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AN ADDRESS ON
PNEUMONIA,

DELIVERED AT BIRMINGHAM, APRIL 24, 1884,
BEFORE THE BIRMINGHAM AND MIDLAND COUNTIES BRANCH OF THE
BRITISH MEDICAL ASSOCIATION.

BY

J. BURNEY YEO, M.D., F.R.C.P.,

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HONORARY FELLOW OF KING'S COLLEGE, ETC. ETC.

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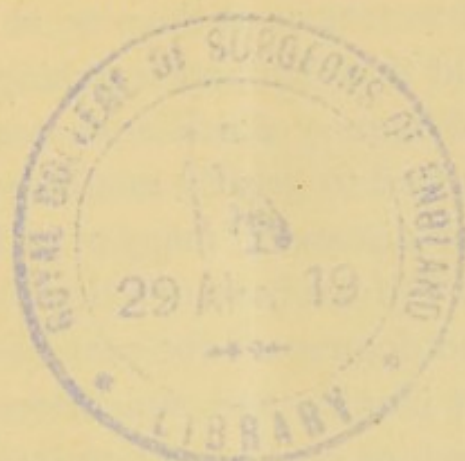
THE HISTORY OF THE
CITY OF BIRMINGHAM

J. B. WHITE, YET. M.D. 1842.

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AN ADDRESS ON PNEUMONIA.*

BY J. BURNEY YEO, M.D., F.R.C.P.,
PHYSICIAN TO KING'S COLLEGE HOSPITAL.

Mr. President and Gentlemen,

When I was asked to give an address for the purpose of opening a discussion on Pneumonia before this, one of the oldest and most influential branches of the British Medical Association, I could not but be sensible that amongst the many distinguished members of this Branch, and especially among the distinguished men who represent our profession in this important town, there were many who were better qualified to perform this task than I am. And if, for a moment, I hesitated to accept your invitation, it was for this reason alone, and not from any lack of interest in the extremely important subject you have selected for discussion.

The pathology, and still more the treatment of pneumonia, have always been debateable subjects, and it is of undoubted advantage that the fruits of progressive research and recent additions to our knowledge should, from time to time, be submitted to such examination and scrutiny as wise and temperate discussion affords.

* Delivered at Birmingham before the Birmingham and Midland Counties Branch of the British Medical Association, on April 24th, 1884.

This was never so true as at the present time. For no one can doubt that the keen and searching spirit of positive experimental inquiry which animates the pathology of our day is destined to greatly modify our conceptions as to the nature of disease, in many of its forms ; that we shall have to break (if we have not already done so) with the traditions and dogmas of the past, so far as they act as fetters to free inquiry and supporters of contented routine ; that much of our vague terminology and many of our hypothetical doctrines are about to disappear ; and that we are drawing nearer and nearer to a knowledge of the nature of many of the most widely-spread and gravest maladies, truer and more precise than any we have hitherto possessed.

This active spirit of positive, experimental inquiry, as applied to pathology, bids fair, I believe, to make our present era one of the most remarkable and one of the most illustrious in the history of medicine.

The mistaken attempt to base a rational pathology exclusively on morbid anatomy, upon the anatomical analysis of morbid changes, has been pushed, perhaps, to its extreme limits ; and without wishing to detract from the value of the immense stores of information it has accumulated, it must be admitted to have failed to contribute to our knowledge of disease in a degree at all adequate to the labours it has involved. It has not inaptly been compared to searching for the causes of a conflagration amongst the ashes and ruins it has left. Yet pulmonary pathology must ever remain deeply indebted to these anatomical studies, and, in particular, for having enabled us to differentiate between many forms of pulmonary consolidation and infiltration, which we could not otherwise have accurately distinguished.

But it is with the results of experimental inquiries in

connection with pneumonia that we are now especially concerned. With the application of that method to the study of pneumonia we have entered upon a new phase in the pathological history of that malady, and it becomes necessary that we should ask ourselves whether the conceptions that have hitherto prevailed as to the nature of that disease are correct or erroneous. It is a subject of the widest and most general interest, and one which has, I think, been wisely selected by this Association for the purpose of Collective Investigation, and by this important Branch for immediate discussion.

It may be well if, in the first place, I endeavour to set precise and definite limits to the subject matter of our inquiry. The subject we propose to examine is *acute lobar pneumonia*, the so-called croupous pneumonia of German authors. We are not now concerned with other forms of pulmonary consolidation and infiltration; we are not concerned with lobular pneumonia, or with chronic pneumonias, or with the so-called secondary pneumonias which arise in the course of many acute and chronic maladies, and with regard to which it has been well said, that most of them "do not belong to the category of true pneumonia, but are either local inflammations, caused by some abnormal state which the pre-existing disease has induced, or conditions of hyperæmia and collapse, in which an inflammatory process plays but little part."—(T. H. Green.) We do not, of course, exclude cases of *inter-current* pneumonia, *i.e.*, cases of true lobar pneumonia, attacking independently a person who may already be the subject of existing disease.

So, also, with respect to the consideration of acute pneumonia itself, there are certain points which it would be quite superfluous to enter into before an assembly like

this. I do not, therefore, propose to discuss either its clinical course, its symptoms, its physical signs, or its morbid anatomy. I propose to limit my observations exclusively to its pathological nature, its etiology, and its treatment.

And first with regard to its pathological nature.

It will be best, perhaps, that I should state at once what is the latest assertion that has been made on this head, and the grounds upon which it is based. It has recently been asserted that acute croupous pneumonia is a parasitic disease, and that it is caused by the entrance into the body of a peculiar and characteristic micro-organism.

In order to establish the fact that a given disease is due to the presence of micro-organisms, it is held to be necessary—

1.—That an organism of definite form and characteristics shall always be found in the blood, or in the diseased organs.

2.—That the blood, or the diseased tissue, when introduced into another animal of the same species, shall produce the same disease.

3.—That when the blood or diseased tissue is inoculated on a suitable soil outside the body, the micro-organisms contained therein shall grow and be propagated indefinitely on similar soil.

4.—That the organism thus separated from the remains of the material in which it was imbedded, thus isolated, when inoculated into a healthy animal, shall re-produce the same disease, and the same organisms shall re-appear in the diseased parts.

It is maintained that in pneumonia, as in anthrax, in septicæmia of the mouse, in erysipelas, in tuberculosis, and in glanders, all the conditions required to establish

the fact that a given disease is due to the presence of a micro-organism have been fulfilled. I will endeavour to set before you, as briefly as possible, the history of this discovery.

So long ago as 1875, Klebs asserted that pneumonia was a parasitic disease, and that he had found, after death, in a case of this disease, a peculiar micro-organism which he termed "*monas pulmonale*." He found it in the contents of the bronchi, in the hepatised lung, and in the fluid of the ventricles of the brain. Eberth was the next to describe a micro-organism in pneumonia, which he had observed in a case complicated with meningitis. Subsequently, Koch stated that he had found a similar organism in the pulmonary and renal capillaries in a case of pneumonia. But Friedlander has the credit of first accurately differentiating this organism, and describing its specific form and characteristics. He found these typical micro-cocci in eight cases of croupous pneumonia, which he examined after death. He found them in the alveolar infiltration, amongst the leucocytes and red blood corpuscles; and some of the lymphatic spaces on the surface of the lung were so stuffed and bulged with these micro-cocci, that they were visible to the naked eye. Günther, of Berlin, found the characteristic organisms in some fluid he withdrew from the chest of a moribund patient. Leyden, of Berlin, contrived, by means of a hypodermic syringe, to withdraw a few drops of blood from the lung of a pneumonic patient, thirty-six hours before death; the puncture was made just below the scapula. The blood was examined by the dry method, and after staining with methyl. blue, the organism was seen in the most characteristic form, as described by Koch and Friedlander.

It may be remarked that there is much difficulty in

obtaining fluid from the lung by this method, and that Leyden attempted it in several other cases, with negative results, and now that the organism has been found in the expectoration, it is no longer needful.

These organisms—specimens of which, by the courtesy of Dr. Dreschfeld, I am able to show you—as described by Friedlander, are micro-cocci of elliptical form, one micro-millimetre in length, and one-third less in breadth. They occur connected together in pairs, as *diplo-cocci*, and frequently they form longer chains: but these chains are distinctly seen to be made up of a series of pairs, a series of diplo-cocci. They are characterised and differentiated from other micro-organisms found in the body by *being surrounded by a capsule*, which is thought by Friedlander to consist of mucin. The capsules, however, are not always present, and are not easily demonstrated. They are thought to be a product of the life of the micro-coccus.

A similar organism, only a little larger, has been found by Kobner, of Berlin, in the lungs of cattle in an epidemic of bovine pleuro-pneumonia; and it has also been found in the lungs of horses similarly affected. Leyden states that there is a certain analogy between the micro-organism of pneumonia and that of erysipelas. Several observers have succeeded in making *pure* cultivations of this organism. It has been cultivated in gelatinised meat infusion, and on potato.

In the gelatinised meat infusion, this micro-coccus grows in a peculiar manner. It grows partly on the surface, and partly downwards into the gelatine; so that it has the form and appearance of a pin, with the head on the surface. On potatoes, it grows in the form of small grey drops; and if again inoculated on the gelatine, it again grows in the form of a pin. When cultivated in

this manner, these micro-cocci are oval or elliptical, and resemble precisely those found in the lung, but they have no capsules.

I need not describe in detail the steps necessary to obtain a *pure cultivation*, i.e., a cultivation of this particular organism, apart from others with which it may be found associated. I may, however, call attention to the extreme importance of these details, and to the necessity, on that account, of leaving these cultivations, for the present, in the hands of experts.

An interesting communication on the "Pathogenesis of Pneumonia" appeared last year in the British Medical Journal, by Dr. Giles, but he does not appear to have followed the method prescribed by Koch and others in his attempts at cultivation; and as he cultivated from sputum, which is generally crowded with other organisms (I have seen four or five different organisms in the same specimen of pneumonic sputum), it is certain he must have operated with other than pure cultivations. Moreover, the organism he describes differs altogether in its characters from that of Friedlander and others. He found also, his cultivated organism fatal to rabbits, whereas the *pure* pneumonia micro-coccus does not affect these animals. Further, it has been shown that the saliva of healthy men sometimes contains an organism which is fatal to rabbits. These facts show how necessary it is to be extremely cautious in working with cultivated micro-organisms.

The *pure* organism obtained by Friedlander and others by cultivation, has been introduced into the bodies of animals, usually mixed with distilled water and injected into the pleural cavity. On 9 rabbits it produced no effect; but it proved fatal to 32 mice in from 18 to 20 hours. The organism was found in enormous numbers in

the engorged and hepatised lung, in the blood, in the spleen, and in the fluid of the pleura. In the latter situation the *diplo-cocci* were encapsulated. When the same fluid was heated so as to destroy the organism, its injection produced no effect. Cultivations from these mice produced the same effect on other mice. Guinea-pigs were less sensitive; out of 11, in 6 only was pneumonia produced. Of 5 dogs so treated only 1 died; that one had typical pneumonia and pleurisy; the other 4 showed a slight rise of temperature and temporary indisposition only.

An attempt was also made to introduce the organism into the lungs of mice by *inhalation*; the fluid containing the micro-cocci being diffused by a hand spray into their cage. In this experiment some mice were affected, some were not. In the case of the latter, it is suggested that but little of the fluid entered the lungs, as they turned their backs to the spray. But there is another consideration which has been somewhat overlooked in connection with the inhalation of pathogenic organisms, and to which I may take this opportunity of calling attention. It is, that under normal conditions the suspended solid particles in the atmosphere taken in with the *inspired* air, are cast out with the *expired* air. The mucus of the buccal and pharyngeal mucous membrane is crowded with micro-organisms, but these, the ciliated epithelium of the air tubes keeps from reaching the air cells; and they, probably, penetrate but a short distance into the air passages with the *tidal* air. This was well-shown in a case of empyema communicating with the lung, which was under my own and Sir Joseph Lister's care, in King's College Hospital, last year. For though there was an opening in the lung permitting the periodical ejection of large quantities of pus, the pus in the pleural cavity

remained perfectly sweet and quite free from putrefactive organisms ; which was no doubt due to this protective office of the bronchial ciliated epithelium.

It has, no doubt, been a difficulty with many in the way of accepting the view that diseases like phthisis and pneumonia are dependent on the inhalation from the air of special pathogenic organisms, that so many escape infection who must, it would seem, occasionally inhale these bodies. But apart from the consideration that the causes of these diseases are complex, and cannot consist of a single factor, we must give due weight to the very obvious reflection I have just stated, viz.—*that in the respiration of most healthy persons, whatever solid particles floating in the air, pass into the lung in inspiration, pass out of it in expiration ; that the air ordinarily exchanged in respiration is the tidal air, not the air of the air cells, and that whatever organisms may penetrate beyond the limits of the tidal air, into the air passages, are arrested and driven back by the action of healthy bronchial ciliated epithelium.*

Now it is possible that one of the factors in the causation of these diseases may be, for instance, *a temporary or permanent paralysis of the ciliated epithelium*, or a denudation of this protection from a portion of the air passages. Or some abnormal condition of the mucous secretion may either, on the one hand, paralyse these cilia, or on the other, *stimulate these germs to overwhelming multiplication.* When we consider that the bacillus which causes septicæmia in the house-mouse is unable to produce any deleterious effect in the field-mouse, we must be struck with the very special nature of the conditions which determine the growth and activity of these organisms.

Considerations such as these dispose also of some of

the objections that have been made, especially by Parjesz, of Klausenberg, to Friedlander's view with regard to the organism he has named the pneumonia micro-coccus. Parjesz says he has found it in the sputa of persons wholly free from pneumonia. But assuming he has been correct in his recognition of the organism, it would seem that this must, necessarily, be sometimes the case, unless we were to maintain that every person who inhales this particular organism *must* have pneumonia; a proposition which no one, hitherto, has been so rash or so foolish as to make. With regard to the other objection made by Parjesz—that he has examined the sputa of many cases of croupous pneumonia and not found this organism—to this it might be replied, that it is admittedly very difficult to find these organisms in the sputum, especially in mild cases, owing to the presence in large numbers of other organisms which may conceal its presence; and that the evidence of one isolated observer, when it is in opposition to that of many others, can never have much weight.

But a remarkable confirmation of Friedlander's observations is contained in an article by Dr. Rudolf Emmerich, of Munich, in the "*Fortschritte der Medecin*" of March last. The medical officer of the prison for men, at Amberg, had been struck by the recurrence, year by year, since 1857, of a considerable number of cases of pneumonia amongst the prisoners, which occasionally assumed an epidemic character. In the year 1870 there had been as many as 66 cases; and the last outbreak, in 1880, was a very malignant one, and lasted from January to the middle of June, during which there had been 161 cases, of which 46 had proved fatal. After weighing every probable cause, he was led to believe that the true cause would be found in the dormitories, for they were all

attacked there, and in varying proportions from 4.7 to 25.5 per cent. He therefore concluded that the cause of the disease was more active in some of the dormitories than in others. After much reflection and examination, it occurred to him that the material which was used to fill up the space between the floors and the ceiling might be a possible breeding-place for a pathogenic organism. Some of this stuffing, consisting of a mixture of sand and mortar, was removed and sent to Emmerich for examination. It was examined chemically, and also by Koch's method of culture, for micro-organisms. Without entering into details, I may say that Emmerich observed, amongst other organisms which he obtained by cultivation of this material, what he regarded and spoke of as a peculiar pathogenic micro-organism; but he had not at that time seen Friedlander's description of the encapsulated pneumonia micro-coccus. When, however, some months after, he became acquainted with Friedlander's paper and drawings, he at once recognised the pneumonia micro-coccus as the same in every respect as the peculiar organism he had found in the stuffing from the prison floors.

In his former observations, he had not been working with pure cultivations, but he now set to work with more of the same material, and by following the necessary details, obtained pure cultivations of an organism resembling in every respect, in its form and size, and behaviour when injected into animals, that described by Friedlander. In this manner it was proved that the material found stuffing the space between the floor and ceiling of the dormitories of the prison where epidemic pneumonia had so long been, as it were, domiciled, was crowded with enormous masses of the peculiar organism which has been stated to be pathogenic of pneumonia.

I have received a letter from Dr. Dreschfeld, of Manchester, referring to some very important and interesting observations which he has been able to carry out in connection with a peculiar form of pneumonia now, or lately, prevalent in Manchester; a specimen from one of these cases he has been good enough to send with the three others before you. I am sorry his letter reached me too late to embody its contents in this address. But I may, perhaps, have an opportunity of alluding to it again in the discussion; at any rate, I am glad to know that his cases will be published, and will form a most valuable contribution to the subject of the etiology of pneumonia.

The observations I have thus briefly brought under your notice, naturally lend great weight to a view of the pathological nature of certain forms of pneumonia which is by no means new; for many writers in this and in other countries have expressed a suspicion that pneumonia sometimes occurs as an infective and contagious malady, and numerous instances of its epidemic occurrence are on record; while many who would be unwilling to admit its infectious or contagious nature, are convinced of its occasional occurrence in what is called a "pythogenic" form.

I should like to call your attention to a few of what appear to me to be the most instructive instances on record of the supposed epidemic occurrence of pneumonia, and its supposed propagation by contagion. And I will first refer to an account given by Stokes of an outbreak of what he terms "asthenic pneumonia," in Dublin, the cases presenting so marked a typhoid character that he was evidently doubtful whether he was right in calling them cases of pneumonia, though the early occurrence of pulmonary consolidation was undoubted; but the clinical thermometer had not then come into use, and he was

without that means of diagnosis. He says, "the occurrence of this disease, as affecting great numbers in a particular locality, was observed in this city some years since. The persons attacked were *young* and *healthy* men, privates in the constabulary force, who were quartered in the then newly-erected barracks in the Phoenix Park; these young men were well fed and clothed, and might be considered as possessing the greatest strength and vigour. It is a remarkable circumstance that at the time of the appearance of this disease, many cases of another and extraordinary affection was observed in the poor-houses and hospitals in and near Dublin. I allude to cerebral-spinal arachnitis, and it is important to mention that several cases of this disease occurred in the force contemporaneously with those of pneumonia. The general characters of the pneumonia were suddenness of invasion and great rapidity of progress; the lung rapidly passing into hepatisation."*

Another interesting account of an epidemic of pneumonia is given by Surgeon-Major Costello, of the Bengal Medical Department. On joining the 1st Punjaub Infantry, at the end of March, 1875, he found a state of alarm there on account of the number of deaths that had taken place from pneumonia, at a time, too, when the temperature was warm and equable. The regiment had lost from 30 to 40 men out of 550, in a few weeks. It was almost confined to two companies, and when it appeared amongst the married, it spread to different members of the family, and when cases were taken into hospital, it spread to other patients, ill of other diseases, and to the nurses and attendants. There was always a catching pain in the side, the infiltration spread rapidly

* Stokes on Diseases of the Chest. Sydenham Society's Edition, p. 303.

through the lungs, and was attended with "typhoid" symptoms. Removal from barracks to tents, isolation of affected persons and their attendants, immediately arrested the progress of the disease. The 3rd Punjaub Infantry lost 60 men from a similar outbreak. He believed it was caused by marching through districts known to be affected with bovine pleuro-pneumonia.*

But to come nearer home :—A very remarkable series of cases, pointing to direct contagion, have been recorded by Mr. Patchett, of Shaw, near Oldham.† A whole family of five persons died, one after the other, in less than a fortnight, of typical and uncomplicated pneumonia. Four brothers and a sister, all unmarried, lived together on a farm, where they had resided all their lives, which was well and healthily situated on a steep hill-side, and was in excellent sanitary condition. There was no suspicion of septic influence. The previous health of all the family had been exceptionally good.

The first case seen was on January 13, 1876, the eldest brother, aged 73 ; he died on the 16th, after an illness of six days, with pneumonia of the right lung. The day of his death another brother, aged 66, was found also to have right-sided pneumonia, which extended to the left lung, and he died in three days, on the 19th. On the morning of January 20th a third brother, aged 63, who was quite well and at work the day before, was found with signs of pneumonia, at both bases. On the evening of the same day a fourth brother, aged 64, felt shivery and cold on going to bed ; the next morning he had double pneumonia, and both died on the evening of the 22nd. The sister, aged 61, who had been in attendance on all the brothers, and appeared to keep up her health

* Lancet. Jan. 29th, 1881.

† Lancet. February 25, 1882.

and strength remarkably well, was seized on the 23rd with right pneumonia, and died on the 26th.

This is, surely, a very remarkable record. But there is another, scarcely less so, by Dr. F. H. Daly, of Hackney Downs.*

In October, 1879, there occurred in the same house, and within a few days, four undoubted cases of pneumonia; and these followed an illness in another child, who had not been previously examined by Dr. Daly, but who was now found to show signs of recent lung consolidation. So that there were five members of this family, four children and the mother, who nursed them. The mother died, and the grandmother, a healthy woman, 60 years of age, who came to nurse her daughter, fell ill of pneumonia, and also died. There was no question here of insanitary conditions—no overcrowding, or bad ventilation, or bad drains; there had been no chill, and no prolonged exposure. The husband and three servants, who were but little in the sick room, escaped. "My cases," Dr. Daly remarks, "exactly resembled, in their mode of attack, case following case, and progress, some zymotic disease, contagious from one patient to another."

Several instances of the apparent communication of pneumonia from person to person have been reported to the Collective Investigation Pneumonia Sub-Committee, and these will be duly set forth in our final report.

Many instances from the time of Courvoisier, Lænnec, and even earlier, are also on record of the epidemic occurrence of pneumonia; cases of pneumonia occurring together in groups, larger or smaller, and in which the suspicion of spread by contagion has not been altogether absent. These instances appear to have occurred independently of any common meteorological conditions, but

* *Lancet*. November 12, 1881.

they have, in many cases, appeared to be in some way connected with local insanitary surroundings. A good example of the latter is the account given by Penkert* of an outbreak of croupous pneumonia in a village of 700 inhabitants, 42 of whom were attacked and 2 died, between the 11th of March and 14th of May, 1881.

The village consisted of one street running down the side of a hill, at the bottom of which was the School-house, and opposite this, the new Cemetery; below and at the lowest level of the village was a small pond. The first cases occurred in children going to this school, which was so placed as to receive the emanations from this pond in a straight line. There was at the same time a rise of sub-soil water on account of previous heavy rains, as well as a considerable rise of temperature, which had previously been very low. There was also a north-west wind which blew from the pond towards the school and other buildings. Penkert thought he could trace direct contagion in 28 cases, and indirect, by the agency of an intermediate person, in 4.

The occurrence of four such epidemics is mentioned in the Provisional Report on Pneumonia in the "Collective Investigation Record." One occurred at Holmwood, Surrey, and is reported by Mr. E. L. Jacob; one at Long Handborough, by Dr. Gilbert Child (a very interesting report); and one by Dr. Gooch, of Eton. In the latter only was the outbreak supposed to be distinctly traceable to defective drainage and sewer-gas poisoning. Other similar instances will no doubt be classified and analysed in the forthcoming final report.

I must not omit to draw your attention to a very interesting and, I think, important communication on an epidemic of pneumonia, by Dr. Bruce, of Dingwall, N.B.†

* Berlin Klin. Wochensch. 1881, 40, 41.

† Brit. Med. Journal. August 11, 1883.

He records 16 cases occurring between May 12 and July 3, with 3 deaths; and divides them into three groups of 8, 4, and 2; the other cases were scattered. As to its origin, he observes—"With some minds the facts as detailed will doubtless lead to the conclusion that the theory of infection or specific germs will alone account for the peculiar outbreak; whilst others will look to an epidemic influence. Certainly the weather was excessively cold, ungenial, and dry, with nightly frosts, about the time the disease prevailed most largely. On the other hand, I heard of some cases some miles away from this district before I saw it myself; and it will be observed that some of the cases occurred later (in June and July), when the weather was seasonably warm. Again, there were three sets of cases, occurring in groups of three each, and one set of two—a very remarkable set—which are very difficult to explain on any other theory than that of direct infection."

In the same number of the *British Medical Journal* is an interesting account of a small epidemic observed by Mr. Raven, of Broadstairs, and which he believed to be traceable to a distinct "pythogenic" cause. The points of special interest in his cases were, he considers, "the absence of typhoid symptoms, and the delay that occurred between the onset of the fever and the development of the lung affection."

You are doubtless aware that the question has been raised, whether these so-called epidemics of pneumonia may not be, in some instances, cases of typhoid, with early development of lung consolidation; and this is a question which may well be inquired into in our discussion. This suspicion occurred to Stokes, in the epidemic of "asthenic pneumonia" recorded by him, and he points out the distinguishing characters which appear to him to separate these cases from typhus. It has also

been observed that epidemics of pneumonia succeed epidemics of typhoid, and epidemics of typhoid succeed epidemics of pneumonia; and it would seem that the presence of putrescent matter is favourable to the culture of both germs.*

In an epidemic observed by Kühn,† at Moringen, in Hanover, the approximation to typhoid in some of the cases was well marked. The cases occurred in an overcrowded prison; 33 cases were observed in one epidemic, and 70 in another. They differed from those of typical pneumonia—there was no initial rigor—and the onset was gradual; the intestinal follicles were sometimes found swollen, and diarrhœa was present in two-thirds of the cases. It was distinctly infectious, and spread to the attendants.

The cases also observed by Dr. Giles, in the Punjaub, appear to differ from the typical form of acute pneumonia, as seen in isolated cases in this country. He says, "a large number of cases, while running the same course, as to pyrexia and general symptoms, as seen in the typical acute pneumonia of Europe, are yet, as to physical signs, instances rather of *lobular* than of acute croupous pneumonia. The cases often run a very rapid course, and death sometimes occurs in twelve hours, with signs of pulmonary engorgement, but no pronounced dulness; and on examination after death, only deep congestion of lungs, and often extensive pulmonary apoplexy."

I have here brought before you but a small fragment of the mass of observations which are on record, and upon which is based the view that pneumonia is an

* Alison. Archiv. Générales de Médecine. Sept. and Oct., 1883.

† Deutsch Arch. f. Klin. Medicin. 1878. xxi., 4. Berlin Klin. Wochenschr. 1879. 73.

infective disease dependent on the presence of a specific pathogenic organism in the body ; and that under certain, somewhat rare circumstances of time, place, or season, it may spread by direct and indirect contagion.

I have thought it best to bring this view of the nature of pneumonia before you, *first of all*, in order that we may the better be able to compare it with the older and more generally accepted views as to the nature of this disease, and see how it is related to them. Probably the most widely accepted view of the nature of pneumonia is, that it is a typical local inflammation, produced by exposure to cold, and that the accompanying pyrexia is merely symptomatic of the local lesion.

Another view, which if not quite so widely accepted as the preceding, is certainly quite as authoritatively maintained, is, that acute pneumonia is a *general disease*, and the lung inflammation is simply the chief local lesion. The inflammatory process in the lung is the *local* effect of a *general* cause. The usual arguments in support of the latter view are these:—That in well-marked cases the fever does not run parallel with the physical signs of pulmonary inflammation. It frequently precedes them by a considerable interval. It does not coincide with them in degree or duration. High fever often accompanies a small tract of inflammation when it is situated at the apex of a lung instead of at the base. The fever often suddenly subsides long before the local signs show a corresponding improvement in the lung. Moreover, it resembles the *specific fevers* in its typical course—the rapid onset, the sudden defervescence. Finally, the anatomical changes in the lung are not such as can be produced by artificial injury of that organ.

It is interesting to look back just a quarter of a century and see how such a philosophically minded physician as

was the late Dr. Edward Parkes, felt exercised in presence of these two views as to the nature of pneumonia,* and with less perfect light than we now possess, how difficult he finds it to decide which view is the right one; though he clearly seems to lean towards the latter. After alluding to the view "that the lung disease is not a primary but a secondary condition, and that it succeeds to and brings to an end, by purifying the blood, a condition of general pyrexia, arising from blood disease," he adds, "without believing that this relation is quite determined (if it were determined the case would be settled), there is no doubt that the fever ends spontaneously, or very greatly lessens, at the time when the inflammation of the lung is very great." "The weak points," he continues, "in this hypothesis, are the want of definite indication of the blood disease, and of its mode of production." Weak points no longer since Friedlander's discovery. "Its strong points are the explanation it gives of the previous malaise; of the sudden outburst of fever, when the diseased blood, at last, implicates the nervous system; of the singular and rapid termination of the pyrexia at a time when the lung lesion is yet intense; and of the enormous elimination of urea during the very first days, before the lung exudation has softened down."

Then he goes on to state the arguments in favour of the first view:—"That the lung symptoms are remarkably early in manifestation, though they may not be very intense. Pain in the side, and cough, are very soon present, and sometimes occur even before the shivering and headache. The pyrexia, although great in the early days, is perhaps not greater than might have been produced by the condition of the lung; and as to the termination of the fever, this may be supposed to occur because

* Clinical Lectures. *Medical Times and Gazette*. 25th February, 1860.

the really truly febrile stage of the pneumonia is *not* the period of complete exudation, but the preceding period of intense hyperæmia. To say that the pyrexia is gone, when the lung lesion is yet most intense, may be an incorrect expression of the fact: the consolidation may possibly, indeed, be most intense, but this may be merely the natural termination of that enormous hyperæmia and blockage of vessels from local changes of nutrition, which is in reality the essential disease. The difference between the two hypotheses would be this:—The fever ends spontaneously; first, because the blood is purified; or secondly, because the local disease ends spontaneously; *i. e.*, the active febrile-making local disease. *This last assumption, however,* he adds, *"is decidedly a very bold and hazardous one."* Having thus stated the two views, and the arguments in support of both with characteristic fairness and impartiality, he adds—"Between these two views it is not very easy, nor perhaps is it desirable, yet to choose, for the blood has not yet been sufficiently examined." This was five-and-twenty years ago. Had Parkes been alive now, no one, I feel sure, would have taken a greater interest in the recent experimental inquiries into the nature of pneumonia than he would have done.

Of these two views, it will be seen that the one is entirely opposed to the theory which regards pneumonia as a parasitic disease, infective, sometimes contagious, and not infrequently epidemic; whereas the other view gains support from and is quite consistent with the belief in the parasitic nature of this disease. But there is yet another view which merits our attention, *viz.*, that pneumonia is *sometimes* an inflammatory and purely local disease, and *at other times* an infective, general disease. This view has been advocated by Dr. Wynter Blyth.* After

* Lancet. Sept., 1878. p. 416.

alluding to some interesting cases of apparent communication of pneumonia mentioned to him by Dr. Christian Budd, of North Tawton, he says, "in all probability there are *two* forms of pneumonia: one probably arising from cold, or a like cause, the other zymotic; and these two forms are, at the present time, confused together, just as typhus and typhoid were. That such an epidemic form exists, infectious, zymotic, self-propagating, I feel in my own mind convinced."

I must own there appears to me to be much that may be said in favour of this *eclectic* view. When we consider what diverse conditions are capable of setting up inflammation and consolidation of the lung, when we consider in what various morbid states secondary pneumonias occur, it is surely not inconsistent to believe that *primary* pneumonia may arise from more than one exciting cause. But even if we admit that pneumonia is caused by exposure to cold, it does not necessarily follow that the *chill* acts directly on the lung; and it is still open to us to believe that the blood is primarily affected, just as it may be in acute rheumatism. But the prevailing want of symmetry in the lung affection would seem to point to the co-operation of some more strictly local influence.

Those, however, who maintain the unity of pneumonia—and so great an authority as Jurgensen is amongst the number, although, if I understand him aright, he would separate certain so-called typhoid forms as not croupous pneumonia at all, but only secondary lung consolidation dependent on septicæmia—those advocates of the unity of pneumonia, notwithstanding the very varying degrees of intensity with which it appears in different periods, contend that these differences are due to the different degrees of activity or malignancy of the exciting cause, of the morbidic germ, at different times

and in different places ;* and they urge, in support of this view, that great changes in the virulency of various pathogenic organisms have been shewn under cultivation ; that well-recognised infectious diseases, like scarlet fever, appear at different times with very varying degrees of virulency ; that the organism characteristic of pneumonia has been found in mild as well as in severe cases ; that an extreme virulency has been observed in cases which *post mortem* examinations have shewn to be cases of true croupous pneumonia. For instance, Jurgensen reports a case that ended in eight hours from the initial rigor, and Mendelsohn mentions two cases, one that of a vigorous man, 38 years of age, who died in thirty-four hours, and another case, communicated to him by a friend, that died in twenty-four hours.

What I have already said with respect to the pathological nature of pneumonia leaves me little to say with regard to its etiology. I have, necessarily, anticipated much of this part of the subject.

Most observers agree that pneumonia sometimes occurs in connection with insanitary conditions, that, in short, there is what has been named a "pythogenic" pneumonia. I am not so fond of the word "pythogenic" as some seem to be. It appears to me to be not altogether free from the charge of being what has been termed a "question begging epithet ;" and medicine has too many such terms in common use. We understand, however, what it means, and may use it provisionally.

But what is the real influence of exposure, of chill, as a cause of pneumonia? Is it simply a predisposing cause, or is it an exciting cause? That it has a causal relation of some sort to many cases of pneumonia, there seems to me to be no reason to doubt. I cannot, in an

* Mendelsohn. *Zeitschrift fur Klinische Medicin.* 1883.

address of this kind, bring before you the various statistical statements that have been published for and against the view that exposure is a common cause of pneumonia. You will find them set forth in the elaborate articles on pneumonia in Ziemsen's Cyclopædia, and in Reynolds' System of Medicine. That we have been in the habit of over-rating its influence as a cause of pneumonia is very possible. It is notorious that pneumonia often occurs during the prevalence of winds, especially the winds of spring; and not with winds from any particular quarter, for it is found to prevail with south-west winds as frequently, or more so, as with east or north-east winds. Now, in this connection, we have rather overlooked the fact, that winds are carriers of dust as well as conveyers of cold (or rather, abstractors of heat); and that while, on the one hand, they carry away heat from the surface of the body; on the other hand, they gather up dust of all kinds, and blow all manner of micro-organisms into our air-passages. It has been noticed, again and again, that all depressing agencies may predispose to pneumonia, such as exhaustion from physical fatigue,* and depressing emotions; and it may be that exposure to a cold wind acts both as a predisposing cause, by the depression of the normal resisting power it produces by rapid abstraction of heat, and also as an exciting cause, by means of the micro-organisms it blows into our air-passages; especially if the view I have already suggested be correct, *that a paralytic or sub-paralytic condition of the bronchial ciliated epithelium* may in this way be induced.

A French writer whom I have already quoted found in the analysis of 80 cases of pneumonia that physical fatigue was apparently a predisposing cause in a great

* Alison. Archiv. Générales de Médecine. Sep. and Oct., 1883.

proportion of them. And I have heard it said that pneumonia occasionally occurs in public school boys after fatiguing games of football. It would be interesting to have further information on this point.

I have left myself little time to speak of the treatment of pneumonia, and what I have to say I must state very briefly. Personally I am content to be what, in political language, is called "an opportunist," in the treatment of pneumonia. I believe in no special treatment, but I believe in a rational, common sense management of individual cases. We most of us know that many cases of pneumonia simply require careful watching and nursing through their malady; and I have seen, in cases so treated, or rather managed, convalescence progress very rapidly. We most of us also know of other cases which tax severely all the resources of our art, and often baffle them all. I may say, also, for my own part, I am entirely opposed to all lowering methods of treatment. I think they are counter-indicated by the tendency to cardiac asthenia which the concomitant pyrexia induces; and also by the consideration that those cases that support lowering measures best, are precisely those cases which do extremely well without any active interference.

There are, however, certain circumstances, certain accidents of the disease, the treatment of which requires careful consideration. These are—

1.—*Excessive dyspnœa* from overwhelming pulmonary hyperæmia and œdema, in addition to the consolidation. This is the only indication I know of for bleeding in pneumonia, and its occurrence is exceedingly rare.

2.—*Excessive pyrexia*, attended by delirium and danger of rapid cardiac exhaustion. Here the cautious application of cold to the surface (not in hyper-sensitive, nervous organisations, which, I venture to think, bear this treatment

badly) may, I believe, be used with benefit, as advocated by Jurgensen; but I am very certain that we should select our patients very carefully for this mode of treatment. I am also inclined to think that we use quinine too little in pneumonia as an a-pyretic. Sulphurous acid will also undoubtedly reduce the temperature in some infectious diseases. Quinine combined with sulphurous acid merits more frequent use for this purpose in low typhoid forms of pneumonia. They should be given from the *commencement* of the illness.

3.—*Pain in the side.* I am in the habit of giving 5 grain doses of Dover's powder every 5 or 6 hours, in the *earliest* stage of pneumonia, to relieve pain, and I find it has an excellent influence, not only *directly*, in the relief of pain, but *indirectly*, by relieving the distress and anxiety of the patient and procuring calm sleep, and so really sparing his strength and protecting him against exhaustion later on. By relieving the pain in the side he is also enabled to make deeper inspirations, and so to make the best use he can of the breathing surface that is left free to him for aerating his blood. Chloroform inhalation has been advocated by Oertel* for the same purpose, and the late Sir James Simpson used to think highly of its use in pneumonia. A few leeches applied over the seat of pain is also a valuable expedient.

4.—*The tendency to cardiac exhaustion, with sleeplessness and delirium.*—The so-called typhoid symptoms. The free use of alcoholic stimulants are here essential. Many cases of pneumonia require no alcohol; many other cases can only be kept alive by alcohol!

One word on the so-called abortive treatment of pneumonia by aconite. I am not ignorant of the excellent effect of aconite in certain ephemeral pyrexiaë, associated

* Respiratorische Therapie.

with local inflammations; and I believe it to be a valuable remedy in cases of slight pulmonary congestion with pyrexia, a condition which should be more precisely differentiated and described than it has been in our text books. But I have no belief in an abortive treatment of acute croupous pneumonia by aconite.

A consulting physician in London tells me he has seen recently three fatal cases of pneumonia in which aconite was being "pushed." I hope, if there is any advocate of that treatment present, he will forgive me for saying that it is to me inconceivable how it should ever be thought right to "push" such a drug as aconite in such a disease as pneumonia.

These few considerations as to the treatment of pneumonia, I submit with much deference to an assembly like this.

It now only remains for me, in conclusion, to say a few words as to the relation of Collective Investigation to the subject of this discussion. I trust the considerations I have here put before you have not failed to convince you how appropriate a subject is that of acute pneumonia for present investigation.

The statistics and other information obtainable from hospital records are of great interest and importance, but we want information as to the course and incidence of disease amongst those classes which are not found in hospitals, but in private practice; and our knowledge of the natural history of a disease like acute pneumonia would be very incomplete without an analysis of the experience of private practitioners.

You will be glad to learn that the number of 1,000 cases which the Collective Investigation Committee desired to obtain, before issuing a final or rather a complete report has been received, and in hands so competent

as those of Drs. Sturges and Coupland we may look for a Report of considerable value to this Association. But in the present unsettled state of opinion as to the nature of pneumonia, we trust to continue this investigation for some time yet, and we shall hope to continue to receive Reports of cases and the experience of observers, especially in disputed points of etiology; such as its occurrence from exposure to chill, its conveyance by contagion, its appearance in the form of epidemics, and the circumstances to which they may be traceable. Any new experience as to treatment or modes of prevention, will also be of great value.

Some complaints have been made that our mode of procedure is, in certain instances, too elaborate. Personally, I have always been in favour of making our inquiries as simple as possible, but we must not sacrifice *precision* to simplicity. I can, however, assure you the London Sub-Committee are thoroughly convinced of the desirableness of doing away with elaborate details as much as possible. You must please remember that this movement is in its infancy, and we can hardly hope to hit upon the very best possible modes of procedure all at once. With regard to the value of the results obtained, that will depend greatly on the movement spreading, and being adopted by the great body of members of the Association.

It has been said, and that in the way of reproach, that our conclusions will be put forward as "final" and authoritative, on questions which are still open to doubt. I cannot think there is any real danger of this kind: the very word "Investigation" seems to contain a protest against "finality."

We have also been charged with pride and conceit-ness, altogether out of proportion to our prospects of

usefulness. That there is some ground for pride in the fact that this great Association has been able to originate and organise and establish a movement of this kind I, for one, am not inclined to dispute—a movement which endeavours to draw into the work of thoughtful observation and careful investigation of disease every member of this Association. As to the conceitedness of which we have been accused, I would ask those who think investigation productive of conceit to try it. I think I can promise them that the first result will be to show them how many errors they have been accustomed to regard as truths, and that so far from tending to conceit, it rather tends to humility; to what has been spoken of as a “divine unrest,” a divine discontent—the parent of fruitful effort, and the source of increasing knowledge.

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