THE AERIAL CONVECTION OF SMALL-POX FROM HOSPITALS.

By JOHN C. McVAIL, M.D., F.R.S.E.

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In opening the discussion on the possible influence of small-pox hospitals in aerially conveying the disease to surrounding population, let me state *in limine* that my purpose is rather to review existing knowledge than to adduce new facts, unless, indeed, some facts to which I wish to draw attention are so very old as to be entirely new to any of the members of the Epidemiological Society. For it is a trite saying that history repeats itself, and even in regard to the discussion of this subject the saying holds true.

A century ago, the celebrated Dr. Haygarth of Chester, and Dr. Waterhouse, Professor of Physic in the University of Cambridge in New England, had a lengthened controversy on this very question, and some of the points then raised are not without interest to us in the present day. Haygarth was the author of *A Plan to Exterminate the Small-pox,* the plan consisting of small-pox inoculation plus isolation. But the isolation he advocated was not necessarily by means of hospitals. He held that the disease could not spread aerially from one room to another in the same house, and that as to transmission through the open air, in moderate cases, a distance of a foot and a half might be looked on as the limit, and that the infection of fevers was "confined to a much narrower sphere"—much narrower, that is to say, than a radius of 18 inches. In support of his views he cited experiments (which, he said, "must strike every reader of sound sense with irresistible conviction") in which *moist* small-pox matter had failed at this distance, entirely forgetting that only dry matter might be expected to be disengaged from its source and carried atmospherically. At the same time Haygarth hotly opposed the thesis of a Dr. Paulet, who held that variola was not to be transmitted excepting by actual contact. In promulgating his own opinions, he therefore carefully steered his plan between Scylla and Charybdis. If, on the one hand, he agreed with Paulet, isolation, as defined by Haygarth, was needless; if, on the other hand, aerial transmission were possible beyond the limits of an apartment or a house, this system of isolation was useless. As to clothing, he asserted that "variolous miasms never adhere to clothes so as to communicate infection", and he challenged any man to prove the contrary. Here, however, it is important to notice that by "miasms" he did not include serum, pus, or scabs adhering to clothing. He argued that the poison of small-pox when conveyed at all by the atmosphere is not particulate, but is dissolved equally all through it. And so he sent out to various correspondents a series of questions, of which one referred to transmission by clothing, and another was as follows: "Did you ever know the small-pox conveyed out of one chamber into another by a person who certainly did not carry any variolous serum, pus, or scab on their clothes, hands, or feet, etc.?" Among the correspondents to whom these queries were sent was Professor Waterhouse. Now, Waterhouse also had a plan of dealing with small-pox, but it differed widely from Haygarth's. Waterhouse was by no means satisfied with isolation in a room or a dwelling. On the contrary, he had his cases transported by water to an island half a mile from the shore, which again was two to three miles distant from the principal town. Elaborate precautions were also taken as to prevention of personal transmission of the disease by the attending physicians. Waterhouse, however, thought that the island, as such, was unnecessary, provided there was used in its stead a hospital, situated a considerable distance from a public road, or on a point of land jutting out into the sea. Such was the New-England method of isolation, and Waterhouse at once joined issue with Haygarth, both as to clothing and as to the theory that small-pox was not aerially conveyable beyond the patient's chamber. Regarding aerial convection, here is his statement: "I would observe that that physician who believed the variolous infection to extend *thirty miles*, and he who supposed it to extend not *thirty yards*, seem, according to our observations, to be equally out of the way. Charleston is separated from Boston by a deep river, *fifteen hundred feet* wide. Close by the water-side in Boston was a house or two infected with the small-pox, in one of those dull, foggy days (such as commonly produces a dark day in London), when there was scarcely wind enough to blow the smoke from the tops of the chimneys; yet what wind was stirring wafted it across the river to Charleston. In its direction on Charleston side was a shipyard, and ten or a dozen carpenters at work, all liable to the disease. They all took the small-pox excepting two, and the eruptions appeared at the usual time from that day.

"We have the clearest proof of the small-pox being communicated to the distance of several hundred roods, viz., from the hospital at West Boston to several families in the neighbourhood. The atmosphere was for many days loaded with aqueous vapours, and the little motion it had was towards the houses where the small-pox made its appearance. The guard (for the hospital was military) which was continually kept prevented all communication." In a subsequent letter, Waterhouse states as to the cases which produced the cross-river infection, that the dissemination occurred "when several houses were highly infected". As to the exact distance, he adds that, since he had first written to Haygarth, a bridge had been built across the river, and the previous statement confirmed by measurement. It would, therefore, appear that there was no bridge when the infection was alleged to have occurred. In trying to reconcile Haygarth's facts and opinions with his own, Waterhouse argues for the variability of small-pox infectivity. In America he had observed "the difference in the virulency of small-pox observed at the different periods when epidemic here", and he urged that there may be differences in climatic conditions, differences in the atmosphere, and differences in individual susceptibility as between one time and another, and one nation and another.

In Haygarth's reply he tells Waterhouse that it was only the New England dread of small-pox that made them hurry patients away to pest-houses, and that in this way they were prevented from ascertaining the truth of the view that small-pox could not be aerially conveyed from one room to another in the same house; that they had plenty of fogs in England, but no record of their spreading small-pox; and as to the disease being carried six miles by means of a wig (as had been alleged by Waterhouse), he maintains that, according to no known chemical principle, could a wig attract miasms from the air and again restore them to it; and that if, indeed, a wig kept on giving forth small-pox virus for six miles it was capable of infecting 15,840 persons standing two feet apart in a line six miles long. Similarly, as to the cross-river case, if small-pox *could* be aerially carried for 1,500 feet, then in England "every person susceptible of the distemper *would* (the italics are mine) inevitably be attacked in a few weeks".

As to the poison being carried several hundred roods from a hospital, he asked Waterhouse to consider how extreme would be the dilution at this distance, seeing "that the variolous poison is diluted 500 times more at the distance of 45 feet than in the patient's chamber". Then Haygarth goes on to argue that negative evidence is "incomparably stronger" than affirmative evidence. He calculates that, "in any place visited by small-pox, if *one* has escaped the distemper, it is 19 to 1 he has not been exposed to the infection"; and that "if three in a family have escaped, the probability that they were not all three exposed is 8,000 to 1."* Therefore, he holds that the escape of persons within a specified distance from a source of infection is almost infinitely strong evidence that the infection could not have been carried so far from its source. And now, in the history of this controversy, we come to the remarkable fact that Waterhouse in reply expresses agreement with this doctrine of Haygarth's as to the value of negative evidence.† Though he continued to defend the various examples he had given, yet in yielding this point he yielded his whole case, and it is not surprising that Haygarth, just at the end of his book, is able to relate that Waterhouse, having subsequently had experience of a small-pox epidemic in Boston, had arrived by negative evidence at the conclusion that the virus was not atmospherically conveyable, as he had originally believed. Unfortunately, this letter of Waterhouse's arrived too late for publication by Haygarth, so that we have no details as to the negative evidence in question. It is, however, an interesting indication of the nature of the proof that Haygarth relates (from Waterhouse's letter) how, in this particular epidemic, a great number of failures occurred in conveying the disease by inoculation, even after four or five successive attempts, and how very few were attacked in the ordinary way, "although many were in the same room with those

* This calculation starts with the opinion (based on facts adduced by various writers) that about 5 per cent. of all persons born are insusceptible to small-pox. If 19 out of every 20 persons are susceptible to small-pox by aerial infection, the chances are 19 to 1 that an exposed person will contract it; and if any person has not taken small-pox, the chances are 19 to 1 that he has not been exposed to it. If two persons in a family do not contract the disease, the chances are 19×19, or say $20 \times 20 = 400$ to 1 that they have not been exposed; and if three persons in a family do not contract it, the chances are $20 \times 20 \times 20 = 8{,}000$ to 1 that they have not been exposed to it.

† Negative evidence is, of course, valuable when compared with affirmative evidence, as indicating the infrequency or frequency of aerial convection, and it is to be hoped that as facts accumulate in regard to the whole question, full use will be made of the comparison; but Haygarth's doctrine was in effect that negative evidence is destructive of affirmative evidence.

who had it full out upon them." Haygarth here runs considerable risk of proving too much by his negative evidence.

Reviewing, shortly, the main points of this interesting correspondence, we find :—

1. That Waterhouse started with the opinions: (*a*) That the small-pox infection depended on variable conditions of virus of atmosphere, and of subject ; (*b*) that it was a particulate poison; and (*c*) that, in regard to the extent of its atmospheric spread, affirmative evidence was valid, and that negative evidence was to be accounted for by one or more of the several variabilities.

2. That Haygarth held : (*a*) That the infection depended on practically fixed and unchanging conditions, alike as to virus, atmosphere, and subject; (*b*) that the virus was not particulate, but that at any given distance from its source the poison was equally dissolved throughout the whole atmosphere; and (*c*) that, therefore, negative evidence was much more valuable than affirmative evidence.

3. That Haygarth's pleading led Waterhouse to believe in this superiority of negative evidence, and that the next occurrence of small-pox in Boston furnished him with sufficient of such evidence to cause him to yield to Haygarth's views, that the spread of small-pox throughout the atmosphere was to be measured by a few inches, not by hundreds of feet or yards, and that even in a last-century dwelling-house it could not be carried by the atmosphere through an open door into an adjoining apartment.

To me Waterhouse's conversion seems a very high compliment to Haygarth's persuasive powers, rather than evidence of the truth of the thesis he sought to establish. In the present day it is not quite impossible that we are just a little in need of being warned by Waterhouse's example to avoid the pitfall into which he tumbled. We are perhaps too apt to forget that the aerial infectivity of small-pox may vary according to the intensity of the virus, the condition of the vehicle by which it is conveyed, and the state of the subject to whom it is conveyed.

About a year ago, when there seemed a likelihood of a prevalence of small-pox in Dumbartonshire, one of the points which I had before me as belonging to the teaching that an outbreak might afford, was this question of the aerial dissemination of the disease by hospitals. I therefore took account of the circumstances of one hospital which was likely to be used for Dumbartonshire small-pox, in order to see how its experience might be utilised in this connection. But at the very outset it became obvious that great, or even insurmountable, difficulties stood in the way. The hospital, which contains altogether 100 beds, 80 being intended for fever and 20 for small-pox, is situated in the country about 1½ miles from the outskirts of Glasgow. Its single gateway opens into a public road. Closely adjoining it on one side is a considerable Board School. The school playground is separated from the hospital ground by a stone wall, and the school gate opens into the same public road 100 yards from the hospital gate. On the other side of the hospital are three rows of miners' houses with a population of about 450, the ends of the rows being close to the same public road, and all of them having their only opening into the road. The distance of the small-pox pavilion from the nearest dwelling-houses in this village is about 340 feet. A dirty ditch runs between the gardens attached to these houses and the hospital ground. On the opposite side of the road from the hospital are a few colliery houses, and the only outlet from these is into the same public road. Now, if I state regarding this population that small-pox began to spread in it within a fortnight of the hospital's containing a given number of acute small-pox cases, and that at the same time certain atmospheric conditions prevailed, and that the hospital administration was practically perfect, and that no small-pox patients walked in on their own feet, and that the ambulance man was a confirmed teetotaller—if all this and more be stated, I have yet considerable misgiving that a reply may be made that the existence of small-pox around the hospital is to be accounted for by personal communication; that the school children were bound to be running after the ambulance waggon; that intercourse must have gone on between the hospital and the village; and that, indeed, no such evidence can be accepted in favour of aerial convection. But, as a matter of fact I have no such story to tell. The general administrative precautions very much resembled those of the London hospitals in 1881. The nursing staff had an afternoon out every week, and were free on every second Sunday. The engineer and some of the ambulance staff lived outside the hospital. Coalmen, a plumber, a man to cut the grass, came and went; school children were sometimes on the road when the ambulance passed; and, in short, the usual possibilities of personal communication existed. But the number of acute cases in the hospital at any one time was small, and neither by aerial convection nor by personal conveyance did a single case occur in the families to which the school children belonged, nor in the mining village. If

I am right in supposing that spread of small-pox here would very likely have been attributed to personal communication, is the absence of small-pox to be set down to the want of ability of personal communication to spread the disease? If not, then we arrive at the position that such a hospital is capable only of telling, either affirmatively or negatively, in favour of personal communication, and never in favour of aerial conveyance.

The point of these observations is, that if we are to arrive at correct conclusions on this subject we must weigh the evidence carefully and fairly as between the *likelihood* of personal communication on the one hand and the *likelihood* of aerial convection on the other. We start with the assumption that small-pox may be conveyed either personally or aerially, and the matter in dispute seems to be narrowed down to this:—

1. Do we know personal conveyance of small-pox to be so very frequent and constant and certain in its operations as to justify us in thinking it the likelier way to account for all the facts of such very remarkable and repeated outbreaks as are recorded to have occurred in the areas around some of the Metropolitan Asylums Board's Hospitals, and especially around Fulham Hospital? or

2. Is it, on the other hand, likelier that some of the facts are to be attributed to an ability of small-pox virus under special conditions to be conveyed aerially to a distance considerably beyond that which has hitherto been generally admitted?

In a recent able report by a Medical Officer of Health the case was put thus:—

"Some observers, oblivious or regardless of the daily and hourly opportunities of small-pox infection being carried out of hospitals by the door, in their desire to account for outbreaks, have even gone so far as to maintain that it had flown out by the windows, and have professed to be able to measure such flights by the quarter mile." But the question here is: Whether does the small-pox contagium resemble more closely the cockroach, which crawls out at the door, or the house fly, which makes its escape by the window; or does it not rather find its analogy in those groups of insects which are provided both with running legs and with wings; and is it not our present object to judge as to which of the two possible methods of progression best explains certain facts belonging to its life history?

In the first place, regarding personal communication, let me quote from a letter by Professor Wall, of Oxford, to Haygarth, contained in the work to which I have already referred :—" How comes it to pass that apothecaries, inoculators, nurses, etc., are continually going from house to house, while they are attending small-pox patients, without any care, and in ninety-nine cases out of a hundred without communicating the disorder to those who have not had it?" Is there not, after all, a good deal of truth behind this query? Everyone believes that a medical man *may* convey infection from one house to another; but if we consider the number of his outgoings from infected houses in any given week, or month, or year, even in the presence of a small-pox epidemic, or in his whole lifetime, and consider relatively to these the number of cases in which any atom of reasonable suspicion exists that he *has* conveyed disease, we get some indication of the value to attach to the entrances and exits belonging to hospital administration, many of them referring to tradesmen and others who never enter a ward, and have no direct opportunity of getting their clothing infected; while as to the nursing staff, the precautions observed as to the ablutions and change of clothing are far in excess of anything that is possible to a general practitioner. No doubt, however, it is here to be borne in mind that persons regularly on duty in a small-pox ward should require more thorough disinfection than a medical practitioner on his daily rounds.

One or two instances have struck me particularly in reading the more recent literature that has grown up around this question. The dustman of one of the M. A. B. hospitals caught small-pox, and though doubtless removed to hospital immediately on the disease declaring itself, he yet succeeded in infecting members of his own household. These people, therefore, were susceptible to small-pox. But, for years before, that dustman had been going in and out of the hospital carrying on his work of refuse removal there, and in and out of his own home, yet he had never brought infection within his doors till he himself was seized with the disease. Similarly, in the course of the Fulham inquiry, no case was found of small-pox occurrence in the houses visited by officers and servants of the hospital, and nine non-resident servants did not carry the disease to their houses in nine different streets. Again, in considering one particular group of cases thought to be due to aerial convection, Mr. Power tried to exclude all as to which personal conveyance could be in the least suspected. One excluded case was that of a man who had had a drink in a beershop along with the driver of an ambulance waggon. How many other people may have been drinking at that time, or on other journeys, along with this driver, and yet not have been infected by him?

This question of the influence of ambulances in spreading the disease around hospitals was before the Commission of 1881 at some length. On one hand it was in evidence that the service was sometimes badly managed; that the driver might stop at a public-house on the way; that children might hang on behind the van. On the other hand it happened that some ambulance routes were pretty well fixed, especially near the hospitals, and that in the streets regularly traversed by them there was no special prevalence of the disease. This fact is noted not only regarding Fulham Hospital, but also by Dr. Bristowe as to Deptford Hospital; indeed, examination of details seems to show that the quadrant of the mile circle, containing the chief line of human intercourse with Fulham Hospital, chanced in 1881 to have a smaller percentage of its houses attacked by small-pox than any of the other three quadrants. It is true that people living in one street might encounter the ambulance in another, and go home and lie down with the disease a fortnight later. But it is equally true that on any sufficient basis of facts a larger proportion of people belonging to the streets traversed by the vans would be exposed to the ambulance influence, than of people belonging to other streets. And in regard to those who met the van in one street and lived elsewhere, it would be a very extraordinary thing if the houses to which these people returned happened to be arranged in numbers diminishing regularly according to the distance from the hospital to which the van was travelling. It would be still more extraordinary if this same regularity of arrangement were found to repeat itself around the same hospital in epidemic after epidemic. It is a remarkable fact that one witness who maintained the view that hospitals were not centres of infection, either aerially or by personal transmission, pointed in support of his thesis to one street in particular, a lane only 10 feet wide, closely adjoining Homerton Hospital, and forming a passage for nearly all the ambulances that entered its gates. There had been only one small-pox death belonging to this street in the decade 1871-80; but, on turning to the number of inhabitants attacked in this thoroughfare, I find from Dr. Tripe's figures that they amounted to 13 per cent. of the average total of inhabitants, a proportion which, in comparison with that of the adjoining streets through which the ambulance did not habitually pass, was higher than some and lower than others.

Similarly, it was pointed out that some of the persons attacked within the "special area" surrounding the hospital might have got their infection in other parts of London to which their daily avocations took them. This is so; but again, is it likely that these people would come home and settle themselves down in regularly decreasing circles around the small-pox hospital? It is not to be denied, however, that, excluding ambulances which had pretty clearly defined routes of traffic to the hospital, and excluding introductions of the disease from other parts of the Metropolis, such small-pox as depended on personal communication from the hospital would tend to range itself in radii corresponding to the lines of communication with the hospital, the amount decreasing with the distance. And I do not for a moment suggest that personal conveyance of one sort and another had not a great deal to do with the matter. Here, as elsewhere, many groups of cases would be secondary to, and directly connected with, foci outside of the hospital, and it is in no way surprising that evidence of conveyance of this sort was able to be put before the Commission. But such foci, unless the mischief done by them was always equal, might tend rather to disturb than to emphasize gradation from centre to periphery; and it appears that in the locality around Fulham Hospital practically all cases were sent to hospital after their discovery. It has never been suggested, however, that aerial convection excludes personal communication, or that the area around the hospital forms an unsuitable field for the latter method of spreading the disease. The question is, Is personal communication the likeliest way to account for *all* the facts?

Coming now to the subject of aerial convection, both in Waterhouse's time and more recently, various theoretical considerations have been adduced with regard to the possibility of the survival of infective power through the requisite space for the requisite time. The distance to which an animal odour like the smell of the skunk can be carried, and the phenomena of hay fever, are instances in point. Too much value may easily be attached to such analogies, and they might be used in support of a thesis that measles, or whooping-cough, or typhus, can be conveyed for long distances. I may note, however, that Dr. Waterhouse quotes from Dr. Aspinwall to the effect that "it is a fact well known to every ploughboy, that a skunk can be smelt much further in a dampish, foggy evening than in serene weather". The length of time during which a lancet charged with small-pox matter can retain its inoculative power is more to the purpose, but very exact and careful observations would be needed to give evidence of value here. All we seem to know is, that certain animal emanations are not affected by long travelling through the atmosphere, but whether or not this is true of small-pox under any circumstances is a question that must be settled by evidence. There may be very important distinctions between the length of duration of the quality of smell by non-living material, and the length of duration of powers belonging to what current pathology teaches to be a contagium vivum. Probably, however, Sir Thomas Watson fairly represents the consensus of opinion in his own day when he says, "There is no contagion so strong and sure as that of small-pox, none that operates at so great a distance."

Throughout this paper I am assuming that the members of the Epidemiological Society are well acquainted with the general lines of the Fulham investigation. I do not therefore propose to recapitulate the evidence, nor to point out how, when small-pox was not being received into the hospital, as in the periods antecedent to March 1877, and from the end of 1881 to May 1884, there was no special incidence of the disease in the one-mile area around the hospital, nor any regularity of distribution in quarter-mile belts within the mile area of such small-pox as did exist; nor how, on the other hand, when the hospital came to be in operation, its surrounding mile area became specially affected, and how at the same time the subdivisions of this area were affected relatively to their propinquity to the hospital; nor how these phenomena have been observed with remarkable regularity in one epidemic after another; nor how the parishes in which small-pox hospitals were completed in 1871, or afterwards, had their small-pox prevalence altered relatively to that of parishes which contained no such hospitals, so that some parishes which aforetime had been comparatively exempt from the influence of small-pox epidemics came under that influence to a very remarkable extent, and displaced other parishes which had formerly headed the list; nor how the positions of the parishes containing small-pox hospitals varied according as these hospitals were closed or in active operation.

In the evidence given before the Royal Commission, two groups of cases occurring within the "special area" of

Fulham Hospital yield an interesting comparison. The first group was reported by Dr. Dudfield, Medical Officer of Health for Kensington. It included 41 cases, occurring, after establishment of the epidemic, in Ifield Road, a thoroughfare about 800 feet from the hospital. Dr. Dudfield indicated that, as a rule, his main concern in such matters was rather with the stamping out of the epidemic than with the tracing of connections between cases, and that, indeed, much of such inquiry was usually made through inspectors. But nearly a year after the first of the cases had occurred, and after some of the people had left the locality and could not be found, he made more particular investigation, and succeeded in ascertaining personal communication from one to another of about half of these cases. They occurred, not simultaneously but in succession, and there can be no doubt as to the soundness of Dr. Dudfield's inferences of cause and effect between these cases.

The other group of cases to which I have referred is reported by Mr. Power. For four weeks after the opening of Fulham Hospital, near the end of 1880, there were no small-pox attacks in Chelsea, Fulham, and Kensington, the parishes nearest the hospital. In the next four weeks there were 11 attacks in these parishes, 7 being within the mile area. These were mostly explicable by personal communication. Then suddenly in the next fortnight the attack in the three parishes amounted to 62, of which 47 were in the special area, and 11 more within half-a-mile of it. Of households there were 56 attacked, 41 being within the special area, and 11 within another half-mile. Next it is noted, that of the 62 cases 42 were attacked during the five days Jan. 26-30, 32 being within the area and 8 more within half-a-mile of it. The facts as to these 32 were very carefully investigated, especially as regards their proceedings a fortnight previously. In 9 cases reasonable suspicion could be entertained of infection from personal communication. As to the other 23, no such evidence was obtainable. The cases were dotted all through the populated part of the special area, and were unconnected with each other, and with any known source or sources of infection, either from hospital intercourse or otherwise. At the time when they must have been infected only one case of the disease, so far as most elaborate inquiry could ascertain, existed in the special area, and small-pox was almost absent not only from the three parishes, but also from the parishes lying between the hospital district and the East-end of London, where there was a certain prevalence of the disease. Meteorological inquiry elicited the fact that the period from Jan. 12th to Jan. 18th (that is, a fortnight preceding the outbreak) was "characterised by still, sometimes foggy, weather, with occasional light airs from nearly all points of the compass". These conditions singularly resemble those which Waterhouse had long before recorded as accompanying similar outbreaks; and, indeed, I may note in passing that the similarity of the conclusions arrived at by the two observers is a very striking fact in view of the obvious inacquaintance of the later investigator with the ground that had been traversed by the New-England Professor a hundred years before.

It is of great consequence here to note what were the inquiries made, so as to exclude the likelihood of personal conveyance. They were stated as follows before the Royal Commission: "I took all possible means. I did not finish off a case, so to speak, at one sitting, but went back to it, and, as well, got additional information from friends, and so on; and then I reconsidered the case, and tried, so to speak, to live over again the attacked person's life during the time when I regarded him as having become infected. And thus I returned again and again to a patient with a view of getting information upon a point I might have overlooked, or that he or his friends might have overlooked. A great many of the people got interested in the question, and thought the thing over with me. For instance, a commercial traveller sent home for all his official books as to his daily doings, and we went through the whole thing together, as to where he was at this and that hour on each of certain days. Wherever they could, the people helped me very much, and the neighbours helped me too. I do not think anything was missed, because I was morning, noon, and night in the hospital or about it, seeing the people and their friends. I used to deal with the patient in the first instance—that is, if he was well enough, and few were so ill in the early stage of their attack that they could not give account of themselves. I would first ask them questions as to whether there had been any illness in their house, however trivial, and then go through with them their doings on particular days, when I supposed they might have got their infection. I would ascertain whether on particular days they were in the district and where, or out of the district and where, with a view to ascertaining whether they could have got their small-pox within the district or outside of it. And I asked special questions as to whether they could have had communication with antecedent cases that I knew of as having occurred in the district, and from which they might have got their infection. I cross-questioned them backwards and forwards in that way. Now and again I did get some sort of information, which led me at last to surmise that they might have come across small-pox infection irrespective of the hospital; but still, in the large majority of cases, I could not get even a suspicion of that sort. I was puzzled altogether about it."

It seems to me that there is a very obvious contrast between these two outbreaks, recorded respectively by Dr. Dudfield and Mr. Power. The one outbreak occurred after small-pox had been established in the district, the other preceded a general prevalence of the disease. The one outbreak belonged to a particular street, the other had its cases dotted all over the special area. The one outbreak had its items occurring in succession either of individuals or households; in the other outbreak the cases occurred simultaneously. In the one outbreak, 50 per cent. of the cases were traceable to personal communication, partly by means of lay inspectors mainly interested in isolation and disinfection, and partly long after the occurrences, by Dr. Dudfield's own special inquiry. The other outbreak gave no indication of causal relationship between 23 cases, even though investigated when the outbreak was in progress by a skilled inquirer specially bent on hunting out all possible sources of origin. Surely, if we accept the one outbreak as indicative of personal communication, we cannot logically refuse to accept the other as indicative of aerial convection. It happens, too, that of the cases recorded by Mr. Power as belonging to the special outbreak of Jan. 26-30th, twelve occurred in Kensington, and, therefore, in Dr. Dudfield's own territory; and I learn from the proceedings of the Commission that, in view of Mr. Power's report, Dr. Dudfield had set himself to investigate these twelve cases, again mainly at a considerable interval after their occurrence. Like Mr. Power, however, he entirely failed to get any trace of their origin, so that we have here a most competent observer who successfully traced, even under the difficulties mentioned, 50 per cent. of one set of cases, and yet was quite baffled by another set which had equally baffled a previous inquirer.

In his evidence before the Commission, Sir John Simon, with characteristic caution, stated that while he attached great weight to the evidence already produced in support of aerial convection, he would wait for further confirmatory evidence. This requirement has to a great extent been fulfilled. As a result of the original inquiry, and of the work of the Royal Commission, Fulham Hospital was put on its mettle. Those in charge of it knew how much might depend on their success or failure in preventing the spread of infection by personal communication, and we need not doubt that no stone was left unturned in the endeavour to prevent subsequent confusion of issue. The outgoings of the staff were reduced to a minimum, and increased stringency was observed in every detail. Yet, when the next outbreak occurred in 1884-85, the result was practically as before. All the extra care taken failed to lessen the incidence in the special area per 100 admissions of acute cases to the hospital. Indeed, the number of acute cases within it was remarkably small when, in the presence of weather conditions resembling those formerly noted, a "special area" outburst occurred. It was further observed, however, that these meteorological conditions, repeating themselves subsequently, were not always accompanied by increased infection around the hospital. In view of such facts, it is obvious that there is still much to learn regarding the whole question, and it is open to us to speculate whether the rhythm of epidemic and non-epidemic periods includes, as by analogy we may easily believe it to include, lesser rhythms of ebb and flow of infectivity, having to do not with whole seasons, but with weeks or days. In this aspect of it, such special outbursts as have been noted might be looked on as spring-tides of infection.

From October 1884, a part of Fulham Hospital not previously used for scarlet fever came to be devoted to that disease. The management, as regards laundry, nurses, and servants, was quite separate from that of the small-pox wards, the only official in common being the medical attendant, who, of course, was well aware of what had been alleged regarding personal communication, and who, we may assume, was correspondingly careful. In the fortnight ending Dec. 6th, following the opening of these scarlatina wards, there was great increase in the admission of acute cases of small-pox, and excessive spread of the disease in the special area. At this time, four cases convalescent from scarlet fever were attacked by small-pox, and all the others were at once re-vaccinated. These four cases constituted 10 per cent. of the total scarlet-fever cases. But no small-pox case was attacked by scarlatina; and it is noteworthy that the hospital did not act as a centre for the spread of scarlet fever or typhus, or any other such disease, in the special area. We know very well that these diseases, and especially scarlet fever, can be conveyed by personal communication; but, as a matter of fact, the administrative procedure of the hospital did not spread even scarlet fever. If it be replied, that perhaps small-pox is conveyable further and more surely by fomites than is scarlet fever, the comment is obvious, that in that case small-pox is likely also to be further conveyable atmospherically.

In conversation recently with Dr. Ernest Marsh, who had charge of the small-pox wards at Belvidere Hospital during the past year, I was informed that one scarlet-fever ward had its windows 48 feet from the nearest small-pox ward, and that the only one of the scarlet-fever patients whose vaccination was neglected to be attended to, was a little American boy who had never been vaccinated, and who straightway was attacked by severe small-pox. The administration here was entirely separate, even as to medical attendance, and a barricade 13 feet high separated the one part of the hospital grounds from the other. Similar instances of aerial convection measured by feet are not uncommon, and the facts are, of course, much more easily proved than where hundreds of yards are in question. Dr. Septimus Gibbons mentioned to the Commission a case in which a distance of 20 yards intervened; Dr. Richardson a case of 30 feet; and Dr. Munk a case of 41 feet, and so on. And long before the theory of distant aerial convection was formulated, Drs. Gregory and Marson, of the original Small-pox Hospital, strongly insisted on a belt of 150 feet around the hospital, and their successor, Dr. Munk, supported the same view before the Commission.

I have already incidentally mentioned, as part of the thesis of aerial convection, that it depends on acute cases, not on convalescents. Probably it will not be denied that personal communication, both of scarlet fever and of small-pox, is likely to occur during desquamation in the one case, and separation of the crusts in the other. But one of the earliest facts ascertained as to Fulham Hospital was, that when it was devoted entirely to convalescents who were conveyed there in large numbers under the same ambulance arrangements as were criticised when acute cases were in question, small-pox did not spread in the special area. At that time the outgoings of servants, and visitors, and tradesmen were singularly powerless. At Darenth Hospital 600 convalescents failed to spread the disease. As with regard to distance, so with regard to limitation of admissions of acute cases, there is observable a tendency to give effect to the thesis while not formally adopting it. Various witnesses mentioned various numbers to which they would restrict the totals of cases under treatment in any hospital. Of mixed cases, such figures as 150, or 100, or 80 were given; and of acute cases, one witness said 50 or 60, another 20, and so on; and in later epidemics the numbers in the intra-urban hospitals were limited first to 50, afterwards to 25, and finally small-pox was altogether excluded from them.

The above are the main points that have impressed themselves on me as supporting the view that small-pox may sometimes be aerially conveyed to a relatively long distance. It is not my purpose to do more than refer to the confirmatory evidence recently yielded by the outbreaks in Oldham and Warrington as recorded by Drs. Niven and Gornall. Probably here, as elsewhere, both agencies may have been at work, but it is certainly of consequence that these observers have not been able to account for some of the facts that came before them unless by acceptance of the atmospheric theory. In the course of his Report for 1893 Dr. Niven says:—"Is this increased incidence round Westhulme Hospital due to aerial diffusion, or to faulty administration of the hospital, leading to direct contact between the untraced cases and the officials of the hospital a fortnight before their attacks? I may say at once that I am satisfied that no such contact has taken place. Whether in isolated instances small-pox may have been caught by coming too near the hospital it is difficult to say. For the most part that possibility could be excluded." Dr. Gornall says:—"The statistics as to the relative incidence upon the specially afflicted area and its several parts most clearly agree with what we should expect with *air-borne infection* as the explanation of our difficulties." As to foreign evidence, a paper by Sir John Cormack, in the *Edinburgh Medical Journal* for 1881, shows that in Paris facts more or less similar had been observed in connection with hospitals there.

Of the arguments urged against the theory, by far the most striking is that regarding the incidence of the disease on certain institutional populations within the special areas. These, indeed, claim to furnish a series of control experiments in which absence of small-pox was suggested to be due to absence of personal communication as a result of careful administration. One of the members of the Royal Commission put the case very strongly to a witness who inclined to support the theory of aerial convection. He referred to "the fact that in some cases workhouses are actually overlooking the grounds of a small-pox hospital with actual immunity from small-pox". Similarly, Dr. Dudfield in his evidence said:—"You know the case of Homerton Hospital, which is from 90 to 100 feet distant from the City of London Infirmary, in which no case of small-pox has occurred since the hospital has existed: that is a case which I have very much relied upon myself, and I know it has had great weight with my colleagues, the medical officers of health." With reference to the same institution, the Commissioners stated in their Report that "the workhouse had scarcely any cases in the epidemics of 1871-77 when the disease was extremely prevalent in the surrounding streets, although at that time the inmates were not protected by re-vaccination. The same may be said of the Hackney Union Workhouse and Infirmary, which are about a quarter of a mile from the Homerton Small-pox Hospital."

The strength of the argument thus set up against aerial convection is so obvious that it is of importance to look into the details of the facts on which the argument is based. The institutions in question were of two classes, workhouses and Poor-law infirmaries, the former being capable of much more thorough protection against personal communication than the latter. Eight of such institutions were mentioned, four being about half a mile from Fulham Hospital; one about a quarter of a mile and another about 200 feet from Highgate Hospital; one (Hackney Workhouse) about a quarter of a mile and another (the City of London Workhouse) about 100 to 200 feet from Homerton Hospital. No details are given as to the institutions related to Fulham Hospital, but, as the distance was considerable, this absence of information is of less consequence, especially as the fullest statement of facts has to do with the institution which was nearest to a small-pox hospital, viz., the City of London Workhouse adjoining Homerton Hospital. In Dr. Bridges' Report of January 1881, republished by the Royal Commission, he wrote:—"I have made careful inquiry for any case of small-pox occurring in this Workhouse during the present or previous epidemics among the inmates, but I have only been able to find one, and this one not attributable to the hospital." It seems likely that this statement may have been that which weighed with Dr. Dudfield and the medical officers of health, and which was also in the mind of the member of the Royal Commission in the above quotation. But in Dr. Bridges' evidence before the Commission, nearly a year later than the date of his Report, he puts the case less decidedly in these words: "I have stated in my Report that in the City of London Workhouse . . . . very few cases of small-pox have occurred." Subsequently, Dr. Aveling, the medical officer of the workhouse, put the exact figures before the Commission (Qs. 5037 and 5059-60). He stated that 17 cases had occurred in the eleven years ending December 31st, 1881, and that certainly 12, and possibly 15, of these contracted the disease within the institution. The number of inmates is given as 450. Next, the facts as to vaccination require careful attention. In the decade there were three epidemics, the first being by far the worst. The second began about the beginning of the summer of 1876, and may be said to have extended to the end of the summer of 1879. It caused about 3,600 deaths in London. Some 650 of these occurred previous to the end of 1876, and the remainder (approaching 3,000) after that date. Now Dr. Aveling states (Q. 5043) that in the workhouse "re-vaccination was begun properly at the end of 1876, so that these eleven years are divided into two series, of six years when re-vaccination was not properly enforced, and a subsequent period of five years when it was tolerably enforced". As to the second epidemic, therefore, we see that less than one-fifth of it had passed before general re-vaccination was instituted, and it is to be particularly noted that of the total 17 cases, 13 occurred in the period ending 1876. In the various streets within a quarter-mile radius of the hospital, the proportion of the population attacked in the ten years 1871-80 ranged from 4 per cent. to 25 per cent., the average being 9 per cent.* In the workhouse, on the other hand, only some 3 per cent. were attacked. The difference is certainly considerable. But the fact that the cases in the workhouse practically stopped after 1876, when re-vaccination became part of the administrative system, goes far to account for the low attack rate calculated over the whole eleven years. Whether, in addition, anything is to be attributed to the age-distribution of the workhouse inmates, or to previous small-pox, or to the well-regulated conditions as to ventilation, cleanliness, etc., under which they were compelled to live, are questions as to which I have no information. But, on the hypothesis of atmospheric diffusion, it seems likely that people who are mainly kept within doors will be less

* See Table, p. 48, *Roy. Com. Report.*

exposed to infection than persons who walk abroad in the special area without restriction of any kind. In regard, however, to the whole comparison, it is to be borne in mind that the theory of aerial convection does not exclude that of personal communication; and in addition to the influence of re-vaccination, some part of the difference above indicated may well be due to excess of personal communication outside of the institutions as compared with the amount inside. Indeed, it is interesting to notice how this very theory comes in with regard to these workhouse cases. The general thesis was that the absence of small-pox was due to the absence of personal communication, and that in particular this absence of personal communication was characteristic of workhouses rather than Poor-law hospitals. But when attention was fastened on the fact that not 1, but 12 or 15 cases *had* occurred within the workhouse, then at once it was adduced in explanation that "the visiting had only been stopped at one place one time, and that for a very short time. Visitors came once a month."

In Hackney Workhouse and Infirmary, which was about a quarter of a mile from the hospital, and had in the eleven years 1870-81 an average of 643 inmates, the total number of small-pox cases originating within it amounted to about 20—again about 3 per cent. It was the custom here to vaccinate very early all children born in the institution, and to offer re-vaccination to all the inmates, but a good many refused it. The facts, therefore, are not unlike those relating to the City of London Workhouse. As to the other institutions mentioned, their officials were not called on for evidence, so that detailed examination of the figures cannot be made.

In going over this part of the evidence in the Royal Commission's Report, I made a note to this effect: Were there no other equal populations, not institutional (and, therefore, with no check on personal communication), within the special area which equally escaped? And near the end of my reading I found attention called to the fact that within a quarter of a mile of Fulham Hospital, within the north-west segment of the circle, there were 121 houses which had entirely escaped from 25th May to the middle of September, though 153 admissions into the hospital had taken place during that time. Here, then, was a place which, as regards administrative effort within it to prevent personal communication, was at the opposite pole from the workhouses, but which yet had not a single case of small-pox during these months. The evidence, however, was adduced, not to throw doubt on the power of personal communication, but on that of the rival theory of aerial convection. But surely the facts are as applicable to the one theory as to the other.

On the whole, I do not think that this evidence regarding these institutions can be looked on as at all sufficient to counterbalance the evidence on the other side.

The practical conclusion of this whole question may be said to have already been arrived at. Small-pox hospitals are not now erected in the midst of towns, and those already in existence are being more and more sparingly used. Indeed, where the power of aerial convection is still doubted, it seems to be assumed that the prevention of personal communication is impracticable, and that accidents incident to the system of hospital treatment of small-pox within populous districts must be accepted as inevitable, so that the only remedy under the one theory, as under the other, is the removal of such institutions to a distance from populous places.

Summing up the evidence, so far as it is possible to sum up on a subject as to which so much is still to be learned, we seem to reach the general conclusion that, as a result of the simultaneous action of causes favourable to the spread of infection, the contagion of small-pox may be conveyed atmospherically to a distance much greater than had been usually admitted, a distance measurable by quarters of miles. In endeavouring to summarise the agencies which have to do with this result, it is necessary to bear in mind that other unknown agencies may also be involved, and that sometimes the joint action of all the known agencies may not be necessary, special activity of some, perhaps, atoning for relative deficiency or absence, and *vice versâ*, of others. Keeping this in view, the factors whose coincident operation can produce the result in question may be subdivided as having to do with: 1, The contagium; 2, The atmosphere; and 3, The population.

1. There must apparently be intensity of virus depending on (*a*) the period of the epidemic, a rising epidemic being important, and (*b*) concentration of acute cases as centres of infection. (*c*) There are apparently also minor waves of epidemicity referring to particular days or weeks, these waves constituting flood-tides of infectivity.

2. (*a*) A foggy condition of atmosphere or light winds appear to be of consequence, as observed both by Waterhouse and Power; (*b*) possibly *only* the atmosphere of

towns or cities may possess the necessary carrying power. No good evidence has yet been adduced of such occurrences in connection with hospitals situated in rural districts; but, no doubt, the failure here may be as much owing to want of population as to atmospheric condition.

3. Bearing in mind the conclusions arrived at by Dr. Whitelegge in his Milroy Lectures, it may well be the case that part of the influence of a rising epidemic is due to the existence in any special area of a greater or less number of persons specially susceptible to small-pox, and easily infected by its first active onset. The supply of such persons would be rapidly diminished or exhausted, and would not be renewed in the course of any single epidemic.