SCARLET FEVER : HOW FAR DO STATISTICS PROVE OR DISPROVE THE UTILITY OF HOSPITAL ISOLATION.*

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It would appear to be somewhat late in the day to question the utility of hospital isolation in a country where that measure has been tried now for a good many years, and where the Government has, during the last ten years, approved of loans amounting to some £2,000,000 for the building and equipment of isolation hospitals. But such a query has been so very energetically put forward in certain quarters recently, and has been accompanied by statistics of so plausible an appearance, that it becomes necessary, not to immediately uproot the public faith in our hospital system by a general inquiry into its alleged dangers, but, first of all, to inquire *how far the statistics available are to be taken as a direct guide in this matter.*

In considering this last question, I think it is quite fair and reasonable to begin by examining the statistics which have already been put forward in various discussions, and I propose that we first have a look at some interesting figures produced by Dr. C. Killick Millard before this Society, and repeated at the Eastbourne Public Health Congress last year. And here I would like to acknowledge my indebtedness to Dr. Millard for very kindly assisting me by letting me see numerous returns on which his figures were based.

In order to keep this criticism within practical limits, I propose to pass over for the present the interesting tables and charts produced by Dr. Millard relating to individual selected towns. They present features which it should be the duty of those immediately concerned to investigate, but as they teach no general lesson, I feel sure that we may leave it to the individual medical officer of health to arrive at the true significance of the figures of his own town. It is only when cross-comparisons are sought to be made and general doctrines enunciated that we, as a body, become involved. Therefore I only propose to deal with the more comprehensive tables of statistics which have been put forward, because they are intended to teach general instead of individual lessons, and they appear to strike at the very root of our hospital system. Moreover, what I

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shall have to say with regard to these will, I think, apply in some measure to all. I will therefore ask you to look at Tables V., VI., and VIII. of Dr. Millard's paper before this Society last year. These are the only tables which he repeated at the Eastbourne Congress, and I think that they are all that are necessary.

Table V. deals with the period 1882-99, and gives the experience of nine towns during those eighteen years. Three of these towns are grouped together as good isolaters, and the other six make a second group of towns with less isolation. The figures for the eighteen years are as follows :

M.—TABLE V.
Eighteen Years—1882-99.

	Percentage Isolated.	Attack- rate per 1,000.	Case- mortality per Cent	Death- rate per 1,000.
Group I.: Huddersfield, Nottingham, Brad- ford 	64	3.99	6.10	0.243
Group II.: Oldham, Halifax, Derby, Birken- head, Bolton, Blackburn ...	28	3.94	6.56	0.259

This looks discouraging for the good isolater, but before we accept any generalizations based on the above, there are several questions to be asked.

In the first place, three towns are compared with six, and although the aggregate population of the two groups is approximately the same, that does not make them comparable. The element of coincidence has a greater chance in three units than in six. Epidemic influence (rise or fall) would have a greater effect on the aggregate figures of three towns for a given cycle of time than it would have on the aggregate of six towns, for there would clearly be more conflict of waves among six towns than among three. Taking three large towns and six smaller ones, and assuming a uniform proportion of infective material to be distributed through all, it is obvious that in the one group there are only three sets of machinery and three hospitals to subdue the disease, while in Group II. there are six sets, and, moreover, the pabulum is distributed.

In the next place, the character of these two groups of towns is widely different. I have not been able to go deeply into their numerous features of sanitary efficiency, but I have worked out one important point—namely, the proportion of population living in

small houses. In Group I., according to the 1891 Census, 38 per cent. of the houses consist of one, two, or three-roomed tenements, while in Group II. the proportion is 17. There cannot fail to be other weighty differences of this nature rendering these two groups incomparable.

Without laying too much stress on these preliminary objections, let us look closer at M.—Table Va. It happens that when the eighteen years is divided into two periods of nine years the figures for the first period appear to be uniformly in favour of the isolating towns.

M.—TABLE Va.

	Nine Years—1882-90.				Nine Years—1891-99.			
	Percentage Isolated.	Attack-rate.	Case-mortality.	Death-rate.	Percentage Isolated.	Attack-rate.	Case-mortality.	Death-rate.
Group I. : Three good isolaters (as above)	54	3.16	8.55	0.269	71	4.73	4.68	0.218
Group II. : Six poor isolaters (as above) ...	16	4.18	8.97	0.375	39	3.73	4.24	0.157

Such a table as the above is capable of cross-comparisons—*i.e.*, we can compare each group with itself during two successive periods, and we can also compare each group with the other. Adopting the first form of comparison, we see that Group I. increased its isolation-rate by 17 per cent., and diminished its case-mortality and death-rate considerably; but it is pointed out that its attack-rate increased. That was due chiefly to epidemic influences, and nobody claims that by isolating less than three-fourths of the cases you can entirely eliminate or command epidemics. It may also have been partly due to improvements in notification. Now, Group II. effected a greater relative increase in its isolating efficiency, with the consequence that a diminution took place in every feature of its scarlet-fever incidence. There is therefore nothing anomalous in the conclusions afforded by these comparisons.

But we are asked to compare the two groups with each other, and to observe that in the second nine years the towns which isolated 71 per cent. fared worse in every respect than those which only isolated 39 per cent. Now, I object strongly to any such direct comparisons between two widely different towns, or between two small groups of towns as above, unless you bring into comparison all the factors involved. There are a great many points of

difference between the two groups, and it is obviously illogical to fix upon one particular divergent item—viz., the isolating policy—and say that this is the explanation of the difference in the incidence of scarlet fever. What about the difference in the social character of the people, the housing accommodation (just referred to), the density, the thoroughness of notification, the enforcement of school

CHART ILLUSTRATING M.—TABLE Va, SHOWING THE FALLACY OF COMPARING TWO SMALL GROUPS OF TOWNS FOR NINE YEARS ONLY, AND OMITTING THE SHADED PARTS.

attendance, and a number of other points which I will refer to later. Just now I will only take two objections against making a direct comparison of Groups I. and II.

First. Epidemic influence. The particular cycle of time (1891-99) is unfortunate for Group I. It found those three towns at the close of a long spell of low prevalence; it carried them up to the crest of one epidemic period down to the hollow, and it left them at the top

of another wave of high incidence. I have prepared a chart which shows that the nine years embraces, as regards Group I., two maxima and one minimum, while as regards Group II. we have something very like two minima and one maximum.

Second. I would call attention to the very great lack of uniformity in the isolating policy of the towns of Group II. In the average it is true that they isolated (as shown in the table) only 39 per cent. of the aggregate cases, but as a matter of fact some of the towns in Group II. were, for a considerable part of the period, actually isolating more than some of the "good isolaters" of Group I.

The following table shows for each year the percentage isolated in four towns, two of which are placed in Group I. and two in Group II.:

K.—TABLE V6.

				Scarlet Fever—Percentage isolated each Year.								
				1891.	1892.	1893.	1894.	1895.	1896.	1897.	1898.	1899.
Group I.:												
Nottingham	...	...	...	84.5	87.9	70.5	80.2	92.9	86.3	90.5	71.5	46.5
Bradford	...	...	...	67.1	62.2	15.8	50.7	65.8	62.5	68.3	69.8	72.5
Group II.:												
Derby	...	...	...	62.3	62.3	38.9	43.9	46.4	71.7	78.2	68.2	57.4
Halifax	...	...	...	11.0	5.9	0.7	34.2	48.1	68.2	49.8	55.1	67.6

Derby, it will be seen, during four or five years actually isolated more than Bradford, while Halifax, in 1896 and 1899, beat Bradford and Nottingham respectively. How can we say, therefore, that hospitals are responsible for the greater incidence of scarlet fever in Group I. when some of the poor isolaters of Group II. have exceeded some of the good ones in Group I. in the use of hospitals? I submit that Table V. makes out no case against isolation, nor even demands further inquiry on those lines.

But upon what lines shall we compile statistics in this matter? It is a general axiom in statistical work that liability to fallacy diminishes when the figures dealt with extend over a long period of time and relate to a large population. Now, in this case we cannot take a long period of time, because in doing so we reduce our towns to a very small number, such as compose M.—Table Va, whose divergent features render them incomparable. It also happens, as we have seen, that prior to 1891 the available records appear to be in favour of the isolating towns, and it is only since that year that the contrary seems to be implied. The question, therefore, is

entirely concerned with the last decade, and that being so, why should we not bring into the comparison the great volume of statistics which is available for the period we are dealing with—viz., the nine years 1891-99?

With this object I have endeavoured to bring together the figures for all the large towns of England and Wales, so as to escape, by dilution, the obvious errors of Table V. I sent out a form to the large towns and to some smaller ones whose experience has been referred to by other writers on the subject, and I have to thank a large number of medical officers for very courteously supplying me with their statistics. I determined that I would incorporate the whole of my results into one table without any selection whatever.

I received returns from ninety-three towns, and worked out the mean percentage of scarlet-fever cases isolated in each town during the nine years 1891-99. I then arranged the whole lot into four groups on this basis, putting together all those which had isolated less than 20 per cent. and so on. The following table shows the results:

K.—TABLE Vc.

Period, Nine Years—1891-99.

	Group A, Towns isolating less than 20 per Cent.	Group B, Towns isolating 20 to 50 per Cent.	Group C, Towns isolating 50 to 80 per Cent.	Group D, Towns isolating more than 80 per Cent.	Total or Mean.
Number of towns in each group ...	25	42	17	9	93
Population of towns in each group ...	1,513,347	5,346,808	2,222,030	1,036,683	10,118,868
Attack - rate per 1,000 ...	4.65	4.27	4.16	3.68	4.21
Case-mortality per cent. ...	3.87	4.51	5.09	3.62	4.50
Death - rate per 1,000 ...	0.180	0.190	0.212	0.133	0.190

Now let us analyze this table to see what (if any) lessons it teaches, and at what points it is faulty. We have just seen that it is free from any objection on the score of picking and choosing towns. The first thing to be noticed is that *the attack-rate diminishes uniformly as the percentage isolated increases*. It may be urged, in reply, that the degree of diminution appears very slight; that, for instance, the towns in Group C, after spending enormous sums to enable them to isolate from 50 to 80 per cent. of their cases, have only succeeded in lowering their attack-rate by about 0.5 per cent. But I strongly object to any such deduction,

because it ignores a number of important disturbing factors, which make all such direct comparisons of towns fallacious.

In the first place, we must remember that in a considerable number of the towns included in the table notification was compulsory under private Acts, and not by adoption of the 1889 Act, and in some of these towns notification of *first cases only* was required. In such towns notification would be incomplete in the places without hospitals. I therefore maintain that the true attack-rate of Group A must have been considerably higher than shown above, because many cases would never be notified, the authority having no hospital or other means of inducing people to notify second cases, or even those first cases which could be concealed. The same remark applies in a diminished degree to Group B, because it contains towns which could only offer isolation to a minority of the notified cases.

For argument's sake, let us approximate the actual attack-rate of Group A as 6·0 per 1,000. I next submit that Group C have diminished their attack-rate not from 6·0 to 4·16, but from a much higher figure than 6·0, which higher figure we cannot ascertain for want of notification figures of Group C prior to the inception of hospitals. Why did the towns forming Group A only isolate less than 20 per cent.? Was it because they had any advanced ideas that hospitals were a delusion? Surely it is a reasonable conjecture that their comparative inactivity, speaking generally, was due to the circumstance that the incidence of the disease in past times was not sufficiently high to compel them to expend money on hospitals. We all know that in those days municipal enthusiasm on hospital matters was, in the majority of cases, the outcome of repeated and severe visitations of disease. According to this view the towns composing Group C have naturally a greater incidence than Group A, with its hypothetical 6·0 per 1,000; but, as a result of isolation, they have reduced their attack-rate from that unknown high figure to 4·16, as shown in the table.

The next thing which we notice in my table is that the case-mortality and the death-rate are decidedly lower in the good isolating towns than in the poor ones, but as regards the moderate isolaters (Groups B and C) they appear to have had more deaths than if they had not isolated at all. Here, again, I say that such direct comparison is utterly fallacious, for the reason just mentioned among others. Are we to conclude that isolation has diminished the attack-rate but increased the fatality? If hospital influence generates a more fatal type of the disease, how comes it that such accentuated type produces a lower attack-rate. This

could only follow if the patients taken to hospital died in abnormal proportion, which, it is admitted, is not the case. Surely it is illogical to argue that hospital isolation increases the susceptibility of the people, and at the same time lets loose a more virulent type of the disease, with the result that the attack-rate falls!

We are forced once more to the conclusion that it is wrong to make direct comparisons of towns or groups of towns in this matter—firstly, on account of the reason just referred to—namely, the different “natural susceptibility” of the towns constituting the various groups; secondly, on account of the fact that Groups B and C contain an undue proportion of towns with congested populations and slum conditions, unfavourable to the recovery of those patients left at home; thirdly, and chiefly, on account of the eccentricity of isolation as practised in Groups B and C. By this last phrase I mean the lack of uniformity in the isolating policy of the towns constituting those groups. It is quite obvious that any town figuring in Group A or in Group D cannot possibly have been very irregular in its practice of isolation. To attain a mean isolation-rate for nine years of less than 20 per cent. or of more than 80 per cent. it is necessary that you should behave very consistently—*i.e.*, you cannot isolate one year 90 per cent. and the next year 10 per cent., and so on. If you did so, your mean for the nine years would fall in Groups B or C. Now, this is a most important point, because it means that Groups B and C are disqualified for comparative purposes on the grounds that they contain all the towns which have had no uniform practice as to the proportion of cases isolated. Many towns have succeeded for a time in isolating a good percentage, but we find that, when pressed with an epidemic of another disease, they have had to close their hospital doors to scarlet fever, or be content with isolating a small and insignificant proportion. All such inconsistent towns (and they form the majority) must, as a matter of averages, fall in Groups B and C, and therefore those groups as they stand can teach little. I am not concerned just now to discuss solitary instances where cessation of isolation has not been at once followed by increased incidence of scarlet fever, or those in which a period of good isolation has preceded a period of decreased prevalence.

We are now left with nothing but Group A (isolating under 20 per cent.) and Group D (isolating over 80 per cent.), and it is interesting to note that the latter has the advantage in every particular, although, as I have attempted to show, Group A consists of towns having a naturally low incidence.

	Group A, isolating under 20 per Cent.	Group D, isolating over 80 per Cent.
Attack-rate	4.65	3.68
Case-mortality	3.87	3.62
Scarlet fever death-rate ...	0.180	0.133

Now, I do not seriously insist on the above figures to decide this question, because I am fully conscious of many objections to that. These two groups include, however, *every town* coming within the limits of isolation indicated, and my point is to show that, unless we are to pick and choose among other towns, we cannot construct any intermediate groups suitable for comparison, and it is unfair to pick and choose. I fully admit that when we discard Groups B and C we may as well throw away the whole table, but I have tried to preserve those groups, whilst eliminating from them the erratic towns which they contain, and I find it cannot be done. If we simply took those towns which have during the period been regular in their isolating, we should be reduced to a very small number of towns of widely divergent character; if we shorten the period, we should encounter the disturbances caused by epidemic influence; if we lengthen the period, we should increase the eccentric isolaters; if we subdivide Groups B and C (putting together all those whose mean isolation-rates were 20 to 30, 30 to 40, and so on), we should still have the eccentrics dropping into their particular columns.

We are therefore drawn again to the conclusion that it is impossible to get groups of towns whose figures we can place side by side for direct comparison without being absolutely misleading.

So far I have only dealt with Dr. Millard's Table V., and endeavoured to show—firstly, that it is fallacious as a guide; secondly, that we cannot construct any such direct statistical indicator, but that when we eliminate disturbing factors, as far as possible, the results are emphatically in favour of hospital isolation.

I will now deal with the other tables which have been put forward, and I need only do so very briefly, because I think all of them are guilty of some of the faults already referred to.

Thus M.—Table VI. deals with the ten years 1890-99, and shows three groups of towns of different isolating efficiency.

M.—TABLE VI.

Period, Ten Years—1890-99.

	Percentage isolated.	Attack-rate.	Case-mortality.	Death-rate.	Names of Towns in Group.
Group I.: Nine towns which isolated over 40 per cent.	62·8	4·45	4·83	0·215	Birmingham, Bradford, Croydon, Derby, Huddersfield, Nottingham, Portsmouth, Salford, Sheffield.
Group II.: Nine towns which isolated under 40 per cent.	28·8	3·89	4·32	0·168	Birkenhead, Blackburn, Bolton, Cardiff, Halifax, Newcastle, Oldham, Plymouth, Sunderland.
Group III.: Four towns with practically no isolation.	—	4·75	4·14	0·196	Burnley, Preston, Swansea, West Ham.

The table also arranges the figures in two quinquennia, with the object of showing that the second five years' records are more than ever against isolation; but there are so many objections to the table as a whole that we need not trouble with the detailed figures.

1. We have only twenty-three towns in the table, although there are statistics available for three times that number for this period.

2. The towns of Group I. have an average population of more than 240,000, while the average of Group II. is less than 140,000.

3. Why is Croydon included in Group I.? So far as my information goes, their hospital was not erected till 1895, so that during the period covered by the table they only isolated 28 per cent.

4. Some of the towns in the table had only partial notification during the period (*e.g.*, Preston, Blackburn, Nottingham).

5. The difference between the isolating efficiency of Groups I. and II. is insignificant compared with other important differences between the two groups.

6. During the second half of the period (on which stress is laid by Dr. Millard) the majority of the towns in Group II. actually isolated more than 40 per cent., and therefore should go in Group I.

In fact, Groups I. and II. overlap each other so much in regard to isolation that they ought to be amalgamated, and when that is done there is no justice in comparing them with Group III., which

only consists of four towns with incomplete notification and widely different social circumstances. Thus in Groups I. and II. about 34 per cent. of the houses have less than four rooms, while in Group III. the proportion of small houses is only 24 per cent.

Coming to Dr. Millard's Table VIII., we observe that it displays fourteen Lancashire towns (for ten years, 1890-99), arranged in two groups as follows :

M.—TABLE VIII.

Period, Ten Years—1890-99.

	Attack-rate.	Case-mortality.	Death-rate.	Names.
Group I. : Eight towns isolating 37 per cent.	4.66	6.07	0.28	Bolton, Eccles, Lancaster, Oldham, Rochdale, St. Helens, Warrington, Wigan.
Group II. : Six towns without isolation.	3.86	5.40	0.20	Accrington, Ashton-under-Lyne, Burnley, Fleetwood, Heywood, Preston.

Now, the towns in Group I. were not good isolaters. Only one of them (Warrington) isolated over 50 per cent. ; all of them acted very erratically, and if they suffered in consequence, we must not blame the hospitals. For example, we find Oldham one year isolating 37 per cent., next year *none*, next year seven, and so on ; while Bolton for half the period did not isolate one-fourth of the cases. Really, how can we expect these large towns with such meagre and irregular isolation to stand better than small towns like Fleetwood in Group II. with even no isolation at all ?

After going most carefully, and, I trust, impartially, through a great volume of statistics, I am perfectly satisfied that it is futile to compare the past records of towns or groups of towns in the manner which has been attempted. It is not necessary for me to labour the question by taking town after town and pointing out the particular disturbing factors which render it unsuitable for direct comparison with other towns. Such factors are so numerous and important as to entirely upset almost all direct comparisons in this matter, yet when due allowance is made for them as far as possible, the indications are all in favour of hospital isolation.

To illustrate what I mean by incomparable towns, let me take Warrington and Preston, both of which I have specially visited. These are the most effective towns in Groups I. and II. respectively of Table VIII., and they are constantly being pointed to as examples

of the folly of isolating and the wisdom of not isolating scarlet fever. Dr. Millard's Table VIII. gives their figures for the ten years 1890-99 as follows :

M.—TABLE VIIIa.

Period, Ten Years—1890-99.

	Population.	Percentage Isolated.	Attack-rate.	Case-mortality.	Death-rate.
Group I. : Warrington	57,200	77	4.2	10.0	0.42
Group II. : Preston	111,930	—	2.3	7.1	0.16

The moral of the above seems to be "don't isolate," but when we come to inquire closer the case is quite different.

As to Position, etc.—Warrington is a small, dense, industrial town situate on the Mersey, exactly midway between Manchester and Liverpool, and surrounded at a nearer distance by typical manufacturing towns on all sides. Preston, however, occupies a fairly salubrious position at the mouth of the Ribble, with beautiful open country stretching far away in all directions but one.

As to Population, etc.—Reference to the census returns must convince anyone that Preston and Warrington are vastly different towns in regard to age and sex distribution, occupation, density, housing accommodation, and other vital points. Warrington has a great variety of industries, and its population is very migratory, as will be understood from its position. Moreover, during the period dealt with, the construction of the great ship canal caused a considerable disturbance in the character and composition of the Warrington population. Preston, on the other hand, was, during the period under review, practically a one-trade town (cotton), supporting some 9,000 more females than males, and having a low birth-rate and a small ratio of inhabitants under ten years of age.

Furthermore, I find that the estimated population of Preston, as supplied to Dr. Millard, is not borne out by the census subsequently published, which shows it to have been over-estimated by some 6,000 in 1901. This, of course, makes a proportionate alteration in the attack-rates, but that is not all: it means that the town has not increased at the expected rate, and this is an item of importance in regard to scarlet fever.

The following statistics will help to emphasize the disparity

between these two towns, far outweighing any apparent differences in their scarlet fever experience :

K.—TABLE VIIIb.

	Warrington Town.	Preston Town.	Warrington Registration District.	Preston Registration District.
Number of persons per house, 1891	5·5	4·8	5·3	4·8
Number of persons per acre ...	26·7	26·3	35·8	22·7
Proportion (per 1,000) of houses with one or two rooms only	88	32	?	?
Proportion (per 1,000) of persons living in ditto	58	19	?	?
Proportion (per 1,000) of chil- dren under ten	270	239	266	233
Mean birth-rate per 1,000 ...	36·3	33·2	39·6	32·5
School accommodation—num- ber of seats per 100 scholars on registers	107	150	?	?

As to Notification.—In both towns notification of scarlet fever has been in vogue for twenty years. In Warrington *every* case was required to be notified, and was paid for. In Preston, however, only the first case in a house was notifiable, and the Corporation would not pay for a second notification within one month after the first certificate. This state of things existed at Preston throughout the whole period we are dealing with, during which they had the low attack-rate of 2·3 per cent.

Without making any allowance for concealed first-cases, which would certainly occur in a town offering no inducements for notification, we may at once raise this apparent attack-rate considerably, because of the numerous second-cases which did not require notifying. In Warrington, on the other hand, there is reason to believe that the recorded attack-rate of 4·2 per cent. is quite up to the actual—in fact, I understand that last year some 10 per cent. of the notified cases were found not to be scarlet fever. It may also be worth mentioning that so soon as complete notification became operative in Preston under the general Act of 1899 the following remarkable incidence was revealed :

K.—TABLE VIIIc.

	Attack- rate.	Case- mortality.	Death- rate.
Preston town, year 1900	4·4	6·4	0·284
Preston town, year 1901	15·4	4·9	0·761

As to Deaths.—It seems clear that in past years notification in Preston has been far from complete; but how about its seemingly low death-rate of 0·16 per cent. in the period taken by Dr. Millard? I submit that the incomplete notification would be reflected in incomplete death-returns, and that the death-rates of such towns are not to be directly compared with those of towns isolating 77 per cent. of the notified cases, and offering every inducement for general openness. This is a phase of the question which it is not possible or desirable to investigate here, but I may mention that during the period covered by Table VIII. the death-rate from urinary diseases was 603 per million in Preston as compared with 378 in Warrington. I am unable to give the nephritis deaths separately, but the above is a notable disparity, which may possibly be partly due to concealed scarlet-fever deaths in Preston, or to sequelæ following the home-nursing of scarlet-fever patients.

General.—I could pursue this comparison further, and bring out many other important points, if necessary. By taking another period instead of that chosen by Dr. Millard, I could invert the positions of Warrington and Preston, so as to show the latter having the highest rates all round; I could criticise items concerning the Warrington hospital in past years, and mention that it served a wide area outside the town; I could give extracts from reports showing that cases removed to the hospital have at times had to wait a week or a fortnight for a vacancy to allow of their admission, such cases being classed as “isolated.” But I have no desire to hold up the sanitary merits and demerits of these two towns. I had no special object in selecting Warrington and Preston to illustrate my points; they were merely taken from Dr. Millard’s Table VIII. as being his most effective examples, and I think I have said enough to show the utter uselessness of collecting figures from various towns and tabulating them for comparison without qualification.

For my own part, I have laid down a rule, which I think no statistician will object to, *that in future the scarlet-fever records of towns or groups of towns must not be directly compared unless accompanied by full details of the local circumstances of each town.* Such details must include particulars of all the disturbing factors to which I have already alluded, and many others. To my mind, the absence of these particulars and the impossibility of obtaining them for years gone by will effectually prevent any satisfactory comparison of towns in the past. And if any general statistical inquiry is necessary to prove the benefits of isolation, it must be prospective, and not retrospective.

To recapitulate, I have endeavoured to show :

1. We cannot directly compare the past records of single towns, except with infinite precautions.

2. We cannot escape the disturbing influences by comparing groups of towns, because those useful for comparison through any length of time are too few.

3. The only remaining course is to compare a given town with itself, with and without isolation.

I have not yet dealt specifically with this last method, though many of the obstacles to which I have referred will be encountered here also. Thus, in most cases the statistics for pre-hospital times are unobtainable or unreliable. When analyzing more recent figures it is obvious that a most intimate knowledge of the local circumstances is required. I therefore think it would be a safe rule to adopt that any analysis of a given town's figures in this matter should preferably be made by the medical officer of health of that town, or by some person who has been in a position to carefully weigh all the circumstances, and is not merely guided by gross figures or rates supplied to him on paper.

To give force to this point, let me refer once more to Warrington and Preston. Warrington has been pointed to as a town with a hospital, yet having an undue incidence of scarlet fever. But the medical officer of health, with his detailed knowledge of the position, is firmly satisfied of the great value of isolation, and the Corporation are just erecting further accommodation. Preston, on the other hand, has been referred to as a town without a hospital, yet apparently enjoying comparative immunity from scarlet fever. But the medical officer of health tells me that he is grievously handicapped without means of isolation, that the lack of it has proved a source of great anxiety and expense to the community, and that the Corporation are now about to go in for a hospital.

This paper has been devoted entirely to the statistical aspect of the question, with the view of showing that adverse criticisms of hospital isolation, when based merely on crude figures, are worthless, and therefore injurious.

But I must not be understood to be defending in all its details the present and past practice of isolation; nor am I one of those unbecomingly described as "realizing that isolation hospitals had failed as a means of prevention, wished to justify their existence as life-saving agencies." I am fully conscious that there have been, and are still, many unsatisfactory points in our hospital system, but the principle is correct, and if the machine is imperfect it

requires gradual improvement, not abolition. And to secure such improvement there are many lines of inquiry waiting for practical medical officers, each in his own town. I fully endorse the opinion of Sir John Simon, who, forty years ago, said that it is the interest of properly conducted hospitals to cause full and truthful records of their respective experiences to be kept. How frequently the hospital is cleaned; how often, if ever, a ward is properly disinfected or allowed to lie fallow; how are the clothes and the homes disinfected, and a host of other questions, are proper subjects for such quiet investigation and record which would yield better results than any general statistical inquiry into the past experience of incomparable towns throughout the country; and, moreover, would not run the risk of disturbing the public faith in what is at present our first line of defence against a disease of which we have still very much to learn.

DISCUSSION.*

Dr. MALET congratulated Dr. Kaye on his paper. It showed that it was practically a waste of time to work out statistics on the lines laid down by Dr. Millard. They wanted to know in what direction they could work them out. He felt that the right way had been pointed out by those who dealt with the small-pox epidemics. Instead of bothering their heads about the number of unvaccinated people in a town, they found the number in a house, because those could be regarded as the number exposed to infection. Why not do the same with scarlet fever? He had very careful inquiries made in cases of scarlet fever, as to the number of people in a house, the number attending school, and whether any had previously had scarlet fever. This information had been kept now for eight years. It was, then, an easy thing to compare in a town like his those who went to hospital and those who did not. Might he mention the results of those inquiries in Wolverhampton for eight years? Of course, the cases removed to hospital came, as a rule, from overcrowded houses. Those left at home were mostly in houses where isolation was possible. It was, therefore, a very severe test to compare the cases taken to hospital with those left at home.

During eight years he had cases from 1,758 houses, in which houses 4,864 infectable children remained, a number sufficiently large for errors to be fairly eliminated. Cases were treated in 228 houses with 468 children remaining infectable. Of the children left at home after hospital removal, 4·8 per cent. contracted scarlet fever. Of the children in the houses isolated at home, 28·2 per cent., or more than six times as many, contracted the disease. In other words, 95 per cent. escaped infection in the more or less slummy houses from which patients were removed, whilst only 72 per cent. escaped in the other cases. A proportion of cases were those infected before removal. Taking these out, they had only 2·2 per cent. after removal, against 19·2 per cent. for home treatment. This calculation did not include "return" cases, of which he had

* See also discussion at meeting of the Midland Branch at p. 502 of present issue.

had 129 in eight years, and even with these added the percentage of 4·8 was only brought up to 7·4. It had been stated that cases did badly in hospitals compared with home treatment. The hospitals at Wolverhampton were not by any means ideal, and at some periods they were badly overcrowded; but in the twelve years during which they had had notification, they had treated in the hospital 4,251 cases with a death-rate of 3·93 per cent.; during the same period 1,182 cases were treated at home with a death-rate of 5·58 per cent. Those figures were sufficiently large, and extended over a sufficiently long period of time to be fairly free from error. He suggested that figures of a similar kind to those should be obtained and tabulated for all the large towns.

Dr. T. ROBINSON said it did not follow because some portion of the management of a hospital was a failure the whole hospital should be done away with. The "return" cases, for instance, formed an important factor in the increase of scarlet fever in towns with hospitals; but if the machinery was perfect, there could be no doubt the hospital would be of great benefit in putting down scarlet fever. He agreed with Dr. Kaye that the grouping was not fair. In the first group of Lancashire towns in the paper Dr. Millard read to the Society, the attack-rate, death-rate, etc., differed so much in the same group that he held there must be something in the towns themselves to account for the very marked difference. It would be better to inquire as to special points of failure in hospital administration, and seek to remedy them.

Dr. ARMSTRONG did not know that it was desirable for them to enter into statistics on such a subject as this, or they might never know the end of it. To raise questions such as those raised on this subject simply astounded him. The way of comparing statistics as they had been compared was beyond his practice or his experience, and made him think they were getting into a region of dangerous extravagance.

Dr. H. BEALE COLLINS said the Borough of Kingston had a population of 35,000, but with the surrounding districts the population was increased to 60,000. Kingston itself was a working-class population. They had no isolation hospital, and never had had one. Notification began by a private Act in 1889, practically on the same lines as the General Act. The figures he had for the last year might be taken as a very fair average. He had divided the cases reported into two tables, one showing the total number and the second the number of *single* cases. Out of 130 notifications, 72 were single cases—*i.e.*, only one case in a house. Only 8 secondary cases arose during the course of treatment in houses, and there were a certain number of other cases notified at the same time as the original case. These could hardly be attributed to infection from the primary case. The number of days under treatment averaged 29·01 per case. The case-mortality was 0·76 and the attack-rate 3·76. Six cases were sent to the London Fever Hospital by persons at their own expense; from these one "return" case arose. He, of course, visited every house himself where cases occurred and interviewed the parents, giving them instructions as to what they should and should not do.

Dr. C. KILLICK MILLARD: It is scarcely necessary for me to say that I have listened to Dr. Kaye's paper with special interest. I am not at all surprised that the storm has burst at last; indeed, I have been expecting it ever since I read my paper before this Society thirteen months ago. I am very glad that Dr. Kaye has taken the matter up, for nothing but good will result from further discussion. At the same time, I must confess to being disappointed. I had hoped that Dr. Kaye had been making an independent inquiry to ascertain how

far the hospital isolation of scarlet fever had succeeded or failed. It would seem, however, that the paper has been conceived and carried out with the chief object of picking holes in my paper, in order to discredit, if possible, my plea for a full and authoritative inquiry into the subject.

Dr. Kaye begins by saying that it is somewhat late to question the utility of hospital isolation after so much public money has been spent upon it. This, I fear, is the lurking sentiment of many who are opposed to any serious inquiry into the subject.

In reply, I would say that it is never too late to mend; and even if we have spent a vast amount of public money on something which we honestly believed to be beneficial, that is no reason why we should hesitate to inquire how far the results have come up to, or fallen short of, our expectations. It is certainly very undesirable that we should continue blindly to spend more and more money every year upon a system of which the value, to say the least of it, is becoming very doubtful. Dr. Kaye goes on to say, "*It becomes necessary not to immediately uproot the public faith in our hospital system by a general inquiry into its alleged dangers.*" That such a result would very possibly follow a general inquiry I quite admit; but if Dr. Kaye had the courage of his seeming convictions, I should not have thought that he need be so apprehensive!

Personally, after having heard Dr. Kaye's criticism, I feel more than ever convinced that an inquiry is called for. Dr. Kaye has not adduced one single serious argument in favour of the hospital isolation of scarlet fever, and, on the other hand, he has left untouched many of my most conclusive arguments against the measure. Lastly, his own figures, in reality, corroborate my own.

Dr. Kaye's criticism amounts to little more than a condemnation of the making of comparisons. Comparisons are proverbially odious, especially when they tell against something in which we are interested; and they are generally open to the charge of not being quite fair or absolutely conclusive. I fully realized this in writing my paper, and, had my conclusions rested entirely upon the figures in Tables V., Va., and VI. (those upon which Dr. Kaye has concentrated his fire), I should have had much less confidence in putting them forward.

As confirmatory evidence, however, I venture to think that they are not without value, in spite of all that Dr. Kaye has said against them.

Now let us see exactly what his objections amount to. He first takes Table V., and objects because in the one group there are only three towns, and in the other six. I admit that a group of three towns is smaller than I could have wished, but my object in this table was to take as long a period as possible, and there are only nine of the great towns with records available for so long a period as eighteen years. Three of these towns had practised hospital isolation to a much greater extent than the others, and formed a natural group of "good isolaters" for the object in hand.

In Table Va. I show that, whereas the six towns ("poor isolaters") reveal considerable improvement during the last nine years, the towns which had "pushed" isolation reveal much less improvement. I submit that such a result is unsatisfactory and calls for explanation. Dr. Kaye suggests that the character of the towns is widely different, and adduces the fact that there are more small tenements in the towns forming the group of "good isolaters." But it is in towns with the greatest number of small tenements that hospital isolation, if anywhere, is likely to be beneficial, and in which, therefore, we should expect to

find the greatest result; so that this is really no explanation at all. To account for the increase in the prevalence of scarlet fever amongst the "good isolaters" Dr. Kaye states: "That was due to epidemic influence, and nobody claims that by isolating less than three-fourths of your cases you can entirely eliminate or command epidemics." Is such a statement as this worth very much? Admitting that isolation, no matter how complete, will not "entirely eliminate or command epidemics," surely we may, nevertheless, expect to see *some* beneficial effect, in the direction of a lessened prevalence of the disease, after isolating over 70 per cent. of cases during a period of nine years. If not, then I assert that hospital isolation has been a failure. Amongst his minor objections to Tables V. and Va., Dr. Kaye refers to "lack of uniformity in isolating policy," and seems to suggest that, because certain towns amongst the "poor isolaters" isolated a high percentage of their cases *in individual years*, therefore they should be classed as "good isolaters." It is, of course, easy to raise objections such as these, but do they really affect the questions at issue? The course I pursued was to ascertain the mean percentage isolated for each town for the *whole period* under consideration, and to put those above a certain figure into one group, and those below it into another. It is difficult to see what fairer way could have been pursued.

I will now deal with Dr. Kaye's criticism of my Table VI., which refers to a much larger number of towns than Tables V. and Va. but covers a shorter period. His first objection is that the figures refer to only twenty-three towns, although, he says, "there are statistics available for three times that number for this period." Dr. Kaye seems to have quite overlooked the fact that I was dealing here with the "thirty-three great towns" only. The smaller towns I dealt with separately, and I did this purposely to anticipate, as far as possible, an objection which he next makes—viz., that the average population of the towns in Groups I. and IV. are unequal.

His third objection—viz., why I included Croydon in Group I., as he calculates that only 28 per cent. of the cases in this town were isolated—appears to arise from some misunderstanding. He will find that cases were removed to hospital in Croydon prior to 1895, and, according to the figures sent me by the Medical Officer of Health, Dr. Meredith Richards, 40 per cent. of the cases notified during the ten years were treated in hospital. To show, however, how Dr. Kaye seems to raise objections simply for the sake of doing so, I would point out that it would have told still more against hospital isolation if Croydon had really isolated, as he suggests, only 28 per cent., inasmuch as Croydon's scarlet-fever statistics are almost the most satisfactory of any of the great towns.

His fourth objection to Table VI. is a more reasonable one. I regret that any towns should have been included in which notification, even for a part of the period under consideration, was not "complete." It was my wish to exclude all such towns, and, had I received any intimation when the returns were sent me that only primary cases had been notified, I should have excluded them. It so happens, however, that there is one "incomplete notification" town in each group, so that the effect, so far as this table is concerned, is, in all probability, neutralized. Moreover, the difference between notification of primary cases only and notification of all cases is very much less than might be thought, as the great majority of the secondary would be discovered by the inspectors when visiting the primary cases.

Lastly, Dr. Kaye turns to my Table VIII. This table undoubtedly tells heavily against hospital isolation, and I do not wonder that Dr. Kaye has tried hard to

discredit it. Here, again, his arguments have rather an artificial ring. He picks out Preston and Warrington, and shows that, in certain respects, Warrington is at a disadvantage; the proportion of two-roomed tenements is greater, and the number of seats per 100 scholars in schools is less. This may be worth something, so far as it goes, and Dr. Kaye is to be congratulated on having found some sort of an explanation of Warrington's disastrous experience with regard to the hospital isolation of scarlet fever. That the explanation is at all adequate I cannot, however, admit. Dr. Kaye then refers to the fact that primary cases only were notified in Preston, whilst all cases were notified in Warrington. I am quite willing to allow that, owing to this, the *attack-rate* for Preston has been *slightly* understated. But just so much has the fatality, or proportion of deaths to cases, been *overstated*. Now, I attached comparatively little importance in my paper to *attack-rate*; the most disquieting feature of my statistics, and the one on which I laid so much stress in my Eastbourne paper, was the *comparatively low fatality in the non-isolating towns*.

The remarks which precede Dr. Kaye's Table VIIIc. are, to say the least of it, very misleading. It is well known that during the past year, 1901, a very extensive epidemic of scarlet fever has visited Preston, the *attack-rate* being no less than 15.4 per 1,000 population. No one who has studied Preston's past experience as regards scarlet fever can be very surprised at this. For many years the town has suffered extraordinarily little from the disease, and consequently a large amount of susceptible material has been accumulating. A "flare-up," sooner or later, was almost inevitable.

Certainly no one can seriously suggest that this epidemic prevalence was explicable by the fact that the 1889 Notification Act had just been substituted for Preston's local Act. Nevertheless, Dr. Kaye says: "So soon as complete notification becomes operative in Preston under the general Act of 1889, the following remarkable incidence was revealed;" and then follow the figures.

His next stroke, however, is, in my opinion, the weakest and most forced in the whole paper. Having discovered that notification was "incomplete" in Preston, which he suggests will account for the very low *attack-rate*, he has still to account for the very low number of deaths from scarlet fever. He therefore seriously and deliberately suggests that, because notification of cases has been "incomplete," the number of deaths reported has also been "incomplete." He says: "I submit that the incomplete notification would be reflected in incomplete death returns!"

To the uninitiated such a suggestion may seem very plausible, but anyone even slightly acquainted with the *modus operandi* of infectious disease notification and death registration must know quite well that the two things are worked by entirely different and independent machinery. Even if notification had been "incomplete" owing to some general slackness in its enforcement, it could have had no influence whatever upon the completeness or otherwise of death registration. As a matter of fact, however, notification was "incomplete" because the Notification Act in force in Preston happened to require notification of primary cases only, and there is no evidence that the medical men in Preston were less careful in notifying than medical men elsewhere. Death registration, on the other hand, is carried on in Preston in precisely the same way and under the same Act as in other parts of the country, and there is no reason, so far as I am aware, for supposing that scarlet fever deaths were not as accurately registered in Preston as in any other town. Yet Dr. Kaye says vaguely that "the death-rates of such towns (Preston) are not to be directly compared with

those of towns isolating 77 per cent. of notified cases and offering every inducement for general openness."

Finally, Dr. Kaye comes to the conclusion that "the absence of certain particulars, and the impossibility of obtaining them for years gone by, will effectually prevent any satisfactory comparison of towns in the past."

This is almost tantamount to admitting that all the labour and expense involved in piling up municipal statistics of disease in past years (so far, at least, as scarlet fever is concerned) has been to no purpose, as the statistics, now that we have them, are useless.

I will now turn back to Dr. Kaye's Table Vc. This is, in my opinion, the most interesting and valuable part of his paper, as it embodies the results of an independent statistical inquiry, and I have therefore left it to the last. I may say at once that I welcome it as striking proof of the correctness of my own conclusions.

Dr. Kaye's procedure, whilst closely resembling my own, differs from it in this important respect: that whereas I treated the thirty-three great towns separately from the smaller towns, he has treated all together. I still think that my procedure is the more satisfactory; for, whilst there may be some objection to comparing the great towns with one another, there must be very much more objection to comparing the great towns with the smaller towns. Some of the latter, for example, are of the nature of watering-places and health resorts, and such places, besides usually possessing great natural advantages and containing a large proportion of well-to-do inhabitants, make exceptional efforts, for the sake of their reputation, to control infectious diseases, not only by isolating a very large proportion of cases in hospital, *but by pushing all other preventive measures as well*. As might be expected, they are able to show much more satisfactory scarlet-fever figures than large manufacturing towns.

Now let us examine Dr. Kaye's table carefully. He divides his ninety-three towns into four groups, A, B, C, and D, according to the percentage of cases isolated in the nine years 1891-99. There is also a column showing the means. I would first call attention to the significant fact that both the case-mortality and the death-rate in Group A (with least isolation) *are below the means for the whole ninety-three towns*. In the next two groups, B and C, there is a progressive increase in both these rates, *pari passu* with increased isolation. In all these three groups the number of towns forming each group is satisfactorily large (25, 42, 17), and the rates, therefore, must be taken as reliable. In Group D, which is a small group of only nine towns, we experience a sudden reversal of the progression, the case-mortality and death-rate dropping most remarkably. Dr. Kaye would have us believe that this satisfactory result is due to the difference in the proportion of cases isolated in this and the previous group; but why the same effect is not seen in groups C and B, as compared with A, is, to me, inexplicable on Dr. Kaye's hypothesis. There is, however, a very much more simple explanation of the apparently inconsistent behaviour of Group D. Dr. Kaye gives us no indication in his paper of the names of the towns composing the different groups. This is an unfortunate omission. I think perhaps I can partially rectify it. I happen to know that Cheltenham, Leamington, Bournemouth, Hove, and Brighton are all towns which have isolated over 80 per cent., and would therefore, I presume, be included by Dr. Kaye in Group D. This accounts for five out of the nine towns, and I submit that it is quite sufficient to account for the favourable figures in this group. A group of so highly specialized composition cannot fairly be compared

with the other groups, and instead of excluding Groups B and C, as Dr. Kaye would like to do, I suggest that we should exclude Group D.

The table then reads as follows:

			Case-mortality. Death-rate.	
Towns isolating less than 20 per cent.	...	3.87	...	0.180
" " 20 to 50 per cent.	...	4.51	...	0.190
" " 50 to 80 per cent.	...	5.09	...	0.212

—that is to say, *the case-mortality and death-rate increase progressively with the proportion of cases isolated in hospital.*

It is true that a fractional diminution in the prevalence of scarlet fever seems to have been achieved by increased hospital isolation, the attack-rate falling progressively from 4.65 in Group A to 4.16 in Group D. Those who still pin their faith to hospital isolation are welcome to this scrap of comfort, but it must indeed be cold comfort when we reflect that this slight reduction in *cases* has been accompanied by a material increase in *deaths*.

There are many other points I might have referred to, but I trust I have said sufficient to show that the doubts which are exercising many of us as to the utility or otherwise of the hospital isolation of scarlet fever are not likely to be dispelled by Dr. Kaye's paper.

Dr. KAYE: I am much gratified by the kind manner in which my paper has been received, and I feel that the great interest shown in it amply justifies the labour expended.

Like Dr. Armstrong, I deprecate the dangerous use of bald statistics on a subject like this, and the object of my paper has merely been to avert, if possible, the danger of allowing previous statistics to go unchallenged.

I think Dr. Malet has set an example worthy of emulation, for, instead of straining after astounding generalizations based upon the crude statistics of a hundred towns, he has industriously worked out the inner details of his own town's experience so as to arrive at the real kernel of truth. With regard to the single year's figures for Kingston put forward by Dr. H. Beale Collins, I fail to see that any argument relevant to the matter in hand can be based upon them.

In reply to Dr. Millard, I am sorry that he should make the complaint that I have "picked holes" in his paper, but he surely does not mean that I have confined myself to cavilling at details. If he objects to my attacking his paper directly, instead of producing an independent set of statistics to prove the opposite, he mistakes my position. I quite admit that his figures, as far as they go, are as nearly correct as can be obtained, and therefore there is no need for the general retrospective inquiry which he advocates. What I object to is his method of employing those statistics, which I think I have shown leaves out of account all the most important factors at work.

Dr. Millard, in his prepared reply to my paper, also objects that I deal only with certain of his statistics. As already explained, I have dealt with all the figures which he launched before the public at the Eastbourne Congress, and which, I take it, were all that he deemed necessary for his purpose. At all events, they are the tables which have produced the most harm in the minds of the lay sanitarians, among whom they were spread broadcast.

I agree with Dr. Millard that it is never too late to mend, and I think that the great advances which have been made in hospital matters in recent years cannot fail to produce good results unless choked by the revolutionary ideas which have unfortunately received general circulation of late. Reform may be

wanted, and is being effected, but revolution was ever the cry of the irresponsible and the theorist. Let us have as much discussion as possible amongst ourselves, but to carry the argument into the street by a public inquiry would produce incalculable harm, no matter how strong a verdict were eventually returned in favour of isolation.

Dr. Millard asserts that I have not adduced any serious arguments in favour of hospitals, but I did not set out with that object, and I fear he must have mistaken the scope of my paper. If one presents a claim upon property, it is not necessary that the person in possession should meet it by a full recital of his title, especially if it were easy to show the new claim to be invalid, by "picking holes" in it.

With regard to Tables V. and VI., I am glad to know that Dr. Millard does not really place much confidence in them, though he still thinks that, in spite of all they may have some value as confirmatory evidence. It is a pity that this complexion was not placed on them at first, because I am convinced that it is just those tables which have done the most harm. Only yesterday I received from the medical officer of health for a small urban district in Yorkshire his annual report, containing a complete reprint of Dr. Millard's Table V., which was placed before the urban councillors, and no doubt fully accepted as proof positive of the wisdom and economy of leaving scarlet fever to run its own course. Dr. Millard does not dispose of my criticisms, and although he attempts to ridicule some of my minor objections, he fails to justify his original position. In speaking of Table V. he absolutely ignores my chief point, which I emphasized by producing a chart; and when I show by Table K. - V. (b) that his two groups overlap each other in certain years, he blandly asks, "Does this really matter?"

With regard to my criticism of Table VI., it will survive all that Dr. Millard has said in reply. Every one of my objections remains true and unchallenged, except possibly my reference to Croydon. The return which I received from the medical officer of health distinctly stated that the scarlet-fever hospital there was not erected till 1895, and I therefore objected to that town being included as a first-class isolator in a table dealing with five years prior to the erection of the hospital. I was quite aware, when I made the objection, that Croydon's records, as far as they go, are in my favour; but I insisted in their being thrown out, on the sound principle that they are incomplete. It is now suggested that there was some error in the statement supplied to me from Croydon, but if that be so, it merely confirms my plea as to the uselessness of basing sweeping deductions upon figures loosely compiled by sending out circulars to medical officers of health asking for past records. The medical officer of health for Croydon has subsequently informed me that "nearly all the sanitary staff (including the medical officer of health) have only traditional accounts of the past history of Croydon previous to about 1896."

As to Table VIII., no fair-minded statistician will now insist upon that as telling heavily against isolation. I have nothing to add to or retract from my criticism of it. Dr. Millard does not attempt to defend his table, but merely abuses my investigation of its most prominent constituents (Warrington and Preston). The "artificial" method to which he objects is, I should have thought, a very natural course—viz., to investigate and set out as I have done some of the special factors at work, which effectually prevent direct comparison. As already stated, I do not seriously object to any of Dr. Millard's figures, but only to his deductions. We can afford to take the Preston records at any figure

imaginable, but the mistake lies in comparing them directly with towns of widely divergent character.

I thought I had sufficiently emphasized that point to have escaped the scorn with which Dr. Millard appears to receive my suggestion that the Preston scarlet-fever deaths have been understated. You can put them at any figure you prefer, but what I say is, do not compare them with the records of a totally dissimilar town, or, in making comparisons, do not ignore the many other differences which exist over and above the difference in hospital isolation. To do so is like comparing the relative height of several bodies without observing that they are standing on different levels. I repeat that we cannot hope for any good to accrue from an inquiry into the past records of this country, because (to retain the metaphor), although our statistics may furnish us with the heights of the bodies we are to compare, they do not supply the information as to the various levels on which those bodies rested, and this information is unfortunately not now obtainable for past years.

Dr. Millard's mania for these illogical comparisons appears incorrigible. In Table K.—V. (c), I presented the gross results of the figures which I have collected. I did this merely to show what form the figures would take without any picking and choosing, but I was most careful to point out that they merely confirm the absolute folly of making direct comparisons. After all the objections and warnings which I gave against any such comparisons being made, Dr. Millard (apparently unable to resist his infatuation) rides roughshod through all reservations and qualifications, and emerges with another direct comparison and a sweeping generalization against the practice of isolation. Yet, so far as I am aware, he has no better measure to advocate.

I would gladly co-operate in the compilation of a form of table for distribution among medical superintendents of isolation hospitals, whereby some reliable and uniform data might be compiled for the future which would embrace many important items hitherto unrecorded. But I am more than ever convinced that the course advocated by Dr. Millard would result in putting back the clock, and would do incalculable mischief throughout the whole field of preventive medicine.

SPORADIC CASES OF ENTERIC FEVER.—While epidemics of enteric fever can, as a rule, be traced with tolerable certainty to their cause, it is far otherwise with those scattered, disconnected cases that crop up here and there, and to which we apply the term sporadic. Such cases afford no certain clue to the sanitarian. It is always right to make careful inquiry into the water-supply and drainage, and to have any obvious nuisances on the premises removed. We may be wide of the mark all the same, and must not forget, while groping among the drains, that the disease may have had no local origin at all, that it may have been contracted elsewhere, that it may have come in a soda-water bottle, or in a hundred other ways past all possibility of finding out. The chief importance of those sporadic cases from a public health point of view lies in the fact that they constitute so many foci of infection from which, in the absence of strict precautionary measures, epidemics may very easily arise.—R. W. D. MACMARTIN CAMERON, *Ann. Rep.*, 1901.