not even forming a contraindication; indeed, Genzmer goes so far as to advocate the operation even though there are already metastatic deposits in the liver. As even in Germany about 20 per cent. to 25 per cent. of the cases applying for relief are not deemed fit for the radical operation, we may say that taking all patients together as they come to the surgeon, not more than 20 per cent. will be benefited by the radical operation; the remainder will do better with colotomy. If we study the successful cases, it appears that in the great majority of instances where real benefit was derived from the operation, the disease was low down, and was not adherent to the surrounding structures. The cases of success as regards prolonged freedom from recurrence where the disease was high up or adherent to surrounding structures, such as the prostate, are quite few in number.

In this matter I quite agree with the tendency of the English school of surgery as opposed to the German, and would exclude from the radical operation cases of rapid growth, cases where the disease forms large masses in the intestine or is deeply ulcerated, cases where it has passed through the wall of the rectum and invaded surrounding parts, as indicated by fixidity, and rapidly-growing tumours high up, even though not yet fixed. In none of these is there any real prospect of cure or of marked prolongation of life. In the fixed tumours local recurrence is almost certain to take place early, and the last condition of the patient will be worse than the first; where the tumour is rapidly growing glandular infection is widespread, and the chances of getting beyond the disease are very remote, while, on account of the rapidity of growth, the glandular disease and the metastatic deposits will soon end the life of the patient. In none of these cases is it worth while for the patient to run a considerable risk, for it is just in these cases that the mortality is highest, while in colotomy we have a means of relieving the patient's sufferings, and of giving him as long a period of life with practically no risk.

THE MILROY LECTURES

ON

THE VALUE OF ISOLATION AND ITS DIFFICULTIES.

Delivered before the Royal College of Physicians of London.

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[ABSTRACT.]

LECTURE II.

THE VALUE OF ISOLATION AS ILLUSTRATED BY SMALL-POX.

AFTER a rapid review of the improvement in sanitary conditions which had resulted from the Public Health Act of 1872, and the Compulsory Notification Act (which already applied to five-sixths of the whole population), the lecturer proceeded to deal with the effect of isolation as exemplified by small-pox in particular. He referred to the investigation conducted by the Local Government Board in 1880-81, giving in full the results of Dr. Thorne Thorne's inspection of isolation hospitals, and of Mr. W. H. Power's inquiry into the connection between small-pox hospitals and the incidence of the disease in the immediate neighbourhood of these institutions (on which the official doctrine of "aerial convection" is founded) together with the gist of the findings of the Royal Commission, consisting of Sir J. Paget, Sir R. Alcock, Professor Burdon Sanderson, Sir W. Broadbent, Mr. Jonathan Hutchinson, and Dr. Alfred Carpenter, appointed to study the whole matter. On the question of aerial dissemination the Commissioners did not see their way to a positive conclusion. Dr. Seaton proceeded:

OBJECTIONS TO THE THEORY OF AERIAL CONVECTION.

While fully recognising the merits of the famous inquiry of 1881, the following points must, I think, still be left open to question:

(1) The possibility, under the circumstances of 1881, of absolutely excluding the other chances of infection in the

case of persons attacked in the month of January. (Several of these, it will be remembered, were outside the half-mile radius. It would certainly have been said before 1881 that there was not even the most remote possibility of their infection at such a distance.)

(2) Assuming the possibility of distal—that is, long distance—infection, what are the probabilities of such a method of infection, as compared with the probabilities of proximal—that is, close distance—infection? (meaning by the latter term the infection which takes place in ordinary personal communications).

(3) Is "distal infection" a dominant factor in the spread of small-pox?

With regard to the first point, I have already expressed my own views.

As to the last point, it would seem from the publications which have been chiefly in evidence of late years, that this was seriously contended for. I will not attempt to follow out this point argumentatively; it seems to me to lead to conclusions which are absolutely absurd.

As to the probability of distal infection, it rests with those who strongly uphold it to indicate its proportions. If we were to judge from the frequency with which this source of possible danger is referred to in the official medical prints, we might be led to conclusions which are very erroneous. It is quite remarkable how, in subjects relating to the public health, an account of an interesting and exceptional outbreak described or referred to over and over again in the officially recognised sources of information, gives rise to the impression that the occurrence of such outbreaks is common. I could offer many illustrations of this fact, derived from my experience as an examiner. We must, therefore, be very careful to distinguish a belief which is due to an impression from that which is the result of a careful study of the facts upon which the statement has been based.

ISOLATION AND THE DIMINISHED PREVALENCE OF SMALL-POX.

I turn now to the experience of the last decade, which seems to be the best corrective of any false impressions which might arise on this vitally important subject. The recommendation of the Commission in 1882 may be given in their words:

We think it right thus to report the chief grounds for accepting or rejecting the theory of distant atmospheric dissemination of small-pox. But we feel that long as it is not proved that "personal communication" is adequate to the explanation of the whole spread of small-pox, and so long as a distant atmospheric dissemination is not shown to be in the highest degree improbable, so long it is essential that in the construction and management of small-pox hospitals both sources of danger should be with the utmost care guarded against. And in this conclusion we are more than supported by the opinions of witnesses of the greatest authority, including Sir William Jenner, Sir William Gull, Sir James Risdon Bennett, and Sir John Simon.

Those recommendations have led to the adoption of the present system of isolating small-pox, that is to say, by the taking of the cases as they occur "out of" the population, and removing them to a distance of several miles to a spot where true isolation could be carried out. Looking back upon the state of things before 1886, at which time this system came into operation, and comparing it with what has happened during the ten years which have elapsed, we have, so far as facts and figures go, one of the most remarkable instances on record of the efficacy of any medical preventive measure. I would repeat the words "so far as facts and figures go," for we do not need to go beyond the history of small-pox itself to find illustrations of conclusions which, as subsequent events proved, were too hastily adopted. Still, as far as we are warranted in basing any opinions at all on mortality statistics, this would certainly seem to be one of the most remarkable pieces of evidence which has ever yet been adduced in support of the efficacy of any preventive measure. For even if it be argued that distal atmospheric dissemination played a dominant or even an important part in the maintenance of small-pox in London in such proportions as caused it to be spoken of as epidemic, we have now the incontrovertible fact that a hospital segregation of cases at a distance has resulted, for a time at least, in a comparative extinction of the disease.

All questions of distant atmospheric dissemination apart, does it not seem as if the whole subject resolved itself into a question of notification with *true isolation*—that is, the removal of cases, as Mead advised nearly two centuries ago, to a distance of some few miles out of the town? The

success of complete isolation of small-pox has been very striking. Previous to the year 1886, when hospital ships were established several miles down the river, the epidemics, as they were termed, occurred with regularity every three or four years or oftener. These experiences extended back for half a century or so, the deaths occurring on each occasion being from 1,000 to 8,000. In the intervening periods the mortality varied, but hardly ever amounted to less than several hundred deaths per annum. In 1885 the deaths were 1,419; in the following year, when the removal to the hospital ships had taken place, it suddenly sank to 24—the lowest then on record—then to 9, finally to zero, not a single death occurring in 1889. After that a slight rise took place, leading to an outbreak in 1893, relatively considerable, but trifling as compared with the years previous to 1886. The whole period may be shown in a tabular form:

Year.	Deaths.	Year.	Deaths.
1885	1,419	1891	8
1886	24	1892	41
1887	9	1893	206
1888	—	1894	89
1889	—	1895	55
1890	4		

[It was pointed out that the revival during the last four years seemed to be attributable to the spread of antivaccination and to the institution of philanthropic shelters, which attracted numbers of tramps to London every winter, and herded them together under conditions most favourable to the spread of disease.]

THE SMALL-POX HOSPITAL AN ESSENTIAL AUXILIARY TO VACCINATION.

The synchronism between the adoption of a new system of isolation and the drop in the small-pox death-rate may just possibly prove another example of the strange vagaries in the behaviour of this disease. If so, the coincidence will be still more extraordinary than that which, some thirty years ago, marked a sequence of events in a neighbouring country. There the disappearance of small-pox immediately followed a change in the system of vaccination and led to conclusions which longer experience showed to be erroneous. But if from the very remarkable experience afforded by the past ten years, we come to the conclusion that thorough isolation and the disappearance of epidemic diffusion stand in the relation of cause and effect, we are forced to recognise an influence of so potent a character as to rank second only to vaccination itself. A small-pox hospital, properly situated, must surely be considered a boon and an essential auxiliary to vaccination in a country where the protection of revaccination, though freely offered, is not systematically enforced.

I am of course aware that there are those whose opinions are entitled to the greatest respect who think differently. They would contend that the State has furnished in vaccination and the opportunity of revaccination the best preventive measures, and that the establishment of small-pox hospitals is an unnecessary tax upon the community generally. To discuss this question would be to enter into a somewhat controversial topic. I would therefore refrain from saying anything more at present. It is my duty to bring to your notice the facts relating to the value of isolation. This would appear to be very conspicuously shown in the case I have submitted to you, and if the lesson taught by London experience confirms that of the large towns of England, we are bound to do what we can to minimise the difficulties attending isolation.

THE LEICESTER SYSTEM.

Let it not be supposed, however, that the change in the system of isolation was the only factor to be credited with the rapid diminution in the small-pox death-rate. At the same time there was taking place an equally essential change in our methods of sanitary administration. I have already called attention in an earlier part of this lecture to the notification and isolation of cases of small-pox which at Leicester have been relied on for the prevention of epidemic diffusion from single cases to a much larger extent than elsewhere. In view of these lectures I have lately studied again official reports, including those by Dr. William Johnston, who was the active medical officer when the notification and isolation system was adopted in the seventies, as well as those of the late Dr. Tomkins, and Dr. Priestley, who have

so ably carried it out. But before I refer briefly to the Leicester experiences, let me once more call to your notice a fact which has not always been remembered even by those in our profession who might be expected to be specially informed, namely, that the term "epidemic" is entirely a relative expression; in other words, that what would be considered an epidemic now would, even at the time when I first undertook the duties of a medical officer of health, have been considered no epidemic at all—indeed, nothing more than ordinary endemic—if it be so called—prevalence of the disease. This it is only fair to the authorities of Leicester to state.

As an illustration of the impressions that arise from the frequency with which a subject is referred to in the official or semi-official prints may be mentioned the small-pox prevalence in Leicester in 1892, which is no doubt classed amongst severe epidemics. This epidemic consisted altogether of 357 cases in a population of nearly 180,000, or less than 2 persons in every 1,000 of the population. The total mortality was only 21, which, when we compare it with that of other towns, was not so very great, and would certainly not have been considered indicative of an epidemic in former times. The "Leicester system," as it has been termed, is in reality a mixture of systems, none of which are carried out in perfection. In the first place, what makes the case of Leicester exceptional is the large proportion of unvaccinated children; this, however, does not imply that the proportion of persons altogether unprotected by vaccination is so markedly in excess as is generally supposed. The population of the town was as a whole well protected by vaccination until late years.

Again, hospital isolation was not of course the main safeguard, inasmuch as the attendants at the hospital were for the most part specially protected. So that around the centres of infection segregated at the hospital were a cordon of protected persons. But over and above vaccination and revaccination Leicester has been following a practice rather than a system which is peculiar to itself. In the first place, Leicester is not one of the towns which have availed themselves of the recommendation of the Royal Commission, and secured a site at a considerable distance from the borough, where something like true isolation could be carried out, and in consequence a good deal of trouble arose there by the infection of persons suffering from scarlet fever, isolated (save the mark!) in the buildings on Freae's ground. Moreover, persons living in a comparatively isolated suburb called Newfoundpool, which is close to the hospital, suffered specially, whether by direct or aerial infection does not appear to be absolutely certain. So that in point of one of its essential isolation equipments the Leicester system was inferior to that of some other towns. On the other hand, most extraordinary precautions for the early notification and detection of possibly infected cases are carried out with great loyalty by the staff of the health department, and submitted to by the population.

[Here the lecturer described the independent observations of Dr. Coupland (on behalf of the Vaccination Commission during the small-pox prevalence at Leicester), which bore directly on the subject of isolation.]

The strong points of the various systems carried out at Leicester are: the very great care taken in the detection, diagnosis, and early reporting of cases, the immediate removal of cases to the hospital, the subsequent quarantining of the inmates of the infected houses, either (a) at special buildings, or (b) at home, and in either case the subsidising of the persons so quarantined, and above all the revaccination of the persons who, by their attendance upon the sick, would be most likely to fall ill. (In this last respect Leicester is only in line with other places.) The weak points of the Leicester system are (1) the large proportion of young children unvaccinated, the danger of which was exemplified in their outbreak of 1892; (2) the position of their hospital, which is too near to a populous suburb, and not sufficiently far from the main portion of the town to constitute what we may term true isolation; (3) but the greatest imperfection of all, according to modern views, was the fact that cases of scarlet fever were isolated almost alongside of small-pox. The dangers attending this last-named practice, namely, that of isolating small-pox in buildings contiguous to and, per-

haps, in communication with those occupied by scarlet fever patients, is one that is specially dwelt upon by Sir George Buchanan, in his introduction to the Report of 1882 on the use and influence of isolation hospitals, where he speaks of the misfortunes within the walls of the institutions themselves. Ten years before the 1892 outbreak at Leicester, there had been experiences in a neighbouring town of the dangers arising from attempting to isolate scarlet fever in a group of huts or sheds arranged on an area of seven acres of ground, when small-pox also at the time of a rising epidemic was being admitted. Small-pox cases took scarlet fever and *vice versa*. Such isolation (so-called) can only be spoken of as quite unpermissible at the present day. It cannot therefore but be recognised that in this respect the Leicester system was especially weak. Taken as a whole, therefore, the lessons taught by Leicester are not nearly so instructive as to the value of isolation as they might have been had the authorities secured a better position for their hospital, and understood the value of true isolation in every respect. In such a case, it is possible they would have still further postponed the occurrence of an outbreak assuming what is termed epidemic proportions. It will of course be understood that I speak of the Leicester experiences strictly from the point of view of isolation, and as illustrating the great influence which was exercised first of all in the provincial towns by precautions other than vaccination, and specially adapted to diseases which are infectious, like small-pox. Elsewhere in the provinces for many years great activity had been displayed in dealing with initial outbreaks of this disease, some towns having acquired not only special powers as to making notification systematic and compulsory, but others having sought for powers which the Sanitary and Police Regulation Committee of the House of Commons were eventually obliged to stop on the ground of undue interference with personal liberty.

SMALL-POX HOSPITALS AND ATMOSPHERIC DISSEMINATION.

I have already expressed the opinion that small-pox hospitals are, under certain circumstances, entitled to be considered a boon. They have, moreover, no appreciable counterbalancing disadvantages. That, at any rate, is the conclusion I arrived at after studying the effects of the extra-urban isolation of small-pox. That also would seem to be the opinion arrived at by Dr. John McVail, who gave a very interesting discourse on the subject of aerial convection of small-pox from hospitals at the Epidemiological Society two years ago.

That discourse contained the following important statement: "No good evidence has yet been adduced of such occurrences in connection with hospitals situated in rural districts." This is true, I believe, in a very wide sense, if we include under the term "rural" institutions which, though not remote from towns, partake of the rural character so far as isolation is concerned. In 1887 I had the opportunity of making a short visit to New York, and of seeing, with the medical officer, some arrangements in that city for the isolation of infectious diseases, for which the public authority was called upon to make accommodation. I was informed at that time by the health officer that no special incidence of small-pox had been noted in the part of the city which would come within the range of distal infection from the island on which the small-pox cases were segregated. Of course the incidence may not have been specially looked for, but it cannot have been marked, or attention would have been drawn to the subject, as was the case round Fulham Hospital in 1880. Within the last few weeks I have received a communication from Surgeon-General J. S. Billings, from which I quote the part relating to small-pox diffusion:

"With regard to your question as to the diffusion from small-pox hospitals, I do not know of any record of anything of the kind in this country, but the small-pox hospitals of our cities are usually located at a distance from any habitations in very thinly settled suburbs. I am familiar with the reports of the Local Government Board on this subject, and think that Mr. Power's argument is a strong one, but I am of the opinion that a collection of small-pox cases may be so managed as to prevent the diffusion of specifically infectious dust therefrom....."

At various times during the last few years I have also had the opportunity of discussing this question with gentlemen officially connected with sanitary administration in Australia and at the Cape Colony, where, so far as my information goes, atmospheric dissemination has not been regarded in any way as a drawback to the hospital isolation of small-pox. Still, as the Royal Commissioners cautiously observed, the utmost care should be taken to guard against it in the event of its becoming necessary, at times of continued fogs or other special circumstances, to retain acute cases of small-pox in towns. It was suggested by Professor Burdon Sanderson, a member of the Royal Commission, that under such circumstances hospitals might be constructed so that the air of the sick wards should be subjected to a high temperature. A small committee, of which I was one, was formed in 1889 to consider the practicability of this suggestion. I believe I am right in saying that Sir Douglas Galton was ready with a plan, but we were unable to persuade the Asylums Board that it was necessary to adopt such a measure. I confess I have been the less disappointed at our failure on learning, as I have done during the last few years, from the reports of medical officers in Surrey that they have been for a long time in the habit of drafting cases of small-pox which occurred in country districts, not, it is true, into the very heart of London, but into a district which, if we take the proportion of persons per square mile, is comparable to a very thickly inhabited town. It is gratifying to note that all the time that process has been going on, we have heard nothing of those dangers which we have been taught to associate with the hospitals for which the Poor-Law Department of the Local Government Board was primarily responsible.

SMALL-POX AN OBJECT LESSON IN THE TREATMENT OF INFECTIOUS DISEASES.

In concluding this lecture I feel that I ought to give some reason for dwelling so long upon the one disease, variola. The proportion of the small-pox death-rate at the present day, since the adoption of vaccination became general, is comparatively insignificant, and if we compare the death-rate from this cause with that of the other principal zymotics, it may be called trifling. On the other hand, this disease affords a most striking proof of the value of preventive measures, such as are administratively possible at the present day. It is from that point of view mainly, I need not remind you, that public health departments have to regard disease prevalence. They must also regard it partly from the view of the lesson which it may possibly afford in the treatment of other infectious diseases. With regard to the prospects held out of a complete riddance of that which a century ago, before the time of our countryman, Edward Jenner, was a terrible plague all over the world, may it not be possible that these lessons we are learning at the end of the century may teach us much which will prove essentially valuable in the preventive treatment of other diseases? The behaviour of small-pox under different conditions, which has been so closely studied, may throw a light on many questions of epidemiology and may eventually enable us in a most practical sense to widen the scope of preventive medicine.

THE PHYSIOLOGY OF THE CARBOHYDRATES.

A REJOINDER TO DR. PATON'S FURTHER CRITICISM.

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(Continued from page 522.)

THE question of the relative state of the blood going to and escaping from the liver is an important one. I dealt with it fully in my *Physiology of the Carbohydrates*, and founded my conclusions on a large amount of experimental evidence. Dr. Paton, in criticising this, brought forward the experimental work of Seegen. I say in my *Epicriticism*, and give the grounds for so saying, that "the experiments of Seegen referred to by Dr. Paton may be unhesitatingly affirmed to constitute experiments from which it is absolutely not permissible to draw a physiological deduction." Seegen's *modus operandi* cannot possibly furnish specimens of blood