AERIAL CONVECTION FROM SMALLPOX HOSPITALS

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Forty years ago the theory of the spread of infection from smallpox hospitals to the surrounding population by means of "aerial convection" was accepted by a majority of epidemiologists and regarded as definitely established. To-day the theory has receded into the background, and many are frankly sceptical as to its validity. Thus Harries and Mitman, in their *Infectious Diseases* (second edition, just published) say, in referring to this question, "There are still some who hold the view," but it is clear that they themselves disbelieve in it. In Jameson and Parkinson's *Synopsis of Hygiene,* published in 1934, the theory is not even mentioned, though they give the Ministry of Health's regulations as to the siting of smallpox hospitals, which are based on the theory. It scarcely seems likely that this omission was an oversight. Parkes and Kenwood's *Public Health* is also against it. C. V. Chapin, formerly Superintendent Officer of Health for Providence, U.S.A., in his *Sources and Modes of Infection,* published in 1910, after discussing the question at some length sums up against the theory; and he gives the impression that this is the general view in America.

The question is by no means of merely academic interest. On the contrary, it is of considerable practical importance. Local authorities throughout the country have to make provision for the isolation of smallpox, even though it be only for odd cases which may never occur; and to comply with the Ministry of Health's regulations such hospitals have to be at least a quarter of a mile from another institution or population of 200, and at least half a mile from a population of 600. To maintain all these institutions, seldom or never used, is not an economic proposition; and to open one in a hurry, at short notice, and provide the requisite staff, is often most inconvenient. If the theory of aerial convection can really be disregarded as of no practical importance, there seems no reason why smallpox cases should not be treated in a separate ward in a general isolation hospital with better supervision and much less inconvenience.

It seems desirable, therefore, that the whole question should be reviewed by going back to the original reports upon which the theory was based; and by the courtesy of the Medical Department of the Ministry of Health I have been given access to the Ministry's Medical Library, where I have been able to refer to these reports and other relevant literature. This article is based upon what I have recently been reading.

History of the Aerial Convection Theory

It was in the great epidemic of 1871 that the Metropolitan Asylums Board began to isolate smallpox cases in hospital; and until 1886, when cases were removed to the Hospital Ships at Long Reach, on the Thames, to the east of London, the smallpox hospitals were situated inside the Metropolis and surrounded by thickly populated areas. It was in respect of Fulham Hospital, in 1881, that W. H. Power, a medical inspector of the L.G.B., demonstrated the influence of the hospital on the prevalence of smallpox in its neighbourhood. He was able to show, and illustrate by a series of spot-maps, that following the use of this hospital for smallpox a graduated intensity of smallpox incidence occurred among the surrounding houses, the percentage of houses invaded becoming gradually less as the distance from the hospital increased. This "influence" of the hospital could be observed up to at least one mile. He satisfied himself, after careful inquiry, that this influence could not be explained by "hospital operations"—i.e., contact with the hospital staff, visits of tradesmen, movements of ambulances, and visits of patients' friends or relatives. He therefore propounded the theory of "aerial convection," or distal conveyance of infection through the atmosphere. Although this had often been suggested before, Power was the first to put the theory forward officially and attempt to justify it scientifically.

The striking facts obtained with regard to Fulham naturally led to inquiry as to whether other smallpox hospitals in London had brought about increased prevalence of the disease in their surrounding neighbourhoods; and in the Report of the Medical Officer of the L.G.B. for 1886 there was published a section—"Statistics of Smallpox Relative to the Operation of Smallpox Hospitals in the Metropolis"—which showed unmistakably that during the period 1876–85 the hospitals receiving acute cases had been associated with an excessive prevalence of smallpox in surrounding areas. Similar evidence was forthcoming from smallpox hospitals in the Provinces. Speaking in 1886 at a special debate on this question before the Epidemiological Society, Dr. G. S. (afterwards Sir George) Buchanan said:

> "We cannot get away from these facts; they are as definite as any known to epidemiology. They had already been ascertained by a multiplicity of careful and detailed observations, in many hospitals, in different epidemics, in London and the Provinces. Recent epidemics have now enabled the question to be tested afresh."

That smallpox hospitals have had a deleterious influence in disseminating the disease in surrounding areas is now admitted, so there is no need for this aspect of the case to be argued further; but it is noteworthy that with no other disease has a similar influence been established. In this respect smallpox stands alone, which proves that its infectivity is exceptional. It is as to the explanation of the influence exercised by smallpox hospitals that difference of opinion exists.

Those who disputed the theory of aerial convection argued that the increased prevalence around hospitals could be explained by human contact in connexion with the normal operations of the hospital, supplemented by irregularities due to laxity of discipline. Against this line of reasoning there was the fact that only in the rarest instances could any contact, direct or indirect, be discovered, in spite of searching inquiry; the great majority of the cases occurring in the vicinity of hospitals were untraceable except on the theory of aerial convection. This is quite contrary to the usual experience in investigating the aetiology of smallpox cases, for careful inquiry will generally elicit some human contact, direct or indirect. There was also the fact that the increased incidence did not follow the main traffic routes to and from a hospital. Moreover, it was pointed out that when hospital staff go on leave they do not visit in the immediate vicinity of a hospital, but rather make for the shopping centre and places of entertainment; also, that tradesmen are not usually appointed because they happen to reside near to the hospital, and that visitors to patients come from all parts of a town where the patients themselves reside. Lastly, even though laxity of discipline might account for occasional spread in the early days, it is to be expected that it would be tightened up after attention had been publicly drawn to the danger. Yet in the case of the Fulham Hospital the influence reported by Mr. Power in 1881 was found to be again in operation in the epidemic of 1885.

Some epidemiologists still seem to think that there is an inherent improbability in aerial convection, so it may be well to say a word about this. It may be taken for granted that the infection of smallpox, as with all infectious diseases, is particulate, and we know that finely particulate matter—carbon, cement, lava—can be carried by the air for great distances. In some manner the infection of smallpox leaves the patient's body and gets into the air of the sick-room or ward, and a susceptible person inhaling that air will readily contract the disease. Infection, then, can be carried by the air for short distances, and this may happen in the open air as well as in the sick-room. The only real question is, For how long can the infection survive exposure to the outside atmosphere? And here it is important to remember that the infection of smallpox, probably more than that of any other disease, can be carried by fomites. This fact certainly suggests that it is exceptionally resistant to desiccation and that it can survive for considerable periods outside the human body. This may explain why aerial convection has been demonstrated only in the case of smallpox. It would seem, therefore, that there is really no inherent improbability in the theory; it is just a matter of weighing the evidence.

Smallpox in the Orsett Union

The theory of aerial convection as opposed to contact conveyance by hospital operations received striking support from

the experience of the Orsett Union, in the epidemic of 1901–2, in connexion with the Hospital Ships at Long Reach, where cases of smallpox from the Metropolis were then being treated. These hospital ships were moored in the Thames near the Kentish shore, while the Orsett Union was on the opposite or Essex side. During this epidemic the Orsett Union suffered severely, and the outbreak was the subject of a special report by Dr. G. S. Buchanan. He wrote:

"The most striking feature of the epidemic is the remarkable intensity of its incidence on the small community of Purfleet. . . . Out of 110 dwellings no less than 44 were invaded by smallpox. . . . As regards the influence of the hospital ships upon the prevalence of smallpox in Purfleet, we have to choose between aerial convection or nothing; there is here no question of infection, direct or mediate, through human intercourse or traffic."

Purfleet was the parish in the Orsett Union which was the nearest to the ships on the Essex side of the river, but all communication with the ships was from the Kentish side. Not only did it suffer by far the most heavily, but it was the first to be attacked. The next most heavily attacked was West Thurrock, which also happened to be the next nearest to the ships. No other explanation of these facts could be given, for in this case hospital operations could be ruled out. Dr. J. C. Thresh, M.O.H. for Essex, pointed out that all this had happened before, and that in the two previous epidemics since the ships had been used for smallpox Purfleet had suffered exceptionally. The distance from the ships to Purfleet was under three-quarters of a mile, but Dr. Thresh believed that the influence of the ships could be traced for a much greater distance to other parishes in the Union.

The Epidemiological Society Debate

In 1904 a full-dress debate on this question took place before the Epidemiological Society of London, when a paper supporting the theory of aerial convection was read by G. S. Buchanan, and many of the leading epidemiologists of the day took part in the discussion. Dr. C. B. Ker, Medical Superintendent of the Edinburgh City Hospital, who opened against the theory, said that he based his objection on his own negative experience at Edinburgh. There the smallpox hospital adjoined not only the fever hospital but the poor-houses and another institution, and there was a population of some 3,000 within the one-mile radius; yet, although for three months they had had an average of nearly 50 smallpox patients under treatment, there was no evidence of infection having spread from the hospital. He concluded that, while not denying the possibility of aerial convection, for practical purposes it might be disregarded. Dr. (afterwards Sir Arthur) Newsholme considered that the theory was not yet proved, and he waited for further confirmation. He thought that excessive prevalence round a smallpox hospital, when not due to coincidence, could be explained by personal contact; and as regards the Purfleet experience, he suggested that surreptitious visits by members of the hospital ships' staffs to the Essex shore might have taken place. (Dr. Buchanan in his reply said that the only instance of this discovered was the case of one man who had visited his sweetheart.) Analysing those who took part in the debate, we find that 12 were in favour of aerial convection and 7 against. Apparently expert opinion at that time was strongly in favour of the theory, and it is noteworthy that since then there has been no further opportunity of testing the theory, owing to the disappearance of epidemic smallpox from this country. In the presence of positive evidence, such as certainly exists from a number of different hospitals in different epidemics, negative evidence from other hospitals cannot settle the question. Innumerable instances have been recorded of failure to infect by personal contact (as where tramps in the early stages of smallpox have wandered about a town without infecting others), yet no one would suggest that this proves that smallpox is not infectious by personal contact. As regards aerial convection, we know little about the conditions necessary for the survival of the smallpox virus outside the human body, or of the meteorological conditions most favourable to this. It has been suggested that it may be related to the presence or absence of ozone, dampness, fog, etc., in the atmosphere.

To sum up, I feel that the case for aerial convection is very strong—much stronger, indeed, than many present-day sceptics imagine. I think that if they could spare time to consult the original reports they would admit this. At the same time it might be justifiable to allow certain hospitals to try isolating smallpox in general isolation hospitals and watch the result.

In the meanwhile, the theory of aerial convection does certainly provide a solution (and here I speak from personal experience) for remarkable facts in smallpox aetiology which cannot otherwise be explained.

CANCER RESEARCH

Prof. H. R. Dean, chairman of Council of the Imperial Cancer Research Fund, presided over the annual meeting of the Fund, which was held at the Royal College of Surgeons of England on April 19. Viscount Halifax was re-elected president of the Fund and Sir Holburt Waring treasurer. Mr. L. E. C. Norbury and Dr. C. H. Kellaway replace Mr. Sampson Handley and Prof. Major Greenwood on the Council as representatives respectively of the Royal College of Surgeons and the Royal Society.

In a brief account of the year's work Prof. Dean said that in cancer research progress was always slow, but during the last twenty years the rate of progress had been accelerated, and this was due partly to the fact that the observation of cancer as it occurred in human beings had been supplemented by the production of cancer experimentally in animals, but even so, the results of one experiment might need to be observed over a number of years, and every report of a research body such as their own must necessarily be of an interim character, recording investigations which might yet be long in reaching their conclusion.

Prostatic Cancer: Two Advances

What appeared to be the most important contribution of chemotherapy to cancer so far made was the administration of diethylstilbeostrol, the synthetic oestrogenic compound originally prepared at the Middlesex Hospital, in the treatment of cancer of the prostate gland. This work had the further interest that it was an example of treating gland cancer with a chemotherapeutic substance having the essential physiological properties of a natural hormone. As soon as this method of treatment was announced, Dr. E. S. Hornung, a member of the Fund's scientific staff, undertook to study the prostate gland in mice, and to attempt to induce glandular cancer by injecting a chemical carcinogen into the epithelium of the anterior lobe of the organ. In this he was successful, and six epithelial tumours were obtained out of fifty mice treated. This work had been continued by Dr. L. Dmochowski, a visiting research worker, and the chemically induced prostatic cancers had now been transplanted and were being studied.

Other Experimental Work

Two other research workers for the Fund, Dr. R. J. Ludford and Miss Hilda Barlow, had been investigating sarcomas arising in the stroma of transplanted breast cancers derived from inbred strains of mice, and had found that when spontaneous mammary cancers of these mice were propagated, each in mice of the strain in which the tumour arose, malignant transformation of tissue stroma was apt to occur. The sarcomas could be separated from the mammary cancers and propagated independently. An opportunity had thus been afforded for close study of these tumours *in vitro* as well as *in vivo*, and at all stages of their development. It confirmed that in some mammary cancers the malignant cell was able to induce malignancy in a neighbouring connective cell.

Another member of the Fund's scientific staff, Mr. H. G. Crabtree, had continued his search for a substance which would inhibit specific metabolic processes on the induction of cancer in mice. He had previously drawn attention to two substances —namely, the groups of compounds represented by chloro-acetone and by bromo-benzene respectively, which, though chemically unrelated, were intimately concerned with cancer metabolism, and to these he had now added a third—namely, maleic acid, used as the more fat-soluble maleic anhydride, which was proving even more effective than bromo-benzene as an inhibitor of carcinogenesis when applied in an appropriate