Introduction

TO A

DISCUSSION ON INFLUENZA

At the Hunterian Society

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INFLUENZA has had, perhaps, an especial interest for me, because, in my childhood, I remember well the outbreak of the epidemic in the Forties, and I had heard something of the manner in which people suffered during the previous epidemic in the Thirties, which seems to have been a very severe one—from what I heard people say, even more severe than the epidemic of 1844. Since that time there have been epidemics mild in degree, but none, of course, with anything like the severity of the outbreak dating from 1889-90. Speaking with the knowledge of what I heard in my childhood, my recollections of the outbreak in the Forties, and the experience of us all in the 1889-90 outbreak, I have ventured to say that this last outbreak was apparently a very much severer epidemic than that which occurred in the Forties. But, of course, it must be remembered that in those times observation was far less accurate, and sequels were not attached to causes with that perspicuity with which they are concatenated now; and no doubt a great many sequels of influenza, especially some of the nervous sequels, were not duly recognized or attributed to their proper causes in the influenza infection.

The disease itself is apparently a very old one. We know of it pretty definitely from the twelfth century onwards. I shall touch very slightly on the epidemic history, because on this subject you will hear Dr. Franklin Parsons afterwards; but one little point of history I may mention—namely, that almost in every century, perhaps, indeed, in every century since the twelfth, there has been a somewhat virulent outbreak, and in almost every century there has been a distinguished observer to record it for us. In the course of other reading I happen to have come across no little information concerning these epidemics. The epidemic of 1540 was very fatal. In the next century—the seventeenth—we have both Willis and Sydenham as observers, and we could not very well have two observers of disease more admirable than these. In the eighteenth century we have Fothergill and Heberden as historians, who, again, were almost ideally-selected observers; we have Huxham's classical account of the epidemic of 1733; of that in 1803 a description by one whom I am inclined to regard as the greatest physician who ever lived, namely, Laënnec; and of the 1847-8 outbreak we possess the well-known monograph of Peacock. I shall only touch upon the course of these epidemics in so far as they illustrate the contagiousness of the disease; Dr. Franklin Parsons will probably carry the matter a great deal further.

Taking the features which are now indisputable, I will remind you that the disease seems to be endemic in Northern Central Asia, as cholera seems to be endemic on the Ganges. For some reasons which we do not comprehend, as cholera raises its head from time to time to begin to travel, so influenza starts on occasional raids. Of the internal affairs of Northern China, infectious and otherwise, we know very little. The outpost from which the epidemic is definitely traced to us is Bokhara, and I may say, briefly, without attempting at this time to set forth all the details, that it follows, and has followed, the three chief trade routes in Asia westwards, and has done so in a very remarkable way. In 1880, at any rate, it tracked from East to West Siberia by the Caspian route, by the more southerly route to Sebastopol and Odessa, and, thirdly, through Merv and Persia. It was in St. Petersburg in November, 1889, and early in December it was in London and Paris. It reached New York in the time that corresponded to the most rapid means of locomotion, that is to say, eight or nine days later. It did not get over the slower passage to Hudson Bay until the following February. Some of the remarkable features of the journey were that the pest travelled in the first instance by express trains, for it appeared at first at a few large stations on the way, where very active centres of disease were promptly established; the intermediate areas were attacked more gradually—by *petite vitesse*. The through travellers appear in the first instance to have been the murderers. It was they who

brought the pest along with them to the main stations by the express trains, which, of course, got much more rapidly over the ground, and reached the several large towns quickly. The main direction of progress was from the North-East to South-West. In 1889-90 the rapidity of arrival in Europe and America was much more rapid than the waves of invasion in previous epidemics, on account of the opening of the Siberian Railway and associated factors.

Since the virulent outbreak of 1890 we know—and it has been true of all previous epidemics—that within the main flood of the epidemic there have been subsidiary or secondary areas of intensity—what we may call vortices in the main curve—which are apparently due—I speak with hesitation in Dr. Franklin Parsons's presence—to the recrudescence of germs of the disease, which, after the first brunt in this place or in that, become latent and burst again into virulence.

It is of importance to emphasize a few of the main features of the passage of the epidemic in order to lay stress upon the contagious nature of it. I suppose every one in this room looks on the contagiousness of influenza as something so certain that it is needless for me to dwell upon it. I can only say that in 1890 this was not by any means the general opinion; indeed, it was the opinion of very few. People may now assume contagion as obvious, but I will tell you how it is impressed on my memory that such was not the general opinion in 1890 and previously. In 1890, for a brief interlude in my life, I was a Commissioner in Lunacy, and I remember well the sudden swoop of this terribly bewildering epidemic upon the lunatic asylums in England. Now it became manifest that those persons who were in contact with the outer world—the medical and domestic staff, the attendants, persons visiting them or the patients—fell down at the rate of something approaching 50 per cent. Amongst the inmates who did not go into the outer world the incidence of the disease was relatively slight, which was some mitigation of a calamity which would otherwise have paralysed patients and administration alike. These events pointed so distinctly to contagion that I looked into the matter very carefully from this point of view, and the only recent author of eminence who definitely pronounced for contagion was Parkes, in Reynolds's *System of Medicine*, who definitely stated his opinion that the spread was by contagion and contagion only. The prevailing opinion, medical and lay, was, on the contrary, that the pestilence swept the folk down like an atmospheric wave as a morbid influence in the air. We cannot wonder at this conviction when people were felled in St. Petersburg at the rate of 40,000 a day, and the question might well be asked how all these could catch it by transference from one to another? We now know, of course, that the terrible suddenness is accounted for by the very short incubation, apparently sometimes even less than twenty-four hours, nearly always less than forty-eight. If we assume a swift and subtle contagion, a very brief incubation and an enormous range of public susceptibility, it is intelligible that the smouldering flax of the comparatively few first victims may well break into a devastating flame in a day or two.

I think it is quite certain, from the very remarkable experience we obtained in asylums—and it is borne out by prison experience and by the experience of lighthouses and so on, isolated villages for instance—that the contagion, whatever it is, may be carried in clothes, upon things as well as in human beings, although probably only for a very short time, perhaps a few days only. Our asylum experience seemed to make it pretty clear that linen and clothes could carry it for a few days. I formed the opinion then—and I have seen no reason to doubt it since—that the disease is propagated in the sputum and spray from the respiratory tract; and this experience is borne out by the further observation of the negative cases—that is, of the non-infectiousness of cases in which the respiratory tract is unaffected. It may be said that this is a very fortuitous and contingent kind of evidence, and so it is; but after some vigilance I am able to assert that I never saw a patient without respiratory affection prove infectious. We see a good many cases with absolutely no respiratory affection. I have two recent examples now under observation, patients in the midst of large households, who had no affections of the respiratory tract, and whose attacks were not followed by any farther outbreak in the house. These are but two of scores of such cases. Therefore I remain of opinion that influenza is infectious only when the respiratory system is affected.

The meteorological conditions of influenza are so variable, as Dr. Franklin Parsons will probably tell us, that I will pass this part of the subject over without a word.

The bacteriology is, however, so important that, hackneyed as it may appear, I must say something about it. There is no doubt now among us as to the specific character of the Pfeiffer bacillus, and about the means of isolating, identifying, and cultivating it. But of its habits, where it is generated, whence it comes, and how it usually survives, we are absolutely ignorant. It may prove to be one of that large crowd of microbes—the flora, as it is the fashion to say—which is with us always in our mouths, and of which this or that species at times takes on, as analogy of other cases would illustrate, a peculiar and extraordinary virulence. It is a matter of the utmost importance to us to know, if we can learn, what the origin and habits of this parasite are, but as yet unfortunately we have no glimpse of them.

The public susceptibility when this epidemic first arrived in 1890 was very great; early in 1890 it was terrific. Nobody seemed immune; people went down at the rate of 50 per cent., and this is probably under the mark, as vast numbers of people had mild attacks which were scarcely noted or were disregarded. I do not know what the general opinion concerning young children is, but my own impression is that they are not very susceptible. When they get to 8, 10, or 12 years of age they become more susceptible and are apt to have very sharp attacks of two or three days' duration; but, except in the ear, they do not suffer much from sequels. The malady in them passes off as quickly, they have not the usual wretched, tedious convalescence. They are very apt, however, to have otitis media, more apt of course than adults. As many people gain immunity, the epidemics subside or peter out, but recrudescence is frequent, because the acquired immunity of susceptible persons is apparently short. A medical man brought a patient to me two or three weeks ago with some of the sequels of influenza, and he assured me that his patient had had a previous attack within two months. Of course, it is not for me to doubt the assertion of a very competent observer, but there was not a bacteriological investigation, and two months is even a shorter period than I was disposed to admit. In my experience five or six months' immunization is generally conferred even on very susceptible people; so that such a person if re-exposed to infection five or six months after a previous attack, is liable to go down again, although not so severely. Twelve months is the more usual respite.

As regards the morbid anatomy I have nothing to say, except that so far as our laboratories go, and my own personal observation, it is an inflammation of an erysipelatous type. Dr. Cobbett carried out a large number of observations and experiments on erysipelas in our laboratories, and with his work I was at one time not unfamiliar. The histology of influenza appears to be very much of the same character, especially in its disposition to small cell and to nuclear infiltration. In the lungs we see this in the coats of the bronchial tubes and also interstitially between the alveoli. There is far more interstitial infiltration than in ordinary croupous pneumonia. The consolidation is never quite of the same appearance as that of the so-called croupous pneumonia; the cut surface is more glistening and more uniform, and it is softer to pressure. What is more characteristic still, if there be a lobar inflammation it is almost always associated with lobular patches. Again, it is very seldom (perhaps never) confined to one lung. In its lobar form, however, it may begin quite suddenly, like an ordinary lobar pneumonia, and thus it may simulate lobar pneumonia closely; no lobular complication may be audible, and it may seem to be confined to one lung. I will not say how far it is to be absolutely replied upon, but it is a useful general rule that in influenzal pneumonia the sputum, of which we have had a great deal at our laboratories, is not rusty; it is not like pneumonic sputum; it is mucous or muco-purulent. Of course, the lobular is far the more common form, but I do not wish now to discuss features, which are well known.

The next point on which I would insist is that, like phthisis, influenza may excavate. I had a very striking case of this kind some few years ago, verified by repeated pathological investigation. The sputum always contained influenza bacilli, with some mixed infection of course, but never tubercle. Yet both lungs were excavated, as in phthisis. The patient was for a time sadly wrecked, but ultimately he got perfectly well, and would have got well sooner if he had not been so susceptible that he had two or three supervening attacks. If tubercle be present, I need scarcely urge what most people here know but too well, how calamitous an event it is when influenza falls on the lungs of a person already the subject of pulmonary tubercle.

Influenzal pleuritic effusions do badly, and pericardial still

worse. And in influenza the empyemas do very badly; nay, you may almost recognize the nature of such a case by its doing so badly. There is generally, of course, streptococcus with Pfeiffer's bacillus.

The heart in influenza is a little inexplicable; it may get into a very perturbed, and even into a perilous condition, but if you come to examine these influenza hearts after death (at any rate to judge by the few I have had the chance of examining), the histological changes amount really to very little. It would be hard to say that there is more muscular degeneration than is to be found in any febrile infection; yet, for all that, the heart is in special peril, in more peril in influenza than it is in many other infectious fevers. I can only suggest that the danger may lie rather in the nervous endowments of the heart than in the muscular fibre itself. Of the cardiac symptoms I will speak presently.

Now as regards symptoms. For some time I have laid stress, especially for diagnostic purposes, on the extreme suddenness with which influenza attacks its victims. If the attack is severe its suddenness is striking enough, in swiftness of stroke exceeding, I think, any other disease. If the attack is mild the suddenness is not so much noticed, but I think it always begins suddenly. Ordinary colds generally creep on. Influenza has a peculiar effect upon the nervo-muscular apparatus, as perhaps many of you here who have had influenza know to your cost. Not infrequently the prostrating effect is terrific. For instance, a gentleman, for all he knew perfectly well, was riding on horseback near my house in Cambridge, when he was seized so suddenly that he had to clamber off his horse as well as he could, and sit down until some passer-by fetched a cab and took him home. A patient of mine, when out for a walk in the country, was seized as he passed from one field to another; he had to hang on for a time to a post, and scarcely knows how he got home. Those are but two instances out of many such. When I described this mode of onset a few years ago at the British Medical Association, Sir William Broadbent said: "I quite agree with that. I remember a patient of mine who drove in at one gate of the Park in perfect health and drove out of the other prostrate with influenza." This is, of course, the form in which it attacks the nervo-muscular system, inducing intense prostration. At the first outbreak in 1890 staff and attendants went down in this sort of way in the asylums, as if mown down. That they were thus knocked off work at once before mixing again with the patients is one reason why the patients so largely escaped; the attendants were laid on their backs and the patients were saved from their contagion. This suddenness of onset is often a useful point of distinction between influenza and typhoid fever. One is very hard put to it sometimes at the outset to distinguish typhoid from influenza; but typhoid generally begins insidiously, influenza suddenly.

In convalescence the malaise very often departs as suddenly as it came on. There are many instances of this brusque recovery in a protracted convalescence. A convalescent goes on for weeks and months feeling unfit for his work, miserably depressed, almost suicidal. Everything is a difficulty and a drag. Suddenly he says to himself, "Dear me, how I should enjoy my lunch or tea," and he falls to, and from that hour is well. Sir William Church has described to me some cases of this sudden release.

The temperature is not high, nor very prolonged, and I need not go much into pyrexia; it is more important to remind you that often it falls considerably below normal, and may remain so, or fluctuate thither for some time in early convalescence. It is usual to divide influenza into the three forms, the respiratory form, the nervous form (which was especially common in the 1890 epidemic), and the gastro-intestinal form. But I would add to these a fourth form—namely, the continuous form, which, I think, is not in the books. It has been my difficulty on not a few occasions—I think four occasions, strictly—to be called in to see certain cases of an unaccountably persistent febrile state; the temperatures are not very high nor the symptoms severe, but a week or two passes and lysis does not take place. There is no typhoid agglutination, nor other sign or symptom to define the case as typhoid. Nor are there any indications of tuberculosis. The first case in which we were able to prove the case to be influenza was under the care of Mr. Lucas, of Bury St. Edmunds. Dr. Graham Smith, my colleague in the Cambridge medical department, went over to Bury and obtained a little mucus from the mouth of the patient. This revealed "a pure culture of the influenza bacillus." There had been no respiratory symptoms, and it was with difficulty that

Dr. Graham Smith obtained a scrap of phlegm. There was no cough, no recognizable expectoration. In a case of fever then of sudden onset, which instead of clearing up in three or four days goes on with an indefinite febrile curve, which might be typhoid though a little less regular, influenza must be borne in mind and diagnosis by bacteriology employed. Moreover, in these influenzal fevers the spleen is not infrequently enlarged and tumid; it was so in two at least of the cases which I am speaking of now, a little large and tender; in 1 of the 4 cases it was neither tender nor palpable.

Other cases begin with some violent neuralgic pains, often in the back and limbs but not rarely in the head. I saw a patient last week who began with violent neuralgia in the submaxillary region and throat, accompanied with slight laryngitis; the vocal cords were a little tumid. The soft palate was a little wet and pendulous, otherwise negative. Pfeiffer's bacillus was present. For a few days the neuralgia was so severe that the teeth, ear, and neighbouring parts were continually under suspicion.

Then there are the ear cases. I have seen a good many of these, and otologists will tell us that during the last few years they have had twice as many cases of otitis media as in a like period before 1890. A member of my own household is now recovering from a mastoid operation by Mr. Cooke of Cambridge. She began suddenly with influenza; intense pain in the ear set in on the first or second day, and on the third or fourth day the drum was seen to protrude. It was incised, and pus was let out, but the mastoid cells had to be opened. In children this is, of course, a very grave anxiety.

There is another point of diagnosis we cannot afford to neglect in these obscure cases. At the beginning of almost every febrile disease we have rather scanty, high-coloured, and often depositing urine. In influenza the urine is not high-coloured, and is not lateritious, as in "chill." This is, I think, a very practical point, and, if you can see the urine early in the case, its aspect will very often lead you to suspect influenza.

Convalescence is terribly slow, and the duration of it is in no proportion to the attack. I had an exceedingly slight attack about a month ago—so slight that it did not take me for an hour off my work. About 5 o'clock one evening I felt ill and found my temperature at 101° , but next morning, as I was a little better, I did not take it again, and forgot the disorder. But in a few days I began to realize an utter weariness and distaste for all exertion. I felt peevish if any one rang the door bell. This went on day after day. The attack could not well have been milder, yet the subsequent depression was very tedious. In influenzal convalescence the loss of appetite, and the distaste for food, are very prominent features in all cases. The taste is lost also for other things, that is to say, in the spheres of the other senses. I went to a concert in my convalescence, and found myself in the position of a man without an ear for music, and said to myself, "How on earth have I and other persons been able to sit listening to this kind of noise for two mortal hours?" Indeed at the end of one hour I had to walk out. Now, the day before yesterday (two days before this address) my disorder passed off quickly.

Neuritic and paralytic attacks we do not hear so much of now, but there were a good many of them in the early Nineties. I have had but two attacks of influenza. One was in 1890, when I was living in London, and it was as slight as the one I had a month ago. In the former case likewise I had forgotten the attack until a few days later, when walking in the street I suddenly felt paraplegic, and had to get a hansom to get forward to Whitehall. Thence I had to take a cab home, and for two or three days I had a little difficulty in walking about. Then the thing passed off. My knee-jerks did not disappear. There are many far worse cases of paraplegia than this, but I do think there has not been anything like the same nerve poisoning of late that there was ten years ago. Many persons then became dangerously suicidal during convalescence.

Peripheral neuritis, as I have said, is a rarer sequel of influenza in the last few years. Of the danger of palsy in the respiratory mechanism I have seen two cases, and was called to a third case, which I failed to see (he was seen afterwards by Dr. Buzzard), in which the patient died. The neuritis may begin in the intercostal area or in the phrenics or may extend itself to them. Any ascending palsy of this kind must be anxiously watched.

Another case which strikes me as worth mentioning is one of a gentleman who had paralysis of the right arm when re-

covering from influenza. He was an old man, and his friends were naturally very much alarmed when he awoke in the morning to discover a palsy of the right arm. He could not feed himself with this hand, sign a cheque, or do anything with it; and this palsy—which went no farther—passed off in a few days. I suppose this was neuritis. Sensation was not obviously affected.

As regards the blood vessels, the veins are not very rarely attacked by phlebitis, and I have seen arterial thrombosis in three cases—one case femoral, two popliteal—just as the same event occurs in typhoid.

I would draw special attention, however, to angina pectoris. A paper of great importance was published on this subject a few years ago by Dr. Ernest Sansom—that is, on Angina Pectoris and Aortitis in Influenza—and in my Cavendish Lecture two or three years ago I went somewhat fully into the matter, and need not reargue it now at length. The same arteritis which, as I have said, may occur in the distal vessels may occur in the aorta. As many of you are aware, I am a heretic about angina pectoris, and believe the seat of the pain to be not in the heart but in the first part of the aorta. Seven or eight years ago a friend of mine, after an attack of influenza, was suddenly seized with an attack of severe angina pectoris, so intense that he could scarcely crawl up the two or three steps into his own chambers. The attack was of the most typical, and was severe in degree. Dr. Humphry, whose patient he was, asked me to see him, for the attacks were repeated very frequently and severely. We kept him absolutely at rest for a few weeks, and gradually the attacks grew fewer and slighter, so that in some three or four months it had entirely disappeared. For some years now past he has been in excellent health. He was a man of some 55 to 60 years of age. I have seen perhaps three or four of such cases, but it is not easy to define in all the relation to a fore-running influenza. I was very much struck with Dr. Sansom's paper, because it precisely bore out and explained by *post-mortem* evidence experiences of my own.

Of sudden death from poisoning of the heart, as in diphtheria, I have had in influenza happily no experience. But of minor, though often very troublesome and tedious perversions of the heart's action, my experience has been only too abundant. In some cases the heart is accelerated, in some it is retarded. The rate may rise to 140, and vary from 125 to 140 for many weeks, or it may fall to such a range as 35 to 50. In these cases of retardation, as in a very closely-studied case I lately saw with Dr. Michel of Cambridge, the temperature fell likewise, and in its fluctuations kept some parallelism with the pulse rate. Whether in acceleration or in retardation, the rhythm is apt to be irregular also, sometimes only by intermissions with the prolonged pause, sometimes quite arrhythmic. Some precordial pain (not angina pectoris) is not infrequent. The arteries seem relaxed; both in the slow and rapid forms the aorta may be seen to beat in the suprasternal notch, and the carotids to throb. The patient may or may not be aware of the beating, but the area of dullness to percussion is extended both to the right of the manubrium sterni, and over the root of the pulmonary artery. Indeed, in respect of physical signs the chief feature is a remarkable extension of the area of cardiac dullness in all diameters. The limit extends well outwards to or beyond the left mid-clavicular line and upwards almost to the second rib. The limit of deep dullness is found 2 cm. to the right of the sternum, and extends upwards into the third space a good fingerbreadth from the sternum. But the veins of the neck are not usually distended.

Murmurs—except, perhaps, a soft systolic murmur in the left third interspace—are not the rule, but a more or less fugitive murmur of mitral regurgitation is not infrequent. These dilated hearts need much careful nursing and treatment, but ultimately do well, for, as I have said, the intoxication is primarily rather of the nervous than of the muscular tissues.

With regard to possible cardiac disability, on one practical point I must insist—namely, on the avoidance of chloroform. I am quite sure that no patient for some time after influenza ought, for empyema or the like, to have chloroform as an anaesthetic, not even if it is given after the manner of Professor Waller or of Mr. Vernon Harcourt. More than once I have had occasion to regret giving it for a simple thing like empyema. The patient may not die there and then, but the heart is placed under a double intoxication, it is still more embarrassed and recovers badly afterwards; perhaps in the course of a few days it may gradually fail.

Dyspepsia persists for a very long time, but this is almost always after the gastro-intestinal form of the infection. Ordinarily the difficulty is an aversion from food.

Another sequel not, I think, mentioned in books is sweats, the obstinate recurrence of profuse paroxysmal sweats for months after an attack of influenza. The sweats are of a very profuse and trying kind, and seem to keep up the debility.

Treatment is a question to be dealt with by my friend, Dr. Burney Yeo, at a later date, and certainly I am very glad I have not to deal with treatment, because I do not know that there is any which can be called specific. There are two points, however, I am unwilling to forget, one of which is well known, but difficult to enforce. This is, that it is of great importance that the patient should do, what we none of us do—that is, go to bed the moment he suspects influenza, and stop there until the acute phase is well past. It is, I fear, a counsel of perfection; people will not do it, but those who do will find in it an enormous saving of time, trouble, and long convalescence afterwards. As regards this long, tedious depression and misery which follows, all I would say is, that I think it is very much shortened by what I may call a non-toxic diet. To dispel it affectionate wives keep their husbands up with beef tea, feed them at short intervals with chops, bring out the old brown sherry, and so forth. Now if all this loaded diet is cut off, and the patient put on a diet clear of purin bodies and much nitrogen, such as milk, custards, and the like, especially leaving out meat, I am quite sure that the misery and depression of the convalescence from influenza will pass off much more quickly.

THE EPIDEMIOLOGY OF INFLUENZA.*

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If we want an abundant vegetable crop we need first the proper seed, in the second place suitable soil, and in the third place the necessary meteorological conditions of climate and weather. Similarly, to produce an epidemic abundance of disease we need first of all the specific organism of the disease, corresponding to the seed; secondly, we need predisposed persons in which the organism may be sown; and, thirdly, we need favouring conditions. These favouring conditions may be such as will enhance the virulence of the specific organism, or such as diminish the resistance of the individual, as, for instance, persons are rendered more prone to fever by famine; or such as assist the multiplication and dissemination of the infective organism, as, perhaps, heavy rainfall after a period of drought may wash infective matter into wells; or there may be other conditions which we do not know, such as were spoken of by the older writers as the "epidemic constitution." At any rate, there are certain favouring conditions necessary, without which the seed may fall upon apparently suitable soil and yet no epidemic follows. In influenza it seems to be generally acknowledged, as Professor Allbutt has told us, that the Pfeiffer bacillus is the specific infective organism, either alone or associated with other microbes, such as the pneumococcus or streptococcus. The personal predisposition, as Professor Allbutt has also told us, is very general. A very large proportion of persons, on the first arrival of influenza in the country, fell victims to it. In London at the time of the 1889-90 epidemic I could not find in any community figures showing a larger proportion than 25 per cent. affected, but still 25 per cent. is a large proportion. Although one attack seems to produce some kind of immunity against another, yet the immunity, as we have heard, is short-lived, and a person may suffer from an attack again and again. I am not quite sure whether he suffers with the same suddenness of onset on subsequent occasions, or whether, indeed, the later attacks are, strictly speaking, fresh attacks. Possibly, the nervous system having been once affected by the influenza poison, the old wound aches afresh when a person's health is lowered by something else, in the same way as patients who have suffered from malaria abroad, coming back to this country, where they do not get a fresh dose of the malarial infection, may nevertheless suffer the old feelings under the influence of circumstances which affect their general health.

As regards the spread of influenza, I believe it is now

generally acknowledged that this spread takes place by personal infection. In 1889 that was not the belief, almost all earlier writers having put it down to vague atmospheric or cosmic or telluric influences. But a number of observers in different countries studied the epidemic of 1889-90, and I believe they all came to the conclusion that infection from person to person was the mode in which it spread. The old stories of its spread over wide regions faster than human beings could travel, and of attacks of persons in isolated places, and sudden attacks of ships far from land, were not confirmed. A number of inquiries were made in this country and elsewhere, but no such instances came to hand. On the contrary, although the epidemic of 1889-90 travelled very fast, it took three months to spread through England. It was pandemic all over the world, but it took more than a year to spread through the world. Some remote places like Gilgit in Cashmere, and the Blantyre Highlands in Central Africa, were not attacked until more than a year after its first appearance in Central Asia. It was very commonly observed, as Professor Allbutt has told us, that in institutions the people who had it first of all were the staff, who were in contact with the outer world. The inmates who remained inside, in workhouses, in prisons, and in lunatic asylums, escaped, or had it very slightly. In families the first to suffer was generally the head of the household, who went out to business, and after him his wife, and then the children. In many instances the subsequent cases occurred at intervals of two or three days from the first case. Numerous histories could be given of its introduction by travellers into places, and then spreading from person to person. The rapidity of the development of the epidemic was not greater than could be accounted for by its short period of incubation, by the early development of infectiousness, and by the widespread susceptibility to the disease.

I think we must assume that besides the well-marked cases of which we have heard, there must have been—as we know to be often the case in other diseases—a number of mild unrecognized cases which might convey the infection, and probably also the infection may be carried by persons who are not themselves obviously affected by it, as we know diphtheria may be. Persons attending on a case of diphtheria may harbour the bacillus in their throats for long periods without suffering in their health in any way, but when those persons come into contact with a susceptible individual they may give him diphtheria. It is possible that influenza may be carried in a similar manner.

Another thing which I think we may assume in influenza is that what we may call the "striking distance" is longer than in some other diseases. We know that in some diseases infection may be conveyed to persons for relatively large distances through the air, and that the infective matter from the lungs may be diffused as spray by coughing and sneezing. Experiments have shown that recognizable bacilli from the mouth may be carried by a speaker to a distance of some 40 ft., and consequently we may suppose that the Pfeiffer bacillus, during the act of coughing or sneezing, may be conveyed a distance of several feet through the air. Hence a person may probably contract influenza by coming within a distance from a person who is suffering from it less close than would be necessary in the case of some other diseases, such as diphtheria or scarlet fever.

But personal infection alone will not explain the behaviour of influenza, and we must assume that some other circumstances are necessary for its epidemic development. The crux of the question is, what are those circumstances? Or to put it another way: What becomes of the influenza contagion in the interval between the epidemics? That is the point we want to know. Does it die out entirely, and is it regenerated afresh by development from some ordinary innocuous microbe? Or does it almost lose its virulence, and regain it under some unknown conditions? Or does it form resting spores and remain in some condition outside the human body, or in some of the lower animals, or what? Those are all subjects on which, so far as I am aware, we know nothing. It will be necessary to investigate these things before we can understand the behaviour of influenza, or what it is that leads to its epidemic development from time to time. Of course, the same difficulty occurs with other diseases. Scarlet fever, for instance, we know spreads by infection, but it is not always epidemic. It recurs at intervals of a few years. In the case of scarlet fever, measles, whooping-cough, and similar diseases, we have good reason to believe the cause of their recurrence at intervals of a few years is the accumulation in the interval of susceptible

* Read at a meeting of the Hunterian Society on March 22nd.