THE INFLUENCE OF HOSPITAL ISOLATION IN SCARLET FEVER.

AN APPEAL TO STATISTICS.*

By C. KILLICK MILLARD, M.D., D.Sc.,

Medical Officer of Health of Burton-on-Trent; Late Medical Superintendent,
City Fever Hospitals, Birmingham.

THE importance of the subject we are to consider this evening is so great that I would much have preferred that the responsibility of opening this discussion had fallen upon someone far more able than myself—the more so as I have found myself obliged, from the evidence before me, to take up a position not altogether in accord with the opinions at present prevailing. It must be clearly understood that I purpose limiting myself strictly to a consideration of the value of hospital isolation as applied to scarlet fever only. There has been a tendency in the past to treat the isolation of the different infectious diseases as all one question. This, I think, has been a mistake. It by no means follows that because isolation in hospital of such diseases as small-pox, cholera, or typhus fever, may have been of great value that therefore the measure must also be of value in such a disease as scarlet fever.

Before proceeding further, it will be well to briefly review what I may call the orthodox position with regard to this measure. If we refer to almost any standard text-book on public health, we find that the provision of hospital isolation is placed in the forefront of measures for the prevention of infectious disease. How sanguine were the hopes at one time entertained that by means of hospital isolation infectious diseases, including scarlet fever, could be almost, if not entirely, stamped out, is shown from the following extract from MacNeil's work on "Epidemics and Isolation Hospitals":¹ "It will thus be seen that by means of hospital provision and proper disinfection the danger of infectious disease spreading among the inmates of even small houses may be reduced to a minimum, and an infected house and family, instead of being a menace to the neighbourhood for months, may be disinfected and rendered harmless in a few days. The various infectious diseases can be recognised sufficiently early by medical men; the time the various fevers take incubating in the system after infection is fairly well known; the period over which the danger of infecting others

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extends can be reckoned with comparative precision; powerful agents and methods for destroying infection in premises and clothing have been devised. *We have only, therefore, to take advantage of the knowledge acquired to be able to check the spread of infectious diseases, if not to exterminate them altogether.*" (The italics are my own.) How sadly these hopes have been disappointed I shall show later.

As to the diseases in which isolation is most important there is no authoritative statement; but the prevailing opinion is (or was until quite recently) that after small-pox (excluding from consideration such diseases as typhus fever and cholera) scarlet fever is the disease calling most for the measure.

It is unnecessary to say more as to the predominant place in our estimation which, in recent years, hospital isolation of scarlet fever has occupied. The substantial and well-appointed isolation hospitals erected all over the country, by far the greater number of beds in which are reserved for scarlet fever alone, are sufficient evidence of this.

Although amongst the public the belief in the power of fever hospitals to do all that was originally hoped from them is probably still as strong, if not stronger, than it ever was, amongst medical officers of health there is evidence of a distinct change of opinion taking place. It is tacitly admitted that the measure has not been the success that was anticipated, and there is a tendency now to assert that not scarlet fever, but diphtheria and enteric fever, are, after small-pox, the most important diseases to be isolated.

The most recent contribution on this subject to current literature is an article by Dr. Newsholme in the first number of the *Journal of Hygiene*, published in January last.

Dr. Newsholme's reputation is such that this article may be looked upon as the latest authoritative pronouncement. The attitude he takes up may be gathered from the following extracts: "The problem deserves consideration, especially as it must be admitted, *in limine*, that the enforcement of hospital isolation has not been so successful in diminishing the prevalence of scarlet fever as might have been anticipated on *a priori* grounds. . . . The limitations as well as the extent of the utility of preventive measures against scarlet fever need to be recognised. Even when all practical preventive measures have been adopted . . . there will, I believe, remain a residuum of infection, the operation of which, with our present limited knowledge, we cannot prevent, owing to the occurrence of 'epidemic influences' beyond our control. . . . So far as we know at present, isolation and disinfection are

the only practicable means for preventing, or at least minimizing, its spread. These measures have not in the past been completely successful because diagnosis has been defective, carelessness has been prevalent, and isolation has been delayed and carried out in an insufficient number of cases, and because disinfection has often been effected in a perfunctory manner."

The last extract is important as it reiterates the theory that the larger the number of cases removed to hospital, the larger the measure of success which is likely to attend it.

The most serious attempt in recent years with which I am acquainted to ascertain, by appealing to statistics, how far hospital isolation has really influenced the course of scarlet fever, was made by Dr. J. T. Wilson, Medical Officer of Health for the County of Lanark. His results were published as a supplement to *PUBLIC HEALTH* in February, 1897.² His conclusions were so clearly worked out, yet so contrary to orthodox teaching, that I certainly expected they would have attracted more notice. They are so important that I venture to quote the following passage from the concluding portion of his article: *

"Active preventive measures against scarlet fever during the two decades, 1870-90, were confined almost entirely to the larger towns. Since 1890 they have gradually become general. The decline in mortality began in the seventies (and coincides with the commencement of preventive efforts), but we have seen that the decline in mortality was not confined to the larger towns which led the way in active prevention; towns without even the nucleus of a sanitary department, and rural districts which made no pretence to sanitary effort of any kind, all participated in the reduction of scarlet fever mortality. Again, in comparing one country with another, we find in some which have not even yet practised hospital isolation (*e.g.*, United States), the mortality has fallen just as in those which have hospital isolation. . . . Without attempting to estimate the prophylactic effects of such measures in the reduction of mortality in localities where they have been in force, we may, in considering the cause of the general reduction which has occurred throughout Great Britain, almost leave them out of account, as the country in general did not enjoy the benefit of such measures to any great extent during the period under consideration.

"As in the days of Graves, at the beginning of the century, when many physicians in Dublin attributed the reduced fatality of scarlet fever, as compared with previous years, to their method of

* This paper, a graduation thesis, was written primarily to demonstrate the causes which had led to the decline in scarlet fever mortality.

treatment, so at the present day it is not uncommon to find reasons, expressed or implied, for the reduction in scarlet fever mortality, quite at variance with those which the result of my inquiry seems to indicate. Diminished mortality has been largely attributed to hospital isolation, and the percentage (70 to 80 in some towns) of scarlet fever cases removed to hospital is frequently held up as a criterion in sanitary administration.

"It is to be feared that the prophylactic value of this excellent sanitary measure is by some authorities over-rated. If we contrast the progress of mortality in some of the principal towns in Scotland with the preventive measures in force, we find that in Glasgow hospital isolation was largely practised without notification; in Dundee notification was in force, but almost no hospital isolation; in Edinburgh both measures were operative, while in Leith neither was to any extent in operation. Yet the reduction in scarlet fever mortality was slightly greater in Leith and Dundee than in Edinburgh or Glasgow. Or, again, take Aberdeen, where the reduction in scarlet fever mortality occurred early, and where during the past two years all modern preventive measures have been in full operation—notification, hospital isolation, etc.—and yet no diminution in prevalence has occurred during these years. In Denmark the experience of Copenhagen, and in England the experience of such places as Nottingham, are instructive in the same direction.

"When we take a wide survey of the movement of scarlet fever mortality, as I have endeavoured to do, it becomes apparent that some more widely operating influence must have been at work in bringing about the reduction that has taken place."

It remains for me to refer to the very serious attack on hospital isolation in general, and on the isolation of scarlet fever in particular, which has recently been made by Dr. E. D. Marriott, of Nottingham, who has taken up the extreme position that isolation hospitals have not only failed to accomplish the objects for which they were intended, but have proved to be a positive evil, by tending to disseminate the very diseases it was hoped they would help to extirpate.

Some of Dr. Marriott's arguments may be rhetorical rather than scientific. His attack, however, cannot on that account be ignored. The substantial accuracy of the facts he adduces has not been seriously called in question; and, whether we like it or not, his views are receiving a large measure of credence amongst intelligent laymen. For this reason alone, if for no other, it is desirable that the subject should be fully inquired into.

It must never be lost sight of that the theory of hospital isola-

tion for scarlet fever was received as an accepted principle of public health practice, without being founded upon any basis of statistical proof. We have this authoritatively from the late Sir George Buchanan, Medical Officer to the Local Government Board, who, in his introduction to the exhaustive report on "The Use and Influence of Hospitals for Infectious Diseases," published in 1882,³ makes the following important statement :

"The *general belief* of districts where isolation hospitals have been erected, that epidemics have repeatedly been prevented by their means, and the multiplied instances of danger proved to have been averted from the units of the community—of disease prevented in the household, in the school, and in the factory—*must be taken for the present in the absence of adequate statistical evidence* as proving the total advantage of the hospital to the entire community. . . . *But longer experience than any now available is wanted before the value of the hospital to the community can be proved by statistics.*" (The italics are my own.)

Nearly twenty years have elapsed since this statement was made. Surely that "longer experience" to which Sir George Buchanan refers has now been obtained. It is significant, however, that the statistical proof which he anticipated has not hitherto been forthcoming.* I have, therefore, taken upon myself to try and collect some of the numerous data which are now available, with a view to ascertaining what the real position of affairs is. I have issued an inquiry form to nearly every town of any importance in the kingdom, requesting information as to scarlet fever under the following heads :

- (a) Population.
- (b) Number of cases notified.
- (c) Number of cases under five years of age.
- (d) Number of cases isolated in hospital.
- (e) Number of deaths registered in each case for as many years back as possible.

I also took the opportunity of requesting the opinion of the medical officers of health as to whether the hospital isolation of scarlet fever in their respective towns had been of value, and, if so, to what extent,

- (1) In reducing mortality ;
- (2) In reducing prevalence ; and
- (3) In reducing fatality (case mortality).

* Such statistical evidence as has been produced has been almost entirely based upon a diminished mortality, which, as I shall show later, is open to serious objection.

I received 113 returns in response to my inquiry, thirty-one being from the "great towns," eighty-one from the smaller towns, and one from the administrative county of London. I would take this opportunity of publicly thanking the medical officers of health of all these towns for the kind and ready way in which they helped me, many of them, as I am well aware, taking much pains and trouble to get out the information I desired. To Dr. E. G. Annis, of Huddersfield, I am specially indebted.

Some of the returns were not sufficiently complete to be of use for my present purpose, chiefly owing to the fact that compulsory notification had only been a short time in force. There remained, however, a large number which I will now proceed to analyze.

As regards the opinions prevailing at the present time as to the value of hospital isolation in scarlet fever, it is a significant fact that fifteen out of thirty-one medical officers of health of the "great towns" preferred to express no definite opinion. Amongst the smaller towns, also, about the same proportion were not prepared to commit themselves. The remainder in either class were nearly unanimous in answering the questions in the affirmative, several using such expressions as "Certainly," "Undoubtedly," "Very great," "Enormously," etc. A few expressed themselves as doubtful. I attach importance to these replies. The fact that so many medical officers of health were unwilling to express an opinion indicates, I think, that a large measure of doubt exists at the present time.

Personal opinion, however, is, after all, of secondary importance as compared with facts; but facts, to be of real value in a question such as this, must necessarily be in statistical form, and not as isolated instances. It is certainly somewhat remarkable, when we consider how readily in many ways the subject lends itself to statistical inquiry, that so little has hitherto been done in this direction. The subject would seem, indeed, to be unpopular. I have been warned more than once whilst making this inquiry that statistics are likely to be inconclusive, if not actually misleading. That fallacies may easily be involved in the use of figures, and erroneous conclusions drawn from them, is, of course, well known; but that is no argument against their legitimate use, and does not usually deter us, as a profession, from appealing to them. As I am basing my position chiefly, if not entirely, upon statistics, I will venture, before proceeding, to again quote from Dr. Newsholme:⁴

"A statistical investigation based on official data of varying degrees of accuracy is apt to be received with a modicum of in-

credulity, the judgment of the reader being prejudiced by erroneous preconceptions. While so many are ready to say that 'anything can be proved by statistics,' but few realize that without statistics nothing can be proved, and that, unless complete ignorance is to persist, even defective statistics must be used to the full extent of their value. . . . In saying this I have no wish—nor is there any need—to suggest any distrust of the data on which the present treatise is based."

In my own case the data from which my statistics have been compiled have been furnished in every instance by medical officers of health in this country. Covering, as they do, a comparatively recent period, during which public health records have been kept with a satisfactory degree of accuracy, they will be admitted, I think, to be reliable.

It is difficult to see if hospital isolation has had any definite effect in diminishing either the spread of scarlet fever or its mortality, why it should not be possible to demonstrate such effect statistically after the measure has been tried long enough, on a sufficient scale, and in a sufficient number of places.

It should be noted that practically all the conditions required for a scientific experiment of this nature are present. By means of compulsory notification we know to what extent the disease has existed both before and after the introduction of hospital isolation. By means of death registration we know the number of deaths caused by it. Allowance can be made for the changes in the populations. The difficulty arising from fluctuations caused by epidemics, and the differences due to local circumstances, can be overcome by taking the average of a number of towns. Lastly, a "control" experiment exists in the case of those towns which have not adopted the measure.

I shall now describe how I have dealt with the large mass of figures I have collected. In the first place, in ascertaining the prevalence of the disease, only such records have been used as are based upon *compulsory* notification. To have omitted this precaution would have involved an obvious fallacy. Secondly, the "great towns" (using the term as applying to the thirty-three "great towns" of the Registrar-General) have been kept quite distinct from smaller towns, the latter referring chiefly to the "sixty-seven other large towns."

For those of the great towns whose records extend furthest back I have constructed diagrams, showing graphically the attack-rate, isolation-rate (number of cases removed to hospital), and the mortality, all per 1,000 of the population per annum.

Mortality.

On examining these diagrams, the contrast between the high mortality-rate which prevailed in the earlier years and the present condition of things is very apparent. The disease was then a veritable scourge, and must have materially affected the general death-rate. Now, the black (mortality) columns have shrunk almost to the vanishing-point. This remarkable change has taken place in little more than one generation, the same generation, moreover, that has seen the advent of isolation hospitals as a recognised institution for the prevention and treatment of scarlet fever. It is scarcely to be wondered at, perhaps, that it should have been assumed that the former was—to some extent, at least—a direct consequence of the latter. Such a belief undoubtedly exists, and not only amongst the public, but also, it would appear from the replies I have received, amongst many members of our profession. Thus, several medical officers of health, in replying to my question as to whether they consider that hospital isolation has reduced the mortality from scarlet fever, reply emphatically in the affirmative, and refer me to the high mortality which prevailed at one time compared with that at the present day. This is chiefly in towns where a small measure of isolation was practised before the general “slump” in scarlet fever mortality had taken place, or, to be more correct, before it had come to an end. We have only to glance at the diagrams to see that the same drop, almost equally marked, has occurred in every town, and in many several years before the hospital isolation of the disease was seriously attempted, or even thought of. Blackburn, Derby, Birkenhead, and Nottingham may be cited as examples of this. Moreover, on referring to international statistics we find that the same decline in scarlet fever mortality has occurred in other countries in which hospital isolation is not in vogue. We can only, therefore, conclude that it is due to some cause or causes independent of the influence of isolation hospitals, and to this extent the hospital isolation of scarlet fever has been living upon an unearned reputation. The reduced mortality is almost entirely due, as has been shown by Dr. Wilson,⁵ to a change in the type of the disease, which nowadays is far less fatal than it used to be. To a small extent it is due to the altered age-distribution of the population. How far it is due to a diminished prevalence of the disease we have no means of definitely determining, as it occurred prior to the days of compulsory notification.

Prevalence.

We will now try and discover how far hospital isolation has affected the prevalence of scarlet fever. Here I must point out

a consideration of great importance. The attack-rates shown in the diagrams are all calculated on the basis of actual population. It is evident, however, that the prevalence of scarlet fever, which is essentially a children's disease, will depend upon the number of children in a community rather than upon the actual population. Now, we know that the age-distribution of the populations in most of our towns has, during the past twenty years, undergone an important change in the direction of a reduced proportion of children, owing to the steadily diminishing birth-rate and a lowered death-rate. In some towns the birth-rate has fallen to such an extent that, in spite of a rapidly increasing population, the number of births occurring each year is actually diminishing. It follows that attack-rates, calculated, as I have calculated them, on the basis of the general population, will tend to make the prevalence of scarlet fever in recent years appear to be less, as compared with earlier years, than is really the case.

Let us now consider the experience of individual towns.

HUDDERSFIELD. (DIAGRAM I.)

I will first take Huddersfield, as being the town in which, more than in any other, hospital isolation has been thoroughly tried. Not only has a larger proportion of cases been removed to hospital than in any other large town, but the measure has been in vogue for nearly twenty-five years, and compulsory notification has been in force for twenty-one. Here then, if anywhere, we may reasonably hope to trace the effect of hospital isolation.

We see at once by referring to Diagram I. that during the decade 1890-99 there has been far more scarlet fever than during the decade 1880-89. But Huddersfield is one of the towns in which a low birth-rate has to be taken into consideration. In 1899, indeed, she had the lowest birth-rate of any of the great towns.

The following table shows the average annual number of births and the average annual number of cases of scarlet fever during the periods under consideration :

TABLE I.—HUDDERSFIELD.

Period.	Average Number of Births Annually.	Average Number of Cases of Scarlet Fever Annually.	Percentage of Cases isolated in Hospital.
1871-79	2,655·5	—	—
1880-89	2,465·9	256·8	67·7
1890-99	2,228·5	427·0	90·4

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ATTACK. — RATE (NO OF CASES NOTIFIED) PER 1000 POPULATION.
 ISOLATION. — RATE (NO OF CASES REMOVED TO HOSPITAL) " " "
 MORTALITY. — RATE (NO OF DEATHS REGISTERED) " " "

DIAGRAM I.

HUDDERSFIELD

DIAGRAM II.

NOTTINGHAM

DIAGRAM III.

BRADFORD

DIAGRAM IV.

HALIFAX

DIAGRAM V.

DERBY

DIAGRAM VI.

OLDHAM

DIAGRAM VII.

BOLTON

DIAGRAM VIII.

PRESTON

(NO HOSPITAL ISOLATION.)

Diagram V.
DERBY

Diagram VI.
OLDHAM

Diagram VII.
BOLTON

Diagram VIII.
PRESTON
(No Hospital Isolation.)

Diagram IX.
SWANSEA
(No Hospital Isolation.)

Diagram X.
LONDON

Diagram XI.
GROUP I.

Diagram XII.
GROUP 2.

Diagram XIII.
SWANSEA
(No Hospital Isolation.)

Diagram XIV.
LONDON

Diagram XV.
GROUP I.

Diagram XVI.
GROUP 2.

We see, then, that whilst the juvenile population must be regarded as decreasing, the actual number of cases of scarlet fever has increased enormously (66 per cent.). The real increase, therefore, is greater even than appears from the diagram. The increase has, moreover, occurred in spite of the fact that the proportion of cases removed to hospital has increased from 67 per cent. to the very high figure of 90 per cent. The remaining 10 per cent. which were not removed to hospital were, doubtless, chiefly patients in better-class houses, in which efficient isolation could be secured at home. It can scarcely be believed that they have materially affected the spread of the disease, or that the nett result would have been very different if they also had been removed to hospital. In Huddersfield, then, we may reasonably consider that we see the result of isolating practically the whole of the known cases of scarlet fever in a large town. And this, too, not during a period of two or three years only, but for ten years, a very large measure of isolation having been effected during the previous ten years.

There is no reason to think that in Huddersfield the measure has not been given a fair trial in the way of being properly supplemented by other preventive measures. Indeed, judging from the sanitary reputation of Huddersfield, her satisfactory general death-rate,* and her low infant mortality,† we may conclude that preventive measures generally‡ are very thoroughly carried out. The mere fact that the town has been willing to spend so freely on hospital isolation (probably the most expensive of all preventive measures) itself points to this conclusion.

We cannot, therefore, escape from the fact that, even under very favourable circumstances, and when carried out to the fullest practicable extent, the hospital isolation of scarlet fever is quite incapable of preventing a great increase of the disease. Nor is this increase due to an accidental epidemic. It is an increase extending over a long series of years. This is a very serious state of affairs, and affords a strong argument for those who allege that hospital isolation has not only failed, but is even itself a source of mischief. But more of this hereafter.

It may reasonably be argued that the facts I have shown so far as regards Huddersfield only demonstrate a partial failure. They prove that hospital isolation cannot prevent the disease increasing, but, it may be urged, they do not prove that the increase would not have been even greater if hospital isolation had not been

* In 1899 the corrected death-rate for Huddersfield was only 18·8, against 21·8 for the thirty-three great towns.

† In 1899 Huddersfield had the lowest infant mortality of any of the thirty-three towns.

‡ Measles is a notifiable disease in Huddersfield.

practised. To support such an argument, however, it must be shown that scarlet fever has been on the increase in other towns, with little or no isolation, and that Huddersfield compares favourably with the average prevalence of the disease in such towns during the same period. Let us see if this is so. The mean attack-rate for Huddersfield for the ten years 1890-99 was 4·3 per 1,000. The mean attack-rate in the four towns which are practically without hospital isolation, viz., West Ham, Preston, Swansea, and Burnley, was 4·4, or almost the same. If we glance at Diagram XII., which shows the combined rates for the six towns in which figures are available for eighteen years, but which have, as compared with Huddersfield, practised isolation but little, we see that no increase has occurred with them during the ten years in question.

In the face of these facts it must be held, I think, that it is a mere assumption to allege that, but for hospital isolation, Huddersfield would probably have had a larger increase.

Before I leave the question of the prevalence of the disease, it will be interesting to consider whether, even if hospital isolation has failed to reduce the number of cases occurring, it has affected their age-incidence. It is a well-known fact that children under the age of five years are less susceptible to scarlet fever than children between the ages of five and ten years. It might be, therefore, that the explanation why isolation has failed to reduce the prevalence of the disease is because the very fact of its succeeding in preventing the disease amongst children under five necessarily increases the number of unprotected children at the most susceptible period of life—viz., five to ten years; the effect being that, whilst fewer children under the age of five contract the disease, more contract it at a later age-period. If this were the case, it might be claimed that hospital isolation had still done good, for the disease is far more fatal under the age of five than it is in older children.

The following table shows the percentage of cases under the age of five years, extending as far back as my figures will permit. I also give the birth-rates for comparison :

TABLE II.—HUDDERSFIELD.

Period.	Percentage of Cases under Five Years of Age.	Mean Birth-rate.
1881-85	—	30·0
1886-90	27·6	25·2
1891-95	23·1	22·5
1896-99	25·2	22·3

From this it is evident that the age-incidence of the disease, as indicated by the proportion of cases occurring under the age of five, cannot have been materially affected in Huddersfield.

Fatality.—It may lastly be urged that, even if the measure has failed to reduce the prevalence of the disease, it must at least have reduced the number of fatal cases. It is often asserted that the fatality of cases treated in hospital is much less than that of the cases treated at home, and tables are published (chiefly in annual reports) which appear on the surface to prove this. It sometimes happens, however, that the comparison tells the other way—*e.g.*, in London, Manchester, and Birmingham—and it is then explained that the two classes of cases—those treated in hospital and those treated at home—cannot be fairly compared. I venture to say that there is grave reason for doubting whether the two classes ever can be fairly compared. Owing to the present low fatality of the disease, the actual number of deaths is comparatively so small that a very few deaths transferred from one group to the other would make all the difference in the result; more especially is this so when the home group is a very small one. That accidental selection of cases must take place to some extent is almost inevitable, as, for instance, cases which are too ill to be moved to hospital, or which prove fatal before removal is effected.* Be this as it may, it is scarcely fair to say that the comparison must not be made when it tells against hospital isolation, and yet to make it when it tells in favour of it. It appears to me that the fairest way is to compare the fatality of all cases in towns which practise isolation to a large extent with the fatality of the disease in those towns which practise it little or not at all.

Before proceeding to make a comparison by this latter method, let us see what is the result in Huddersfield of making a comparison by the method I have condemned.

The figures for the ten years 1890-99 are given in the following table:

TABLE III.—HUDDERSFIELD.

1890-99.	No. of Cases.	No. of Deaths.	Mean Fatality.
Treated in hospital -	3,862	123	3·2
Treated at home -	408	69	16·9
Total for whole town	4,270	192	4·5

* As Marriott has pointed out, in estimating the home fatality no allowance is made for the unrecognised cases. This alone is sufficient to invalidate the comparison.

The difference in the result in the case of Huddersfield* is so great that no one, I think, would venture to suggest that it is due to the superiority of hospital treatment. Yet this is what is frequently done in towns where the difference is less striking. Now let us see the result by the second method. In Huddersfield, as I have shown, the mean fatality for the whole town during the ten years 1890-99 was 4·5. In a group of nine large towns, which I shall deal with hereafter, in which only 28·8 per cent. of the cases had been isolated (less than a third of the number isolated in Huddersfield), the mean fatality during the same period was 3·8; whilst in the group of four towns already referred to, in which hospital isolation was practically non-existent, the mean fatality was only 3·3. I must leave it to others to explain away these figures.

NOTTINGHAM. (DIAGRAM II.)

I have considered Huddersfield somewhat fully because it happens to be the town which has tried hospital isolation most thoroughly. Nottingham, however, is another town which occupies a foremost position in this respect, and may be placed second. Nottingham has an excellent modern hospital conveniently situated outside the town, and great pains and trouble have been taken to render it efficient. During the past thirteen years 71 per cent. of all cases have been isolated, which means that no less than 10,025 cases out of a total of 14,006 have been removed from their homes and treated there. Acting on the generally-accepted theory that hospital isolation, to be successful, must be thorough, and that the larger the proportion of cases isolated the larger the measure of success that was likely to attend it, the proportion of cases isolated in Nottingham was increased until it reached over 90 per cent. In spite of all efforts, however, the disease continued to increase, and in 1899 got practically beyond control. Nottingham's experience has been the more unfortunate in that the hospital was opened after a period of several years in which the disease had been unusually low.

For the twelve months ending March 31st, 1900, Nottingham's bill for hospital isolation, including interest on loan, sinking fund and maintenance, was £10,756, the bulk of this expenditure being for scarlet fever. Her capital outlay at same date was £49,311.

* During the two years 1898 and 1899 the hospital and home fatality in Huddersfield have been as follows:

				HOSPITAL.	HOME.	WHOLE TOWN.
1898	...	...	...	1·8	18·2	2·9
1899	...	...	...	1·5	28·5	2·1

These figures, I think, amount to a *reductio ad absurdum*.

BRADFORD. (DIAGRAM III.)

The town which comes third is Bradford. During the eighteen years the hospital has been open 68 per cent. of the cases have been isolated. The progress of the disease is shown by the diagram. For the ten years 1890-99 the mean attack-rate was 4.1, and the mean fatality 5.9.

During the five years ending March 31st, 1900, the expenditure for hospital isolation was £30,100. This is exclusive of small-pox, the great bulk of it being on scarlet fever.

HALIFAX. (DIAGRAM IV.)

Now let us look at some of the towns which have not practised hospital isolation so thoroughly. Halifax is a town which only fully adopted the measure in 1897, while compulsory notification has been in force since 1882. It is therefore a good town for comparing the prevalence of the disease both before and after the measure was begun. *For the fifteen years 1882-96, with practically no isolation, the mean attack-rate was 2.8. For the four years 1897-1900, during which 61 per cent. of the cases were isolated, it was 5.6.* This may, of course, be merely a coincidence, but it is an unfortunate one.*

DERBY. (DIAGRAM V.)

Derby is another town which is instructive. Compulsory notification came into force in 1880, and the isolation hospital was opened in 1890.† Derby, therefore, furnishes ten years' experience of notification without isolation, and ten years' experience of notification with isolation. The mean attack-rate for the ten years 1880-89 without isolation was 5.2. For the ten years 1890-99, isolating 58 per cent., it was 4.8, or a decrease of 0.4 per 1,000. This decrease, however, is only apparent. Derby is one of those towns in which, with an increasing general population, the number of births is actually decreasing. Hence, the juvenile population cannot be increasing. Therefore it is fairer to consider the actual number of cases of scarlet fever occurring rather than the rate per 1,000. The figures are shown in the following table:

* It has been suggested that this coincidence is explained by the fact that a limited milk epidemic occurred in 1897. As, however, the source of infection was promptly discovered and stopped, and only forty-eight cases were traced to it (most of which were at once isolated in hospital), it seems quite inadequate to explain the occurrence of over 1,800 cases during the following three years.

† Prior to 1890 some cases of scarlet fever were treated in the Royal Infirmary. I have been unable to ascertain their exact number, but for the purpose of the present comparison they may reasonably be left out of account.

TABLE IV.—DERBY.

Period.	Total Cases of Scarlet Fever.	Total Number of Births.
1880-89 (without isolation)	4,454	29,964
1890-99 (58 per cent. isolated)	4,737	29,016

From this we see that Derby is no exception to the rule that scarlet fever shows no tendency to decrease in our large towns in spite of hospital isolation.

OLDHAM. (DIAGRAM VI.)

Oldham is a town in which a very interesting experiment was carried out.

It is sometimes asked: "What would happen if you were to suddenly stop removing scarlet fever cases to hospital? Would you not at once have a big epidemic?"

This experiment was actually performed in Oldham in 1892, when, owing to an outbreak of small-pox, the hospital had to be reserved for that disease alone. No cases of scarlet fever were admitted that year, and only a few cases towards the end of the following year, yet the disease fell off to such an extent that during the years 1892, 1893, and 1894, there was less scarlet fever in Oldham than in any other three consecutive years during the twenty years that notification has been in force. This may be another coincidence, but it is a striking one, nevertheless. During the five years ending March, 1900, Oldham has spent £10,200 on hospital isolation, with a capital outlay standing at £23,800.

PRESTON. (DIAGRAM VIII.)

Let us now look at Preston. Preston is unique in that compulsory notification came into force so long ago as 1883, yet, with the exception of a few cases, chiefly domestic servants in better-class houses, which are removed to the Royal Infirmary, hospital isolation is unknown. The condition of this town, as regards the prevalence of scarlet fever, is seen in Diagram VIII., and the contrast between it and towns such as Huddersfield, Nottingham, and Bradford (which have "pushed" isolation most) is very striking. The mean attack-rate in Preston for the eighteen years 1883 to 1900 was only 2·8.

It must not be thought that I wish to suggest that, but for hospital isolation, other towns would all have suffered equally little from scarlet fever; yet if we compare Preston with Bolton (Diagram VII.), another Lancashire town of about the same size, we observe that whereas in Bolton, prior to hospital isolation, the prevalence of scarlet fever was not very different from what it has been in Preston, yet since adopting the measure it has been unmistakably higher.

SWANSEA. (DIAGRAM IX.)

I will now turn to Swansea. This town is also interesting, as being one of the few with no hospital isolation. It has evidently had a very remarkable history with regard to scarlet fever. The persistence with which fatal epidemics formerly recurred, and the frightful mortality caused by them, are not shown to the same extent in any of the other towns I have records for. Clearly, then, Swansea is a town in which the local conditions are exceptionally favourable to a high mortality from scarlet fever, yet she affords an instructive example of what may happen when the disease is not interfered with so far as hospital isolation is concerned. The steady and continuous fall in the height of the epidemic mortality peaks is very striking. The mortality has continued to fall until it may now be said to have almost vanished. In spite of the fact that all cases were treated at home, the fatality of the disease during the five years 1895-99 was only 2.2, and in one year—1896—no less than 549 cases were notified with only three deaths.

The decrease in scarlet fever mortality has been greater in Swansea than in any other large town.

As regards the prevalence of the disease, the mean attack-rate during the nine years that compulsory notification has been in force, 1891-99, has been 5.7. This figure is rather above the average, but when we consider the past history of Swansea as regards scarlet fever, and note, too, that the attack-rate is rapidly falling, the result is not one with which the inhabitants of Swansea have any reason to be dissatisfied.

Had time permitted, I should like to have prepared diagrams of all the "great towns." Those which I have prepared deal with towns in which notification has been longest in force, and are on that account most instructive.

I will now consider the effect of grouping towns together, and so obtaining an average result. The advantage of doing this is that other differences, such as varying local conditions, will thereby tend to be neutralized, and the difference under consideration—viz.,

as regards hospital isolation—to be emphasized. I have in each case made the groups as large as the available data permitted.

First I have taken the three towns which have tried hospital isolation most thoroughly, viz., Huddersfield, Nottingham, and Bradford, and have compared them with the six towns in which the measure has been much less thoroughly tried, viz., Oldham, Derby, Bolton, Halifax, Birkenhead, and Blackburn. In all these towns notification has been compulsory since 1882. I have constructed diagrams for these two groups (Nos. XI. and XII.), showing the mean rates for successive quinquennia. These diagrams sufficiently speak for themselves. Can it be held that Group I. shows any better result than Group II.?

The mean rates for the groups are given in the following table :

TABLE V.—18 YEARS PERIOD, 1882-99.

Group I.—Three Towns with most Isolation.

Period.	Mean Percentage Isolated.	Mean Attack-rate.	Mean Fatality.	Mean Mortality.
18 years 1882-99 ...	64 per cent.	3.99	6.10	.243
9 years 1882-90 ...	54 per cent.	3.16	8.55	.269
9 years 1891-99 ...	71 per cent.	4.73	4.68	.218

Group II.—Six Towns with less Isolation.

Period.	Mean Percentage Isolated.	Mean Attack-rate.	Mean Fatality.	Mean Mortality.
18 years 1882-99 ...	28 per cent.	3.94	6.56	.259
9 years 1882-90 ...	16 per cent.	4.18	8.97	.375
9 years 1891-99 ...	39 per cent.	3.73	4.24	.157

We see from this table that in both groups the mean rates for the whole period are about the same, in spite of the fact that the number of cases isolated in Group I. is more than double the number isolated in Group II. We see also in Group I. that, in spite of the very high percentage of cases isolated during the last nine years, the disease has distinctly increased.

10 YEARS PERIOD, 1890-99.

There are a considerable number of great towns in which notification has been in force since 1890. I have taken all these towns, including those I have already considered, and have divided them into three groups, as follows :

1. Those towns which during the ten years 1890-99 have isolated 40 per cent. and over of cases.
2. Those towns which have isolated less than 40 per cent.
3. Those towns which practically do not isolate.

In Group I. there are nine towns, viz,

Huddersfield,	Salford,
Birmingham,	Derby,
Nottingham,	Portsmouth,
Bradford,	Sheffield,
Croydon.	

The average population of each of these towns during the period in question was 218,813.*

In Group II. there are also nine towns, viz.,

Oldham,	Plymouth,
Halifax,	Cardiff,
Bolton,	Birkenhead,
Blackburn,	Sunderland,
Newcastle-upon-Tyne,	

the average population for each being 130,167.

In Group III. there are, unfortunately, only four towns, viz., Preston, West Ham,† Burnley, and Swansea. In the two last-named towns compulsory notification was not in force till 1891. Therefore, in their case, the figures for 1890 have been omitted from calculation. The average population of each of these towns was 135,200.

The following table (Table VI.) shows the rates in each group for the whole period, and for the first and second half of the period separately.

* In case it may be objected that the average population in this group is greater than in the other two, I may point out that the increase is due to the inclusion of Birmingham, with its half-million of inhabitants. The rates for Birmingham, however, are below the average for the whole group.

† West Ham has isolated 5 per cent. during the ten years. Her hospital was not opened till 1896. For the four years 1896-99 she has isolated 15 per cent.

TABLE VI.—10 YEARS PERIOD, 1890-99.

Group I.—Over 40 per Cent. Isolated.

Period.	Percentage Isolated.	Mean Attack-rate.	Mean Fatality.	Mean Mortality.
10 years 1890-99 ...	62·8	4·45	4·83	·215
5 years 1890-94 ...	59·4	4·39	5·69	·250
5 years 1895-99 ...	65·9	4·51	4·04	·182

Group II.—Under 40 per Cent. Isolated.

Period.	Percentage Isolated.	Mean Attack-rate.	Mean Fatality.	Mean Mortality.
10 years 1890-99 ...	28·8	3·89	4·32	·168
5 years 1890-94 ...	17·5	4·15	4·82	·200
5 years 1895-99 ...	40·3	3·65	3·82	·140

Group III.—Practically no Isolation.

Period.	Percentage Isolated.	Mean Attack-rate.	Mean Fatality.	Mean Mortality.
10 years 1890-99 ...	—	4·75	4·14	·196
5 years 1890-94 ...	—	6·13	4·75	·291
5 years 1895-99 ...	—	3·62	3·29	·119

The figures in this table show that those towns (Group I.) which have practised hospital isolation most thoroughly show no better, but rather worse, results than those towns (Group II.) which have practised the measure very much less. Taken in conjunction with the figures in Table V., they must, I think, be held to prove that the theory that the success or otherwise of hospital isolation depends upon the extent to which it is carried out is quite erroneous. In Group III. (the towns with practically no isolation) the attack-rate for the ten years is higher than in either of the other two groups, but it must be noticed that it has rapidly fallen in the last quinquennium, till it is actually the lowest. From the point of view of considering (as I shall do later) what influence, beneficial

or otherwise, hospital isolation has had upon the *type* of the disease, the most important rate is the fatality. It is necessary, therefore, to call special attention to the fact that the fatality of scarlet fever during the past ten years in the great towns *appears to vary directly with the extent to which hospital isolation has been practised.*

Nine of the "great towns" have been omitted from the above tables.* In most of these compulsory notification came into force subsequently to 1890. In one or two cases the returns were not received in time to be included.

SMALLER TOWNS.

Of the smaller towns from which returns were received, there are forty-seven in which compulsory notification has been in force since 1890. These I have divided into two groups: (I.) Towns with hospital isolation; (II.) towns without hospital isolation.

Group I.

There are thirty-six towns which isolate scarlet fever in hospital, viz.:

Bangor.	Eastbourne.	Macclesfield.	Stockton.
Barnsley.	East Ham.	Malvern.	Teignmouth.
Bournemouth.	Exeter.	Newport.	Tynemouth.
Chatham.	Finchley.	Reigate.	Wallasey.
Cheltenham.	Guildford.	Rochdale.	Warrington.
Chester.	Hanley.	St. Helens.	West Bromwich.
Colchester.	Handsworth.	Southport.	Wigan.
Coventry.	Jarrow.	Stafford.	Willesden.
Darlington.	Lancaster.	Stockport.	Wimbledon.

These towns have an average population, for the ten years 1890-1899, of 41,350 each.

Group II.

There are eleven towns which do not isolate scarlet fever, viz.:

Accrington.	Dartmouth.	Oldbury.	Smethwick.
Ashton - under -	Fleetwood.	Pontypridd.	Stalybridge.
Lyne.	Heywood.	Shrewsbury.	Workington.

These towns have an average population for the ten years of 27,400 each.

There are nine additional towns which would have been included in Group II. as being without hospital isolation, but either compulsory notification only came into force subsequently to 1890, or

* The mean rates for these towns will be found in the appendix to this paper.

hospital isolation has quite recently been begun. I have therefore formed these into a subsidiary group (Group II.a).

The towns in question, with the years for which their figures have been included, are as follows :

Group II.a.

Batley, 1891-99 ;	Nelson, 1893-99 ;	Rowley Regis, 1891-99 ;
Coseley, 1893-99 ;	Nuneaton, 1890-96 ;	Walsall, 1891-99 ;
Keighley, 1890-96 ;	Rhondda, 1894-99 ;	Willenhall, 1894-99

—having an average population of 41,500 each.

The mean rates for the different groups are shown below :

TABLE VII.—SMALLER TOWNS.

	Percentage Isolation.	Attack-rate.	Fatality.	Mortality.
Group I., 1890-99 ...	44	4.23	4.00	.16
Group II., 1890-99 ...	Nil	4.76	3.44	.16
Group II.a, portion of period only ...	Nil	4.99	4.40	.21
Group II. and Group II.a combined ...	Nil	4.87	3.91	.19

Before any attempt is made to determine what conclusions should be drawn from the figures I have given in Tables V., VI., and VII., it is necessary to consider whether a comparison may fairly be made between these various groups of towns. The only factor which was considered when deciding in which group a town should be placed was the extent to which isolation had been practised. It may, however, be suggested that the towns in which isolation has been most practised are just the towns which from natural causes tend to suffer most from the disease. If this hypothesis were a correct one, it would largely diminish the significance of my figures. But it cannot, I fear, be maintained. The considerations that influence municipal corporations in deciding whether or not they will build an isolation hospital for scarlet fever, and the extent to which they will practise isolation, do not have reference to the fact that the scarlet fever mortality is or is not unduly high. Rather is it a question of the degree of "enlighten-

ment" (as it is usually considered) of the individual members of a local authority, and of the energy and perseverance of their medical officer of health.

If we refer to the diagrams, we see that in Huddersfield, Bradford, and Nottingham the mortality of scarlet fever was *not* excessive, whilst in Swansea and Preston, which did not build hospitals, it certainly was.

There is another important consideration, and that is the extent to which other preventive measures besides hospital isolation have been carried out. It seems only reasonable to believe that those towns which have persistently neglected to adopt a measure which has hitherto been considered as amongst the foremost means for preventing disease will also have been more indifferent to preventive measures in general; whilst those towns which have shown their activity and enterprise by building a hospital must be regarded as more likely to be advanced in other directions also.

There is yet a further consideration closely allied to this, viz., that it is usually held that isolation hospitals have an indirect value in educating the public to pay more regard to infectious disease. This must surely be the case to some extent.

We should expect, then, from these considerations, that a comparison made between the two classes of towns would come out in favour of the isolating towns, quite apart from any direct influence of the practice of isolation.

In the case of Table VII. (Smaller Towns) there is a further consideration tending to make the comparison tell in favour of the isolating group, viz., that the latter contains a number of towns which are health resorts, or of a residential nature (*e.g.*, Bournemouth, Cheltenham, Eastbourne, Malvern, Southport), which cannot fairly be compared with industrial towns. There are no towns of this class in the non-isolating group, as can readily be understood.*

* I have received returns from a number of the smaller towns for the year 1900, the figures for which year have not been included in the tables given above. Taking all these towns without any omissions, I find that amongst twenty-seven "isolating" towns (average population 53,860) the mean fatality was 2.89; whilst among fifteen "non-isolating" towns (average population 36,610) *it was only* 1.86.

In case it should be thought that the somewhat larger average population of the isolating group is responsible for the difference in these rates, I have taken the fifteen smallest towns of this group (excluding, however, the following health-resorts: Bournemouth, Cheltenham, Eastbourne, Folkestone, and Southport), and I find that then, with an average population of 41,500, the mean fatality for the year for the isolating group was 3.26.

I have been much impressed with the degree to which the fatality of scarlet fever has fallen in the "non-isolating" towns. It would seem possible that in a few years more it may in these towns have vanished altogether.

To overcome this objection, I have made a separate table for the Lancashire towns, the majority of the towns in Lancashire being fairly comparable. I have divided them, as in Table VII., into those which isolate and those which do not. The mean rates are given below :

TABLE VIII.—LANCASHIRE TOWNS, 1890-99.

Group I.—With Hospital Isolation.

Town.	Average Population.	Attack-rate.	Mortality.	Fatality.	Percentage Isolated.
Oldham ...	140,080	3·1	·20	6·5	37
Bolton ...	122,480	5·5	·25	4·4	33
St. Helens ...	78,290	7·1	·49	6·8	22
Rochdale ...	72,810	4·3	·21	4·9	36
Wigan ...	58,670	4·5	·32	7·1	47
Warrington ...	57,200	4·2	·42	10·0	77
Lancaster ...	35,300	2·7	·14	5·0	49
Eccles (1891-99) ...	32,850	5·2	·18	3·5	28
Whole Group	597,680	4·66	·28	6·07	37

Group II.—Without Hospital Isolation.

Town.	Average Population.	Attack-rate.	Mortality.	Fatality.
Preston ...	111,930	2·3	·16	7·1
Burnley (1891-99) ...	94,890	5·3	·25	4·7
Ashton-under-Lyne ...	42,150	3·6	·20	5·6
Accrington ...	41,140	4·5	·19	4·3
Heywood ...	25,235	4·6	·25	5·5
Fleetwood ...	11,507	4·3	·21	5·0
Whole Group	326,852	3·86	·20	5·40

Liverpool, Manchester, and Salford have been omitted because their inclusion might be objected to on account of their magnitude. Blackburn has also been omitted, as hospital isolation has only

been in force there since 1895. The rates for these towns are given separately below :

TABLE VIII.a.—ADDITIONAL LANCASHIRE TOWNS.

Towns.	Percentage Isolated.	Attack-rate.	Fatality.	Mortality.
Liverpool (1891-1900)	46	4.4	6.5	.289
Manchester (1891-99)...	68	3.3	7.0	.235
Salford ...	61	4.3	8.3	.367
Blackburn ...	Hospital only started in 1895	2.1	5.0	.107

Referring to the figures in Table VIII., it will be seen that two towns, Warrington and St. Helens, have suffered exceptionally from scarlet fever. It will be well, therefore, to consider these towns separately.

Warrington has almost, if not quite, the highest fatality for scarlet fever of any town in the country. It also happens to be a town which has "pushed" hospital isolation with altogether exceptional persistence. Beginning as early as 1877, from 1880 to 1889 77 per cent. of all notified cases were isolated, a proportion which, for those comparatively early years, is the highest I have any record of (Huddersfield during the same period only isolated 68 per cent.). The mortality for this period was not above the average, and for the ten years preceding hospital isolation, 1870-79, *it was below the average*; but whereas the mortality in other towns has fallen greatly in the last decade, 1890-99, in Warrington it has only fallen slightly.*

Turning now to St. Helens, which has also suffered so severely, we find that the hospital isolation of scarlet fever only dates from 1890. For the ten years preceding hospital isolation, viz., 1880-89, the mortality from scarlet fever in St. Helens was only 0.35, the average for the whole of England and Wales being 0.37.†

I do not, of course, assert that in either Warrington or St. Helens the practice of hospital isolation and the exceptional incidence of the disease are necessarily cause and effect. I have merely related the facts; but taken in conjunction with the other

* The infantile death-rate in Warrington, in spite of the high scarlet fever mortality, averaged only 166 for the ten years 1887-96.

† The infantile death-rate for St. Helen's for the eight years 1891-98 was 176, which cannot be considered excessive for a Lancashire town.

facts I am adducing, such a possibility, unpleasant though it be to contemplate, has to be seriously considered.

In deciding the relative value to be attached to the various tables, there are certain considerations to be borne in mind.

It is desirable (1) that the period over which the figures extend should be as long as possible, and (2) that each group should be as large as possible. Otherwise, epidemic influences and varying local conditions may introduce disturbing factors which would detract from the value of the result.

In Table V. the groups are small, but they have the advantage of extending over an exceptionally long period. In Table VI., Groups I. and II. are satisfactorily large, but Group III. is small. In Table VII. (Smaller Towns) both groups are very large, but the comparison is scarcely a fair one for the reasons explained above. Table VIII. is more satisfactory in this respect. As regards the value attaching to the various rates, the *attack-rate* is open to the objection that, except when the groups are large or the period covered long, it is liable to be materially affected by accidental epidemics. For this reason mean attack-rates for single towns for limited periods only are not of much value for comparative purposes. The *fatality*, or percentage of cases proving fatal, is much the more reliable rate for present-day comparisons, as it is not materially affected by epidemics. It is useless, however, for making comparisons between past and present years, owing to the fact that the fatality of scarlet fever has everywhere declined. It is from overlooking this fact that so much benefit has been ascribed to hospital isolation in the way of reducing fatality.

The *mortality* (number of deaths in proportion to population) is really only the resultant of the two factors—*attack-rate* and *fatality*. It is, however, useful as showing the precise amount of life lost from scarlet fever, and it is obtainable prior to the years of compulsory notification.

A high attack-rate is of comparatively little moment if the fatality be low. Now, the most surprising fact, I think, brought out by my inquiry is that *the fatality is lower in those towns which have not practised hospital isolation than in those towns which have*. Tables V. and VI., indeed, would seem almost to show that in the towns under consideration the fatality varies directly with the degree in which isolation has been practised.

The question necessarily arises: Is this higher fatality in towns which have practised hospital isolation purely accidental, or *has it any causal relationship with the measure?*

In view of the gravity of the issues involved in the answer to

this question, it would be unwise to jump to a conclusion. On the other hand, however, the very importance of the question makes it imperative that it should be most carefully considered. The facts I have adduced are, I think, sufficiently significant to merit full investigation.

Conclusions.

After careful consideration of all the facts I have brought forward, I believe I am justified in making the following conclusions:

- I. That the hospital isolation of scarlet fever (in towns) appears to have failed to materially reduce the prevalence of the disease.
- II. That the hospital isolation of scarlet fever appears to have failed to materially reduce the fatality of the disease.
- III. That the fatality of the disease has fallen most in those large towns which have not practised hospital isolation.

Explanation.

Although my object in writing this paper is not to theorize, but to lay before you facts, I may perhaps be allowed to consider how it is that hospital isolation, which appeared to be so well conceived, and upon the carrying out of which so much time and money have been spent, has yielded such disappointing results.

The explanation so often given—viz., that the measure has not been carried out with sufficient thoroughness—will not hold good in face of the fact that those towns which have isolated most thoroughly—*e.g.*, Huddersfield, Nottingham, and Warrington—show no better, but even worse, results than towns that have done less. Another explanation put forward is that the spread of the disease is due to unrecognised cases which escape both notification and isolation, and are thus placed under no restriction. There can be no doubt that such cases do play a considerable part in the dissemination of the disease, and they would be a perfectly adequate explanation of a failure to entirely stamp out scarlet fever, but they will not explain why the disease should remain as prevalent as ever, in spite of 80 or 90 per cent. of all known cases being isolated in hospital.

We are forced to turn to hospital isolation to see if there is no flaw in the system itself. The theory of isolation in hospital as a substitute for isolation at home, where the latter is difficult to obtain, appears on the surface to be perfectly sound. It cannot be denied, however, that *isolation in hospital is not by any means the same thing as isolation at home*. As Dr. Marriott rightly asserts, it is *aggregation* rather than *isolation*, and this aggregation has often reached a high degree. Is it not conceivable that aggregation

may possibly be productive, in ways hitherto hardly suspected, of evils which have neutralized the good which would otherwise have resulted? Speaking from my own experience of hospital isolation, I see nothing improbable in this. Indeed, I am bound to admit that at present it is the explanation which seems to me to have most claim for consideration. We have positive evidence, in the occurrence of "return cases," that infection is *sometimes* carried from a fever hospital and spread outside. The actual number of such "return cases" does not, as we must all admit, represent the full extent of this evil. It may well be doubted if it is even an approximate indication of it, for "return cases," as is well known, are defined to include *only such cases as occur in the actual household* to which a patient discharged from hospital returns. Only a proportion of these households will contain susceptible individuals, for it must be remembered that six weeks or two months previously they were necessarily exposed to infection when the original outbreak of the disease occurred, and many, if not most, of the susceptible members would then be "weeded out."*

There is considerable evidence that infection carried out from hospital differs in some way from the infection of scarlet fever contracted in the ordinary manner. For one thing the infectivity of the patients who convey the infection would appear to be of abnormal duration, and the fatality of the "return cases" (the product of this infection) is, we know, increased;⁶ in other words, the infection is more virulent.†

Now, it is admitted that we have no certain means of determining

* The question raised here is one of great importance, viz., What is the real proportion of cases returning from hospital still in an infective condition which these return cases represent? It has commonly been supposed that if there are children in the house who have never had the disease, and if none of them take it, then it is a proof that the case returning from hospital must be quite free from infection. *This by no means follows.* A fact which has never been sufficiently emphasized is that only a certain proportion of children (even though unprotected by a previous attack) are susceptible to scarlet fever at any given time. There is abundant proof of this. The following figures, which have been supplied to me by Dr. E. Casey, Medical Officer of Health for Windsor, are of great interest in this connection. In Windsor, all cases of scarlet fever are treated at home. During the ten years 1891-1900 *no spread of the disease to other members of the household has occurred in 77 per cent. of the houses infected, and in only 10 per cent. did fresh cases occur more than three days after the first notification.* The actual proportion of cases per house infected in Windsor was only 1·34; whilst in Bath, a town which during the period 1890-99 isolated 75 per cent. of its cases in hospital, the proportion of cases per house infected was 1·89, or *practically the same.* I have for some time suspected, and these figures show, that the spread of scarlet fever in an infected house usually takes place (when it occurs at all) in the early stage of the disease, and that generally the mischief is done before removal to hospital, however prompt, can be effected. I hope, however, to be able to collect further evidence on this point.

† Dr. Annis, Medical Officer of Health, Huddersfield, informs me that, in his experience, the fatality of return cases is very much above the average.

whether a patient leaving hospital is really free from infection or not. Let us assume for the sake of argument that a far larger number of patients leave hospital still in an infectious condition than is at present suspected. Is it not possible that if this went on for a number of years the whole type of the disease prevailing in a town might become modified to some extent in the direction of a higher fatality and a more persistent infectivity? We should, as it were, get a "hospitalized strain" amongst our scarlet fever.

Such a theory would entirely account for the continued prevalence of the disease in spite of all efforts to control it. It would also account for the fact that its fatality is higher, as I have shown, in those large towns which have isolated most, and *vice versâ*.

It may be argued against this theory that, if the disease were really being disseminated by hospitals, it must have been discovered before. I will ask, in reply, whether it is not a fact that very many—perhaps a majority—of cases of scarlet fever in large towns are never satisfactorily traced to any particular source. It is generally supposed that the infection in such cases is contracted at school through the medium of unrecognised cases. Is it such a very improbable idea that many of these supposed "unrecognised" cases are in reality nothing more than *cases which have recently passed through the isolation hospital*? Nor is this suggestion entirely theory. It is already admitted that infection may occasionally be carried in this manner. It is, after all, only a matter of degree, and a few years ago we should not have admitted as much as we do now. Many medical officers of health are beginning to attach great importance to the part played by these "return cases."

It is sometimes asserted that the difficulty of "return cases" can be got over by the provision of a convalescent hospital. My own experience leads me to doubt this. The following passage from the Annual Report for 1899 of the Medical Officer of Health for Huddersfield (Dr. Annis) is somewhat significant:

"We are now able to carry out the plan of allowing patients after desquamation has ceased, and where they appear as far as possible free from infection, to be isolated in a separate ward for one week before going home, in order as far as possible to overcome the great blot upon the system of isolation in hospital—that is, the occurrence of 'return cases'—but I am bound to confess that up to the present there has been no appreciable difference in the number of such cases."*

* Dr. Annis has written me to say that with longer experience he has been more successful with this method. The passage is important, however, as showing that in the past "return cases" have played a part in Huddersfield.

That the aggregation of large numbers of cases in highly infected buildings might have a directly injurious effect upon the patients treated therein is a fact which was foreseen by the early advocates of the measure. It was assumed, however, that by allowing ample cubic space the danger could be entirely obviated. We have, perhaps, taken this assumption rather too much for granted. Warning notes have from time to time been sounded. I quote the following from a paper by Dr. J. B. Curgenvin :⁷

"If we would never think of placing cases of wounds in a ward with others suffering from erysipelas or hospital gangrene, neither should we place cases of scarlet fever, diphtheria, or even catarrhal sore throat, in a ward with others suffering from ulcerated scarlatinal throat, suppurating adenitis, or from scarlatinal rhinorrhœa or otorrhœa. It is not at all surprising that a large number of the cases in the crowded infectious hospitals suffer from this streptococcus infection, and become affected by suppurative complications to an extent that is never witnessed in private practice."

This statement was published seven years ago. It was denied, but no attempt, so far as I am aware, was made to prove or disprove the truth of it by collecting reliable statistics as to the extent to which secondary complications occur amongst home-treated cases. It is one of the points, and a very important one, which a commission of inquiry might help to clear up.

Personally, I have long had suspicions—and I believe many others have had too—that some at least of the secondary complications met with in fever hospitals are infectious.

Professor W. J. Simpson, in his special report to the Metropolitan Asylums Board on "Return Cases,"⁸ very distinctly hints at such a possibility. He says: "There are other questions connected with the classification of these cases into which it would be important to enter. One of these is whether the rhinitis and otorrhœa occurring in infectious hospitals are propagable to other patients suffering from a mild form of the disease, and whether septic or more severe types cannot in this way be produced. This, however, must lie over." Dr. W. Gayton, Medical Superintendent of the North-Western Hospital, who has had, perhaps, a longer experience of fever hospitals than anyone in this country, appears to be in no uncertainty on the matter. He says (p. 30),⁹ referring to Professor Simpson's remarks :

"That otorrhœa and rhinorrhœa are propagable, and very important for consideration, is proven by the fact that, given a case of either kind, not infrequently it is communicated, not only to patients in contiguity with the one affected, but to others in remote parts of the ward."

Seeing that it is just these complications which, as we now know, are the chief cause of protracted infectivity, it is not difficult to understand why the average period of detention in hospital has to be so greatly prolonged, or why, when a patient does at last leave hospital, infection is so often carried out. It is useless to urge that proper hospital administration, even though aided by every appliance that money can buy, should be able to obviate the danger here indicated. Whilst numbers of cases, of every shade of severity and in all stages, some complicated and some uncomplicated, are treated together in the same ward, the atmosphere of that ward, no matter how good the ventilation and how great the cubic space allowed, must contain innumerable pyogenic organisms of all varieties. Dare we assert that such an atmosphere is quite innocuous? When recovering from scarlet fever, children are, as we know, specially liable to septic complications. *They provide the soil; the hospital atmosphere may be trusted to provide the seed.*

It must never be forgotten that scarlet fever is now a totally different disease to what it was when Simon, in his classic reports to the Privy Council, advocated the practice of hospital isolation. The disease was then a scourge. It has now become so mild (less fatal, probably, in many places than measles) that there is no longer room for much benefit to result from the advantages of skilled treatment and nursing. It is quite conceivable, therefore, that the drawbacks inevitably associated with hospital treatment on the aggregation system may now more than counterbalance any benefit to be derived.

As regards the effect of aggregation on the duration of infectivity, I venture to again quote from the series of papers on the subject of "Return Cases" previously referred to. On p. 36 Dr. F. M. Turner, Medical Superintendent of the South-Eastern Hospital, says:

"The facts given in Dr. Simpson's report suggest that the herding of large numbers of patients together in hospital wards is responsible for the prolonged duration of infection, and that the substitution of an ordinary house for a hospital and separate rooms for large wards would result in a more rapid disappearance of infectivity."

Professor Simpson himself, in his report, is scarcely less outspoken. He states expressly in his summary "*that mere duration of detention in hospital is no standard as to the limit of infection, and no guarantee that the patient shall be freed of infection, and that long detention in infective wards will not reduce the percentage of return cases,*" and "*that the isolation in hospitals is, if anything, too long, and that return cases are not due to premature discharge.*"

It would seem as if our views as to the effect of hospital

isolation were undergoing a complete revolution! The highest authorities are now opposed to one another. Scarcely two think exactly alike. Uncertainty and doubt are the order of the day. It is becoming an open question whether the measure has in the aggregate done good or harm. Nearly 50 per cent. of medical officers of health prefer at present not to commit themselves to a definite opinion. No official statistics are available. Dogma and surmise take the place of scientific proof. And yet, though the expenditure incurred is already enormous, we are complacently increasing that expenditure and plunging further into debt, as if the whole system had proved a conspicuous success, and had accomplished all that it was expected to do.

Surely it is not unreasonable to urge that the time has come when a full and searching inquiry into the whole system is not only desirable, but absolutely imperative.

¹ "Epidemics and Isolation Hospitals," by Roger McNeil (1894), p. 53.

² "A Contribution to the Natural History of Scarlet Fever," by John T. Wilson, M.D., D.P.H. *PUBLIC HEALTH*, vol. ix. (Supplement), p. 21.

³ Medical Supplement, Tenth Annual Report, Local Government Board.

⁴ "Epidemic Diphtheria," A. Newsholme, M.D., 1900, p. 2.

⁵ *Loc. cit.*

⁶ Boobyer, *PUBLIC HEALTH*, vol. x., p. 69; Wright-Mason, *ibid.*, vol. x., p. 218; Millard, *British Medical Journal*, September 3rd, 1898.

⁷ *Medical Magazine*, 1893.

⁸ "Return Cases of Scarlet Fever and Diphtheria: Reports and Papers relative to the Board's Inquiries." Metropolitan Asylums Board, 1901.

⁹ *Loc. cit.*

APPENDIX.

TABLE SHOWING MEAN RATES FOR SCARLET FEVER IN THE THIRTY-THREE GREAT TOWNS FOR TEN YEARS 1890-99, TOGETHER WITH AVERAGE BIRTH- AND DEATH-RATES.

TOWN.	SCARLET FEVER RATES, 1890-99.				Population, Census 1891.	General Death-rate, Average for 10 Years, 1890-99.	Infantile Death-rate, Average for 10 Years, 1890-99.	Birth-rate, Average for 10 Years, 1888-97.
	Per Cent. Isolated in Hospital.	Attack-rate.	Fatality.	Mortality.				
Birkenhead	22	3·7	3·8	0·143	99,857	19·4	171	31·7
Birmingham	84	4·1	4·8	0·199	478,113	20·9	186	32·2
Blackburn	*	2·1	5·0	0·107	120,064	21·2	201	33·2
Bolton	33	5·6	4·4	0·247	146,487	22·0	180	32·5
Bradford	58	4·1	5·9	0·245	265,718	19·1	174	26·8
Brighton (1892-99) ...	82	3·1	2·1	0·065	115,873	17·7	156	24·9
Bristol (1891-1900) ...	32	3·6	3·1	0·115	289,280	18·4	149	28·9
Burnley (1891-99) ...	None	5·3	4·7	0·255	87,016	20·2	212	32·2
Cardiff	25	4·5	3·6	0·165	128,915	17·6	164	35·9
Croydon	40	3·1	2·1	0·069	102,695	14·7	137	25·5
Derby	57	4·7	2·7	0·129	94,146	17·3	156	29·2
Gateshead (1894-99) ...	15	4·0	4·0	0·165	85,692	19·7	173	35·5
Halifax	39	3·5	4·3	0·155	97,714	18·8	158	25·4
Huddersfield	90	4·3	4·5	0·194	95,420	17·6	156	22·9
Hull	33	4·2	4·1	0·172	200,472	19·7	176	33·1
Leeds (1895-1900) ...	36	3·7	4·9	0·183	367,505	20·3	178	32·5
Leicester	54	6·5	3·4	0·225	174,624	18·1	197	31·7
Liverpool (1891-1900) ...	46	4·4	6·5	0·289	629,548	25·7	192	33·5
Manchester (1891-99) ...	68	3·3	7·0	0·235	505,368	24·3	190	33·9
Newcastle-upon-Tyne ...	27	3·7	3·5	0·133	186,300	20·6	174	34·4
Nottingham	74	5·1	3·7	0·192	213,877	18·7	179	28·8
Norwich	†	4·1	2·7	0·114	100,970	18·9	180	31·7
Oldham	36	3·1	6·6	0·208	131,463	21·1	183	28·2
Plymouth	27	3·7	3·9	0·148	88,926	20·2	172	29·8
Portsmouth	55	3·6	2·8	0·103	159,278	17·5	158	30·1
Preston	Practically none	2·3	7·1	0·167	107,573	23·7	236	34·7
Salford	61	4·3	8·3	0·367	198,139	24·2	203	33·7
Sheffield	44	4·5	6·1	0·277	324,243	21·4	184	34·0
Sunderland	23	4·7	4·3	0·208	131,872	21·9	175	35·6
Swansea (1891-99) ...	None	5·2	5·1	0·271	90,349	18·9	165	32·6
West Ham	‡	5·3	3·3	0·180	204,903	17·3	164	34·3
Wolverhampton	79	5·7	4·3	0·251	82,662	22·1	191	33·9
London	55	4·8	4·1	0·199	4,228,317	19·8	160	30·5

* Hospital only open since 1895.

† Hospital only open since 1894.

‡ Hospital only open since 1896.

NOTE.—The writer would have preferred, had space permitted, to have given the detailed figures for each town for each year. In the absence of this, the above table may be found useful for reference. It is necessary, however, to bear in mind that mean rates for *individual* towns, if considered apart from individual circumstances and past history, may easily be misleading. Thus, in the case of Swansea, the mean rates, as shown in the above table, give no indication of the fact that the rates, which were formerly exceptionally high, are now at the present time exceptionally low. It is in this respect that the diagrams are valuable.

DISCUSSION.

Dr. JOHN T. WILSON: Dr. Millard's paper, which I am sure will occupy an important place in the publications of the Society, is to be followed by a motion for inquiry, and it seems to me the primary object of the paper is to show cause for inquiry. The evidence adduced in support of the motion relates, first, to the diversity of opinion which prevails as to the value of isolation hospitals in diminishing the prevalence or fatality of scarlet fever; then to the difficulty of obtaining any good statistical evidence that isolation hospitals have done so; and finally, it is suggested that such hospitals may be a source of danger by increasing the infectivity of the disease, and giving rise to what might be called a form of "hospitalism" tending to increase the prevalence of the disease. I should have liked to support the motion, in so far as I believe there is room for inquiry; but dislike the terms of the motion, which ought to have been more general.

The great diversity of opinion among medical officers of health elicited by Dr. Millard, if it be due to an overestimation of the value of isolation hospitals as a preventive measure, may contain an element of danger. There is, no doubt, room for some difference of opinion, as the success of any preventive measure will depend largely on the manner in which it is applied, but the tendency to overestimate the value of isolation hospitals as a preventive measure seems to rest chiefly with the general public. In this connection permit me to read the following extract from the evidence of the late Sir Richard Thorne Thorne given before the Royal Commission on Vaccination. The witness had handed in a diagram showing the death-rate from small-pox, measles, scarlet fever, and whooping-cough, and when questioned as to the cause of the fall in the mortality of scarlet fever, replied:

"There has unquestionably been a very marked diminution in the total amount of scarlet fever, although infants have not benefited by it in so far as the share per cent. of deaths is concerned. This period of diminution, from 1881 to 1887, is in some respects very difficult of explanation; my own impression is that it is a phase of scarlatina, which we can hardly rely upon as lasting. I know that some people attribute it to hospital influence; but wherever we have been able in a given large town to inquire how far any diminution has been due to isolation, we cannot make out that the diminution has in any sensible way been due to that practice. The truth is, that the decline of scarlatina in London commenced at a time when only about 7 per cent. of the total deaths were taking place in hospital; that is to say, not a very large proportion of the total persons attacked with scarlet fever were in hospital. Then, taking Leicester and Warrington, I presented a report to the Local Government Board upon the influence of isolation on infectious diseases, and I was keenly disappointed to find how little effect the isolation of scarlet fever cases in Leicester and Warrington had had on epidemic scarlet fever."*

Notwithstanding this evidence, we find in the final report of the Royal Commission on Vaccination the decline of scarlet fever mortality referred to thus: "We think that the steps which have been taken, in various ways, to isolate persons suffering from scarlet fever have largely contributed to this decline"; while the dissenters tell us, without hesitation, that regarding small-pox, scarlet and typhus fevers in London, "the recent development of proper hospital isolation has been most strikingly effectual in reducing, almost to insignificance, the

* The First Report of the Royal Commission appointed to inquire into the subject of Vaccination, with the minutes of evidence—1889, p. 40.

mortality from these diseases, in the case of which it has been most largely resorted to." These quotations seem, on the one hand, to claim very little for isolation hospitals in the control of epidemic scarlet fever by one of the very highest authorities, and, on the other hand, a tendency by non-official persons to exaggerate the value of isolation hospitals. I do think it is important that we should hold correct views and form sound judgments as to the real value of such an important preventive measure; but inquiry should not be confined to scarlet fever, as I believe there is, even in the prevention of small-pox, a tendency to exaggerate the value of isolation hospitals. If the importance and value of any preventive measure be overrated, it tends to bring discredit upon it, as, for example, in Jenner's estimate of the duration of protection against small-pox afforded by vaccination.

The difficulty of obtaining any good statistical evidence that isolation hospitals have been instrumental in diminishing the prevalence of scarlet fever has been shown, first, in the statistics collected by Dr. Hay, of Aberdeen, and myself, and now by those collected by Dr. Millard. These latter statistics relate exclusively to English towns, and therefore have a special interest for such officers of these towns as are present. I would therefore prefer not to speak of these, but to hear what others have to say. These statistics seem to afford valuable evidence as to the comparative prevalence and fatality of scarlet fever in towns with and without hospital isolation; but the question arises as to whether such statistical evidence affords a proper means of estimating the value of hospital isolation. I have no doubt evidence exists, and in Lanarkshire, where isolation hospitals have only been in full operation for five years, there is already some evidence of comparative value. With regard to my thesis paper, from which quotations have been taken, I would remind you that it was not written to prove the value of isolation hospitals, but to determine what were the causes of the great reduction in scarlet-fever mortality. At the time it was written I was not aware of Sir Richard Thorne Thorne's evidence, which I have now quoted.

With regard to the disadvantages of hospital isolation, as manifested in "return cases"—so-called—I think there is great need for inquiry of a detailed scientific character. I am aware of what Dr. Millard and some others have written on the subject; but such inquiries have not been conducted on proper lines. Protracted infectivity occurs in other diseases than scarlet fever, and I have now evidence to show that in scarlet fever it is not a hospital phenomenon, but is manifested in cases treated at home. Three such cases have come under my observation during the past year. "Return" cases of scarlet fever are vexatious and puzzling to the medical officer of health, and seem in many instances to result from the development of an infectious condition in a discharged patient after leaving hospital.

With regard to the means of inquiry, I have already suggested to the President that this should be left to each medical officer of health; and as to the result, I am certain it would lead to an improvement in our methods of dealing with scarlet fever, not to a disuse of hospital isolation.

Dr. SHIRLEY MURPHY doubted whether the statistical method adopted was applicable to the case. He did not believe there was any basis for arriving at any conclusions as to whether the hospitals did good or failed to do good. The data presented appeared to point to the conclusion that hospital isolation had not reduced the prevalence or mortality of scarlet fever; but it would at once be asked, Reduced the prevalence and mortality from what? There were no data for determining what the prevalence would have been without the hospitals.

That was an unknown quantity. While always anxious for inquiries to be prosecuted, he thought the appointment of a committee of inquiry would be useless. They could all make inquiries and observations in their own districts. The mere isolation of cases could not be expected to influence the laws of nature nor the prevalence of disease; but isolation did lessen the probability of the communication of the disease from one person to another. There were other factors besides personal communication, as, for example, milk. There might also be others not yet dreamt of. Who of those present would take a case of scarlet fever into their own house? If one of their children developed the disease, would they not be glad to remove the case to hospital, and so lessen the possibility of spread by personal communication?

Dr. BOOBYER: In the first place I must congratulate Dr. Millard upon the energy and ability he has brought to bear upon the task he has undertaken, the grave task of weighing the isolation hospital—so far, at least, as scarlet fever is concerned—in the statistical balance. One approaches this subject of necessity with much misgiving, with much searching of heart. No man acquainted with its practical aspect can fail to realize the gravity of the question here raised. Enormous sums of public money have been expended in providing these hospitals, and people have been slowly but steadily brought to appreciate the apparent and immediate advantages of the accommodation they afford. We of the Public Health Service have been accustomed to regard them and to speak of them without a moment's hesitation as one of the most conspicuous and valuable monuments of sanitary progress in our day. Each one of us is continually accumulating evidence of their value in cases to which no other conceivable remedy is applicable. The authors of the destructive criticism we have lately heard so much of have nothing better to offer us, be it observed, than a counsel of despair. Infection invades the crowded tenement, the public institution, both alike devoid of isolation accommodation. What are these to do if deprived of the relief which the fever hospital affords? Opinions lightly formed and rashly expressed at this juncture, especially on the part of those, like ourselves, who are held to be specially well qualified to form opinions on the subject, may have far-reaching and disastrous effects, without affording any compensating advantage whatever. I wish, however, to say at the outset that I have not come here to attempt to bolster up the now discredited system, too prevalent we know in the past, of crowding scarlet-fever cases and reputed scarlet-fever cases indiscriminately in hospitals altogether too small and insufficiently equipped for their accommodation. Overcrowding of this kind has had its day. It is not likely to be practised to any great extent again. It has certainly done harm, in very much the same way, though not in the same degree, as want of cleanliness and the crowding of septic cases with others of various kinds did harm in general hospitals in the remoter past. All I shall insist upon is, that it has not done anything like the amount of harm that many people would have us believe. Scarlet fever has been a good deal more prevalent of late than we like to see it; but the case mortality has remained almost uniformly low, and is still apparently going down, even in large towns. This fact discredits at once the theory of the general production of a species of scarlatinal hospitalism of mixed infection, as suggested by Dr. Millard and others, in towns and districts where the disease is largely isolated in hospital. Scarlet-fever mortality has been low now for more than fifteen years; but we know the liability of the disease to sudden changes of type from the days of Sydenham onwards. If we could be sure that its present mildness would continue, there might be some ground for the statement, so often

heard nowadays, that we are no longer justified in regarding it as a dangerous infectious disease; but the fact is, we do not want to run the risk of being awakened suddenly, as at any time we might be, to the consciousness of having a very general and fatal pestilence to deal with, and no hospital accommodation available for its victims.

I have said that isolation hospitals, even when somewhat misused, have not done the amount of harm that is often alleged; and this cannot be gainsaid, their death-rates speak for themselves. But I have seen as much of "return" cases, of primary and secondary attacks (in hospital), and of the septic secondaries of scarlet fever as most people—be it observed, however, that these all occur at home as well as in hospital—and I have put on record a good deal of what I have seen. I have also known times in the history of hospitals under my observation when, in order to render them at all safe and fit for habitation, they required to be closed and thoroughly cleansed. Still, while cognizant of many of the drawbacks which can be cited against the continuous use of hospitals for isolation purposes of this kind, I am also alive to the good work they are capable of doing when properly managed. It is a certain amount of reform, or—shall I say?—stricter and more systematic regulation, that is called for; it is, of course, absurd to talk of the abolition of the hospitals. There are administrative improvements that many of us could specify which would add greatly to the value and efficiency of most of these institutions, while removing much of the reproach which at present too often attaches to them. I would venture to suggest that the Local Government Board should issue a memorandum on the subject of the management of isolation hospitals for the guidance of local authorities and hospital committees.

I will now turn very briefly to Dr. Millard's tables. The first thing, I think, that strikes one in his tables is the relatively slight difference (either as regards attack-rates or case-mortality-rates) between the figures for the several groups of towns. What difference there is, too, is against isolation, except in the case of the last section consisting of two considerable groups of smaller towns, where it is in its favour. I may note here, as I think Dr. Newsholme has recently said, that it is impossible to demonstrate the *ceteris paribus* in comparing two or more towns in this relation. If it were not for the question of the influence of isolation we should be disposed to say that the comparatively insignificant difference apparent was due, (1) to the superior diagnosis and more accurate certification of the larger towns, where most attention had certainly been paid to the disease; and (2) to the less favourable conditions for life and health among the poor of these larger towns—overcrowding in slums, etc. But, taking all the facts into consideration, we should go further and say, that in the larger towns like Birmingham, Sheffield, Nottingham, Bradford, Salford, of the first group, the disease is more likely to remain steadily endemic than in the smaller, and for various reasons, the principal of which are, (1) the size of the town, and (2) its vulnerability from without. Most large towns have suburban and surrounding districts which are under somewhat looser control than their own areas, and these districts are a continual source of danger as regards the importation of infection into the towns. Nottingham is the market centre of a huge surrounding district, in which but little precaution is usually taken against the spread of infectious diseases, and from all parts of which on market-days people flock into the city. A glance at the ten-years tables in the Registrar-General's Annual Summary for 1882 will show how steadily endemic and how frightfully fatal was scarlet fever in many of our large cities during the seventies. The annual deaths in Liverpool ranged

from 215 to 1,911, in Manchester from 325 to 545, and in Birmingham from 122 to 993 during the decennium.

Before leaving the subject of figures I should like to observe, with regard to the reputed proportions of all cases removed to hospital, that these are often altogether illusory, as may be gathered from the extraordinarily high apparent mortality among home cases. The simple explanation of course is, that large numbers of cases escape both detection and notification.

Until we know more than we do at present of the life-history of the *Streptococcus scarlatina*, or whatever the particulate cause of the disease may be, it were perhaps idle to discuss the possible influence of insanitary and other conditions and surroundings upon its propagation. The exact pathological significance of the Hendon cow disease has probably yet to be determined, as well as the meaning of many other curious facts coincident from time to time with scarlet-fever outbreaks. I have myself been witness to the coincidence of dry seasons (as noted by Greswell and Longstaff), and also the disturbance of impure soil on a large scale, with outbreaks of unusual magnitude. But whether such agencies are or are not operative in fostering the spread of the disease, the impression grows upon one more and more with the growth of one's experience that scarlet fever is capable of spreading by other channels than that of personal infection. Still, impressions with only more or less disjointed facts to justify them are not of much scientific value; I shall content myself, therefore, with saying in conclusion, that until we know definitely whether the essential micro-organism of scarlet fever is obligate or facultative (either as saprophyte or parasite), we cannot satisfactorily decide whether in this disease we have to deal with a simple matter of personal infection, or with a much larger matter, in which the phenomena known collectively as scarlet fever are simply an incident.

Dr. NEWSHOLME thought that the most valuable part of the paper was that dealing with the statistics for single years of single towns. He hoped they would appear in full in the Journal, because they would be valuable for comparative reference. When Dr. Millard began to make averages he erred. Averages for groups of years or for groups of towns were useless, for not only might the circumstances of the several towns differ, but the maxima of prevalence falling in different years deprived them of all significance. For instance, Huddersfield and Nottingham were included in a group of six or seven towns. The epidemic peak at Huddersfield came in 1887, but there was no such peak in Nottingham in that year. Such grouping of towns introduced a great probability of error, particularly with reference to the periodicity of epidemics. The attempt to prove that hospital isolation did no good assumed that isolation was the only factor influencing the spread of scarlet fever, whereas it was only one of many factors. Any limitation of the spread of infection by personal contact was bound to reduce the prevalence of the disease to some extent. That was a general principle. He would require much more forcible arguments than those submitted that evening to convince him to the contrary. He maintained there was no evidence to prove that a different strain of scarlet fever was produced by the aggregation of cases in hospital. Secondary cases in households where the patients were treated at home were accompanied with higher fatality than primary cases. The facts relating to such secondary cases had not been examined as had been done in connection with hospital cases. He endorsed Mr. Murphy's views as to the uselessness of any investigation by a committee. What was really wanted was investigation by each medical officer of health in his own district, clinical and pathological as well as statistical.

Dr. BROWNING said the point which struck him most was that hospitals had practically no effect on the mortality. Isolation was often improperly carried out in a private house, where it was perfunctorily done; whereas in a hospital it was too often combined with overcrowding. He had just experienced a small epidemic, in which nine out of every ten cases were sent to hospital. There had not been a single "return" case.

Dr. T. ROBINSON (Leicestershire) observed that Dr. Millard had said that by grouping towns he overcame the difficulty of dealing with the different sanitary conditions of towns. Dr. Millard had selected certain towns in Lancashire which he said could fairly be compared. The speaker did not think so. Oldham and Bolton with the same population isolated the same proportion, they would therefore expect the attack-rate pretty much the same; but, as a matter of fact, in one case it was 3.1 and in the other 5.5. Therefore there must be something in Bolton or Oldham which had to do with the endemicity of scarlet fever, irrespective of isolation. Wigan and Warrington had the same attack-rate, and they would naturally expect therefore that isolation was carried out on the same lines; but in one case 47 per cent. were isolated, to 77 per cent. in the other, a very considerable difference. They could not therefore fairly compare the results of one town with another on the lines suggested, and there must be some influence beyond that of the hospitals to account for the disparity in the figures.

Dr. W. A. BOND said, if the investigations proved anything, it was that there had been a very great drop in the mortality from scarlet fever in *all* the towns, which indicated a very marked change in the type of the disease. The Metropolitan figures, for instance, were: 1861-70, a death-rate of 1.13; 1871-80, of 0.60; 1881-90, of 0.33; 1890-99, of 0.20. It might be argued from Dr. Millard's figures that, with an increase of case-isolation, there was a diminution in the mortality. In Group I., the death-rate fell from 0.25 per 1,000 in the five years 1890-94, to 0.18 in the five years 1895-99, coincident with increased percentage of the cases isolated. In Group II., the mortality fell from 0.20 to 0.14, and Group III. had a still more marked reduction. There was a similar fall in the death-rates in Dr. Millard's eighteen years' period. In Group I., for the nine years 1882-90 the death-rate of 0.27 fell in the next nine years, 1891-99, to 0.22, notwithstanding an increased percentage of cases isolated in hospital. And for the corresponding years of Group II., with a larger increase in the percentage of cases isolated in hospital, there was a still greater reduction from 0.37 to 0.16. This reduction had gone on in spite of a probable increase in the infectivity of the disease. The infectivity of a disease did not depend on its severity. Influenza, for instance, was exceedingly infectious, although the death-rate was very small. It was also the same with measles. That was a point which ought to be considered in connection with the advantages of isolation hospitals. He was also of opinion that the increased appreciation of isolation hospitals by the general public led to a larger percentage of cases being notified the more adequate the hospital accommodation, and the greater facility for removal led to earlier notification, and therefore, also, to the notification being greater in those towns than the real number, owing to an increase in the cases erroneously diagnosed as scarlet fever. As Mr. Shirley Murphy had said, there were probably many other sources of infection which were not recognised. Measures ought, he thought, to be taken to obviate the attendance of patients ill with scarlet fever in the out-patient rooms of general hospitals. Some cases were of so mild a type that no medical advice was sought, or, if sought, not until other persons had been infected. The increasing difficulty of diagnosis in many cases

was also a feature in the increase in the number of cases. He had often noticed in his investigations of limited outbreaks that as many as five or six cases had arisen from an unrecognised or undiagnosed case.

Dr. GWYNN dealt with the risks of patients sent to hospital with scarlet fever becoming infected with other diseases. He cited a case in which a patient, on his return home, spread diphtheria among the other children of the family, with fatal results. If the first case had been treated at home, secondary cases of scarlet fever would probably have occurred; but as the first case went to hospital, fatal diphtheria was brought home. He was not, however, prepared to throw over the hospitals. The relief experienced in being able to send the cases into well-equipped hospitals was so great that he would not be without them for anything.

Dr. BEALE COLLINS said the cost of isolation hospitals had to be considered. In a district with which he was connected, the cost per case had been £250. If isolation hospitals were to be established in small districts, the cost of maintenance would require to be less than that. In the town in which he lived, there was no isolation hospital. The attack-rate was about 3 per thousand, and the case-mortality was about 2 per cent. A considerable number of cases were probably not recognised at all in bygone days. It was very generally said that scarlet fever of the present day was a very mild disease compared with the disease as it prevailed years ago. Then an attack was a very serious matter, but now patients went into hospital, and were apparently well all the time.

Dr. NASH said the charts Dr. Millard had produced showed a general prevalence of scarlet fever throughout the kingdom, the increase being apparently irrespective of isolation. At Huddersfield, where the isolation was thorough, there was an apparently lower attack-rate than in other towns, and therefore he thought it might be argued that isolation did influence the prevalence of the disease.

Dr. C. SANDERS deprecated any impression being conveyed by the Society to the lay world that isolation hospitals were not accomplishing the objects for which they were introduced. He was disposed to think that Dr. Millard, and those who agreed with him, had expected too much. If they expected isolation hospitals would eradicate the disease from the country, then such hospitals were foredoomed to failure.

Dr. McVAIL said, whatever view those present took of the subject under discussion, they were at one in thanking Dr. Millard for the immense trouble he had taken in preparing his paper. He was certain the Society did not desire in the slightest degree to burke discussion of the question, of which they recognised the importance. He thought, however, that the opinion of the Society was that there was not sufficient evidence available on which to base any conclusion. The fatality of scarlet fever had of late years been so reduced that the general public had come, on that account, to look upon the disease with some contempt. Isolation had been adopted at a time when fatality and prevalence were diminishing, and that, perhaps, was one of the reasons why there was so little evidence of any material result of isolation.

Dr. KILLICK MILLARD thanked the members for the very kind way they had received his paper, especially as he had brought forward views not altogether orthodox. With Dr. Wilson's remarks he was so much in accord that they called for but little comment. As regards "return cases," Dr. Wilson, and Dr. Boobhyer also, said that protracted infectivity was not purely a hospital phe-

nomenon. That might be so, but there were no statistics to show that it was equally common amongst home-treated cases. On the other hand, in Professor Simpson's report to the Metropolitan Asylums Board on return cases, figures were given showing that it was much less common. Dr. Casey had informed him that in Windsor, where all cases of scarlet fever were treated at home, in the ten years 1890-99 he could only recollect one case occurring after final disinfection, which was usually performed at the end of seven weeks. Dr. Shirley Murphy objected to the statistical method. He could only refer him to such an eminent authority as Dr. Newsholme, who said that, "without statistics, nothing can be proved, and that, unless complete ignorance was to persist, even defective statistics must be used to the full extent of their value." As to whether there was sufficient ground for forming a definite conclusion, that could only be decided after a full inquiry had been made and all available evidence collected. It would be time enough then to assert that there was insufficient evidence. It was generally considered that, in a big question of national importance such as the present one, the collecting of evidence could best be accomplished by an authoritative Commission specially appointed for the purpose. Dr. Murphy suggested that individual medical officers should make inquiries in their own districts. They were supposed to have been doing that ever since hospital isolation was first begun, but so far with almost negative results. There were practically no statistics available, and no one could say precisely what the influence of the measure was. Dr. Murphy also objected that it could not be said that hospital isolation had not reduced the prevalence or mortality of scarlet fever, as they had no means of knowing what these would have been without the hospitals. His answer was, that it was just here that the "control" experiment, furnished by the towns which had not adopted isolation, helped them. It was impossible to ignore the fact that those towns without hospitals were, on the whole, as well off as, if not better than, the towns with hospitals. Swansea, with no hospital isolation, was in a better condition at the present day, so far as scarlet fever was concerned, than any other large town. Warrington, on the other hand, which had pushed isolation so much, came out one of the worst. Yet before the days of isolation Warrington had suffered far less than Swansea. Next as to Dr. Boobbyer. With much of what this speaker had said he found himself entirely in accord. Dr. Boobbyer admitted the evils which had resulted from overcrowding in fever hospitals. But this "overcrowding" simply meant that when aggregation reached an extreme degree, the consequences of it became so marked that one could not shut one's eyes to them. We had no scientific proof that the evil effects produced when septic cases were aggregated together with only, say, 1,000 cubic feet of space would entirely vanish if we provided, say, 3,000 cubic feet. We might not be able to notice them clinically, yet statistics might reveal their presence. Besides, no one would assert that the danger arising from cross-infection, outbreaks of measles, whooping-cough, post-scarlatinal diphtheria, etc., could be got over (no matter how great the cubic space) so long as aggregation was continued. Dr. Boobbyer said that they could not be certain that the present mildness of scarlet fever would continue, and that they should keep on with hospital isolation on the chance of its old virulence returning. There was no sign whatever of that at present; indeed, scarlet fever was getting milder every year. But, in any case, it seemed very foolish to be building new hospitals all over the country (as was being done at the present time) on the chance of their being needed in the future. Even if that contingency should occur, and it was purely hypothetical, it was more than probable that these new hospitals, built on the old lines, would be quite out of

date by the time they were wanted. He believed that one of the latest isolation wards in Paris had been built on the principle of a separate room for each patient, the partition walls being of glass. Perhaps it would have to come to that. They would then get true isolation. Dr. Boobhyer argued that the theory he (Dr. Millard) had suggested, viz., that hospital aggregation might possibly have affected the type of the disease, could not possibly be true, because in certain towns where hospital isolation was largely practised, as in Nottingham, the fatality of the disease was very low. It was very low, he knew, and it was getting lower every year; but the facts he had ascertained showed that it had fallen still lower in towns without hospitals. Moreover, there was no reason to suppose that the same effect would follow in every town, or in direct proportion to the number of cases isolated. It would all depend upon the way the hospital was worked, and the number of bad cases crowded together in proportion to the accommodation available. In Nottingham the hospital was only started (as could be seen by referring to the diagram) after the disease had entirely lost its fatality, and it was built on modern principles as to ventilation, etc., which undoubtedly would tend to minimize the evil. There had been very few bad cases, therefore, to affect the others. In Warrington, on the other hand, hospital isolation was begun more than twenty years ago, when the fatality of the disease was still very high and hospital construction little understood. All the evils of aggregation would thus have been intensified. The present type of the disease in Warrington he had already shown. It was higher than in any other town he had records for. One point raised by Dr. Boobhyer, and also, he thought, by another speaker, was that possibly the greater prevalence of the disease in the large towns was due to superior diagnosis and more accurate notification. He could not quite see that this would operate more in towns with hospitals, but in any case it cut both ways. If, where the prevalence appeared highest, it was due to more complete diagnosis, then the fatality, or proportion of fatal cases, should come out lower, but he had shown that the fatality was actually higher. Dr. Boobhyer also referred to the fact that some large towns, such as Nottingham, suffered from importations of the disease from surrounding districts where restrictions were enforced to a less extent than in Nottingham itself. Before much weight could be attached to this argument, it would be necessary to make quite sure that the prevalence of the disease was greater in such surrounding districts. Dr. Boobhyer had not informed them as to this. Dr. Newsholme objected to his grouping towns together, but surely in no other way could comparison be made, and the essence of statistics was comparison. To select a single town without isolation, and compare it with another single town with isolation, would obviously be open to objection. It was only by grouping that they could obtain an approximately average result with which to compare. To restrict comparison merely to each town with itself was of comparatively little use in view of the changing character of the disease and the effect of epidemic waves, but he would point out that, so far as this method went (the one Dr. Newsholme praised), it told against the utility of hospital isolation very strikingly. Dr. Newsholme and Dr. Boobhyer suggested that possibly other factors, at present unrecognised and apart from personal infection, were concerned in the spread of scarlet fever. He had himself come to hold that opinion, but in the absence of any proof he had not thought it was of much use advancing it. Whilst, however, it would certainly help to explain why hospitals had failed, it was no argument whatever in their favour. It must not be lost sight of that the question to be decided was not so much, "Are hospitals doing harm?" but, rather, "Does the isolation of scarlet fever justify

the very great expenditure it entails?" No evidence whatever had been adduced by any of the speakers to show that it did. It was argued that there was not sufficient evidence to condemn it. Had any systematic attempt been made (since the last Local Government Board inquiry twenty years ago) to obtain evidence? If not, surely it was now time that such an attempt were made. That was the reason he urged that a full inquiry should be instituted, and he therefore moved the following resolution:

"That, as the hospital isolation of scarlet fever has now been tried on a large scale for a number of years, the time has arrived when a full inquiry into the subject is desirable."*

ON THE CULTIVATION OF TYPHOID BACILLI FROM ROSE-SPOTS.—Richardson (*The Philadelphia Medical Journal*, March 3rd, 1900, p. 514) points out the scientific and diagnostic value of the rose-spots in typhoid fever as determined by recent research. That the rose-spots are due to a metastasis of the typhoid bacilli from the intestine to the skin has been frequently demonstrated since the discovery of the typhoid bacillus by Eberth. In investigating this point, however, many negative results had been obtained.

Neufeld has recently reviewed the subject, and gives some remarkable results obtained by himself. He found that the efforts to cultivate the typhoid bacillus from the rose-spots had hitherto been so often negative that they had little value either from a scientific or from a diagnostic standpoint. Neufeld's results, however, have been extraordinary, in that he cultivated the typhoid bacillus in thirteen out of fourteen cases. He attributes his good results to the fact that he as quickly as possible transferred the drop of blood from the rose-spot to a tube of nutrient bouillon. In this way he was able to dilute the bactericidal substances of the blood so thoroughly and quickly that they no longer destroyed the typhoid bacilli, as they do when cultures are taken in the ordinary way. To the lack of observance of this method of technique Neufeld attributed the frequent negative results of previous workers.

Neufeld compared the date of the earliest appearance of the Widal reaction with the earliest day of the disease on which he had obtained the typhoid bacillus from the rose-spots. This was done in eight cases, and he found that in seven out of the eight cases the diagnosis made from the rose-spots was, on an average, six days ahead of that made by the Widal test, whereas in only one case was the Widal reaction obtained first.

Curschmann, stimulated by Neufeld's work, undertook a similar investigation, also with remarkable results. He found the typhoid bacillus in fourteen out of twenty cases examined bacteriologically.

Richardson made a bacteriological examination of the rose-spots in six cases, using the same technique as Neufeld, with the exception that the rose-spot was frozen with a spray of methyl chloride before it was incised. He obtained the typhoid bacillus in five of the six cases. In all the cases the positive results of the cultures from the spots antedated the Widal test by an average of six days. His work confirms a further point ascertained by Neufeld, namely, that not every spot can be depended on to yield bacilli. Five or six spots should be incised in every case.—*American Journal of Medical Science*.

* The voting on this proposition was recorded in our last issue. *Vide* p. 440, *supra*.