The Relation of Housing to the Isolation of Scarlet Fever and to Return Cases.

By Miles B. Arnold, M.D.

In this paper I propose to show the effect, upon the spread of infection of scarlet fever, of isolation in hospital as compared with isolation at home. The survey is confined to 3,000 consecutive cases reported in Manchester in 1907 and the early part of 1908, the subsequent cases occurring in connexion with them having been traced through the three months following the notification of the three-thousandth case. I wish to take this opportunity of cordially thanking Dr. Niven for so kindly allowing me to use the Manchester records. Many of the methods of classification used will be found in his Annual Reports.

Whatever view one may take as to the value of institutional methods in general as compared with management of home affairs by the family concerned, it is obvious that there are certain advantages in the hospital treatment of infectious diseases. On the general question I shall touch only briefly and on the most prominent points:—

(1) If a source of infection is discovered and removed to hospital there is obviously less chance of its spreading immediately to the family than if the source of infection is left in the isolation which may be arranged in the home.

(2) The social and business relationships of the family concerned are rendered easier and safer if the member suffering from an infectious disease has been removed.

(3) In severe cases at any rate it is quite impossible for sufficient attention to be provided in the average home.

On the other side two main criticisms have been made. The first is the suggestion that a patient in an isolation hospital is subjected to the risk of contracting a severer form of the disease for which he was admitted and may occasionally contract some other disease in addition. Certainly it occasionally happens that measles or chicken-pox breaks out in a ward, but, excepting that the resistance of the inmates is probably somewhat lowered by the primary disease, this is a risk which is shared to a considerable extent by children's general hospitals, by crêches, and by schools. As to the increase in the severity of any one disease through collecting patients together opinion is still divided.

I do not consider that with modern methods of nursing this may be seriously considered as an overwhelming objection to isolation hospitals. Comparisons of the death-rates in and out of hospitals are not of much value, as there is undoubtedly a tendency to send into the hospital cases which are severe and to attempt to nurse at home those which seem to promise a mild course. Even then an unexpected attack of nephritis or the occurrence of a mastoid abscess not infrequently leads to the admission of a patient to hospital late in the disease. In any case very low death-rates are often recorded by large hospitals, so that the suggested increase in severity cannot be a constant factor of serious extent.

The second main objection raised to hospital isolation is that patients when discharged have a power, to an extent not found in those isolated at home, of infecting susceptible persons. It has even been suggested that this infectivity is so great as to balance any known gain from hospital treatment. This is the main criticism which believers in isolation hospitals have to answer. The old answer that infection of susceptible persons, following on the return home of a patient from hospital, was due either to coincidence or to a disinterring of articles which had escaped disinfection must be abandoned. The investigations of Dr. Newsholme, Dr. Niven, Dr. Chalmers and Dr. Killick Millard, and last but not least the reports of Dr. Turner and Dr. Cameron to the Metropolitan Asylums Board, are conclusive as to the infective power of some patients on discharge from hospital.

Another explanation offered is that the protracted or recrudescent infectivity is due to discharged patients catching cold on the way home. Though precautions against this should clearly be taken, this reply proves too much when we remember that we are working in a country in which discharging in inclement weather is almost unavoidable.

Before coming to the details of the analysis of figures bearing on this question, I shall have to give rather a tedious explanation of the definitions to which I have conformed. In the first place, it should be explained that though 3,000 consecutive notifications were dealt with, only 2,192 remain in the following tables. The analysis is mainly concerned with any relationship which may be found between the density of the population in the homes and the success of the isolation of a patient suffering from scarlet fever either in the home or in hospital. It is unnecessary to detail the various reasons for which notifications had to be ignored, as a few examples will suffice. Obviously "overlooked cases" are not to be included when the efficiency of isolation is being considered, and these have been dealt with in another paper. All cases

occurring in institutions had to be ignored as the density of the home population could not be accurately estimated. Then, again, as an enumeration of the susceptible persons remaining in the home is included, any sheet in which this information was incomplete had to be disregarded. Deaths also had to be taken out. These examples will suffice. The following definitions were adopted:—

Primary invasions of a household include the first case and all subsequent cases which occurred within seven days of the isolation, either at home or in hospital, of the primary or subsequent cases. In this respect the figures are a better guide to the truth than many of those published. It is obvious that a case occurring four or five days after another in the same house proves nothing as to the advantage of the particular form of isolation adopted. It is generally agreed that hospital cases are drawn from the more populous homes. The chance of a subsequent case occurring by direct infection in such homes is obviously greater, especially as the parents in these homes are often less careful in observing the onset of disease. For this reason I have adopted the definitions of a "primary invasion" as including all cases in which, the interval being less than seven days from known exposure, the infection could not be safely attributed to other causes.

Susceptible persons are all living as members of the family who are said not to have had scarlet fever and who are under 16 years of age. Any others who actually contracted the disease are also included.

The density of home population is the number of inhabitants per room. Children of all ages are included as inhabitants.

Incidental infections are infections of susceptible persons occurring during the period between seven days after the beginning of isolation of the invading cases and before the release from isolation of any of the patients.

Return and recovery cases are any occurring amongst the susceptibles after the release from isolation of a previous case. There is no time limit except that the search for subsequent cases was not carried further than three months after the date of notification of the three-thousandth case. On the other hand, only infections occurring in the home of the patient are counted, such a history as having "played in the street with a child recently suffering from scarlet fever" is ignored.

It seems very important not to have a small time limit in considering return cases. If the question under investigation is that of protracted or recrudescent infection it is surely begging the question to refuse to include any case occurring after three weeks or a month.

Dr. Niven has always refused to accept a time limit, with the result that his reports are a mine of valuable information on this point. There are numerous other minor points as to the methods used in constructing the tables in this paper, which it would be tedious to relate. It should be mentioned that although the figures appear here only as totals, I have grouped them also for my own satisfaction in three consecutive thousands and the important percentages did not vary greatly.

It is of course generally recognized that patients treated in isolation hospitals are not drawn from the same class socially as those treated at home. Dr. Niven has shown that the rentals of the houses in which patients suffering from scarlet fever were kept at home in Manchester were considerably higher on the average than the rentals of those houses from which patients were removed to hospital. Again, in Dr. Turner's report it was demonstrated that whilst no relationship could be established between the incidence of return cases and the particular hospitals of the Metropolitan Asylums Boards in which the patients had been, there was a definite relationship between the incidence of return cases and the borough from which the patients had come. This suggests at once that some at least of the factors producing return cases are to be found in the home.

The figures show the following facts :—

(1) The numbers isolated at home and in hospital.

(2) The numbers of susceptible persons remaining in the homes in two groups.

(3) The number of persons per room in the houses concerned; the grouping being: (*a*) less than 0·5 person per room, (*b*) 0·5 but less than 1 person per room, (*c*) 1 but less than 1·5 persons per room, (*d*) 1·5 but less than 2 persons per room, (*e*) 2 or more persons per room.

(4) The number of incidental cases occurring.

(5) The number of cases which occurred after the release from isolation of an invading case, either from hospital, i.e., return cases, or from home, i.e., recovery cases.

Before giving the details, the fact should be emphasized that they are of no use except for the purpose in view. For instance, of the 2,192 cases with which I am dealing, only 57 per cent. were removed to hospital. This is not the percentage removed in Manchester. The actual percentage is greater and the difference is caused by several eliminations, such as cases from institutions, &c., which affect chiefly the hospital cases. Again, the return case-rate is very high. It is always a higher figure in Manchester than in most towns, because no time

limit is fixed and all cases are included. But in this instance the figure is much higher than the truth, the reason being that return cases are always specially investigated and therefore all sheets relating to them were filled in perfectly. On the other hand, a number of sheets where there had been no return case had to be left uncounted because of some ambiguity of statement.

Two thousand one hundred and ninety-two invading cases occurred in 1,976 houses; 1,529, or 57 per cent. of the cases, were removed to hospital. Of the group treated at home 72·5 per cent. occurred in houses in which there was less than one inhabitant per room. In these houses where home treatment was adopted there were 1·05 susceptible persons per house remaining. Of the hospital group only 36·2 per cent. came from houses in which there was less than one inhabitant per room, and in the houses in the hospital group an average of 2·01 susceptible persons remained. It is clear, therefore, that in the hospital group there are not only more susceptible persons, but they have to live in more intimate contact.

For the moment, we shall leave return and recovery cases out of consideration and follow the course of events during isolation only. In the 1,359 houses in the hospital group there remained 2,736 susceptibles. Only 44 began to be ill with scarlet fever between the period from seven days after the removal of the invaders to the time of their return home. The "incidental" cases in this group therefore amount to 1·5 per cent. of susceptible persons. A point of some interest is the similarity of the percentage of incidental cases to susceptible persons in the hospital groups of 0·5 to 1, 1 to 1·5, and 1·5 to 2 inhabitants per room. These cases are due either to lingering infection in inanimate objects in the house or, more probably, to mild overlooked cases, or to persons who have become breeding grounds for the infecting organism without having had any recognizable reaction—i.e., carrier cases. Apart from coincident infections, the only other possible source would be an external breeding ground, such as a midden or a defective drain, which might have been the cause of the invading cases. This last hypothesis has few supporters at present, though ten to fifteen years ago the journals contained numerous papers in which the authors evinced an unqualified belief in such sources of infection. The number of these incidental cases in the hospital group bears a constant relation to the number of susceptible persons, as also does the percentage of all cases, incidental plus return cases, infected. The return case-rate, as will be seen later, rises in the different house groups, at the same rate that the proportion of susceptible persons increases.

In the 617 houses in which patients were nursed at home, 653 susceptible persons remained, and during the period between seven days after isolation began and the time that the patient was released, 67, or 10·2 per cent., of incidental infections occurred. There does not seem to be any explanation of the big difference in the incidental infections in the two groups except that in spite of the good home conditions isolation was so imperfect that a large leakage occurred.

It has been already mentioned that, although the figures so far are entirely in favour of the hospital, the housing conditions are so different that the full extent of the gain is masked. Unfortunately, it is difficult to get comparable groups of sufficient size. Taking the hospital and home groups where the home population is from 1 to 1·5 per room, we have in the hospital group 509 houses in which 1,076 susceptible persons remained, and 15, or 1·3 per cent., incidental cases occurred. In the home group of 136 houses, 268 susceptible persons remained and 36, or 13·4 per cent., of incidental cases occurred. This enormous hospital advantage is, of course, much diminished when return cases are compared with recovery cases.

So much for what happens during isolation. We now come to the question of return and recovery cases. It will be convenient to consider recovery cases first. The 663 home-treated cases were followed by 11 recovery cases, or 1·6 per cent. The recovery case-rate seems to be very variable. Investigations by Dr. Niven show both higher and much lower rates: 1·6 per cent. is higher than is usually found. Eight of the recovery cases occurred in the group of 0·5 to less than 1 person per room, in which houses 406 had been isolated.

Before coming to the figures for return cases, I propose to discuss what one would expect to find, keeping in mind certain facts. In the first place it seems very probable that a number of patients, after an attack of scarlet fever, retain their power of infection for a long period, irrespective of the type of isolation adopted. Dr. Newsholme, in his paper on "Protracted and Recrudescent Infection in Diphtheria and Scarlet Fever," says:[1] "In the preceding pages instances have been given of protracted infection in scarlet fever in which there had been no recent contact with acute cases of that disease, and of cases in which there had been no contact during the patient's illness with any except personal infection; and it has been shown that in some of these cases infection recrudesced after an interval of apparent freedom from infection. It has also been shown that such instances of protracted and recrudescent

[1] *Med.-Chir. Trans.*, 1904, lxxxvii, p. 583.

infection occur in diphtheria when patients are treated at home; and in this disease no suggestion, so far as I am aware, has been made or could be supported, of any special hospital influence favouring 'return cases.' The known close relationship and analogy between the two diseases suggests that the explanation of the above occurrences for one disease will apply equally for the other."

I venture to refer to a chart prepared to show the rate of decrease in infectivity during the successive weeks in scarlet fever. The facts were obtained from the results of infections from overlooked cases. The details of the method employed would take rather long to explain. They were published with the chart in *Public Health* for August last year.[1] My reason for mentioning it now is that the curve, which I admit to be only an approximation to the truth, does not suggest that infection has by any means disappeared at the end of the fourth week. Assuming that an unknown percentage of patients remains unrecognizably infectious at the end of the usual period of isolation, we expect that a certain number of susceptible persons will be infected. Now it is clear that the home and hospital groups will have very different opportunities offered to them. The home cases are released to houses which are much roomier. In 74 of the 617 houses in this analysis there was less than 0·5 person per room, and in 453 of the 617, or 73 per cent., there was less than 1 person per room. On the other hand, of the hospital cases, in only 31 houses out of 1,359 was there less than 0·5 person per room, and only in 521 out of 1,359, or 38 per cent., did the patient return to a house with less than 1 person per room.

In the first place, then, the released hospital patients would, quite apart from probably receiving less careful attention and being in less favourable surroundings, be forced into more intimate contact with susceptible persons. Now, when the hospital patients were removed, they left behind them 2,736 supposedly susceptible persons, or slightly more than an average of two in each house. The home patients were isolated in homes which had 653 susceptible persons in 617 houses, or rather more than one on the average in each house. During the period in which incidental infections occurred, 44 were amongst the 2,736 in the hospital series, and these must be subtracted from the total to arrive at the presumably susceptible population awaiting the home-coming of the patient. This figure is 2,692, which is an average of 1·9 per house. The home group of supposed susceptibles was 653, amongst which 67 incidentals occurred, leaving an average of 0·9 per house. But this is

[1] *Public Health*, 1910-11, xxiv, pp. 414-17.

not a fair statement of the matter. Susceptibility is simply an assumption, and it is certain that of the group of supposed susceptible persons in each case many would be insusceptible either from forgotten previous attacks or natural immunity, or from the resistance which many have probably acquired by their fifteenth year. So that of the supposed susceptibles in the home group 10·2 per cent. have been attacked incidentally, and they are both proved to be susceptible and at the same time removed from the group, leaving it with a probably higher percentage of insusceptible persons included. Of the hospital group only 1·5 per cent. of the presumed susceptibles have been attacked by incidental infection, and therefore this group retains a much higher proportion of truly susceptible persons. The conditions, then, are that the hospital cases—which, on the average, have been more severe in type—return to meet, on the average, much greater numbers of supposed susceptible persons from which there has been practically no weeding out of the truly susceptible, and, in addition, they are forced into much more intimate contact with them, owing to the density of population in their houses.

It would then be extraordinary if "return" cases did not far outnumber "recovery" cases. The figures here are as follows: The 1,529 cases returning from hospital produce 98 return cases, or 6·4 per cent. on the cases discharged. This occurred amongst 2,692 supposed susceptible persons, hence 3·6 per cent. were infected. The home cases, 663 in number, on release from isolation produced 11 recovery cases, or 1·6 per cent. These occurred amongst 586 supposed susceptible persons, hence 1·8 per cent. were infected. Now if return cases occur simply because of the special chances which the hospital cases have of infecting, it should follow that the "return" case-rate should be high or low according to the degree of the chances conferred on the discharged patients. Five hundred and twenty-two cases in 490 houses with from 0·5 to less than 1 person per room returned to 538 susceptibles and produced 19, or 3·6 per cent., "return" cases; 591 cases in 509 houses with from 1 to less than 1·5 per room returned to 1,061 susceptible persons and produced 40, or 6·7 per cent., of "return" cases; 384 cases from 329 houses with more than 1·5 person per room returned to 1,091 susceptible persons and produced 39, or 10·1 per cent., of return cases. But in the same groups the percentage of all susceptible persons infected has remained constant. It seems certain that the home conditions have a preponderating influence on the occurrence of "return" cases. The figures for recovery cases are very small, but the same tendency is to be observed.

Lastly, there is the total effect to be considered. How far does the return case reduce the value of the saving in incidental cases. The total of incidental and return infection produced by the 1,529 hospital cases is 142, that is to say, that each hundred cases possibly infected 9·2 others. The number of supposed susceptible persons in these houses was 2,736, so that 5·1 per cent. of susceptibles were infected. The 663 home-treated cases possibly produced by incidental and recovery infections 78 cases, that is to say, each hundred cases was followed by 11·7 others. The susceptibles in the houses numbered 653, so that 11·9 per cent. were infected. So that the hospital cases, though in less favourable homes, infected less per case than the home-isolated patients and the proportion of susceptible persons in their homes infected was less than half. Picking out the most fairly comparable groups we find that in the hospital cases 522 came from 490 houses with from 0·5 to under 1 inhabitant per room, and that 546 susceptible persons were left, of whom 27 became infected. That is to say, that each hundred cases was possibly responsible for 5·1 others, and that 4·9 per cent. of susceptibles were infected. In the same house group of the home cases 406 patients were nursed at home in 379 houses in which there were only 299 susceptible persons. Nevertheless there were 28 subsequent infections. That is to say, each hundred of the primary invasions was possibly the cause of 6·8 infections, and in spite of the better conditions 9·3 per cent. of the susceptible persons were infected.

As the conditions for furthering infection increase in the two groups so the numbers naturally rise, but it may be noted that the rise in the home group is much steeper than in the hospital group, and it is evident that if home isolation had had to be adopted throughout, the number of cases of scarlet fever in the period investigated would have been very much greater. The figures, however, are not on a large enough scale to justify a detailed calculation as to the saving. Indeed, the cases kept at home in unfavourable conditions are so few that many years would have to be taken to obtain a group sufficiently large.

Dr. Malet gives in his annual report for 1907 some interesting figures:—

TOTAL FOR FOURTEEN YEARS, 1894-1907.

	Hospital	Home
Total houses	3,213	405
Cases recurred	324	135
Number of children after primary cases ...	8,472	777
Number of these attacked	405 or 4·8 per cent.	187 or 24·1 per cent.
Number possibly due to failure	201 or 2·4 ,,	124 or 16 ,,
Number of children escaping	8,067 or 95 ,,	590 or 75·9 ,,

This table, however, does not include return cases, which were estimated at 254. Dr. Malet has always taken a great interest in return cases, and it is instructive to compare his views in 1907 and in 1893. In 1907 he said: "Analogy suggests that in scarlet fever also, in some cases infection may be prolonged far beyond the complete recovery of the patient." Now for many years Dr. Malet has given special attention to this matter, and the change in his view is striking.

In 1893 he wrote: "Infection could only be taken home by a discharged case in three ways. First, in his own body, through his not yet being thoroughly free; second, in his clothing, through some defect in disinfection; third, by some germs settling upon him (as dust) after he had been cleansed for discharge. I am confident that in none of our cases was the first cause operative. The stay in the hospital was too long, and every case is carefully examined by myself before I discharge it."

There is no doubt that there has been a general trend of opinion towards the belief in the greater part played by personal infection.

Many good observers believe that it is possible to decrease the normal percentage of infective discharged patients, and hence the return case-rate, by special treatment in hospital. This may well be true. The advocates, however, vary considerably in their routine, and no one has yet persuaded his colleagues, in spite of their eagerness for any new thing which promises to prevent return cases, that his method is successful. However, allowing that certain lines of treatment in hospital may diminish the number of return cases, this is only equivalent to saying that the percentage of cases discharged whilst in a condition of protracted infection has been reduced. It does not in any way follow that the infectivity which has been got rid of was acquired through segregation in hospital. Any successful routine could presumably be equally well applied to the home cases with a consequent reduction in the recovery-rate.

Conclusions.

(1) In Manchester the great majority of the cases of scarlet fever nursed at home are in houses with less than one inhabitant per room. A considerable majority of the houses from which patients are removed to hospital have one or more inhabitants per room.

(2) The average number of susceptible persons per house is twice as high in the homes of the hospital cases as in the homes of the other group.

(3) The difference between the percentage of susceptible persons infected in the two groups during isolation (counting only cases occurring after the seventh day) is so great that it is evident there is a great leakage of infection occurring during home isolation.

HOSPITAL ISOLATION GROUP.

Density of home population	Number of primary invasion cases	Number of houses in which cases occurred	Number of susceptible persons left	"Incidental" infections	Return cases	Total number of susceptible persons infected	Percentage of "incidental" cases to susceptible persons	Percentage of return cases to patients isolated	Percentage of susceptible persons infected	Number of susceptible persons infected to each 100 primary cases
In this line the figures relate to homes with less than 0·5 inhabitant per room	32	31	2	0	0	0	—	—	—	—
0·5 to less than 1 inhabitant per room	522	490	546	8	19	27	1·4	3·6	5·1	5·1
1 to less than 1·5 inhabitants per room	591	509	1,076	15	40	55	1·3	6·7	5·1	9·1
1·5 to less than 2 inhabitants per room	313	268	882	13	32	45	1·4	10·2	5·1	14·3
2 or more inhabitants per room	71	61	230	8	7	15	—	—	—	—
All hospital cases	1,529	1,359	2,736	44	98	142	1·5	6·4	5·1	9·2

HOME ISOLATION GROUP.

Density of home population	Number of primary invasion cases	Number of houses in which cases occurred	Number of susceptible persons left	"Incidental" infections	Recovery cases	Total number of susceptible persons infected	Percentage of "incidental" cases to susceptible persons	Percentage of recovery cases to patients isolated	Percentage of susceptible persons infected	Number of susceptible persons infected to each 100 primary cases
Less than 0·5 inhabitant per room	75	74	14	4	0	4	—	—	—	—
0·5 to less than 1 inhabitant per room	406	379	299	20	8	28	6·6	1·9	9·3	6·8
1 to less than 1·5 inhabitants per room	147	136	268	36	3	39	13·4	2·0	14·5	26·5
1·5 to less than 2 inhabitants per room	33	26	66	7	0	7	—	—	—	—
2 or more inhabitants per room	2	2	6	0	0	0	—	—	—	—
All home cases	663	617	653	67	11	78	10·2	1·6	11·9	11·7

(4) When return cases and recovery cases are considered with the incidental cases there is, in spite of the worse home conditions, a big gain remaining in the hospital group, the percentage of susceptible persons infected being less than half that in the group of home cases, and the actual number of cases credited to each group of one hundred primary invasion cases is less than that in the home group in spite of the great difference in the chances for infection offered. If home and hospital isolation were adopted in a large series of similar households the gain demonstrated in hospital isolation would presumably be much greater.

(5) If it is allowed that a small percentage of all scarlet fever patients remain unrecognizably infectious for longer periods than the usual time of isolation, then the conditions to which the hospital patients return are, in comparison with the home treated cases, so favourable for the production of further infections, that it is probably unnecessary to seek any other explanation of the known great excess in the percentage of return cases over that of recovery cases.

Appended is a table which gives all the figures on which this paper is based. It should be kept in mind that the table refers to a certain period of fifteen months, and the percentages would probably be found to vary considerably from year to year.

DISCUSSION.

The PRESIDENT (Dr. Theodore Thomson, C.M.G.) thanked Dr. Arnold most heartily, in the name of the Section, for his admirable paper. It was a model of lucidity and brevity, and of close and consecutive reasoning. The subject was very interesting to all who had had to discharge the duties of Medical Officer of Health, and it also interested those who were Medical Superintendents of Infectious Diseases Hospitals. Much trouble had been caused by complaints of householders when the disease broke out after the return of patients from the hospital. Dr. Arnold had carried the war into the enemy's camp, and instead of attacking the hospitals he had attacked the homes. The case had never before been brought home to him so well as by this paper. The figures were presented in a very impressive way, and the contribution showed that hitherto sufficient attention had not been paid to that side of the question.

Dr. TURNER thanked the author for his valuable paper, and for the thoroughness with which the facts recorded were worked out. He gave the following figures, the result of a year's records at his own hospital. In 269

cases more than one patient had been admitted from one house, and the following shows the interval in weeks from the admission of the first to the second case :—

INTERVAL IN 269 SECONDARY CASES.

Weeks	No.	Weeks	No.	Weeks	No.
1	75	11	16	21	—
2	29	12	11	22	—
3	12	13	8	23	—
4	13	14	6	24	1
5	11	15	3	25	3
6	5	16	7	26	2
7	9	17	8	27	—
8	15	18	4	28	—
9	16	19	4	29	—
10	15	20	2	30	—

The first 75 cases would, in Dr. Arnold's scheme, be grouped with the primary cases. Of the remainder, 32 were " incidental " and 162 " return " cases.

Dr. E. W. GOODALL said that those who were responsible for the management of fever hospitals must have listened to Dr. Arnold's paper with very great satisfaction, if they adopted without hesitation the conclusions at which he had arrived. But the author had guarded himself by pointing out that the number of cases with which he had dealt was rather small, compared with the large amount of scarlet fever prevalent throughout the country. Doubtless the author would agree that still further investigation was needed to put the subject on a secure basis. He was glad to see that, incidentally, the author negatived the idea that patients suffering from scarlet fever became more infectious, and that the virus was increased in potency by the sojourn of the patient in hospital. It was an idea he (the speaker) had never believed in. In 2,000 cases at the Eastern Hospital which he had investigated, the results went to show that there was no valid evidence either in favour of the view that the patients became more virulent while in hospital, or that the different complications which were supposed to be a source of infection were handed on from one patient to another. He did not know whether Dr. Arnold would go so far as to say that infected articles were no longer to be taken into account. He (the speaker) would not go so far as that, because he regarded the scarlet fever organism as one which it was difficult to kill, and which was capable of adhering to all sorts of articles for a long period. Dr. Arnold had said nothing about those instances, well known in fever hospitals, in which patients had been ordered to be discharged, but at the last minute had been detained. Yet in two or three days another patient was admitted from the same house suffering from the disease. Such cases supported the view that infection might hang about articles as well as about the patient himself. But he agreed with the main thesis, that it was the patient himself who had to be looked to, and that the infection lingered about him more than was generally thought. He was glad the author had emphasized the time limit. In the Asylums Board hospitals they had always taken three months. But there should be no limit at all if

the facts were wanted. By limiting the time to two or three weeks one would fail to detect the chronic cases of infection. In a discussion on the paper read by Dr. Butler three years ago he (Dr. Goodall) had brought forward an instance of a boy who was chronically infected for the best part of two years, and was intermittently infectious, just as one found typhoid fever carriers were. Papers like Dr. Arnold's and Dr. Butler's showed that it was not so much the infected house which was the factor as the person. And Dr. Arnold's paper was particularly interesting because he had investigated the conditions under which the discharged patients lived. He was disappointed that Dr. Turner had not said more on that topic, because he had paid special attention to the question in regard to the report which Dr. Cameron made to the Metropolitan Asylums Board some years ago. In Dr. Cameron's report it was distinctly shown that the incidence of return cases was larger in some of the London boroughs than in others. In the observations which the Medical Superintendents made to the Board on Dr. Cameron's report, that question was still further gone into, and Dr. Turner worked it out with great labour, and showed that so far as the figures went, the borough was a greater evil than the hospital in respect of return cases. A question which Dr. Arnold touched very slightly on was the condition of the patient; and if, as was hoped, he intended to continue the inquiry, it would be a great help if he would investigate the condition of the discharged patient from a clinical point of view. There was an impression that the patients who had suffered from certain complications, by no means severe, such as chronic discharges from the nose and morbid conditions of the throat, were more likely to cause return cases than others. Since the publication of Dr. Cameron's report and the criticisms of the Medical Superintendents, Dr. Turner, after further inquiry, had concluded that too much stress had been laid on the importance of these morbid conditions. So that one could not say, with regard to the condition of the patient, whether he would be chronically infectious or not. Dr. Arnold's paper raised the whole question of the continued prevalence of scarlet fever in this country. For many years past attacks had been made on the fever hospitals. It had even been said that, so far from lessening the prevalence of scarlet fever, they tended to perpetuate it. Some of the papers on the subject had been mere statements unsupported by evidence, but Dr. Millard, the Medical Officer of Health of Leicester, had endeavoured to prove by statistics that the isolation hospital had had very little effect on the prevalence of scarlet fever. Yet if one accepted the view that the patient was the source of infection, there should by now have been some evidence that the hospitals had had some influence on the occurrence of the disease during the last twenty years. Up to a few years ago, however, there was very little evidence to that effect. Whether the difference in the case—mortality of the disease—was due to the hospitals was another question, which it required a larger body of evidence to settle than Dr. Arnold had at his disposal. But he thought that those who were responsible for the administration of fever hospitals, whether superintendents or medical officers of health, were entitled, after reading a paper such as this, to congratulate themselves that, after all,

the hospitals had not been such harmful places as some people had tried to make out, but that they had been of distinct advantage to the community.

Dr. BUTLER expressed his thanks to Dr. Arnold for the paper, which treated a very interesting problem in a definite way. He believed that the method of classification according to the density of the population in the homes was new, and it seemed to have definite relationship to the number of return cases. Holding the views which he did, that return cases were not due to hospital isolation, he found cause for gratification in such a paper as this. But one had very indefinite data in return cases. He supposed it was customary to accept occurrence of a case after return as evidence of an infecting case having been discharged from the home or from the hospital. One was apt to slide over a difficult administrative problem in neglecting all the other return cases which were not so classified but which might equally come from that infecting case. It would be much better if one could use as a datum not the return cases themselves, but the return infecting case. It was curious that attention should be almost limited to a single return case. If the case from hospital was in an infectious condition, it was scarcely conceivable that it should cause only one case. And as that number would be variable according to the home or other conditions, any figures based upon it would be misleading. If infected cases were taken as a basis of classification, more definite results would be obtained. For several years he had given both the return infecting cases and the return cases to which they gave rise, so far as they had been traced. But he was now convinced that, if the machinery were sufficiently perfected, they would find return cases bore a greater ratio than was supposed to return infecting cases. During the last few years he had noticed that the number of return infecting cases, expressed as a percentage of total cases discharged, increased as the incidence-rate for scarlet fever increased in a district, and declined with it. He would read the figures for 1905 to 1911, the percentage of return infecting cases and incidence-rate per thousand population. The percentage of return infecting cases to total cases was as follows:—

1905	...	...	3·5	1909	...	...	8·7
1906	...	...	3·6	1910	...	...	4·7
1907	...	...	4·2	1911	...	...	3·11
1908	...	...	6·9				

During the same years the incidence-rate was as follows:—

1905	...	...	2·8	1909	...	...	4·4
1906	...	...	4·5	1910	...	...	2·0
1907	...	...	4·4	1911	...	...	2·1
1908	...	...	5·0				

It was curious that the return cases should follow the same law as the incidence of the disease. If the return case was due to the home conditions or to the condition of the patient, it need not bear the same ratio to the total cases as those cases did to the incidence on the population. If it were so, it suggested that what had to be recognized was that the return case was not a result

but probably a cause; that the conditions which made for epidemicity of the disease made also for increased infectivity of cases which gave rise to returns. And in that might lie the explanation of the perpetuation of scarlet fever and other diseases of like character. The evidence that persistence of infection was due to the hospital received little confirmation as experience was increased, but he had been struck with the fact that the migration of the return infecting case was a very important matter. He was not sure that that was not an explanation of the difference between the recovery cases and the return cases, because even the figures which Dr. Arnold had produced showed an increase of return cases for the same class over the recovery cases. There were fewer recovery cases for a population of a given density than return cases for the same population, notwithstanding the fact that the recovery cases occurred among a better class of the population as a whole. The cases did not migrate after their recovery, but maintained the same conditions. Hospital cases came from a different environment, and both recovery cases and hospital return cases seemed to occur with much greater frequency in patients who moved about. Patients discharged from the Metropolitan Asylums Board hospitals caused a much larger proportion of return cases in his own district than did patients discharged from his own hospital. If he could follow his own cases he would probably find they would give rise in other districts to a greater proportion of return cases. Cases which had been sent to the seaside during convalesence, even when they had been isolated at home, gave rise, when they went back, to a greater number of cases than others. That was only an impression, and one which it was difficult to establish from figures.

Dr. CROOKSHANK said his pleasure at hearing the paper was enhanced by the fact that the conclusions to which the author had come were welcome to him, as he had always advocated the hospital method of treating scarlet fever, as opposed to the home method. But he had some misgivings as to the validity of the application of the statistical method to this problem, because the factors concerned were so variable that it was difficult so to reduce them as to make a just comparison. For that reason, when dealing with the matter in the past he never had recourse to statistical methods, but had fallen back on the so-called unscientific process of reasoning from general principles and observations. One factor favourable to hospital methods was brought out by the author, but he (the speaker) feared lest some counter-argument might be brought forward by statisticians on the other side. In attempting to estimate the value of the two methods attention should not be concentrated in looking for results on the "notified cases"; and he was in considerable agreement with what Dr. Butler had said. Notified cases were what the public called "concrete facts," and it appealed very much to the public if one was able to say that there were so many "notified cases" following one sequence of events, and so many after another. But the "spread of infection" did *not* equal the occurrence of "notified cases." Though in opening the author had said: "In this paper I propose to show the

effect upon the *spread of infection* of scarlet fever, of isolation in hospital, as compared with isolation at home"—what he had shown was the effect upon "notified cases" of scarlet fever treated in one set of circumstances as compared with scarlet fever isolated at home. He submitted, again, that the "spread of infection" differed from the "occurrence of notified cases." He was not referring for the moment to the mild cases of scarlet fever, which sometimes escaped recognition, or even to "simple" carriers, though the production of carrier cases—if carrier cases could be produced by cases returning from hospital—was not easily revealed by "notifications," even after a considerable time. He agreed with Dr. Goodall as to the inadvisability of fixing a time limit, because he thought a case might come from hospital and set up cases which were carriers and produced scarlet fever after a considerable time, which would not be caught in the meshes of Dr. Arnold's statistics, but would yet be due to the cases returning from hospital.

He had been reading a paper by Calmette—a paper which had not yet appeared in the English journals, but would shortly so appear—in which that author made some very interesting suggestions, leading to the notion that there might be a considerable diminution of cases of fatal phthisis in a community, due to the fact that the mass of the people were becoming more "tuberculized," and thus "immune." Perhaps that had been the case in this country during the last thirty years. He (Dr. Crookshank) did not suggest that everything said about chronic infections like tuberculosis applied to scarlet fever, but this illustrated a point which should not be forgotten in considering the effects of hospital as opposed to home treatment. Following up Calmette's ideas on immunity, the phenomenon of the home secondary cases occurring within a day or two of infection of the first case depended on the associates of the first case getting exposed to a massive and overwhelming infection, to which they fell victims. But, supposing these associated children manifested some resistance at first, or supposing the infection not to be too massive, and the child suffering from scarlet fever still kept at home, the associates in the house probably got repeated moderate infections, immunizing in effect. And that was why scarlet fever patients did not turn up at home during the fourth to the sixth week that the child first ill was kept at home. But if the scarlet fever patient were removed at an early stage, before he had given any massive infection to his brothers and sisters, and if he came out of hospital at the sixth or seventh week still in an infective condition, he probably brought back a massive infection to what was practically virgin soil, because the children at home had not been exposed to repeated small infections. Then one got the return case. And in that lay one explanation of a fact which Dr. Butler brought out, that one got return cases particularly when the case discharged from hospital went straight away to the seaside, because it went among a family which had not had immunizing doses of scarlet fever for the previous six weeks. The same explanation had some reference to what he had also noticed—that when better-class patients were admitted into hospital more return cases occurred amongst the families than in poorer cases, although

in this instance there might be, according to Dr. Arnold's showing, fewer susceptible persons. The author used the term "susceptibility" in a sense different from that in which he would use it himself. He would use it as having reference to the fact that a person had not been exposed to repeated immunizing infections. If a child were kept at home and could immunize its family, although the family might not furnish a definite number of notified cases, still the immunized family were probably carriers, in the French meaning of the term. And the question was whether more harm was done by a child remaining at home and immunizing its family and converting the members into potential carriers who spread the disease later on; or by allowing the child to go to hospital, even though it might come home in an infectious state and set up a frank case of scarlet fever, which could be removed. He thought the greater harm was done by the child remaining at home.

There were three ways of dealing with infectious diseases: The old individualist way, allowing considerations of public health to take care of themselves. That was obsolete. The second was to immunize the community, as had been done for many years in the case of small-pox, and, unconsciously, in the case of tubercle for the last thirty years. If scarlet fever cases were kept at home, it was also being done, in a half-hearted way, in connexion with that disease. Measles was nowadays treated at home, but with more care than formerly, and so we were ceasing to immunize the community in the way we used to. The other method was to try to destroy the sum-total of the virus, and that was what had been attempted in the case of malaria. It had been done also perhaps in the case of typhus by doing away with bed-bugs. And by removing every case of scarlet fever to hospital and treating it on disinfecting lines, one was doing something to reduce the total amount of the virus, but not causing immunization. The two latter methods were not comparable, and the truth would not be arrived at by estimating results from statistical investigations. It used to be thought that the "immune person," he who did not get a disease twice, was a cured patient. But if one fell back on analogy with another chronic disease, that was not so. When a patient suffering from syphilis was cured of his syphilis by salvarsan, and a negative Wassermann reaction procured, that patient ceased to be immune. In France people had been found to have returned to the hospital two or three times a year with the primary disease after having been "cured" of it. They were "immune" only so long as they were carrying about in their bodies a weak focus of the disease. The same was true of tubercle. People in adult life, who were called "immune," were those infected in early life, who had some tuberculous glands hidden away which were keeping up their "immunization." If those people were "cured" of their tubercle they ceased to be "immune," and were likely to fall victims again. He suggested that all these new facts and conceptions must be reckoned with, and that they tended to increase the strength of the general arguments used in favour of the hospital isolation of scarlet fever as opposed to home isolation, although they showed how difficult it was to estimate results by statistical methods.

Dr. G. W. JOHNSTONE said that Dr. Crookshank had drawn a somewhat alarming picture, which if it were true would indicate the necessity of all of us speedily becoming infected in order to render ourselves immune from all sorts of infections. It would seem, moreover, that the power of resistance to disease possessed by the ordinary healthy human being to which we have hitherto trusted was visionary. This, however, seemed to be carrying the theory too far.

Mr. M. GREENWOOD, jun., remarked that the author had advanced reasons for believing that home conditions greatly influenced the frequency of "return" cases. Were this not so, it might be anticipated that the ratio of such cases to the number of susceptible persons at risk would increase steadily as the ratio of discharged hospital patients to susceptibles increased. In the three groups formed, the ratios of hospital discharges to susceptibles were $\frac{522}{589}$, $\frac{591}{1061}$, $\frac{384}{1091}$, but the incidence of "returns" upon susceptibles was sensibly constant. This seemed to indicate the very great importance of home conditions, as urged by the author. Further work upon this point seemed desirable, owing to the difficulty of weighing the chances of infection in different groups. The only other point which appealed to him, as an outsider, was that the method which had been adopted by Dr. Arnold was the only one applicable to a subject of this kind. It was easy to propound any number of theories as to why certain facts were as stated, but the first duty was to ascertain whether the facts *were* as stated. From the statistical point of view he did not think it could be questioned that Dr. Arnold had adduced good reasons for believing that the result of the hospital method was the production of a smaller total number of cases than the other method. As to the ultimate reason of that difference, it might be a matter of speculation. Such speculations might be interesting, but in the absence of scientific evidence were not of very special importance to those who had to carry out the administration.

Dr. HAMER asked whether Dr. Arnold had considered the question of distribution in time of "return cases" to which Dr. Turner alluded, and more particularly the increase in the number of cases notified from homes, to which hospital patients returned, occurring after the lapse of some seven or eight weeks. Dr. Turner gave the figures for successive weeks as follows: 29, 12, 13, 11, 5, 9, and so on; succeeding that came a rise in the numbers up to 15, 16, 15. The same phenomenon was very well brought out in Dr. Seaton's figures, namely, the rise at the eighth, ninth, and tenth weeks. That increase had been held to furnish conclusive proof of the operation of a special infectivity manifested by returned hospital cases, and he confessed he had always felt disposed to admit the validity of that contention. But of late he had been much struck with the enormous part which suggestion seemed to play in this connexion. Dr. Crookshank had alluded to that, and had pointed out that they were dealing with notified cases, not with infections. He (Dr. Hamer) wondered whether they might not here find explanation for the additional number of cases from the eighth to the tenth week. They were dealing, of course, with notified cases,

and they found that the number of cases notified in those weeks was larger than in preceding and following weeks. Might not the explanation be that the fact that the child had returned from hospital led to notification being made, while it would not have been made if the child had not just returned from hospital? Thinking on these lines one was rather confirmed in the view suggested by the smallness of the rise which occurred in the eighth, ninth, and tenth weeks. If the cases sent out from hospitals were especially infective, surely the rise in the numbers might have been expected to be much more marked than it was actually found to be. Following the matter further, one was carried along to Dr. Butler's point, that when a case migrated infection occurred. If the child went, say, to Brighton—i.e., to a new environment—it was in people's minds that there ought to be a return case. So too if a Metropolitan Asylums Board patient were sent back into Willesden, it might be that there was a more critical study made of circumstances relating to the case than would have been undertaken had the patient merely been returned home out of the Willesden hospital.

Dr. WILLIAMSON said, in regard to the difference between the results of the systems of hospitals and home isolation, that in some figures which he got out and contributed to Dr. Seaton's book, he showed that two-thirds of all the secondary cases from either home isolation or hospital isolation occurred before the isolation of the first case at either home or hospital. He did not know whether in Dr. Arnold's tables the figures relating to secondary cases excluded those early cases. In his own figures two-thirds of the secondary cases could not be prevented. No one knew of the first case until the secondary cases had occurred. So that in comparing the percentage of cases, even if the number of later hospital-isolated cases was small, those earlier two-thirds must be added to the smaller figures in the one case, and the larger figures in the other, and the total difference was not so marked as it would otherwise appear.

The PRESIDENT said he thought the satisfactory solution of this question could only be attained by study in the direction in which it had been carried out by Dr. Arnold. A lively time had been experienced in regard to attacks on fever hospitals, and the manner in which figures on the subject had been collected and presented did not always carry conviction, because of the many obvious fallacies involved. For instance, comparison had been made between districts with hospital isolation and districts without hospital isolation. To base argument on that was not the way to get useful results. The home conditions in these districts might vary widely. The possibility of children spreading infection would vary widely with the condition of the home. And it was also necessary to consider the hospital conditions, because not all hospitals were equally well administered. Moreover, the degree of infectivity of scarlet fever outbreaks in different districts was little likely to be the same. He thought that those who had claimed that there was no benefit to be derived from hospital isolation were foolish. The merit of Dr. Arnold's paper—and it was not the

first time he had expressed his appreciation of the manner in which Dr. Arnold did his work—was that he compared conditions which were properly comparable. In the main the principle was sound, and it was along that path that a satisfactory conclusion was likely to be reached. There was nothing he himself enjoyed more than a theory based upon general impressions. The fewer the data the easier it was to theorize; but the administrator wanted hard facts of which he could be sure, and comparison of data which were justly comparable.

Dr. ARNOLD, in reply, expressed his thanks to Members for the way in which they had discussed his paper. In answer to Dr. Williamson, all the cases which occurred in the houses were included in one or other of the columns, but the aim was to avoid any case being put down as incidental which might have been infected by a previous case directly before it was isolated in the house. Taking the hospital group, there was a big difference between the number of primary invasion cases and the number of houses in which they occurred. Cases occurring in the first week, probably from direct infection, did not get into the incidental cases, nor did cases occurring later if there had been an intermediate case. Column 1, with column 6, would give the total number of cases. The total primaries were 1,529, 44, and 98. Dr. Turner's original work was most interesting, and it was looking at his demonstration of return cases and the rate in different boroughs which made him (the speaker) think it was worth while going into detail concerning the home conditions. In reference to Dr. Goodall's remarks, complications at times seemed to be secondary elements which might be infective; but he did not think hospitals had any other influence upon cases. He avoided making any direct statement in regard to infected articles, but he believed that from time to time infection might be carried in that way. The clinical condition could not be investigated in this series, because the inquiry was conducted two years afterwards. He had taken the Health Office sheets, and nothing was known as to the exact condition of the patient on discharge, if no return case occurred. Dr. Butler's remarks were most instructive. One seemed to have more trouble when things began to be busy with return cases than later on; when the incidence-rate dropped, the return case-rate dropped at the same time. Dr. Crookshank's remarks were of wide interest. With regard to the notified cases, it was a suggestion of caution. In reference to Dr. Hamer's remarks as to the returns at the ninth to eleventh weeks, his own cases were too few to enable conclusions to be drawn. He had the figures, but he did not think it worth while to include them.