examples, and the morbid changes, as shown by post-mortem examination in the fatal cases, do not differ in any appreciable way from those which are manifest in cases having a distinct history of rheumatic causation. It would therefore appear most probable that the correct conclusion is, not that mitral stenosis is independent of rheumatism, but that it is associated with the less pronounced forms of it—with its insidious, and not (so to speak) with its *explosive*, varieties.

I bring before you examples of (1) presystolic murmur developing insidiously without signs of rheumatism; (2) systolic murmur at apex becoming changed to presystolic murmur; (3) a presystolic murmur developing insidiously, subsequently found to be accompanied by a systolic murmur. The evidence, I consider, tends to show that in a considerable number of cases the origin and course are insidious and gradual. The disease is not independent of rheumatism, but is unaccompanied by pronounced rheumatic phenomena; it is initiated by the form of endocarditis which I sketched in my first lecture as manifested by no subjective sign, accompanied by no prominent symptom, and yet differing in no essential feature from that which occurs in obvious relation with rheumatism. The endocarditis which results in mitral regurgitation is more violent, so to speak, whilst that which initiates stenosis is more protracted, giving rise to a slower formation of fibrous, quasi-cicatricial tissue that under the even pressure of blood in the auricle tends to form the smooth septum which has erroneously suggested a possible congenital causation.

Not all the cases of mitral stenosis, however, originate in this manner. In some there has been, first, the induction, in association with the phenomena of acute rheumatism, of the lesion of mitral regurgitation; then has occurred probably a slow welding of the curtains, and in the repeated attacks of endocarditis the changes have been slower than those which result in retraction of curtains, cords, and columns to the ventricular wall. By either of these modes produced, it is probable that secondary changes take place in the diseased tissue—under the tension of blood the fibrous septum thickens, for it has to bear the chief strain of the auricular pressure, and not the ventricle, as in the case of mitral regurgitation. In some cases it undergoes calcareous degeneration, and probably in others where gouty signs are manifest it becomes infiltrated with the earthy lithates.

Compensation in cases of pulsal stenosis may be maintained, as in mitral regurgitation, for long periods. It may be even more simple in the former case than in the latter, for it is only a hypertrophy of the right ventricle, and not of both ventricles, that is needed to sustain it. So long therefore as a good nutrition maintains the muscular power of the right ventricle and left auricle, any special methods of treatment of a simple condition of mitral stenosis may be unnecessary. In course of time, however, the right ventricle or left auricle, or both, may begin to fail. Usually it is the former, but I have quoted a case in which it was markedly the latter, and in this I have no doubt the muscle failed on account of the great privations which the patient had undergone. The right chambers dilate on account of the pressure which is maintained within them if the compensating muscular power begins to fail. Then ensue the dyspnoea, the oedema, ascites, &c., which we are familiar with in analogous cases of mitral regurgitation. To restore compensation we may use for the most part similar means to those which we have considered in regard to mitral regurgitation. Even when orthopnoea and dropsy have supervened, I have in many cases found that rest, combined with the administration of nutrients and tonics, with digitalis, have restored the *status quo ut ante* often for a considerable period. Coincidentally with the use of means for increasing muscular power, I consider that small and repeated abstractions of blood are even more valuable in mitral stenosis than in mitral regurgitation. The tension of the right heart may be sensibly relieved even by a leech or two applied over the præcordium.

As regards the special action of digitalis in restoring compensation in cases of mitral stenosis, I am not convinced that this is as markedly proved to be beneficial as in the cases of mitral regurgitation. I have found that in some instances, as shown by the sphygmograph, digitalis has restored regularity, whilst in others it has increased irregularity of pulse. I believe it to be most valuable where stenosis and regurgitation are combined. Where the right ventricle is chiefly at fault, I do not think its good effect is so manifest; where it can induce an efficient systole of both ventricles and coördinate them, then I think it is the more valuable. In failure of the right heart, therefore, in extreme mitral

stenosis I look more hopefully to caffeine and to convallaria majalis. M. Sée has recorded instances showing the good effects of the latter in mitral stenosis.

Though the production of the obstructive lesion is more gradual, I do not think that the prognosis is more favourable than in the case of the regurgitant. We can point to many instances of mitral regurgitation where fair health has been maintained for long periods of years, where compensation is perfect, and where disease of the valves does not progress. Such instances are, I think, less common in stenosis; there is not a like quiescence, and degenerative changes or intercurrent morbid phenomena are more likely to occur.

In mitral stenosis, as well as regurgitation, it is not alone with the simple dynamical problem of restoration of compensation that we have to deal. Certain complications are almost of the essence of the disease, and call for consideration when question arises as to treatment. Such are (1) recurrence of endocarditis, or pericarditis, or both; (2) embolism. I consider that one of the great interfering causes, spoiling the chance of compensation in children and young people, is the occurrence of pericarditis with adhesions. Embolism of some sort is a source of danger, and an element to be considered in a large proportion of cases of valvular disease, especially of mitral stenosis. Clinical examples are brought forward showing the occurrence of this complication (1) on the venous, and (2) on the arterial sides of the circulation.

Embolism of a branch or twig of the pulmonary artery is a very common cause of the broncho-pneumonia observed in mitral diseases, a characteristic feature of which is hæmoptysis. The occurrence of such symptoms in a case which has hitherto been questioned tells us that compensation is disturbed, that circulation is sluggish in the right chambers, and we are called on to strengthen the ventricles by cardiac tonics as well as to maintain rest. In all such cases it is desirable to restore the tendency to coagulation of the blood by the administration of alkalies, especially ammonia.

I bring forward also examples where *arterial* embolism has been the first clinical sign of mitral stenosis, the embolism giving rise to hemiplegia, hemichorea, epilepsy, &c. In cases under treatment for cardiac diseases the one sign which I have found to indicate the probability of the occurrence of embolism is a sudden *rise of the temperature of the body*. In all such cases the most perfect rest should be enjoined. I think also that an attempt should be made to render the deposit of fibrin less likely by the administration of ammonia or other alkalies. The induced phenomena, of course, call for treatment according to the organ or situation in which infarction occurs.

[ERRATUM.—In the last lecture (THE LANCET, Feb. 3rd, 1883, p. 185), for “the sign of such compensation (supposing the amount of blood regurgitating to be very small) is an accentuated second sound,” read, “the sign of such compensation (supposing the amount of blood regurgitating to be *not* very small) &c.”]

Hunterian Oration

DELIVERED AT THE

ROYAL COLLEGE OF SURGEONS OF ENGLAND

On February 14th, 1883,

By T. SPENCER WELLS,

PRESIDENT OF THE COLLEGE.

(Concluded from page 267.)

I MUST now obey the other direction in the trust deed, and endeavour to express something as to the “merits in comparative anatomy, physiology, and surgery” of John Hunter. The direction seems a happy one, for Hunter based his surgery upon physiology, and his physiology upon comparative anatomy. What can more strikingly illustrate this than his greatest improvement in practical surgery, the abolition of amputation for popliteal aneurism, and the practice of tying the artery in a sound part at a distance from the seat of disease? This has already saved thousands of human lives; and it has been well said that if Hunter had done nothing else, “on this account alone he would have a right to be classed among the principal benefactors of mankind.”

One who perhaps more than any other of our con-

temporaries resembles Hunter in completing the union of thought with action—the wisdom of the philosopher with the skill of the surgeon—Sir James Paget—says, “It was really a splendid achievement, and its utility is not half told by counting the thousands of lives that it has saved. Its higher value is that it still abides as a great testimony of the power of the scientific mind in surgery. I think it has done more than any other of Hunter’s works to make, not only surgeons, but surgery, scientific.” And observe how comparative anatomy and physiology led to surgery—how thought and experiment prepared the way for action. It was probably without any idea of surgery that Hunter was first led to investigate the phenomena of the annual growth and shedding of the antlers of the stag or buck. But he did so, and tied the artery which supplies the growing antler and the soft covering called the “velvet,” which conveys the vessels supplying the material of growth. Pulsation in the vessels in the velvet ceased, and the antler soon became colder. A week afterwards the vessels were again pulsating, the velvet was warm, and the antler growing. The buck was killed, the tied artery found to be obliterated, but the circulation was carried on by vessels above and below the ligature, ordinarily very small, but enlarged under the new conditions.

Now I am well aware that some modern misanthropical zoophilists have said that Hunter had no right to make this experiment—that human morbid anatomy had taught him that when the current of blood ceased to flow into an aneurismal sac, the blood clotted and was absorbed. But it is quite indisputable that Hunter was doubtful whether, after the supply of blood to a part had been cut off by obliterating the main arterial trunk, the circulation would be kept up by other vessels, or the part would die. And it is certain that his experiment on the deer removed his doubts, for, as Professor Owen tells us, there was a coachman in St. George’s Hospital, with popliteal aneurism, who had consented to amputation. But Hunter thought that if the anastomosing vessels in the man would carry on the circulation after obliteration of the femoral artery, as they did in the antler of the buck after obliteration of the carotid, he could cure the aneurism and save the limb. Professor Owen emphatically says that Hunter explained to his assistant and pupils the results which he believed would follow a repetition on the man of his experiment on the deer. And, just as he predicted, there was the same stopping of pulsation, the same cooling of the part from which the supply of blood was cut off, the same return of natural warmth, and in six weeks the man walked away cured. This account, Professor Owen tells me, was given to him by Mr. Clift. In this and two subsequent cases artery and vein were both tied; but in his fourth case Hunter tied the artery only, not the vein. This was in the year 1787. The patient was then thirty-seven years old. He lived till he was eighty-six, and died in 1837. At his death, Mr. Wormald obtained the limb from his widow, and found the femoral vein pervious, the artery obliterated, the tortuous anastomosing vessels as you see them in this specimen which Mr. Wormald presented to our museum, and the aneurism represented by the small calcareous body not larger than a filbert.

This association of surgery, physiology, and comparative anatomy is manifest in the leading idea or plan of Hunter’s museum, which is to show each step from the most simple conditions in which life can be traced upwards to man himself. We are told, observing that in the advanced stages of successful incubation eggs did not putrefy, he was first led “to recognise LIFE as a power, and ORGANISATION as the mechanism by which life operates.” Observing some snails drowning, and noticing the effects produced upon lizards if they were brought too early in winter out of their lurking-places, he ascertained some of the facts “which guide and encourage us in our attempts to restore suspended animation.” Here we see the man who both observes and thinks. “For one person who can think,” says Buckle, “there are at least a hundred who can observe: an accurate observer is no doubt rare, but an accurate thinker is far rarer.” Can anyone look up to this portrait of Hunter, by Reynolds, and not agree with Lavater, who, when he saw it, said, “This man thinks for himself”? Hunter used to say that “he delighted in thinking;” and the great artist who is said to have peculiarly excelled in *painting the mind* of his sitters, has caught Hunter in the attitude of meditation. In that absorbed and upward gaze “from earth to heaven,” “as imagination bodies forth the forms of things unknown,” we can trace the hoped-for explanation of some of the myste-

rious phenomena of nature by the application of some great leading principle, the elucidation of some physiological problem, the unravelling of some as yet unfolded revelation. Home and others who knew Hunter have said that this portrait gives a very faithful representation of his countenance, person, and manner. While painting the portrait, Reynolds induced Hunter to have a cast taken from his face. From this cast, which has been preserved in our museum, and is here before you, Flaxman’s marble was sculptured. He and Chantrey studied both cast and portrait, for the College is fortunate in possessing two busts of our great master, as well as the full-length sitting figure in the museum by Weekes. And we have a second portrait, by Sir Nathaniel Holland, which was said by Mr. Clift, and by Sir John Dorat, Hunter’s last surviving pupil, to be better as a mere likeness than the idealised portrait by Reynolds. Perhaps Reynolds’s portrait is more suggestive of the man who thought, and Flaxman’s bust of the man who observed, experimented, acted.

Though Hunter loved to think, he followed Bacon in insisting on observation and experiment as the only foundations of true science. “If you check experiment you stop discovery” is one of his aphorisms. And he once wrote to a friend, “I think your solution is just. But why *think*? Why not try the experiment? Repeat all the experiments as soon as you receive this, and they will give you the solution.” Thus in Hunter we find the mind which investigates the laws of disease, and the hand which improves the art which cures disease, not only philosopher and pathologist, but surgeon—as rare a combination as that of a profound jurist and an eloquent advocate—an agricultural chemist and a farmer—an astronomer and a pilot. We have a combination of Faraday investigating the laws of electricity and magnetism, and Swan or Edison applying the knowledge in the electric lighting of towns.

It is unnecessary to repeat now, what Mr. Wormald proved in this place twenty-five years ago, that Hunter was well aware of the possibility of curing aneurism by pressure on the artery as well as by tying it—in this, as in so many other instances, anticipating recent improvements in practice supposed to be new. Just as we find that modern views of phlebitis and pyæmia had been advanced by Hunter; and Burdon Sanderson, in his lectures on inflammation, acknowledging that “we find ourselves once more coming back to the notions, which at one time were thought obsolete, of the great father and founder of physiological pathology”; so we may be certain that Hunter foresaw much of that progress in abdominal surgery in which it has been my own pride and pleasure to assist. In 1762 William Hunter distinctly suggested that it might be advisable to expose an ovarian cyst by a small incision—“tap the bag and draw it out.” In 1785 John Hunter said, “I cannot see any reason why . . . we should not make an opening into the abdomen and extract the cyst itself. Why should not a woman suffer pain without danger as well as other animals do?” The influence of Hunter’s teaching upon John Bell, of Bell upon McDowell, and the history of ovariectomy since McDowell’s first operation, is a theme which I have treated at length in this theatre and elsewhere; and however strongly tempted to enlarge upon it, and upon the more recent extensions of peritoneal surgery—the removal of uterine tumours, of the spleen, of one kidney, of hydatids, of gall-stones, of omental and mesenteric tumours, of fibroplastic and fatty tumours of various origin from the abdomen, of the entire uterus, or of part of the uterus with the fœtus of an extra-uterine foetation, of the pylorus, and of portions of diseased intestine—all operations which have been performed with increasing success—I regret very much that I have not time to do more than point to the preparations on the table, which show how our younger hospital surgeons are joining in the most recent advances of abdominal surgery. Mr. Treves’s specimens show how perfectly union between two portions of intestine may be effected after the removal of an intervening diseased portion. And the three large gall-stones removed from the gall-bladder of a woman in the Samaritan Hospital by Mr. Meredith are gratifying evidences of boldness and skill. Both Mr. Treves and Mr. Meredith, at my suggestion, were desirous of experimenting on some of the lower animals as to the best mode of uniting divided edges or surfaces of peritoneum; but the trouble and delay of the present system of licensing has hitherto restricted their operations to man and woman. I wish I could say more of this, but I am compelled to devote the very few minutes allotted to me to a hasty sketch of what we hope may be

gained in the not very distant future by combined association for the advancement of medicine, in its higher sense, by research.

And first let me say with how much pleasure I can state that the Association lately founded under the auspices of all the leading men in our profession in the United Kingdom has already begun useful work. On the part of the Association, Mr. Watson Cheyne visited Dr. Koch at Berlin, and Professor Toussaint at Toulouse, and has since carried on investigations, the results of which enable him to explain their opposite statements with regard to the micro-organisms associated with tubercle. Mr. Cheyne has also made experiments with reference to the specific nature of tubercle, which tend to confirm the view of the specific nature of tubercle first promulgated by Villemin, and so strongly supported by Koch's observations. Mr. Cheyne's further observations lead him to the conclusion that the bacilli of tubercle multiply by preference in the epithelium of the alveoli of the lungs, and lead to inflammatory exudation in the walls of the alveoli. According to the number and rapidity of growth of the bacilli in the alveoli, we have the two conditions of fibroid phthisis or caseous pneumonia, which by many have been looked on as different processes. On this view is also explained the difference in the effects produced by these organisms in man and rodents. Rodents, when inoculated subcutaneously, always develop general acute tuberculosis. That disease is extremely rare in man when compared with the frequency of pulmonary tuberculosis; because in man the bacilli are not inoculated, but are received into the bronchial tubes by inhalation, and their entrance into the circulation is prevented in the first instance by the inflammatory changes which occur around the alveoli in which the bacilli grow. If man were inoculated as we inoculate rodents, all analogy would lead to the conclusion that acute tuberculosis would probably be developed. Mr. Cheyne is carrying on these researches, the animals experimented on being kept under exceptionally favourable hygienic conditions, and it is not too much to hope that they may lead to a successful mode of treating phthisis. However strongly tempted to say more on this immensely important subject, I must leave it for a future Hunterian orator, and pass from tubercle and its bacilli to other diseases, more commonly classed as contagious or infective.

Of all the discoveries of modern times, perhaps the most important is that of the dependence of fermentation and putrefaction upon microscopic organisms; and of a number of communicable diseases upon specific microbes—a convenient word to include the micrococcus, the bacterium, the bacillus, the vibrio—microzoa or microphytes, whichever they may be.

At Cambridge, eighteen years ago, I attempted to show the relation between the work of Davaine and Pasteur, and the causes of excessive mortality after surgical operations. Allow me to read one sentence from my address in 1864, partly to allude to advances gained since that year, and partly to point out some lines of future research. I said: "Applying the knowledge for which we are indebted to Pasteur of the presence in the atmosphere of organic germs, which will grow, develop, and multiply, under favourable conditions, it is easy to understand that some germs find their most appropriate nutriment in the secretions from wounds, or in pus, and that they so modify it as to convert it into a poison when absorbed—or that the germs after development, multiplication, and death, may form a putrid infecting matter—or that they may enter the blood and develop themselves, effecting in the process deadly changes in the circulating fluid."

In the history of this discovery we find Davaine discovering bacteria in the blood of animals suffering from charbon. Then, having studied Pasteur's researches on butyric fermentation, Davaine found that he could propagate a fatal disease, not only by a purulent virus, but by a drop of infected blood. It was left for Pasteur to separate and identify the microbe, to propagate it through successive generations, and to arrive at a general rule that a whole series of contagious diseases could be produced artificially, and that the microbe which was the cause of each disease could be so modified by successive cultivation, some with, some without, access of oxygen, as to be rendered almost inert—nay, more, even to protect the recipient for a time from a second invasion, and secure immunity to the offspring of infected mothers.

I must not go back to the history of vaccination as a

protective against small-pox, nor remind you that Jenner was a pupil of John Hunter, nor refer to many of the letters which passed between them; but I may notice a resolution of the Council of this College carried sixty years ago—"not to inoculate small-pox, but to pursue, and to the utmost of our power promote, the practice of vaccination." We persevere in this course. We require every candidate for our diploma to produce proof of practical knowledge of vaccination. We support the law which protects the many from the danger to which a few ignorant opponents of compulsory vaccination would expose the whole population. And as we glory in the life-saving work of our countryman, Jenner, so we welcome the teaching of the illustrious Frenchman, Pasteur, and his extension of the protective influence of vaccination to other diseases—every year adding to the list of new vaccines which protect man and animals against virulent diseases.

When I first thought of the subjects for this oration, I had a very exaggerated idea of what it might be possible to do in sixty minutes. I hoped that (in addition to what I must say of our deceased brethren, and of John Hunter himself), of recent additions to our museum, and of the new Pathological Catalogue, I might be able to indulge in a review of the progress of modern surgery, and perhaps compare the present state of medical practice in London with that in Hunter's time, or even to take a hasty review of the progress of the nation since his death, and refer to the rapid increase of population and wealth, the discovery of steam, the influence of railroads and steamships, the use of gas, of the electric telegraph, the spread of education, the effects of newspapers and cheap literature, of reforms in our laws, improvements in our habits, and many other things which tend to make a people wiser, happier, and better. I even thought it might be possible to sketch very rapidly the share which the medical art, in its larger and wider sense—not only as curing but as preventing disease—has had in assisting national progress, and to prove that neither medicine nor surgery has lagged behind the general advance. I hoped I might be able to prove beyond dispute that since active sanitary work has been undertaken in this country death rates have fallen very greatly, and fallen most in those places—the great towns—where sanitation has been most active. I was very anxious to show how the knowledge gained by the statistical work begun by Dr. Farr, and since carried on by Dr. Ogle at the General Register Office, had led to sanitary legislation, and how sanitary work has been followed by a lower general death-rate and smaller mortality in single forms of disease, as in typhoid fever, as well as after wounds, injuries, and surgical operations. I wished also to show how statistics lead to the saving of life by throwing light on the natural history of disease, on the prevalence of various zymotic diseases at different seasons, and on the indisputable proofs that small-pox has declined considerably with the extended use of vaccination, that it is false to attribute that decline to general sanitation, exclusive of vaccination, and, what is a more novel or less generally known fact, that we have statistical proof that the preservative effects of vaccination wear out more rapidly and surely than the preservative effects of small-pox itself. I imagined also that I might be able to sketch what the nation might gain if State Medicine were really administered by a well-organised department of the Government, if politicians of both parties could be roused from their indifference to social or domestic legislation and give some small share of their attention to the health-interests of the people—to their food, drink, occupations, house accommodation, care of infants, prevention of infective diseases, local sanitary administration, and many details of sanitary reform, such as a permission or encouragement of cremation as a substitute for the present mode of burying the dead.

My intentions, sir, may have been good, but their fulfilment is impossible; and, in conclusion, I can only refer to the prospect which expands before us as we are shown that more than thirty destructive diseases, including tubercle and typhoid fever, ague and yellow fever, scarlatina, diphtheria, erysipelas, syphilis, and septicæmia, in man; in the lower animals, splenic fever, fowl-cholera, cattle-plague, glanders, hydrophobia—all depend upon specific microbes. In many of these thirty diseases, the specific microbe has been identified beyond dispute. In some, the process of attenuative culture has transformed a poisonous virus into a protective vaccine. In others, we may confidently hope that the same happy result will soon be reached; and as we

have already acquired the knowledge of certain specifics, it is not illogical to infer that if we can in one instance command the good results we see with mercury, in another with sulphur, and in a third with quinine, we may before long obtain such a knowledge of the various microbes which are the cause of communicable diseases, as may teach us how to destroy these organisms, or to arrest or mitigate their morbid influence, and so check, if not stamp out, some of the diseases which are now our most formidable difficulties. We may then congratulate mankind that the science of our own time has conferred not only upon man, but on the whole animal kingdom, benefits equalling any which it owes to the lucky empiricism of past ages, or to the philosophic genius and marvellous industry of John Hunter. And we his followers may be encouraged by the conviction that we so cultivate our science and our art (devoted as they are to the public good) as to justify us in keeping our old motto,

"QUÆ PROSUNT OMNIBUS ARTES."

REMARKS ON THE RELATION OF THE TUBERCLE BACILLUS TO PHTHISIS.¹

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SINCE the announcement of Koch's celebrated discovery of the tubercle bacillus, attention has been closely directed to its detection in the sputum of phthisis, and subsequent observations have confirmed its intimate connexion with the disease, though the exact part it plays in its pathology and causation will require much further research to elucidate. The first question of importance which meets us is, Are the bacilli always present in the sputum of phthisis, and absent in that of other diseases? Important evidence has been given on these points by Drs. Balmer and Fraenkel, D'Espine, and Drs. Heron, Whiphram, and Dreschfeld, and now I propose to give the results of the examinations which have been carried on in the Brompton Hospital for some months with a view to test Koch's conclusions. The number of patients whose sputum has been tried is one hundred and thirty. Some earlier examinations were made, but as they were few in number, and not systematically carried out, I have omitted them. The method used was that of Dr. Heneage Gibbes, the staining being accomplished by his magenta aniline solution and chrysoidin; in some later slides methylene blue was substituted for the chrysoidin. The specimens tested were either taken from the sputum collected during the twenty-four hours or from that expectorated in the early morning, and the rule adopted has been in the case of a negative result to repeat the examinations two, three, or four times so as to ensure accuracy. Forty-five of the patients were under my care, and eighty-five under the care of my colleagues, Drs. Douglas Powell, Tatham, Reginald Thompson, and Roberts, who kindly allowed me to make use of the examinations and records made in their wards. In one hundred and three cases the slides were prepared and examined by my clinical assistant, Mr. G. Perez, and in twenty-seven by my other clinical assistant, Mr. Waugh. Dr. Powell's thirty cases have been carefully checked by him, and I have myself gone through all the others with a good Zeiss microscope with an F objective and a No. 4 eyepiece, giving a magnifying power of 1020. Twenty-one patients were examples of various lung affections other than phthisis; two were cases of asthma, five of emphysema, two of bronchitis and emphysema, three of bronchitis, one of pleurisy, three of bronchiectasis, one of pneumonia, one of empyema, two of pulmonary congestion (the result of heart disease), and one was an obscure case of lung induration. In no one of these did the sputum contain bacilli. The one hundred and nine phthisical cases consisted of acute and chronic forms, and included instances of tuberculo-pneumonic phthisis, of scrofulous pneumonia, of fibroid, catarrhal phthisis, and a large number of cases of chronic tubercular phthisis. Cavities were detected in one or both lungs in eighty-one of these patients, nine were in the stage of early consolidation, the

rest were undergoing softening or were cases of old tubercular induration with emphysema and fibrosis. In the 109 phthisical cases we detected bacilli in 106—that is, in all but three; and even of these three, in one it could not be affirmed with certainty that they were absent. There was a difference of opinion between the two observers, and unfortunately the patient left the hospital before we obtained a further specimen. In another, a case of contracted cavity, under my charge, the sputum was so scanty that it was very difficult to obtain any, and I have my suspicions that the secretion, owing to the blocking of the cavity bronchus, was entirely bronchial. In the other case the slides were not very successfully mounted and circumstances prevented our obtaining more sputum, but I thought it just to include it in the list.

So far our results agree with those of previous observers as regards the specific character of the bacilli, and the fact that none were found in the cases of bronchiectasis, in which the expectoration was extremely fetid and abundant, separates the tubercle bacillus from any of the numerous organisms connected with fermentation and decomposition.

Most of our consumptive patients had cavities, but it will be observed that nine were cases of early consolidation. These were all cases in which both the history and the physical signs forbade any suspicion of a cavity, and I offer them as a proof that the bacilli are found in connexion with tubercle formation, and not only with softening and excavation. It will be remembered that in some of Koch's specimens they were seen together with miliary granulations. Dr. Heneage Gibbes' distinction of two kinds of tubercle, one containing bacilli and another devoid of it, is most important.

The second question seems to be as to the relation between the numbers of the bacilli and the progress of the disease. Is their abundance coincident with great activity, and does their disappearance indicate quiescence? In dealing with this point we must bear in mind the different expectorating powers of patients, some of whom, especially in the later stages, have great difficulty in ejecting the secretion, large accumulations of which are often found in cavities after death. Then, again, we must remember Dr. Gibbes' caution as to ensuring that the material comes from the lungs and not from the throat. A third consideration is, What do we understand by activity of disease? I take it as signifying either extending tuberculation or softening and excavation, or a combination of both processes. The occurrence of pyrexia, and specially of prolonged pyrexia, indicates some form of activity, though it is possible, as I have shown elsewhere, the disease can progress and extend without this concomitant. Where, however, it is present, we may be quite certain that active tuberculous processes are going on.

Temperature observations were carefully taken in all the 109 cases, and in 51 pyrexia, ranging from 100° to 105° F., was present at the time of the examinations. In some of these pyrexial cases the bacilli were very abundant, but in others, though the sputum was abundant, bacilli were few, and this in spite of numerous careful observations. In one patient under my care, a case of acute tuberculo-pneumonic phthisis, which terminated fatally in ten weeks, the temperature ranged from 100° to 103° F. Cavities formed in both lungs, and the expectoration was fairly abundant. Bacilli were present, but always in small numbers. In another case of somewhat acute disease, where a cavity existed in one lung and a second one was forming in the opposite lung, the temperature ranging from 99° to 103° F., it was only after four trials that any bacilli whatever were detected. In some chronic and quiescent cavity cases the bacilli have been found in fair number, and even abundantly. In two cases of hæmoptysis, under the care of Dr. Tatham, bacilli were detected in the blood expectorated. With regard to the proportion present during periods of quiescence or arrest of the disease, I should regard their total disappearance as an eminently favourable sign. Out of four cases of contracting cavities where very favourable changes were progressing, bacilli were detected in three, but in small numbers; in the fourth case, above mentioned, none were found. Therefore we are hardly justified in concluding that there is any definite ratio between activity of disease and number of bacilli, though as rule they are few in cases where the disease is quiescent.

Now, what bearing has the bacillus on the question of the contagion of phthisis? And to answer this question we must ascertain whether they exist in the air exhaled by consumptive patients, as well as in their sputum. Dr. Ransom has found them in the air of a room containing several advanced cases of phthisis, and they have also been detected in a respirator worn by a patient by Dr. Charnley Smith.

¹ Made at the meeting of the Medical Society of London, Feb. 12th, 1883.