman at Berlin to send him some, because he wanted some fresh material, but he had been informed that none could be had, because all the cases had been cured by Koch's treatment. He was convinced that there had been too much trying of late to find some remedy to apply in particular to tuberculosis, leading to general measures being neglected. He mentioned the case of a gentleman who had been recommended to go to the Cape, but had delayed his departure because of the florid accounts that came from Berlin. He had given up the idea of trying Koch's treatment now, but his chances of recovery had been forfeited by the delay. The attempt to bring something forward which should kill this bacillus, either directly or indirectly, had almost

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pushed the ordinary remedies aside. It could not be impressed too much upon them that there had only been thirteen cases of alleged cure out of 1200 in which it had been tried. Personally, he would rather take a voyage than trust to this treatment.

Mr. W. WYATT WINGRAVE read the following note on *Microscopic Specimens with Lungs and Larynx from a Patient treated by Koch's Remedy*, being the report of the *post-mortem* appearances in a case of laryngeal tuberculosis treated by tuberculin. (Mr. Lennox Browne's case No. 8, in his essay on Koch's remedy.)

LUNGS.—*Right.* The pleural surfaces were slightly adherent at the apex. The upper lobe was studded with caseous tubercles, which here and there were actively breaking down. The remaining lobes were free from tubercles, but showed marked congestion and œdema.

Left. Pleura everywhere so adherent that the lungs were much torn in removal. Both lobes were in an advanced stage of destruction, being riddled with suppurating chambers containing grumous non-fœtid pus. There were three large cavities, two in the upper lobe, and one situated in the base of the lower lobe posteriorly; but practically no part of the lung was free from necrosis.

The LARYNX presented the usual features of tubercular ulceration mainly involving the right vocal cords and ventricular bands, with marked thickening of both ary-epiglottic folds. The right arytenoid cartilage and its cornicula were bare, and the ulceration had also invaded the left sub-glottic region. Excepting the left kidney, in which was discovered a small abscess, the remaining viscera were entirely free from tubercle. Microscopic examination proved the tuberculous nature of the morbid processes.

REMARKS.—Perhaps some apology would be deemed necessary for presenting such an ordinary specimen to the Society, were it not attended by circumstances of more than ordinary interest. It is only fair to the remedy and its discoverer that all available evidence (especially that of the cadaver) should be openly and impartially investigated, and the rarity of such an opportunity in England may be taken as a sufficient justification for the exhibition of these specimens.

The patient, on admission, presented some decided, though distinctly localized pulmonary disease, of three years' duration, and apparently quiescent; also unmistakeable laryngeal tuberculosis of less than twelve months' duration. His temperature was carefully watched for eighteen days, and it maintained a mean level of 99°F. After the first injection, pyrexia rapidly developed and increased, the lung symptoms were as it were "lit up," and his general condition became so grave that the treatment was stopped after the fourth injection of ·006 grammes. Still he grew rapidly worse, and fifty days after the last injection, necropsy reveals an extent of disease contrasting strongly with the conditions which the physical signs on his admission indicated.

It is interesting to note that the disease was distinctly limited to the viscera which were primarily attacked (with the exception of the one kidney), so that notwithstanding the violent pulmonary "eruption," tuberculous processes were not established elsewhere, although there was ample time for such. Was the pulmonary tuberculosis secondary to the

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laryngeal? is a question which naturally suggests itself. It is quite possible that the early pulmonary lesion was non-tubercular. The freedom of the other viscera from tubercle is in favour of such a view. It must not be overlooked also that the patient had for many years been addicted to spirit drinking.

Still, whatever interpretation this evidence may suggest or justify, it must be admitted that the facts are deficient neither in interest nor significance.

Dr. STOKER related a case in which an ***Epitheliomatous Growth of the Larynx*** had been ***removed by intra-laryngeal operation*** nine months ago. The patient had been seen by several men. Two portions of the growth had been examined by competent pathologists, and had been pronounced to be epithelioma, and the result was the same with three subsequent portions. There was the usual ulceration, and the question arose as to the further treatment. It was decided that no extra laryngeal operation was desirable on account of the age of the patient, and because he was suffering from chronic bronchitis. Last, but not least, the patient firmly declined to allow of any operation. With a good deal of trouble the growth was entirely removed by intra-laryngeal methods, and the base was freely cauterized, and the patient went away. He saw him again in October last, and there was no sign of any recurrence, though there was still some congestion of the larynx. He had written to him the other day, and he had received the answer that he was not troubled in the least by his throat. He had an opportunity of examining him on the previous Saturday, and all that he found was some congestion of the left vocal cord. He did not wish to prophesy that there would not be any recurrence, but, in any event, the patient's life had been prolonged by the operation. There was, of course, the question of diagnosis, but although some persons might decline to be guided by negative symptoms, nobody could deny the positive ones. In regard to the operation itself, he wished to express his entire disapproval of forceps of every kind. He preferred the wire: in the first place it was a fine body, and it was, consequently, possible to see something of the growths that were to be removed, a thing which was impossible with forceps; further, it destroyed the roots from which the growth took its origin. With the forceps, one could not be sure of the point one was going to touch; and, lastly, there was the necessity of subjecting the patient to a prolonged course of training, which rendered manipulation easy. He did not agree with what had been said in regard to the use of cocaine. He thought it was of far greater benefit to the human race than either chloroform or ether, and he thought it had brought about a complete revolution in the matter of intra-laryngeal surgery. The aggregate of human pain was not made up of large operations, but of an infinity of little pangs such as were caused in the removal of tonsils, small growths, and the like. To come back to his case, he said the result of the operation had been very gratifying to himself, and probably to the patient, but he felt bound to testify to the courage and endurance of the patient in submitting to the treatment.

Mr. LENNOX BROWNE said that Dr. Stoker had omitted to state that he (Mr. Browne) had advised immediate operation. They could quite

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understand how Dr. Stoker had succeeded in persuading the patient where neither Teuton nor Englishman had been able to overcome the reluctance of the patient. He had impressed the patient with the necessity for immediate operation, but his wife was totally opposed to it. He showed a drawing which had been made of the laryngeal appearances by the gentleman who first saw the case. It was positively a work of art. This gentleman had told the patient that it might be a fibroma (there was only a small projection on one vocal cord) or develop into a cystic cord. That was in 1886, and the patient was then sixty-two years of age. He did not see the patient again until 1888. At that time there was no doubt as to its being cystic. He believed he had seen him again in 1889, but there was not much change ; but the third time the progress was marked, and he had again strongly urged the necessity for an operation. While he congratulated Dr. Stoker upon his success, he pointed out that there was no sign of any enlarged gland, no emaciation, no laryngeal spasm or shortness of breath. He reminded Dr. Stoker that he had reported the case of a man under Fraenkel, of Berlin, who had been treated for an undoubtedly malignant growth on one vocal cord, and successfully, by means of the galvano-cautery ; and that was the second case he had read of. He was delighted to hear Dr. Stoker speak as he did of the use of the forceps. Speaking with an experience of nearly twenty-six years, he said he had never introduced a pair of forceps into the larynx, although he had from nine to twelve cases a year, and he thought one could get on very well without them. With regard to cocaine, he could only add that in many patients it proved more of an irritant than an anæsthetic, and he mentioned a case in which the first operation having been carried through under cocaine, the patient, on a second occasion, said he suffered less without it.

Dr. DONALD STEWART recalled a case brought before the Medical Society of Vienna by Schnitzler of a growth recognised to be carcinomatous by several independent pathologists. That was many years ago, and the patient lived for many years after. That probably was a case of the same category. He had had only two cases of intra-laryngeal growth, which he had attempted to remove by the forceps, and he was fain to confess that he had failed to achieve his object, only securing pieces thereof. These cases were still under his observation, the patients being fairly comfortable, but not entirely removed. He was very pleased to hear of the advantages attaching to the use of the galvano wire.

Dr. CAMPBELL corroborated what Mr. Browne had said about cocaine acting as an irritant. That very day he had injected some of a 20 grain to the ounce solution for the removal of some cysts of the scalp, and he never remembered so much pain being complained of.

Dr. STOKER pointed out that such a solution was not likely to procure any relief from pain ; nothing under a 20 per cent. strength was likely to be of service.

The Society then adjourned.

[The report will be concluded in the next number of this Journal, in which will also be published the papers read by Mr. Lennox Browne and Dr. Macintyre at this meeting.]

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