dependence upon medical advice. As a profession, if not as individuals, we leave untouched treaties with foreign nations, many of the great commercial questions, nay even the unity of the Empire, to minds with a different bias from ourselves. We concern ourselves with the well-being of the masses of mankind, the possibilities of the advancement of the race, the annihilation of all things that militate against the progress of the nation.

To us the poor, the blind, the deaf, the feeble-minded are not merely divisions of the human family to be fed and cared for, but brothers and sisters of our own to be raised still higher in the economy of Nature.

Our thoughts are given to the prevention and relief of suffering; our highest ambition is to leave each generation better than we found it; and by the results of our investigations and by the influence of our lives to form foundations for our successors to rise higher still, and to elevate the race to a point impossible for ourselves, to deserve to have it said of each one of us, as it was of the great Head of our profession, "He went about doing good, healing all that were diseased, for God was with him."

ADDRESS IN MEDICINE,

BY

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*At the Annual Meeting of the British Medical Association held in
Bristol, August, 1894.*

THE NATURE AND TREATMENT OF INFLUENZA.

It is not my intention to ask you to consider to-day any question of medical politics or of medical education, for these are inappropriate to the address in medicine. Nor do I purpose to advance any wide general statements, for these are apt to be vague and wanting in practical utility; nor to lay before you the results of any recent work of my own, for that has not been of a nature suitable for our present meeting, but I desire to take up a subject which has of late years largely occupied the attention of the profession, and with regard to which it may be of some service if we can formulate views and conclusions fitted to throw light upon the nature of the process and its prevention and cure. The recent epidemics of influenza have been studied with a care hitherto unequalled, and by methods which the profession did not formerly possess; and I wish to consider what we have really learned with regard to the nature and treatment of this great morbid process.

REFERENCE TO OLD EPIDEMICS.

It is scarcely necessary to demonstrate nowadays to an audience such as this that influenza is no new epidemic. I shall cite evidence of only two unmistakable outbreaks—one occurring on the Continent and one on our own island. We learn from many sources that an epidemic broke out in Italy, and particularly in Florence, during the months of January and February, 1387, that that epidemic spread rapidly, so that by Lent it prevailed in Strassburg and throughout France, as well as in Southern and Northern Germany. Buoninsegni in his Florentine history speaks of it as "*una influenza*" which affected people with cold and fever, and was fatal to many, especially the old. Velascus of Tharanta observed that young children enjoyed a comparative immunity from its attacks; and Jacob von Königshofen tells us that in the Strassburg district it was marked by cough and running of the nose, that it attacked nine out of every ten of the population, and that it was fatal mostly to old people who were otherwise weak. No one can look into the records of the period without being satisfied that that outbreak was an epidemic of influenza.

For the instance which I select as proving its existence in our own island we are indebted mainly to a shrewd observer who was not a medical man, but a diplomatist, and who

represented Queen Elizabeth at the court of Mary Queen of Scots. This Thomas Randolph or Randall was keen-witted enough to be worthy of working along with Cecil and the other great Ministers of Elizabeth, and one of his letters, dated from Edinburgh in 1562, contains the following passage:

"May it please your honour, immediately upon the Queen's arrival here, she fell acquainted with a new disease which is common in this town, called here 'the New Acquaintance,' which passed through her whole court, neither sparing Lord, Lady, nor damoiselle, not so much as either French or English. It is a pain in their heads that have it, and a soreness in their stomach, with a great cough that remains with some longer with others shorter time, as it findeth apt bodies for the nature of the disease. The Queen kept her bed six days; there was no appearance of danger, nor many that die of the disease, except some old folks. My Lord of Murray is now presently in it; the Lord of Liddington hath had it, and I am ashamed to say that I have been free of it, seeing it seeketh acquaintance at all men's hands. By reason of these occasions I have not seen her Grace since she came to town."

It can scarcely be doubted that this disease was influenza. Its sudden appearance, its epidemic character, its clinical features, its degree of fatality, all correspond to what we recognise as characteristic of that malady. And our ancestors gave it a name as suggestive as the more modern ones, "influenza" and "*la grippe*," for they speak of it as the "New Acquaintance." This is the earliest definite account that I have met with of its occurrence in our island, but since that time it has reappeared at longer or shorter intervals. It has thus been a subject of study to the physicians of each generation. Each successive epidemic has exhibited essentially the same features as those with which we have of late been only too familiar. The question which I wish to consider is, what use we have made of our opportunities, what real advance we have made.

CLINICAL FEATURES.

I must say a few words regarding the clinical history of the disease in order that we may understand one another clearly when we come to consider other more obscure points. I should not think of speaking in detail of matters familiar to every practitioner in regard to the ordinary features of the malady, its general characteristics of pains in the head, the back, the limbs; the sense of debility of body and mind which rendered exertion impossible, the degree of fever, and the long-continued weakness. Nor of the three great groups which we all came to recognise: that affecting chiefly the respiratory tract, that involving for the most part the gastrointestinal mucous membrane, and that in which the aches and pains and nervous symptoms predominated over all others. But I cannot pass quite so lightly over the complications and sequelæ which arose in so many cases, and shall endeavour to sketch in bold outline some of my own experiences in regard to the involvement of the different systems of the body. In doing so I hope that I shall recall experiences that many of you have had, and I have no doubt that many present could supplement and extend what I say by describing cases which have fallen under their observation.

The *Alimentary System* was affected in a great variety of ways. Apart from the disturbance attending the acute attack or primary disease many patients suffered for weeks or months afterwards from various digestive troubles. As an example, I may refer to the case of a gentleman from Forfarshire who consulted me on several occasions. He seems to have a special proclivity to influenza, for he has had three attacks since 1890. The first was sharp enough, but left no serious effect behind. The second, which occurred in 1892, was much more formidable. During the primary stage the temperature reached 105°, and he manifested great depression. Having risen from bed and gone out too soon, he suffered a relapse, with sharp bronchial catarrh and severe cough. When I saw him first these conditions persisted, but his chief complaint was of gastric irritation. Every morning, whether he remained in bed or got up, whether he took food or abstained from it, he was taken at about the same hour with a chilliness, often amounting to rigor, followed by sickness and violent retching. He got no peace until the stomach was

emptied of any food he might have taken, as well as of a large quantity of glairy mucus sometimes mingled with bile. During the rest of the day he was comparatively free of the sickness, and able to go about in a feeble fashion. He was often chilly, and when he went to bed his feet and legs used to be cold and clammy, in a way that they had never been before—and towards morning he would break out into copious perspiration like that attending advanced phthisis. The illness lasted for many weeks and yielded only very gradually to treatment, but at last recovery was quite satisfactory, and during the past winter he had another attack of influenza which left no sequel behind it.

Now, no one who witnessed the facts could doubt that the gastric symptoms were due to influenza, but it was not easy to say whether the gastric mucous membrane was the structure primarily affected, or whether the nerves supplying the stomach were at fault. That an inflammatory condition existed was clear enough, but in my judgment it was equally apparent that the nervous system had an imperfect share in the process, considering that the phenomena occurred periodically and with such regularity. I do not mean that organic change in the branches of the vagus or at its roots necessarily existed, but that their functions were undoubtedly involved.

The *Hæmopoietic System* exhibited innumerable changes. Ordinary anæmia was commonly observed, and forms so severe as to warrant their being called pernicious now and then occurred. But among the most important of the blood changes which I noticed was Purpura Hæmorrhagica, of which I had an example under my care in the Royal Infirmary. I sifted very closely the evidence which connected the process with influenza, and was quite satisfied of its reliability. The patient, a man aged 31, had been suffering from chronic phthisis when he had his attack. He had so far recovered as to be able to go out for a short walk, but on coming home from his first attempt of that kind he felt uneasiness in one leg, and this was followed by swelling, evidently the result of thrombosis. The first purpuric blotches appeared in the area whose vein was blocked, but soon afterwards in other parts as well. Successive crops of hæmorrhages appeared under the skin, attended by the other symptoms peculiar to the disease. But gradually he improved, and about a month after admission he seemed convalescent, the hæmorrhages having passed off, and the thrombosis disappeared. The only anxiety was due to the condition of the lungs, which became more affected, the sputum containing tubercle bacilli, and fever also increasing, especially towards evening. Suddenly a fresh eruption of purpuric spots appeared, and along with them hæmorrhages from various mucous membranes, and particularly from the lungs. It was followed by a speedily fatal result.

It is difficult to say how these blood changes are brought about, but we must keep in view the fact that at least one of the structures standing in important relations to the blood is affected, namely, the spleen; that it is often enlarged in influenza as it is in so many fevers, and it is not unreasonable to suppose that the bone marrow and other structures may be similarly involved, and that the blood changes may be due to these.

But, apart from structural alterations of the blood, chemical changes have been frequently observed to result from influenza. Perhaps the most typical is the occurrence of Gout during convalescence from an attack. A patient of gouty constitution, who had perhaps suffered repeated attacks of the disease, was taken with influenza, and, without other cause that could be ascertained, suffered a fresh paroxysm of gout. We know that anything which causes debility may be followed by like effect in such a patient, and cannot wonder that a malady so debilitating as influenza should give rise to it.

Perhaps in connection with the blood changes I may also refer to Diabetes Mellitus. It has been found to stand related to the influenza process in two ways—for those who have been suffering from diabetes and have taken influenza showed a proclivity to diabetic coma, and, in a considerable number of cases, the diabetic process itself took origin just after influenza. On looking over the records of my hospital practice I find at least three cases in which, so far as one could judge, the influenza induced diabetes. Changes in the central

nervous system may afford the explanation of this clinical fact, or it may be that organs such as the liver or the pancreas, which stand related to the transformation of sugar in the economy, may be directly affected by the process.

The *Circulatory System* exhibited some of the most interesting postinfluenzal phenomena. They presented considerable variety. I select as my first illustration the case of a member of our own profession, a man, aged 54, who during the winter of 1891 passed through an attack of influenza. The illness was so slight that he was never laid aside from his ordinary avocations, but he complained of catarrh, with aching of the limbs, the head, and great debility. During the weeks which followed he continued feeble and less fit for exertion than usual, but was able to perform all his professional duties, which were both varied and exacting. At length one evening he was startled to find himself very weak, and to notice that his heart's action was slow and irregular. Notwithstanding the most urgent advice he persisted in attending to his duties the following day, and after some hours of laborious work he was compelled to give in, feeling absolutely unfit for further effort. I had the opportunity of feeling his pulse then, and of watching its state occasionally during the subsequent weeks. It was sometimes as low as 36, rarely above 40 in the minute, and there was a curious irregularity as if there were occasional abortive beats, and then a slowly delivered but not weak or small one. The heart, which was somewhat enfeebled from old rheumatic disease, but not unequal to its ordinary duties, dilated rapidly and well marked murmurs developed in the mitral and tricuspid areas. There was even some suspicion of aortic incompetence. There was no cardiac pain and no faintness, but the sensation of delayed action of the heart with abortive attempts at systole, and the full bound of the organ when ventricular systole did take place produced a very unpleasant sensation. There was marked dyspnoea on exertion, but never a trace of pulmonary oedema or of swelling about the feet. During several weeks the heart showed very little improvement; only by slow degrees did it come up to 50 in the minute and act with greater regularity. Debility of the muscles generally attended that of the heart, and the patient, who had been an active man, described as quite a new sensation his reluctance to descend three or four steps from the dread of the cardiac effort which was involved in ascending them again. At the same time, so far as could be made out, the mental action and the functions of the body generally were little affected. Only at night his legs and feet became deadly cold, and towards morning he was troubled with violent perspirations and was often obliged to change his night dress more than once. These perspirations seemed in one sense to relieve, in another to exhaust, the patient. Under rest and careful treatment he gradually improved, and at the present time he is equal to all his old activity, both of mind and body. It was clear enough that at any time during the weeks he was at his worst a fatal result might easily have been induced, either by bodily effort, nervous shock, or mental excitement. The question is, What was the precise condition of the heart of that patient and how was it to be explained? Was the fault situated in the muscular fibre of the organ; was it in the motor nerves; was it in the inhibitory nerves? I believe that it was not in the muscle itself, because the action of the ventricles, although delayed, was vigorous and powerful when it did take place. It is not probable that the fault lay with the inhibitory nerves, because such changes as existed must have been due to an exaggeration rather than a diminution of inhibitory influence, and a diminution is a much more likely result than an exaggeration of function. Still, one might argue in favour of that view to a certain extent. But it seems most likely that the real cause was a diminution of the action of the motor nerves whereby the rhythm became irregular and the movements slow and delayed.

But there were many cases during and after the influenza epidemic which showed rapid rather than slow cardiac action. I might cite instances of this kind with pulses ranging from 80 to above 100 in the minute, with more or less irregularity of rhythm and without any marked tokens of backward pressure. Cases of that kind I was inclined to ascribe to morbid action on the part of the inhibitory

structures, the inhibition being diminished, the rate of action increased. If there were time I might narrate very interesting cases in which the heart showed rapid and irregular action, with marked dropsy as a result of increased backward pressure from failure of the heart's action. In some of these cases it is probable that degenerative changes occurred in the muscular fibre of the organ. And, lastly, I might describe certain cases in which a fatal result occurred from sudden failure of the heart's action, sometimes with, and sometimes without, fatty degeneration of the muscular fibre. I might dwell also upon the frequency of pericarditis, and the occasional occurrence of endocarditis, and perhaps of myocarditis also.

The *Respiratory System* suffered changes more frequently in connection with influenza than did any other organs of the body. We all noticed cases in which bronchitis persisted for a longer or shorter time. In some of these, no doubt, that inflammation was of a specific influenzal character, although so much prolonged, the bacilli being demonstrable in the sputum all the time. In others it was, perhaps, of a simple non-specific kind. I have seen the influenza process stand strikingly related to bronchitic asthma. In illustration of this I may refer to the case of a gentleman resident in the South of England, who happened to be in Edinburgh lately, and to consult me as to the state of his chest. I found that although hereditarily predisposed to asthma, he never had a serious attack until he was nearly forty years of age. He then had influenza, and it was followed by so severe and prolonged an attack of bronchitic asthma that he was obliged to winter in the South of France, and only gradually did he recover, so as to be able to stand, as he had formerly done, the severity of an English winter.

What was the explanation of this aggravation of this nervous malady? Probably it was to be found partly in the increased severity of the bronchitic irritation due to the local effects of the influenza process, but partly also to disturbance of the nerves supplying the bronchial tubes, which stand so definitely related to asthma.

Pneumonia was by far the most formidable sequel of influenza, and displayed, according to my experience, marked differences of type from the normal course of the disease. Moreover, its fatality appeared to vary in the recent epidemics. In 1890 I lost case after case in my wards, and found treatment of little value, while in two later outbreaks the deaths were extremely few. Ordinarily I am accustomed to receive into my department in the Royal Infirmary of Edinburgh from 6 to 12 cases of pneumonia in the year. In 1890 I admitted 14; in 1891, 38; in 1892, 27; and in 1893, 28 cases. Every variety of pneumonia presented itself, but the catarrhal was most frequent, and that of an acute and peculiar type.

The croupous form was also frequent, and pleurisy and empyema accompanied it in a considerable number of instances. In explanation of these complications we may assume that the catarrhal pneumonias were due to extension of inflammation from the bronchi to the ultimate air-cells. Possibly the bacillus of influenza was itself capable of inducing this inflammation. But in many of the cases of croupous pneumonia the organisms of Friedländer and Fraenkel were abundant enough, and streptococci were found in the empyema cases. There was no reason to suppose that these organisms were more abundant or more active during the influenza epidemic. The difference lay in the greater susceptibility to the action of these organisms induced by the influenza process.

The relationships between influenza and phthisis pulmonalis were equally striking and varied. I am satisfied that in not a few instances phthisis originated in an attack of influenza, and frequently phthisical patients, whose condition had been quiescent or scarcely advancing, suddenly developed pronounced symptoms, and went on rapidly to a fatal result. The relationship between the two processes seems to depend on the fact that influenza so affects the system as to render it more susceptible of the action of the tubercle bacillus.

Among the morbid conditions of the *Integumentary System* which attended upon the process, I may mention the critical efflorescence so often observed at the height of the disease, and the desquamation which speedily followed on that in-

flammatory action. But I must not omit to refer to the herpetic and various other eruptions, nor to the tendency to chilliness of the skin, and to copious and exhausting sweating which often persisted for months after the acute symptoms had passed away.

The *Urinary System* was affected in many ways: sometimes there was a mere passing albuminuria, sometimes a formidable hæmaturia or serious inflammation of the kidneys, and in some epidemics, as in one of those recently described in Egypt, such changes were frequent, and in a considerable number of instances proved fatal by uræmia. On the other hand, I found some patients exhibit a susceptibility to vesical inflammation. A member of our own profession sat down at a railway station overheated while still suffering from the exhaustion of influenza, and in a few hours developed a sharp cystitis which he had never had before, and which lasted for a month or six weeks.

The *Reproductive System* also showed manifest changes in both sexes—the functions becoming greatly impaired.

But the *Nervous System* suffered most frequently. In every case of influenza during the acute stage disturbance of that system was pronounced, and nervous sequelæ were extremely common, serious, and persistent. If I had time I should be interested to show in detail how nerves and spinal cord and brain, how membranes and deeper tissues, and how every kind of function of the system was altered. I should like to speak of the well-marked peripheral neuritis affecting the hands and feet which I have seen in some instances, of the changes in individual nerves—superficial and visceral—giving rise to important symptoms; of the changes in the functions of the cord and of the brain resulting in bodily and mental symptoms; of the periodic or irregularly recurring paroxysms of fever, with *malaise*, aching, loss of appetite, nausea, jaundice, and profuse perspiration, which I have seen occur and sometimes persist through a period of years. I should have liked to give you details as to typical examples of meningitis, and of congestive and inflammatory changes in the medulla, the pons, and other parts; and as to the mental alienations ranging from mere lowness of spirits to the most pronounced and dangerous melancholia, or other serious forms of insanity.

I should have liked to speak of the numerous eye changes which resulted from the process—inflammation and ulceration of the cornea, conjunctivitis, iritis, glaucoma, asthenopia, optic neuritis, optic atrophy, paralysis of accommodation, dischromatopsia, blepharo-spasm, and many more which have been observed by ophthalmologists. I should have liked to speak of the diseases of the ears, the deafness, from various causes, but especially the otitis which occurred during the epidemics. I select a single fact in illustration of the formidable character of this from Gubler's statistics of his hospital practice in Munich. In three months when influenza was not prevailing he treated in all 52 cases of catarrhal, and 44 of acute otitis. In three months when the epidemic of influenza was at its height, the catarrhal rose to 309, and the acute to 316. I should be glad also to tell you of changes in the sense of smell and of taste; sometimes in the way of diminution, sometimes in the way of perversion and sometimes in the way of exaggeration of function. One of the heads of our profession told me, in illustration of this last-named and rare variety, that during his convalescence he attained to an acuteness of perception of odours and flavours much in excess of his normal condition before or afterwards.

The *Locomotory System* also sometimes showed important changes in respect of bones, joints, and muscles, but upon them I must not linger.

Now, Mr. President, one would be inclined to say, unless we had ourselves witnessed the facts, that surely there must be exaggeration when so many processes are referred to a single cause, but I appeal to you as practitioners, I appeal to the general experience of the profession, whether all these sequelæ and complications have not been definitely proved to stand related to the influenza process. I am quite aware that loose thinking and loose talking have been indulged in in some quarters, but I am confident that the most critical analysis would vindicate the accuracy of all that I have said, and of much that I have not been able to mention, or that has remained unknown to me.

SEQUELÆ REFERABLE TO MANY DIFFERENT CAUSES.

When we look over this long and varied catalogue, it becomes clear that it includes morbid processes which arise in a great variety of ways. An etiological classification of morbid processes generally may perhaps serve to render this more distinct. We may recognise:

1. *Diseases which owe their Origin to the Invasion of Foreign Organisms, particularly Micro-organisms.*—Among the maladies which follow influenza there are undoubtedly several belonging to this group, as, for example, tuberculosis and pneumonia.

2. *Diseases due to Faulty Chemical Action within the Body*, either in respect of (1) the introduction of toxic substances from without; (2) faulty gastro-intestinal digestion; (3) faulty chemical transformation of the blood and other elements of the body; or (4) faulty elimination of waste products. This group is well represented among the after maladies of influenza, the gastro-intestinal cases affording illustrations of faulty digestion; the anæmias, purpura, diabetes, and many cases of gout affording examples of faulty metabolism; while in some instances gout and other maladies may be induced by faulty elimination.

3. *Diseases dependent upon Faulty Mechanical Conditions.*—We find illustrations of this group in the swellings of the limbs and other changes following upon thrombosis of veins, and in the dropsies and other results of backward pressure following upon dilatation of the heart.

4. *Diseases dependent upon Faulty Innervation.*—Of this group we have abundant examples, as in some of the cardiac and gastro-intestinal disorders, in the feverish attacks, and the profuse perspirations.

5. *Diseases dependent upon Morbid Nutrition of the Tissues, either in the way of Inflammation, New Formation, or Degeneration.*—Influenza has shown numerous illustrations of the inflammation, as, for example, in the cases of meningitis, pericarditis, pleurisy, neuritis, the inflammation of the skin, while the degenerations are illustrated by the fatty changes in the cardiac muscle.

From this outline it is apparent how numerous and how varied in their origins are the morbid processes which are entitled to rank among the sequelæ of influenza, and I think that we may claim that our knowledge of the malady, at least in this department of its clinical history, has been immensely increased during the recent epidemics.

PATHOLOGY.

If we can thus claim to have increased our knowledge of the clinical features of influenza, it is certain that still greater advances have been made in regard to the essential nature of the influenza process. We are now able positively to recognise its essential cause, by its presence or absence to test our diagnosis in cases otherwise doubtful, to judge better than formerly as to its mode of spreading, and with considerable confidence to explain many of its complications and sequelæ.

For the discovery of the essential cause of the process we are indebted to Dr. R. Pfeiffer; one of the chief members of the staff of the scientific department of the Institute for Infectious Diseases in Berlin. During the epidemic of 1889-90 he saw and photographed the bodies which are now known as Pfeiffer's bacilli, but their real import was not made out until two years later. In the end of 1891 he found, on examining the sputum, that it contained enormous numbers of bacilli which presented distinctive characters in respect of appearance, staining, and cultures.

Even to the naked eye influenza sputum is characteristic enough. Of a yellow-greenish colour, viscid and tough, generally brought up in lumps or balls, and presenting a nummular appearance in the dish, the sputum varies in quantity from a very small amount up to several c.c. in twenty-four hours. In almost every case it is derived from the nose and pharynx, but in those of any severity largely from the bronchial tubes down to their utmost ramifications, and even from the air-cells of the lungs. Now Pfeiffer, knowing that the mucous membrane of the mouth, nose, and pharynx constantly contains a great variety of micro-organisms, and that the bronchi under ordinary conditions are entirely free from these, sought to investigate specially bronchial expectoration, and there he constantly found the characteristic organism. From such bronchial sputum spread out on sterile

glasses he cautiously selected purulent parts with a platinum loop, prepared coverglass films in the usual way, and proceeded to stain. Loeffler's alkaline methyl-blue did well, but the best preparations were obtained by steeping for five to ten minutes in a dilute solution of carbol fuchsin. The rodlets become very distinct with this staining, and are found free as well as within pus cells in the protoplasm round the nucleus, but never within the nucleus. They are short, slender rodlets; the extremities colour more distinctly than the rest.

With regard to their conditions of culture some very unusual facts were elicited. They can be cultivated only with great difficulty and upon quite special soils. Pfeiffer found that neither in gelatine nor agar plates could the colonies be got; neither by aerobic nor anaerobic process could he obtain colonies of the characteristic bacilli except when he inoculated the sputum itself or purulent matter from the lung direct upon the agar, and then he obtained minute water-clear colonies visible only with the lens, and composed of the fine rodlets.

I shall not follow in detail the beautiful observations by which Pfeiffer showed that the presence of blood in the agar soil was essential for the growth, and that in the blood it was the red corpuscles, not the leucocytes or the serum, that was essential; that in the red corpuscles it was not the stroma but the hæmoglobin that was the necessary part; and that the value of the hæmoglobin did not depend upon the oxygen-absorbing power, but upon the iron it contained. When once he had ascertained the conditions, he was able to carry on successive cultivations through a period of many months.

The proof that Pfeiffer's bacillus is the true essential cause depends upon the fact that the organism is met with constantly in the catarrhal discharges of influenza patients, and has never been met with under other conditions. The only evidence that is wanting to complete the chain demanded by Robert Koch in his famous rules is the demonstration that the cultures are capable of producing the disease when inoculated. Now the difficulty is that man appears to be almost the only animal which affords an appropriate nidus for the organism. It is true that, according to the experiments of Pfeiffer, rabbits were to a certain extent influenced by such cultures, for they got dyspnoea and a muscular debility, amounting almost to a paralysis, when they received such injections; but seeing that no multiplication of the organisms took place within their bodies, and that the effects were equally distinct when the bacilli had been killed by chloroform previously to their injection, it was certain that no true infection occurred in these animals, but that the effects were due to the toxins introduced along with the cultures.

Among apes he succeeded on several occasions in inducing disease, which presented such analogies to human influenza as made him believe that probably they are capable of true infection. For example, he injected into the trachea of a monkey a piece of typical influenza sputum of the size of a pin head containing bacilli rubbed up with 1 c.c.m. of sterile bouillon. Some of the injection passed into the cellular tissue behind the trachea. On the seventh day the monkey was found dead, and, on examination, there was an abscess behind the trachea and a number of patches of collapse throughout the lung, the bronchi leading to these patches being blocked with greenish pus. These plugs, as well as the pus from the abscess, showed no micro-organism except the influenzal one, and that only in small quantity. But Dr. Klein, in a very able research, in most respects confirming Pfeiffer's conclusions, differs from him as to the results of experiments on animals. Among 30 rabbits into which he introduced influenzal material in no single instance did a disease recognisable as influenza make its appearance; while among 18 monkeys inoculated only in 1 did clumps of influenza bacilli make their appearance, and in that instance they were associated with other micro-organisms. It thus appears that we have no sufficient proof of the communicability of the disease to the lower animals, but this is the less important, seeing that other observations make it almost certain that influenza is incapable of affecting any animal excepting man, for Dr. Klein points out that while influenza was raging in London there was no increased sickness observable in domestic animals or in those in the Zoological Gardens, the monkey house in these gardens remaining

as healthy as usual. As experiments on the human subject are scarcely permissible, we are compelled to be satisfied without this kind of evidence.

With regard to the distribution of the bacilli in the bodies of those infected, they are as a rule present in enormous numbers among the mucus of the respiratory tract. They are to be found also in the mucous membrane, and to some extent in the neighbouring tissues. They are met with in the inflamed patches of the lung, along with the ordinary micro-organisms of pneumonic processes. They are occasionally found, but in comparatively small numbers, in the blood, and when there they appear to be dead or dying, being no longer capable of cultivation. Klein has shown that they may block blood vessels in patches of inflamed lung, and they have occasionally been found in different parts of the body, as, for example, in the kidney and the liver, but I am not aware that their presence has been determined in the gastric or intestinal mucous membranes in cases in which the stress of the disease falls on these parts.

HOW THE BACILLUS ACTS.

Accepting, then, the fact that Pfeiffer's bacillus is the essential cause of the disease, we have next to inquire how it produces the various symptoms, and how it stands related to the complications and sequels. Does the bacillus itself, which so abounds in the sputum, produce the catarrh of the respiratory tract? I believe that it does. We know that the bodies of bacteria are capable of setting up inflammation and producing pus, as the experiments of Buchner have shown. We know that the bodies of the bacilli have the power of attracting white blood corpuscles towards them by positive chemotaxis, as was shown in the experiments of Gabritchewsky. This is typically seen in gonorrhœa, where the gonococci attract large numbers of white blood cells, which constitute the inflammation, and it is most probable that in the case of influenza the substances contained in the bodies of the micro-organisms so attract white cells and produce the characteristic inflammatory changes.

It cannot be said that this is absolutely demonstrated, for it is conceivable that the products of Pfeiffer's bacillus may be essential to the production of the inflammation. Passing, however, from the local to the general constitutional effects—the degree of fever, the aching pains, the general debility, which are ordinary symptoms of the malady—can these be ascribed to the direct action of the bacilli or to that of their products? The gonococcus has served as a type of direct action. I shall select the bacillus anthracis as a type of the indirect. That organism is injurious mainly by its action upon albuminous substances. When it develops in them it so acts as to produce from their materials albumoses and alkaloids. If it be cultivated upon a non-albuminous medium it does not evolve the very toxic substances which it produces when grown upon alkali albumen. Here, then, we have a type of the indirect effects of bacteria.

In the case of diphtheria it seems pretty certain that there is a double action; on the one hand certain changes locally produced by the Loeffler's bacillus itself, and on the other hand constitutional changes and changes disseminated throughout the system, results of the products of these bacilli.

Now it seems probable that the constitutional symptoms of influenza result indirectly, rather than directly, and that for several reasons:

1. That the bacteria are not found living, active, numerous in the blood and throughout the tissues of the body generally, and therefore cannot be supposed to be operating so widely.

2. That the character of the symptoms corresponds to those that we know to be due to such poisoning; or, in other words, that the analogy of diseases of which we can speak more positively points that way.

If such be our conclusion as to the general symptoms which occur in all cases of influenza, still more must it apply to the complications and the sequelæ. We have already traced and illustrated the chief groups of these; let us now go over them and see the bearings of each.

Our first group included complications and sequelæ which were due to the action of other micro-organisms. How does the influenza process stand related to these? I believe in a considerable number of ways:

1. By affording a nidus in which germs may readily multiply. We know well how a simple inflammation of the throat or that attending measles or scarlet fever provides a nidus in which the bacillus of diphtheria readily finds material in which to grow, and similarly it appears that the inflammation produced by the influenza process in the pulmonary tissue around ultimate bronchi affords a ready nidus for the development of Fraenkel's diplococcus or the bacillus of Friedländer, which are so closely related to pneumonia, or for the tubercle bacillus and its tuberculous consequences.

2. By lowering the general power of resistance to micro-organisms. The importance of this element has been demonstrated both by observation and experiment. One of the ships of our navy landed some men on a portion of African coast affected with malaria. A certain proportion of these men were soon afterwards exposed to great fatigue, while the others were resting quietly in their hammocks. The latter suffered slightly from malarious fever, while of the former many were severely affected. It seems reasonable to conclude that the exhaustion of the bodies of the overworked men rendered them less capable of resisting the action of the malaria organism. Experiment confirms this, as in the case of the white rats which Charrin and Roger have described. They inoculated eleven with anthrax bacilli, and of the eleven allowed five to rest, while the other six were put upon a species of treadmill. The five who were allowed to rest remained unaffected, while those who had been subjected to the peculiar form of exercise died in periods varying from twenty-four to thirty hours.

3. It is possible that some of these may illustrate the principle which is known to scientific workers as mixed infection. It has been found by experiments on rabbits that when two germs are simultaneously or in rapid succession introduced into the body, sometimes the one prevents the growth of the other, sometimes it renders the growth more rapid and luxuriant. Take first an example where the growth is checked. If we introduce the green pus bacillus and the bacillus anthracis into a rabbit they neutralise one another, and the danger to the animal is greatly lessened. On the other hand, I shall select two instances. Take first the green pus bacillus in association with the bacillus of tubercle—the combination leads to rapid and severe development of the tuberculous process within the body. Take again the case of erysipelas; the streptococcus which is the essential cause of this disease, if introduced into the ear of a rabbit, produces some local irritation which passes off in the course of a few days, without any blood infection or general result; but if at the same time 1 c.c. or less of the sterilised products of micrococcus prodigiosus, which is itself not a very dangerous organism, be injected subcutaneously at another part of the body, the process becomes generalised, and the animal dies. After death the streptococcus of erysipelas is found abundantly in the blood and throughout the organs generally. Roger states that even two or three drops injected into the blood stream sufficed to bring about the result, and the effect is believed to be due to the products of the organism.

Such facts being thoroughly well ascertained, it is reasonable to suppose that perhaps the bacillus of influenza or its products may be capable of conferring increased activity upon certain other micro-organisms when it happens to be associated with them.

Our second great group of morbid processes is that in which faulty chemical action is evolved. We have seen how some of the processes which pathology refers to faulty chemistry have occurred as complications or sequelæ of influenza. Take gout as a typical example. A patient of gouty constitution is very liable to have an attack after influenza. This may be explained by reference—

1. To the general debility whereby the gouty patient becomes more liable to the operation of the causes which induce his paroxysm.

2. The special depression of nervous system which has long been known to stand related to outbreaks of this sort.

3. To the interference with the processes of gastro-intestinal digestion from interference with the functions of that organ.

4. To interference with the chemical changes in the liver, in the blood, perhaps the spleen, and other organs, which interfere with metabolism.

5. To defective action of emunctories whereby waste products, failing to be removed by the natural channels of elimination, accumulate within the system, and exert a toxic influence.

6. Perhaps the chemical products of the influenza bacillus itself may interfere directly with chemical transformations, and so the gouty process be established.

In the case of others faulty chemical processes which have so often been seen to follow influenza, such as diabetes mellitus, similar explanations may hold good, or specific influences on the nervous system or on the liver or pancreas, with which we are not yet acquainted, may play a more important part.

It is not necessary for us to dwell upon morbid conditions due to disturbance of the mechanism of the body; but our next great group—that connected with faulty innervation—is certainly of great importance. The cardiac, the gastric, and the bronchial and the intestinal maladies all afford important illustrations of this group, for, as we have already seen, it is through the nervous system that these organs are most frequently disturbed. But we must ask in what way the nervous system itself becomes altered. I have no doubt that in most cases it is due to poisoning, without organic change which we can recognise, or with alterations which only the most recent methods are enabling us to make out, but sometimes it is a result of well-marked structural alteration.

Our fifth group includes the series of inflammatory and degenerative processes, and of these we have many illustrations in a great variety of tissues and organs. The inflammations of mucous surfaces, of serous membranes, and of solid organs; those of nerves, of nerve centres of the membranes of the brain and cord afford types; while of the degenerative changes the fatty degeneration of the heart affords, perhaps, the best example.

I have already shown reason for thinking that these morbid processes are due not to the bacillus itself but to its products. As yet we have no information as to the precise nature of these products and as to their modes of action. At the present time Dr. Pfeiffer is engaged in a research with regard to them; but meanwhile we can only judge by analogy with other diseases of kindred character. It is not to be wondered at that difficulty should be experienced in connection with this inquiry, considering how difficult the cultivation of Pfeiffer's bacillus is, how minute the colonies are which it produces on cultivation, and that its action appears practically limited to the human subject, and so experimental inoculations are almost impossible.

The diseases which by analogy throw most light upon this subject are anthrax and diphtheria, and the researches which seem to me the most valuable are those of Dr. Sidney Martin. He ascertained that the anthrax bacillus gives origin to two classes of products, one group of digested proteids, namely, albumoses, the other non-proteid, a definite organic base with a specific action. When the anthrax bacillus is introduced into the body, it begins to digest the proteids, the products of digestion cause local oedema, but the absorption of these products produces the symptoms of the disease, and the stupor, ending in coma and death, is due to the gradual accumulation of the anthrax base.

In diphtheria Sidney Martin found the albumoses associated with an organic acid and much more abundant than the acid. In the case of a child only a year old who died of diphtheria, he obtained 15 grs. of albumose from the kidneys. He found, further, that the spleen is the chief repository of the diphtheritic poison of the body. The albumoses of diphtheria cannot be distinguished from those of anthrax or peptic digestion by chemical means. Their physiological effect is the only method by which their specificity is made evident. When the diphtheria albumose is injected subcutaneously it produces local oedema, and in guinea-pigs no well-marked rise of temperature, while the anthrax and peptic albumoses produced distinct fever. Intravenous inoculation in single doses or in repeated smaller doses produced a varying amount of fever, but, what was much more striking and constant, paresis of the muscles. It did not amount to paralysis, and it affected the movements both of the limbs and respiration. There was no special wasting of muscles, no change of pupil, no sensory alteration and

diminution of the knee-jerks was strictly in proportion to the paralysis. Another striking effect is loss of weight in the animal, and that not in proportion to the amount of fever; also diarrhoea; and attention further must be drawn to the remarkable prolongation both of the fever and of the general wasting. *Post-mortem* examination showed that the blood was slow of coagulation, and no wide distribution of germs throughout the body could be demonstrated by the microscope or by cultivations. Thus it was certain that not the germs but the products were the cause of the symptoms. Histological changes of the greatest interest were also discovered. The muscular tissue of the heart was found to undergo fatty degeneration in a marked degree by the action of the albumoses, but to some extent also by the action of the organic acid. The nerves also were found to undergo important organic changes. The white substance of Schwann was diminished or completely lost. Sometimes it was pale, granular, broken up; the axis cylinder was in many instances broken below the point at which the white substance disappeared. Not every branch was affected, and therefore only paresis and not paralysis resulted. Dr. Martin's general conclusion is that the diphtheria albumoses select for their action the peripheral nerves just as strychnine selects the motor cells of the cord; and it is probable that the effects may consist either simply in functional alteration or in true organic processes.

Now, applying this knowledge to the influenza processes, we seem entitled to conclude: (1) That the bacillus, which is not widely disseminated throughout the body, produces its constitutional effects by its chemical products. (2) That these chemical products may be of the nature of albumoses or of ferment-like bodies which may act directly or indirectly by converting albumens into albumoses, organic alkaloids, or even, it may be, organic acids. (3) It is probable that the production of these substances takes place to some extent in the spleen, which is so often found enlarged in cases of influenza. (4) That these poisonous products act upon nervous and other tissues, producing the great variety of functional and structural changes which are met with in the later stages of the influenza process. The peripheral neuritis affecting the nerves of the extremities we can well understand. A similar process affecting the phrenic nerve, or affecting the motor or the inhibitory nerves of the heart, affords an explanation of many of the clinical phenomena with which we have become familiar. Degenerative changes in the muscular tissue of the heart might account for other conditions. Changes in the nerves of the stomach and intestines, or in the mucous membrane itself, might account for the gastric and intestinal symptoms, while blood changes might result from affections of the spleen or of the red marrow spaces of bone. Such is in outline the view of the pathology of influenza which recent discoveries seem to me to justify; and in this respect we may safely claim that we have made immense progress during the recent epidemic.

MODE OF SPREAD OF THE DISEASE.

Up to the time of the recent epidemics it may be safely asserted that little importance was attached to the spread of the disease by contagion, and much to its dissemination by the atmosphere. The striking and typical cases which have so long figured in the textbooks and lectures, in which ships at sea were suddenly overtaken by the disease, were vividly impressed on the minds of the profession and the public. What student of medicine is there who does not remember the story of His Majesty's ship *Stag*, which on April 3rd, 1833, the very day on which London was stricken throughout its length and breadth with influenza, was making its way up the English Channel along the Devonshire coast? At 2 o'clock it was off Berry Head, and all on board were well, but an easterly breeze sprang up and blew over the land to the ship, and in half an hour forty men were down with the influenza. By 6 o'clock the number was increased to sixty, and by 2 o'clock the next day another hundred of the crew had been rendered helpless. The story is alike telling and picturesque. The inference was obvious that that east wind blew over a district contaminated with influenza, and bore with it the contamination which induced the outbreak in the ship. The case seems at first sight conclusive. But when we consider the geography of Berry Head, it becomes

clear that the *Stag* must have been tacking far in to the estuary of the Dart before any easterly breeze could have blown over Berry Head so as to reach the vessel. In fact, it seems impossible to conceive that a vessel could have occupied the necessary position; and one cannot but suspect that some inaccuracy, due to faulty memory or exuberant imagination, had crept into the narrative. The extreme suddenness of the attack—forty men taken ill within half an hour—surpasses anything that I know of in the recent epidemics, and with our present knowledge one cannot help wondering whether at Falmouth or at Plymouth there had not been communication with infected people on the shore.

Many have searched earnestly for similar cases during our recent epidemics. In the masterly report by Dr. Parsons, published by the Local Government Board, we are told that every effort was made to discover instances of outbreaks occurring in ships at sea, among lighthouse and lightship keepers, who, having had no communication with the land, had become infected through the atmosphere; but not a single fact was elicited which lent support to that view. Inquiry was made among the trawlers, who often remain many days at sea, fishing on the Dogger Bank and other places which fish frequent; but in no instance was it found that they had become affected, unless when in contact with influenza patients. The same was true of the keepers of the lighthouses and the lonely lightships which mark the channels or indicate dangers.

While recent facts have thrown doubt upon the old belief in atmospheric spread of the disease, evidence of an overwhelming force has accumulated in favour of its spread by contagion. From Germany, from Switzerland, from Austria, from Bavaria, and from France innumerable instances have been put on record which prove that the malady spreads from the sick to contaminate the sound, but no evidence more important has been advanced than that of our Government officials, to whom I have already referred. No one who reads the report of Dr. Parsons can hesitate to accept the opinion which he so ably advocates. Not that the idea of contagion is new, for a hundred years ago Cullen described influenza as *catarrhus e contagio*, and every good observer since may be said to have recognised the same fact. Still, recent experience has given a fulness to our knowledge, and, I think, an exclusiveness to this method of spread which had not been formerly claimed for it. As Dr. Parsons says, we cannot but attach weight to the fact that the epidemic was independent of season and weather, that it spread contrary to the prevailing winds, and that it was never known to travel faster than men can travel, that it never occurred among persons so placed as to preclude communication by human agency, that, as a rule, it appeared first in each country in the ports of entry or in the frontier towns, and in the busy capitals which were most actively in intercourse with neighbouring lands which were already affected; that, as a rule, the country places remained longer free than the busy towns, even although they might be nearer the countries already invaded by the disease; that the individual members of communities who were most in contact with others were first affected, and that from them it spread to others with whom they were associated; that persons who were wont to congregate in crowded, and especially in ill-ventilated, buildings were specially involved, and that where people were closely associated as in public institutions, the disease came more speedily to its height and was sooner got over than when men were more secluded from one another.

Another important fact which has been definitely made out is the shortness of the period of incubation. Sometimes the symptoms appeared almost immediately after exposure. Professor Eichhorst, for example, states that he saw in consultation at Zurich four cases of severe influenza in a small and ill-ventilated room, and the circumstances seemed to him to indicate that the infection had not been in his system before. Half an hour later on his way home he was himself seized with symptoms of the illness. Although such extreme rapidity of action was rare, it was quite common to find the disease break out within twenty-four or forty-eight hours of exposure. Now, if we consider the brevity of this period of incubation, we can well conceive how its epidemic spread should be so rapid. Contrast its power of spreading from an individual patient with that of small pox. The

small-pox patient comes in contact, let us say, with ten susceptible individuals. At the end of ten days these ten may not improbably have taken small-pox; but one case of influenza has in two days produced its ten cases; in other two these ten may have infected a hundred; in other two the hundred has become a thousand; and in other two days ten thousand cases may have been produced. Moreover, as the great majority of people are susceptible of the action of this poison, and as one attack confers no immunity from another, and as it often spreads from cases which are not severe, and appear trifling and unimportant, its extension in a community, even by contagion, ultimately surpasses what anyone could expect.

But the spread is not exclusively by direct intercourse between the sick and the sound; it is often by means of fomites. Post office and railway officials were specially apt to suffer early, and from them as centres it sometimes spread through the community, and these facts afford instances of the tendency the disease had to spread by letters and by parcels. Such facts as these made most people confident that a micro-organism was the essential cause of the disease, and prepared the profession to accept the discovery of Pfeiffer as soon as it was announced.

POINTS IN LIFE-HISTORY OF BACILLUS.

It cannot be said that our knowledge either of the laws of the epidemic or of the life-history of Pfeiffer's bacillus is so complete as to enable us to correlate all the facts together, but many points in the life-history are ascertained, and seem to me to be alike interesting and important.

The bacillus seems to be capable of affecting the human subject only. It requires quite special food elements for its growth. It cannot grow above $42^{\circ}\text{C.} = 107.6^{\circ}\text{F.}$, nor at the utmost below $23^{\circ} = 73.4^{\circ}$ or $24^{\circ}\text{C.} = 75.2^{\circ}\text{F.}$ Now, as in temperate climates such a temperature is not often reached, and is certainly never maintained for any length of time, and as the disease spreads in cold weather as well as warm with little or no regard to temperature, it is clear that it cannot multiply outside the human body. This has been abundantly proved by Pfeiffer.

The question whether the influenza bacillus is capable of retaining its vitality in water and of reaching the human subject in that way, as cholera so often does, has also been worked out by the same investigator. He took some influenza bacilli and inoculated blood agar with the mixture. In due time colonies appeared. Twenty-four hours later, the water having been in the meantime kept at the ordinary temperature, inoculations were made on the same medium, but they produced little result. Eight hours later further inoculations were made, and all remained sterile. Thus certainly it cannot be communicated by water. But it may in some fluids retain its vitality for a longer period. In bouillon, for example, it will live for fourteen to eighteen days, and it is probable that if sputum retain its moisture it may live in it for at least as long a period. In this way we may explain the convection of the disease by fomites.

The influenza bacillus is extraordinarily sensitive to drying, and the more complete the drying the more rapid is its death. At $37^{\circ}\text{C.} = 98.6^{\circ}\text{F.}$, there was a diminution of the number of colonies produced by inoculation, and in one to two hours no colonies could be obtained. In the rather moist air of Pfeiffer's laboratory the process of drying was slower.

The most careful search has failed to elicit evidence of any spore or resting form (*Dauerform*) of bacillus, and so we know at present no seat of its multiplication except the respiratory tract of the human subject, nor of spread except from that source.

PREVENTION AND TREATMENT.

In conclusion, we have to inquire as to what real progress has been made in regard to the prevention and treatment of the malady. I shall speak first of prophylaxis. The facts just indicated are fitted to guide us in the inquiry as to how we shall be able to deal with the influenza bacillus itself. Outside the body of the healthy person it might be prevented from producing the effects, if we could effectively isolate those who have become affected with the disease. Every sneeze, and every cough probably, scatters the germs throughout the atmosphere, and if we could only isolate those

who are affected, the danger would greatly diminish. But isolation in a general way is impossible, and that because many cases of true influenza are so mild as to escape detection, and because even where the case is severe infection is often spread before diagnosis is possible. But, on the other hand, now that people have ceased to regard influenza as a trifling and unimportant malady, and have come to know that it is certainly contagious, precautions have often been taken that were scarcely dreamed of in former epidemics. Infected individuals have isolated themselves, and thereby have saved their neighbours from contamination. People suffering from heart or lung disease have been kept scrupulously away from the infected, and have thereby escaped what would have been to them a terrible danger. Schools have been closed, with the result of diminishing the severity of certain outbreaks, and I have no doubt that when the next epidemic of influenza occurs, precautions of this kind will much more generally be adopted.

But even where isolation is impossible much good may be done by destruction of the sputum and of the nasal secretion. If patients expectorate into vessels containing sufficient disinfecting fluids, or into pieces of linen which are immediately thrown into the fire, innumerable quantities of noxious germs may be destroyed in each case. So precautions as to the discharges, if once efficiently taken, would tell materially upon the epidemics. It may be asked what is the best disinfectant for destroying the bacilli. Fire and water are no doubt efficient, but considering the very sensitive nature of the bacilli, it is probable any of the antiseptics ordinarily in use will suffice.

If we can thus deal with the bacilli discharged from the body, the question next arises whether we might not deal with them within the bronchial tubes. If we could do so, we might prevent their multiplication, we might prevent the development of their toxins, we might greatly diminish the noxious power of the sputum. Inhalations of many antiseptics have been recommended for this purpose; eucalyptus and menthol have been, perhaps, among the most popular. For my own part I have decided to try, whenever I have opportunity, the injection into the trachea of such substances as have proved serviceable in other diseases of the respiratory tract. In some cases of bronchiectasis I have obtained most satisfactory results by the intratracheal injection of a 10 per cent. solution of menthol in olive oil, with or without 2 per cent. of guaiacol superadded; and I look forward with interest to trying the like method in cases of influenza.

It may be asked whether, during epidemics of influenza, such intratracheal injections might not confer a degree of immunity upon those who are exposed to the contagion; and I should certainly carry out this method of treatment among my phthisical patients if the epidemic should recur. I should incline, also, to give it a trial with others who may be necessarily exposed.

Another question which emerges is whether we could discover any means of conferring immunity by rendering individuals insusceptible to the action of the germs. Now the mere fact that one attack of the disease confers no immunity from subsequent attacks makes one less sanguine as to the probability of such a material being discovered, but I observe that Pfeiffer in his great paper remarks that it may perhaps be one day within our power to supply a preventive treatment by the use of the derivatives of the specific bacillus.

Again, it would be of great service if we could discover substances by means of which we might destroy or neutralise the poisonous products of the bacilli, and so prevent the secondary results. If we could in the case of a patient with prolonged debility of heart or with great nervous or mental depression neutralise the poison which is bringing about these results, we should rob the disease of many of its terrors. The antitoxins, which have been discovered to have the power of combating, as it is supposed by chemically destroying, the poison evolved by the "drumstick" bacillus of tetanus, afford a type of what may one day be accomplished in this way, but meanwhile no such material has been discovered. It is also conceivable that substances may be discovered which would act as physiological antagonists to the influenza toxins; but this also is in the meantime mere matter of conjecture.

The last question which I shall raise is whether we can so

act upon the system generally or upon the organs specially affected as to combat the injurious effects. There is one lesson which most people have learned from the recent epidemics, namely, that those who are taken with the disease even in a mild form ought not to struggle against it and go on with their work, but should yield to the malady for the time, in the hope of escaping or lessening its after-effects. It appears as if tissue change and bodily exertion had an extraordinary power of favouring the action of the bacillus and the development of its toxins. I have found that when the after-effects manifest themselves rest from bodily and mental effort is essential, and that careful attention to diet, to protection from cold, to the moderate and wise use of stimulants by those who are accustomed to employ them, and of such tonics as nux vomica, quinine, arsenic, and iron, with special cardiac or other tonics when they are indicated, prove eminently beneficial.

On looking back over the field which I have thus hastily traversed, one is satisfied that real progress has been made, and that the profession are already better prepared to meet the next epidemic than they were to meet the last, and that we may yet hope for the promulgation of discoveries which may prove of more signal service.

ADDRESS IN SURGERY

BY

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At the Annual Meeting of the British Medical Association held in Bristol, August, 1894.

THE ART OF THE SURGEON.

It is, I am sure, a source of pride and of pleasure to many in this room besides myself that a Bristol surgeon and the teacher of surgery in the Bristol Medical School should have been chosen to deliver the Surgical Address at our meeting here. On behalf of our city and our school in the first place, and on my own behalf in the second place, I desire to make most grateful acknowledgment of the honour.

In seeking for a subject on which to address you I have been careful to consult precedent. The surgical orator has, I find, most often and most properly assigned to himself a task akin to that of the minstrel at the feast in old times. He has been our pæan-writer; he has sung of our victories over science, and he has glorified our scientific heroes. In telling the tale of good deeds done some of our orators have been able to point the way to new conquests; a few have been critics alone; and in almost every case the speaker has dealt with the science of surgery.

I have thought that I may for once leave the science of surgery alone—it is, indeed, well able to take care of itself—and, instead of the science, I shall speak of the art; instead of the making of surgery I shall speak of the making of the surgeon. The Art of the Surgeon, and how we train men to practise it—this will be my theme. My ambition is to keep fresh in your minds the fact that not only have we knowledge to acquire but also an art to learn. The art may be of little importance as compared with the science of surgery; there can be no dispute that it is; still, as the servant, the interpreter, occasionally even the improver of the science, it has its importance, and may be worth thinking about and worth discussing.

What is a surgeon? A stranger looking at the doorplates of an English town might reasonably infer that there was no lack of material on which to base an answer. But then the stranger does not know, as we do, that the title "Surgeon" on an Englishman's doorplate usually means that the possessor thereof is prepared to practise any and every branch of the healing art save and excepting surgery. This covering of the whole field of the healing art by the titular surgeon is the more remarkable that he alone amongst craftsmen should have retained the name "handworker"—*χειρουργός*. Not less remarkable, perhaps, is the fact that the physician should