THE HOSPITAL ISOLATION OF SCARLET FEVER: SOME POINTS OF UNCERTAINTY.*

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I THINK that no one who has been interested in recent discussions will deny that the subject of the hospital isolation of scarlet fever admits of further and fuller discussion than it has yet received.

Nor can anyone deny, in the face of the paramount position hospital isolation occupies in the municipal machinery for disease prevention, that the subject is of sufficient importance to justify such discussion.

The present short paper has been put together merely with the object of promoting discussion rather than with the idea of adducing fresh facts, and with this end in view I have thought it best to enumerate certain definite points which are still surrounded with uncertainty, and which I anticipate will be the subject of considerable difference of opinion. I have formulated these points in the form of questions. I will preface my remarks by saying that, as the hospital isolation of scarlet fever has now been in vogue for some twenty years or more, it is very desirable that all *à priori* arguments should, as far as possible, be replaced by arguments based upon the results of actual experience.

1. *What effect has hospital isolation had in reducing the prevalence of scarlet fever?*

Can we point to towns which have built isolation hospitals and isolated the disease on a large scale, and which can show an appreciable reduction in the average number of cases of scarlet fever notified annually? As the result of an investigation which I made last year with the special object of ascertaining this (the results of which I published in *PUBLIC HEALTH* for last April), I have had to come to the conclusion that we cannot do so. We may search for evidence, however, in another way. Is scarlet fever less prevalent in those towns which isolate the disease in hospital than in those where the disease has to be treated at home? Here again the result of appealing to statistics is most disappointing. If the mean attack rate from scarlet fever be worked out for the past ten years, it will be found to be practically the same in the towns with isolation as in those without. It would seem, indeed, that the possession of an isolation hospital makes no appreciable difference in

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the prevalence of the disease. This is a most serious state of affairs, and justifies the further question, *Can the hospital isolation of scarlet fever be any longer looked upon as an effective preventive measure?*

2. Another point, and one which could easily be settled, is: *Does the practice of isolating cases in hospital reduce the number of cases per house attacked?*

We should certainly anticipate that it must do so. Such isolation as is possible in the houses of the poor is of the most imperfect kind, and even where parents conscientiously try to carry it out it must frequently break down. In spite of this, I believe it will be found, if a careful comparison be made, that the difference is very slight. I am now collecting evidence on this point in Leicester, but my figures are not yet large enough to be worth publishing. Dr. A. Newsholme has published some in his Annual Report for 1900, which would seem to show that in Brighton the number of cases per house attacked is rather less in those houses in which the first case is removed to hospital. On the other hand, if we compare the number of cases per house attacked in Windsor (a town without an isolation hospital) during the ten years 1891-1900, with that in Bath (a town which has isolated 75 per cent. of its cases during the ten years 1890-1899), we find that it is practically the same, viz., 1.34 and 1.39 respectively. The explanation of this failure probably lies partly in the fact that where scarlet fever spreads in a household it is generally at the very outset. Again and again have I noticed this. The first case is promptly removed to hospital and the house disinfected; a day or two later another case is reported and also removed, and perhaps a third case follows a few days later still. I recollect a case where four members of a family thus followed one another to hospital, yet in the same family some years before, when one of the older children had the disease and was treated at home, no spread at all occurred, in spite of the fact that there were seven other children in the house at the time unprotected by a previous attack. I mention this case as a set-off against the cases so often cited as a supposed argument in favour of hospital isolation where the first case is removed to hospital and no spread occurs. I may say, in passing, that in my opinion (contrary, I think, to the popular one) it is the exception and not the rule for scarlet fever to spread in a household, whether hospital isolation be resorted to or not. It is very desirable that further statistics on this point should be collected.

3. *To what extent has the reduced mortality of scarlet fever, which has been such a marked feature of our vital statistics during the past thirty years, been the result of hospital isolation?*

If we examine the question carefully, we find :

(a) That the decline in mortality began some years before the introduction of hospital isolation.

(b) That it has affected the whole country alike, and other countries as well as this.

(c) That it has actually been greatest in certain towns, such as Swansea, without hospital isolation.

In the face of these facts, can we assert that the reduced mortality has been in any way due to hospital isolation? I think not.

4. *Is the fatality of scarlet fever, or proportion of deaths to cases, any less in those towns where the disease is treated in hospitals than in those where it is treated at home?*

After careful examination of available statistics, I am obliged to believe that, so far from this being so, it is if anything rather the reverse, extraordinary though this may seem. Before leaving this point I must emphasize the fallaciousness of comparing the fatality of cases treated in hospital with that of those treated at home. The two classes of cases are not comparable, for the essential reason that a certain amount of selection of the cases which go to hospital inevitably takes place. In proof of the non-comparability of the two groups, I may adduce the fact that in some towns, *e.g.*, Manchester, the fatality of the hospital-treated cases is actually higher than that of the cases treated at home; whilst in other towns where it is less, *e.g.*, Huddersfield in 1899, the difference in the fatality in the two groups is so great (home-treated group, 28·5; hospital-treated group, 2·1) as to be clearly inadmissible.

5. I now come to an important practical point: *Is the average period of isolation in hospital too long?*

There is considerable reason to think that it is. Professor Simpson came to this conclusion as the result of his special inquiry on behalf of the Metropolitan Asylums Board. It used to be thought that the occurrence of "return cases" was an indication that patients were being sent home too soon, and, in the hope of getting rid of these unfortunate occurrences, the average period of isolation has gradually been increased, until in many hospitals it is now, I believe, over sixty days.

In spite of this, however, return cases still occur, and it is now recognised that they are principally in connection with patients that have been detained longer than the average. Certainly the isolation which is practised at home is as a rule of far shorter duration than the time I have mentioned, and yet the results, as I have shown, compare favourably with those of cases isolated in hospital.

Believing that the usual period of isolation in hospital could with advantage be shortened, I have adopted the plan at the Leicester Hospital, during the past few months, of having patients "shown up" at the end of twenty-eight days. Many of the patients have not completely desquamated on the feet by that time, but, provided the process was not too obvious, I have thought it unnecessary to detain them on that account. The results (so far as this short experience goes) I consider to have justified my action. During the three months ending December 14th, 1901, 190 cases have been discharged (excluding cases admitted through mistaken diagnosis), and the mean duration of stay in hospital has been only 35·3 days (or counting from the day of onset, 39·1 days). The number of return cases resulting from them was six, equal to 3·1 per cent., but one of the six cases may, I think, be fairly excluded as having contracted the infection from another source. In Birmingham in 1896-98, where the average stay in hospital was fifty-seven days, the percentage of "return cases" was 3·5.

I fully recognise, however, as I shall explain later, that the number of "return cases" is no true indication of the number of patients sent out still infectious.

The undoubted advantages gained by shortening the average stay in hospital are :

(a) The amount of air space per patient can be largely increased, and the recovery of the patient, as I submit, thereby expedited. In other words, there is less temptation to overcrowd, with all the vicious train of evils which accompany it.

(b) The cost of hospital treatment per patient is materially reduced.

(c) In some cases (chiefly in adults) it saves the patient serious pecuniary loss.

6. This leads us to the question : *What indication have we that a patient is still infectious or has ceased to be infectious ?*

Whilst there is no indication which will enable us to positively determine this point, it is now becoming generally recognised that discharges from the nose and ears are the most important indications of continued infectivity. Even when all recognisable lesions are absent, however, patients may, and not infrequently do, remain infectious long after the usual period. This, I think, will be admitted by all who have had much experience of the disease. As to the importance of desquamation as an indication of continued infectivity, I have long since ceased to have faith in it. If a patient be found in school with hands peeling, it is an important indication that the child has recently suffered from scarlet fever,

and hence, indirectly, that he is probably still infectious. But in a patient sent out from hospital after all the facts of the case have been well considered, and the patient pronounced free from infection by the medical officer in charge, I hold that the mere presence of protracted desquamation certainly does not justify anyone in pronouncing him still infectious. This is a most important practical point, upon which, I think, it is most desirable that an authoritative opinion should be expressed.

7. *What proportion of patients are sent out from hospital still in an infectious condition?*

Is the number of recorded "return cases" any approximate indication of this? Personally I am convinced that it is not. A "return case," as usually defined, includes only such cases as occur in the actual house within a limited period of the return home of another case. It must very frequently, if not generally, happen that all the susceptible members in a family are weeded out at the beginning of the outbreak, so that the household of the patient is in reality a not very likely place to look for the spread of infection after his return home.

In Leicester in 1892, in consequence of small-pox having broken out in the scarlet fever hospital, it was decided to clear that institution, and with the exception of thirteen who were too ill to be moved, all the patients—144 in number—were sent straight to their homes. These patients were in all stages of the disease, and very many must undoubtedly have still been highly infectious. Yet, extraordinary as it seems, not a single so-called "return case" of scarlet fever occurred in the houses to which these 144 patients returned, although they were carefully followed up and watched for weeks afterwards.

An experience such as this proves, I maintain, that the number of "return cases" is no indication whatever of the number of cases leaving hospital still in an infective condition. It follows that the mere fact that only a few of these "return cases" may be recorded is no proof that a fever hospital may not be playing an important part in the spread of the disease. I am well aware that this is a somewhat startling and unpleasant suggestion, but it can scarcely be more so than was the suggestion (now universally accepted) that small-pox might be spread by small-pox hospitals.

Before leaving the subject, may I refer to the sequel of the Leicester experiment? The hospital was closed to scarlet fever on October 31st, 1892, and was thereafter reserved for small-pox. It was not again used for scarlet fever until 1894. During the interval all cases of that disease had to be isolated as best they could at

home. It is interesting to note the result. During November and December, 1892, following the closure, 408 cases were notified, as against 923 during the previous ten months.

The following year, 1893, proved to be an epidemic one, and 2,308 cases were notified. It was eight years, however, since the last big epidemic, so that it is reasonable to suppose that this epidemic may have occurred quite independently of the closure of the hospital. Be that as it may, the then medical officer of health, Dr. J. Priestley, referring to the experience in his Annual Report for 1893, wrote as follows :

“It is interesting to compare with 1893 the last epidemic scarlet fever year, viz., 1885, more especially as the former represents no isolation, whilst the latter, strict isolation (at least 80 per cent. of the cases being taken to the hospital). . . .

		No. of Certificates.*		No. of Deaths.
1885	...	1,816	...	113
1893	...	2,308	...	81

The type of the 1893 epidemic must have been very mild, as the death-rate (calculated on certificates) is only 3·5 per cent. as compared with 6·2 per cent. for 1885, whilst the number of notification certificates received is increased by one-fourth, so that one naturally (taking these facts into consideration) asks the question: ‘Is a sanitary authority justified in spending enormous sums of money to keep an isolation hospital for scarlet fever patients full, and in this way to endeavour to stamp out the disease?’ Our own statistics would seem to answer the question in the negative, for during 1893, without any isolation at all, we have suffered less *in reality* as regards our scarlet fever deaths, and *in comparison* as regards the number of our scarlet fever cases. . . . The Committee have decided that in future only such cases of scarlet fever shall be admitted to the wards of the hospital as cannot possibly be isolated or nursed satisfactorily at home—for instance, in the case of scarlet fever breaking out in a two-roomed house containing three or four children, or in a family who have not the means of treating the disease properly. In other words, the Committee have definitely decided not to again crowd the hospital in an endeavour to *stamp out* scarlet fever in the town by means of isolating all the cases as they occur.”

* In Leicester (prior to 1900) a local Notification Act was in force whereby primary cases only were notified. The population was 136,000 in 1885, 184,000 in 1893.

The doctrine here inculcated by Dr. Priestley seven years ago is exactly what I am trying to preach to-day. It is worthy of note that during the past year, 1901, whilst there have been 764 cases of scarlet fever notified, there have been only five deaths, a case-mortality of *less than 1 per cent.* Have the Sanitary Committee of Leicester any reason to be dissatisfied with this result? Can towns which have "pushed" isolation show anything better?

8. I now come to the question: *Does the infection of scarlet fever which is carried out from hospital differ at all from infection contracted in the ordinary way?*

The infection carried from hospital is bred, so to speak, from carefully selected cases, and cases which are all in a late stage of the disease, whereas infection spread in the usual way is from cases in all stages, and without selection. There is nothing very unreasonable, therefore, when we consider the methods adopted in the laboratory for modifying virulence, in the suggestion that this selection may be sufficient to modify the virulence of scarlet fever. Whatever the cause, however, there is undoubtedly strong evidence pointing to the fact that the infection carried out from hospital does differ from that contracted in the ordinary way. We have it on the authority of several independent investigators that the fatality of "return cases" is above the average, and the only explanation I can think of is that the virulence of the infection carried out from hospital has in some way been "exalted." Is not this also the most probable explanation of the fact that the average fatality of scarlet fever appears to be lowest at the present time in those towns which have practised hospital isolation least, and *vice versa*?

9. *Does overcrowding in hospitals tend to prolong the duration of infectiousness?*

There is, I think, a growing belief that this is so. The following statement was made to me by Dr. Alex. Johnston, Physician Superintendent of the Glasgow City Fever Hospital: "I have been much impressed, however, by the apparent relation between *overcrowding of wards and return cases* of scarlet fever. When wards get chronically overcrowded, as they are apt to do in September, October, and November, the sequel is usually a run of return cases in November, December, and January" (extract from a letter to the writer, February 4th, 1901).

10. *Does overcrowding in hospitals tend to increase the proportion of septic complications and secondary infection?*

By septic complications I refer to such complications as otorrhœa, rhinorrhœa, scarlatinal septicæmia, etc. Here again I believe

there is an increasing tendency to admit that it does. Personally I have little doubt about it, nor, I think, has a distinguished past-president of this Branch, Dr. P. Boobyer, who has spoken out strongly on this question of overcrowding.

11. In this connection the question arises: *Are such complications as otorrhœa and rhinorrhœa directly communicable from patient to patient in the same ward?*

No less an authority than Dr. W. Gayton has stated his unhesitating opinion that they are, and many others have suspected the same thing. When we consider that it is these very complications which at the present day are the most serious feature of scarlet fever, it must surely be admitted that it is of the very first importance that this point should be settled as quickly as possible. If it should be found that Dr. Gayton and those who think with him are right, and that these complications are communicable, it would practically condemn the present system of treating large numbers of patients promiscuously in large wards. I may mention here incidentally that the Corporation of Birmingham have just erected two double-ward pavilions to hold no less than eighty beds each.

12. I now come to the question: *What constitutes overcrowding in a fever ward?*

I take it that technically overcrowding exists when the number of patients treated in any given ward is such that the amount of cubic space per head is less than what experts have decided is necessary. It is obvious that the basis of the amount of cubic space required in health cannot possibly be accepted. I wish, however, to suggest that we have no scientific basis upon which the amount of cubic space required in the case of scarlet fever can be even approximately determined until some of the questions I have here raised have been settled. It is quite possible that the basis usually accepted—viz., 2,000 cubic feet for such diseases as scarlet fever—may turn out to be too little. In the meantime, I hold that it is the duty of all responsible to at least see that this basis is acted up to. It is no justification for overcrowding that no very obvious evil effects are at once observable. We no longer have the excuse that it is a choice of two evils. There is no proof now that it is a real evil if some cases have to be left at home.

13. *Is not the term "hospital isolation" a misnomer?*

Can it be regarded as "isolation" in the true sense of the word? Is it not, as a matter of fact, "aggregation"? And is not aggregation liable to be attended by certain very definite evils (in addition to those already indicated), such as outbreaks of post-scarlatinal diphtheria, measles, whooping-cough, etc.? Is it not desirable, if

we are to arrive at the real value of hospital isolation, that an attempt should be made to keep a true balance-sheet in which the losses should be reckoned up as well as the gains? The number of deaths caused by intercurrent infectious disease contracted in hospital is anything but a negligible quantity, as all who have had much experience of fever hospitals are, I am sure, well aware. It is a painful thing to have to write "whooping-cough" on the death certificate of a child admitted for scarlet fever. It is a painful thing to be told by the grief-stricken father of a child who was admitted with scarlet fever, and has died from measles, "You have killed my child: if my child had stayed at home it would have been alive now!" It is a painful thing when a patient is admitted through an erroneous diagnosis, and afterward develops scarlet fever in a severe, and perhaps fatal, form. It is a painful thing to see the idolized and only daughter of well-to-do parents, sent to hospital with a slight attack of scarlet fever, in the belief that it was the "right thing to do," die of post-scarlatinal diphtheria.

Nor are these deaths actually occurring in hospital the only ones which have to be debited against the practice of aggregation. It is by no means an unknown thing for scarlet fever patients to carry fatal infection to their homes, not only of scarlet fever, but, which is much worse, of other diseases also. I have heard of a case where a scarlet fever patient took the infection of diphtheria home, and two fatal cases resulted. But the worst case I have personally known was where a child who had recovered from scarlet fever developed measles a week after its return home, gave the disease to the two remaining children, and all three died—a whole family wiped out!

It is true, of course, that these are isolated instances drawn from an experience of several years, but one has only to study the records of the Metropolitan Asylums Board, or, indeed, of any hospital, to realize that in the aggregate they form a most serious, albeit, inevitable, drawback to the modern system of hospital isolation.

So long as it was believed that the hospital isolation of scarlet fever was productive of substantial good in the shape of a lessened mortality from the disease, it was, perhaps, justifiable to minimize this drawback and keep it in the background; now, however, that it has been shown that such a belief is scarcely any longer consistent with the facts, when the only undisputed plea which can be put forward on behalf of hospital isolation is that it is a *great convenience*, it seems to me to be high time that the whole truth should be made known. I feel sure that if the public fully appreciated, on the one hand, the serious risk which aggregation may entail, and on the other hand the trifling risk as a rule entailed by keeping cases at

home, the modern practice of rushing children off to hospital, even amongst the comparatively well-to-do with plenty of means of treating the disease at home, would not be nearly so popular as it has now become.

Perhaps the strongest reason the public have for not caring to treat their children at home is fear of the social ostracism which the existence of a case of scarlet fever in a house has now come to entail. This is the outcome of a dread of the disease altogether out of proportion to its severity. Such dread is partly due to a recollection of scarlet fever as it existed thirty years ago, but chiefly, I think, it is a manufactured product, the result of the excessive though praiseworthy efforts of municipal authorities to control it.

THE December number of the *Studio* contained an interesting article describing Mr. George Cadbury's successful solution of the difficult problem of the housing of the working-classes at the beautiful village of Bournville, near Birmingham.

THE HOUSING QUESTION IN ADELAIDE.—Dr. Thomas Borthwick, in his report for November, 1901, states: "There were of necessity a large number of defective houses in the city, seeing that many of them were built in the early days of sanitary science, and before a Building Act was in operation. The chief defects found to exist were limited cubic space, insufficiency of ventilation, sometimes combined with inadequate lighting, dampness of walls, and general dilapidation. When any of these conditions co-exist, the houses can only be regarded as death-traps. Given a bedroom with damp walls and defective ventilation, with no external windows, the only light obtainable being borrowed from the adjoining rooms; if you add the dirty conditions which must be the consequence, and overcrowding which is too often an accompaniment, the result to the inmates must be general ill-health and a liability to contract infectious diseases, and especially tubercular disease. During last year 112 such houses were demolished by order of the Local Board, while thirty-six were converted to other purposes than human habitation. During the present year 127 such houses were condemned as unfit for human habitation, of which ninety were ordered to be demolished, the remainder being renovated to the satisfaction of the Board. So far no hardship has arisen from the action taken, but it cannot be extended without something being done to provide suitable accommodation for the people who are ejected. The houses are, as a rule, occupied by persons who cannot pay more than 3s. 6d. to 6s. per week rent, and who in many instances are poor old couples, widows with one or two children, and families whose breadwinners are temporarily out of work. They are able just to eke out a living in one way or another, but could not do so if they had to pay a higher rent. How a housing scheme can be best carried out must remain for consideration, but it must not be delayed unduly. It may be accomplished either by the local authority, and I think without loss of revenue, or by philanthropic enterprise. Should the latter come to the rescue, I would suggest that any scheme proposed should be submitted to the Local Board of Health for suggestions as to the necessary requirements before it is finally adopted."