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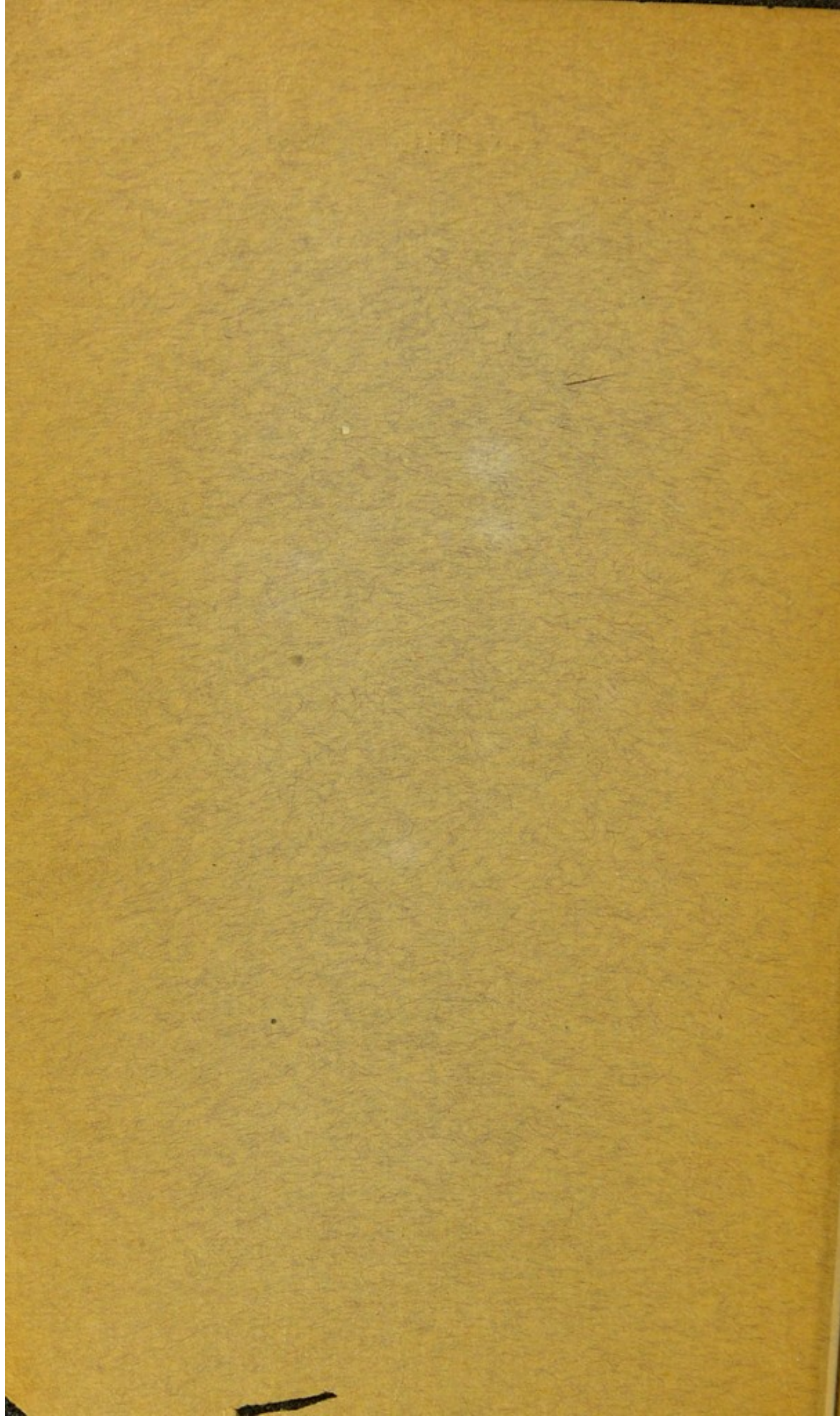
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SMALL-POX HOSPITALS

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IN 1894 the subject of the spread of small-pox occasioned by Small-pox Hospitals, in its general aspects, was brought before this Society in the admirable paper by Dr. J. C. McVail, which appears in the Thirteenth Volume of our *Transactions*. For more than six years afterwards there was practically no opportunity of adding to our knowledge of the matter, as during this time the country was almost free from small-pox. Now, after the usual fashion, a period of small-pox prevalence has succeeded; and at one time or another during the last four years—1900-01, 1901-02, 1902-03, and 1903-04—epidemics of considerable magnitude have occurred in London and in various parts of the kingdom.

The object of this paper is to inquire whether the ability of small-pox hospitals to cause the spread of small-pox in their vicinity has been again demonstrated during this epidemic period, and to consider whether the later facts which have been ascertained on this subject support or are opposed to the explanation of this circumstance by the assumption that particulate infectious matter from a hospital containing acute cases of small-pox is on occasion conveyed through the atmosphere in such a way as to infect susceptible people at considerable distances from the hospital.

During the four years in question, the number of hospitals available for the isolation of small-pox has increased all over the country. It may be said, indeed, with an approach to truth, that during this period every case of small-pox in the kingdom which has been detected has been isolated in a hospital of some kind. The exceptions to this statement, though themselves numerous, are re-

latively insignificant. At first sight, therefore, we should expect a superabundance of material to be available for our proposed inquiry. But it is, of course, otherwise. The administrative lessons of former epidemics have nearly always been taken into account by the medical advisers of sanitary authorities, in determining whether the use of particular hospitals for acute small-pox cases should be permitted, or in selecting the site on which a new small-pox hospital is to be erected or on which temporary accommodation for small-pox is to be improvised. For example, the use for acute cases of small-pox of a hospital which has a large population living within half a mile of it has been altogether exceptional. In the majority of instances it is useless to inquire whether a given hospital has spread small-pox, after the fashion of town hospitals in former years, among those who dwell in its neighbourhood or otherwise are habitually brought into its vicinity. The number of such persons is too small; the most minute inquiry as to the incidence of small-pox amongst them could not, in the nature of things, bring out data sufficient to enable inferences to be drawn with any degree of certainty.

We must turn, therefore, to special cases. I propose to refer to the experiences of London, Glasgow, Liverpool, Manchester, and Gateshead. Each of these five great towns has suffered from epidemic small-pox in the course of the last four years; and for each the incidence of small-pox on locality in relation to the small-pox hospital or hospitals at which the patients were isolated has been carefully studied. Fortunately for the brevity of this paper, the results in each instance are published and recorded in considerable detail, so that I need only refer to the principal facts of each.*

* *London: Hospital Ships in Long Reach.*—Dr. J. C. Thresh, Trans. Epidem. Soc., 1901-2. Dr. G. S. Buchanan, Report to Local Government Board on Small-Pox in the Union of Orsett, 1901-2.

Glasgow: Belvedere Hospital.—Dr. A. K. Chalmers, Report to Corporation of Glasgow on Small-Pox, 1900-1902.

Liverpool: Three Hospitals.—Dr. R. J. Reece, Report to Local Government Board on Small-Pox and Small-Pox Hospitals in Liverpool, 1902-1903.

Manchester: Clayton Vale Hospital.—Dr. J. Niven, Annual Reports of Medical Officer of Health for 1902 and 1903.

Gateshead: Sheriff Hill Hospital.—Dr. G. S. Buchanan, Report to L. G. B. on Small-Pox in Gateshead and Felling, 1903-1904, in Relation to Sheriff Hill Hospital.

N.B.—Each of these Reports contains detailed maps of the districts in question.

LONDON.

Metropolitan Asylums Board Hospital Ships in Long Reach.

It is natural to begin with London, where formerly small-pox hospitals played so prominent a part in determining the local distribution of small-pox. A brief reference to this earlier experience is desirable, in view of what follows. It will be remembered that the Metropolitan Asylums Board began to isolate small-pox during the great pandemic of 1871, and that until 1886 the hospitals used for the purpose were situated within or on the outskirts of the Metropolis, being in each instance surrounded by, or at the edge of, thickly-populated areas : *e.g.*, the hospital at Fulham (the present "Western"); at Stockwell, ("South-Western"); at Deptford, ("South-Eastern"); and at Hampstead, ("North-Western"). It was in respect of Fulham Hospital, during the epidemic of 1881, that Mr. W. H. Power demonstrated the influence exercised by the hospital on the prevalence of small-pox in its neighbourhood. Mr. Power showed that, following on the use of this hospital for small-pox, a graduated intensity of small-pox incidence upon the surrounding houses was observed, the percentage of houses invaded by small-pox in the neighbourhood of the hospital becoming gradually smaller as the distance of these houses from the hospital increased. The influence of the hospital in this way was most marked within half a mile. It was manifested, however, in the zone lying between half and one mile from the hospital. This graduated incidence from centre to periphery was not to be explained by the hospital operations, and it did not follow lines of communication and traffic to and from the hospital.

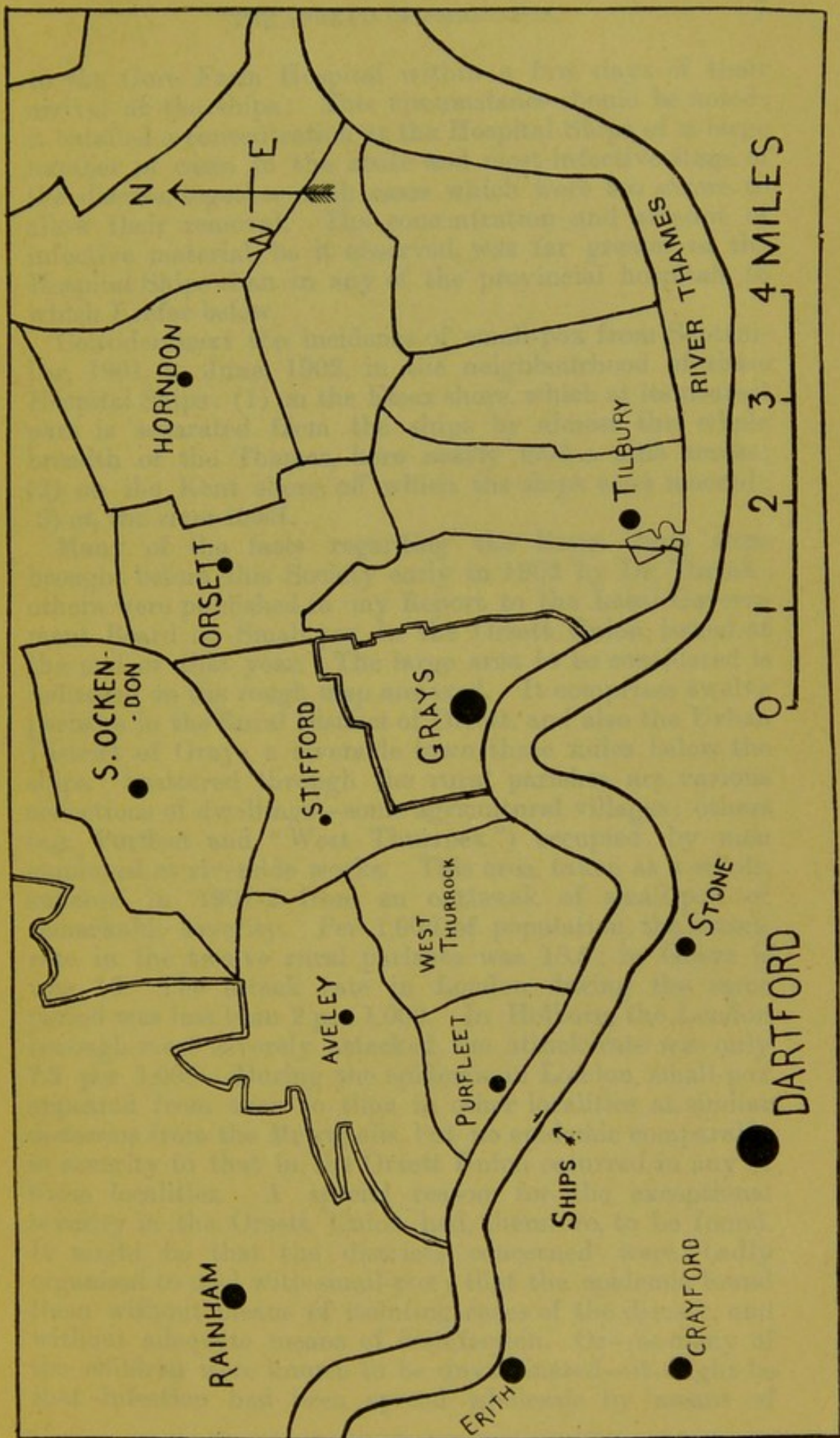
The striking facts obtained with regard to the Fulham Hospital in 1881, which formed the basis of the theory of aerial convection of small-pox formulated by Mr. Power, inevitably led to inquiry whether other London small-pox hospitals had caused spread of the disease in their neighbourhood; and whether, or to what extent, such spread could be prevented by stringent administrative regulations and precautions. Answer to the first question was furnished by Mr. Power's "Statistics of Small-pox incidence in the Registration Districts of London, relatively to the operation of Small-pox Hospitals in the Metropolis," published in the Report of the Medical Officer of the Local Government Board for 1886—a series of chapters dealing with local incidence in the Metropolis for 1876-1885. This

elaborate investigation made it clear that, for whatever reason, the local distribution and intensity of small-pox in the Metropolis during the epidemics included in the period under review, had been determined to a very marked extent by the operations of the London hospitals receiving acute small-pox cases. In particular it brought out instances in which the opening for small-pox of a hospital in a part of London hitherto comparatively free from the disease, (*e.g.*, Hampstead, in 1884), had been speedily followed by a severe local outbreak; and in general, the facts afford strong evidence that the persistence of small-pox in London, and the excessive prevalence of the disease in the Metropolis relatively to that in the provinces during this period, were to a large extent attributable to the use for small-pox of the London hospitals of the Metropolitan Asylums Board.

Opportunity of investigating the second question meanwhile had come during the epidemic of 1884-1885. The incidence of small-pox in the neighbourhood of Fulham Hospital in that year was again studied with great care and in great detail by Mr. Power. Notwithstanding a great variety of special and stringent precautions adopted at this hospital during this period, with a view to prevent opportunities of infection arising through hospital communications and traffic, the experience of 1881 was repeated. Again, there was exceptional incidence of the disease on dwellings near to the hospital, manifested within a few weeks of the admission of acute cases thereto, and again the characteristic graduation was observed.

Repeated demonstrations, therefore, of the mischief occasioned by treating acute small-pox cases in London hospitals left the Metropolitan Asylums Board no option. Henceforth they took all cases of this disease to hospitals outside the Metropolis altogether—namely, to the Hospital Ships in Long Reach, in connection with which was the Gore Farm “convalescent” small-pox hospital at Darenth.

I come now to the experience of London during the epidemic of 1901-2. This epidemic began in August, 1901. During the first fortnight in September the number of cases brought to the Hospital Ships was 75; in succeeding fortnights the corresponding numbers were 74, 71, 89, 211, and 240. Between November, 1901, and June, 1902, the average daily number of patients under treatment at the ships was 164, the maximum being 271 and the minimum 97. The reason that these figures were not even larger was that the bulk of the milder cases were sent on



to the Gore Farm Hospital within a few days of their arrival at the ships. This circumstance should be noted: it entailed a concentration at the Hospital Ships of a large number of cases in the acute and most infective stage of the disease, together with cases which were too severe to allow their removal. The concentration and amount of infective material, be it observed, was far greater in the Hospital Ships than in any of the provincial hospitals to which I refer below.

Consider next the incidence of small-pox from September, 1901, to June, 1902, in the neighbourhood of these Hospital Ships: (1) on the Essex shore, which at its nearest part is separated from the ships by almost the whole breadth of the Thames, here nearly half a mile across; (2) on the Kent shore, off which the ships were moored; (3) on the river itself.

Many of the facts regarding the Essex shore were brought before this Society early in 1902 by Dr. Thresh; others were published in my Report to the Local Government Board on Small-pox in the Orsett Union, issued at the end of that year. The large area to be considered is indicated on the rough map annexed. It comprises twelve parishes in the Rural District of Orsett, and also the Urban District of Grays, a riverside town three miles below the ships. Scattered through the rural parishes are various collections of dwellings—some agricultural villages; others (*e.g.*, Purfleet and "West Thurrock") occupied by men employed at riverside works. This area, taken as a whole, suffered in 1901-2 from an outbreak of small-pox of remarkable severity. Per 1,000 of population, the attack rate in the twelve rural parishes was 18.5; in Grays it was 16. The attack rate in London during the same period was less than 2 per 1,000. In Holborn, the London borough most severely attacked, the attack rate was only 7.2 per 1,000. During the epidemic in London, small-pox appeared from time to time in other localities at similar distances from the Metropolis, but no epidemic comparable in severity to that in the Orsett Union occurred in any of these localities. A special reason for the exceptional severity in the Orsett Union had, therefore, to be found. It might be that the districts concerned were badly organised to deal with small-pox: that the epidemic found them without means of isolating cases of the disease, and without adequate means of disinfection. Or—as many of the children were known to be unvaccinated—it might be that infection had been spread wholesale by means of

schools. A large section of the population might have been refusing vaccination and re-vaccination, and the magnitude of the epidemic might, consequently have been due to excessive small-pox among "contacts." Or, again, the organisation of the sanitary departments concerned might have broken down, with the result that cases were not promptly detected and dealt with. Answers in considerable detail to these and other conjectures which naturally were forced upon anyone undertaking a systematic inquiry into the outbreak, will be found in my Report. From the first, there was no failure in provision of hospital isolation for small-pox cases. Throughout the epidemic, notified cases of small-pox were promptly removed to hospital. Disinfection and other preventive measures adopted by the sanitary authorities concerned, were, upon the whole, efficiently carried out. Each district had the services of an excellent and hard-working sanitary staff. Excessive prevalence of small-pox among "contacts" was not a conspicuous feature of the epidemic, and in at least four-fifths of the dwellings invaded by small-pox, no case of the disease occurred among the "contacts." The occurrence of small-pox among unvaccinated children had been frequent, and was responsible in material degree for the magnitude of the epidemic, but there had been no instance of wholesale spread of small-pox among children, such as occurred in Gloucester in 1895. After the first few months of the epidemic, the susceptibility of the general population to small-pox had been very greatly decreased, in consequence of the enormous amount of vaccination and re-vaccination offered and accepted in the Union.

It was, however, impossible to neglect the obvious key furnished by study of the local incidence of the disease. The place at which the epidemic had its beginning (in September and November, 1901) was the small community of Purfleet. Not merely to begin with, but throughout the whole epidemic, small-pox prevailed in Purfleet to a much greater extent than in other portions of the Union. The places which, next after Purfleet, were most heavily attacked were the portions of the Union nearest to Purfleet. In parishes between two and five miles from Purfleet, including Rainham, which is outside the Orsett Union, the introduction and recurrence of the disease were in many instances found to be attributable to infection contracted at Purfleet. This was also the case at Aveley, Grays, West Thurrock and South Ockendon. In each of these parishes the first cases of small-pox appeared to have contracted

their infection at Purfleet. To a less degree, the history of Purfleet in regard to small-pox was repeated during the latter part of the epidemic in the larger community of "West Thurrock,"* which lies between Purfleet and Grays, from two to three miles in direct line from the Hospital Ships. The facts and figures as to local incidence are given in detail in my Report.

What, apart from their proximity to the Hospital Ships, were the peculiar circumstances in which this collection of six hundred residents at Purfleet, together with another hundred or more people working at Purfleet and returning every day to dwellings within a few miles of it, came to suffer so severely? Apply to Purfleet the conjectures with which we approached the whole Union. The matter was carefully investigated by Dr. Corbet, Dr. Thresh, and myself. The comparative smallness of the population concerned here made the answers easier. And they were definite. Defects in isolation, in disinfection, or in contact vaccination; school infection; re-vaccination refusals; multiple cases in houses—none of these familiar factors in small-pox epidemics, and no combination of them, could be accepted as furnishing anything like a sufficient explanation of the recurrence of the disease week after week and month after month in this community. The local medical practitioner, the medical officer of health, the sanitary inspector, the inhabitants themselves—all were on the lookout for the occurrence of the initial symptoms of small-pox—I would dwell particularly on the promptness with which cases of small-pox in Purfleet were detected as they occurred, and on the rarity of "missed" cases there. Most persons in Purfleet were vaccinated or re-vaccinated during the early part of the epidemic, and the number of residents susceptible to small-pox by March was apparently reduced to something under forty, a number of these being persons newly arrived in the place. Nevertheless, cases continued to occur after March among these few remaining susceptibles.

And all this, as Dr. Thresh showed, has happened before, though on a smaller scale. Both in 1884-5, and in 1892-5, years in which numbers of small-pox cases were under treatment at the Hospital Ships, Orsett Union suffered from small-pox. For 1892-5 facts were available, and are given by Dr. Thresh and in my Report, to show that in each year corresponding to the use of the ships for small-

* As in my Report to the L. G. B., I have subdivided the large parish of West Thurrock into (1) Purfleet, and (2) "West Thurrock."

pox, Purfleet and West Thurrock suffered out of all proportion to the rest of the Union.

As to the Kent shore: I will pass over facts which I have recorded as to the first occurrence of small-pox in 1901 at Erith, and in parishes of the Dartford Rural District: places two miles or more from the ships; and deal solely with the uninhabited flats within a mile of them. On these flats, within a quarter of a mile of the ships, a large number of workmen were employed during and after December, 1901, in erecting a temporary hospital on shore. Up to February 27th, 1902, no case of small-pox was received into any shore buildings. From time to time during these three months, small-pox occurred among the workmen—at least 60 such cases were detected. Personal communication between members of the staff and these workmen cannot, however, be excluded as one of the causes of the small-pox among them.

As regards vessels ascertained to have cast anchor in Long Reach near to the Hospital Ships, instances are given in my Report, in which members of the crew of such vessels were found to have sickened with small-pox about twelve days afterwards, when in the ports of Rochester, Faversham, and London.

Revert for a moment to the outbreak on the Essex shore in the Orsett Union. The exceptional character of that outbreak demanded explanation, and one which would account for the wholly exceptional and sustained incidence of the disease on Purfleet and on places associated with Purfleet—the most conspicuous feature in the Orsett epidemic. Purfleet village and the riverside works there form the only population within a mile of the Hospital Ships; nearly all this population was within three-quarters of a mile. During the Orsett outbreak the administration of these ships entailed the concentration, almost by the hundred, of acute and severe cases of small-pox therein. These ships had been put there because hospitals in London receiving acute small-pox cases had been shown to spread small-pox in their neighbourhood: their influence in this respect having been traced on dwellings a mile from the hospital. In previous epidemics the operations of the Hospital Ships had coincided with prevalence of small-pox in Purfleet; during the present epidemic, crews of vessels moored in the river near the ships had become infected, while small-pox became rife among workmen on the opposite shore. All this affords strong grounds for inference that the exceptional severity of the epidemic in

Purfleet, (and, to a large extent also, directly or indirectly, the magnitude of the outbreak in the whole of the Orsett Union), stood to the operations of the Hospital Ships in the relation of effect to cause.

As regards *the provinces*, I will not attempt any retrospect of the various instances in which hospital influence has been traced in the case of provincial hospitals during small-pox epidemics in years antecedent to the four which we are now considering. A useful list of certain of these instances is given in Dr. Reece's Liverpool Report, to which I propose to refer later on.

GLASGOW, 1900-1904.—*Belvedere Hospital.*

We are fortunate in possessing a most instructive report by Dr. A. K. Chalmers, Medical Officer of Health for Glasgow, which deals in a variety of aspects and in well-arranged detail with the epidemic of small-pox in that city from 1900 to 1902. In dealing with the relative intensity of small-pox incidence during this period on various divisions of the city, Dr. Chalmers takes for his basis of comparison the number of cases which occurred in each division relatively to its population: no doubt for the reason that the tenement and flat system in Glasgow would introduce too many uncertainties if he attempted to make invaded dwellings his basis of reckoning. Dr. Chalmers divides 1900-1902 into pre-epidemic, epidemic, and recrudescence periods of small-pox, and he has studied each period fortnight by fortnight. The figures as to number and rates of attack in the seven large Divisions* of Glasgow are as follows:—

Small-pox in Divisions of Glasgow, April, 1900, to May, 1902.

Division.	Population.	Cases Reported in—		
		Pre-Epidemic Period, April to December.	Epidemic Period, January to July.	Recrudescence Period, November to May.
Central	111,784	83	139	48
East	173,104	216	863	205
North	166,825	26	100	103
South	132,718	41	200	65
West	61,092	23	29	24
South Suburban	64,205	7	48	9
North-West	51,984	1	10	15
Whole City	761,712	397	1389	469

* The proper name for what I have described as "Divisions" is "Administrative Districts." "Division," however, may save risk of confusion between these large "Administrative Districts" and the smaller "Sanitary Districts."

Attack Rate per 100,000 of Population in

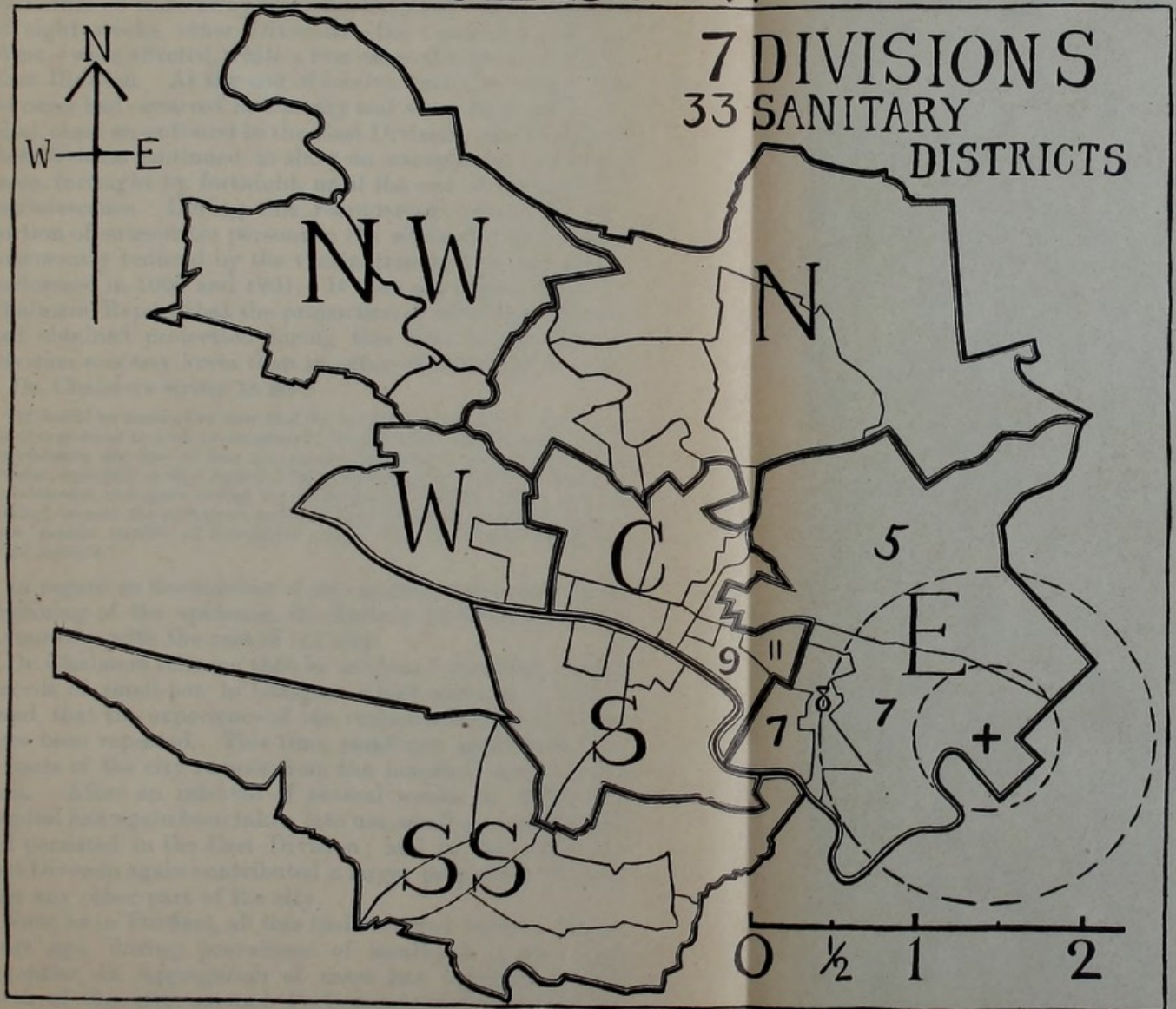
Division.	Pre-Epidemic Period.	Epidemic Period.	Recrudescent Period.
Central	74	124	43
East	124	498	118
North	15	60	62
South	30	151	49
West	37	47	39
South Suburban	11	76	14
North West	2	19	29
Whole City	52	184	61

Even when the figures are thus massed together the disproportionate incidence on the East Division of the City during all three periods becomes evident. Belvedere Hospital, the sole institution used for the isolation of Glasgow small-pox cases, stands within this East Division, and on the eastern edge of it. This is shown in the rough map annexed. So much of the area within a mile of this hospital as is within the City of Glasgow lies wholly in the East Division. It contains a large population, viz., nearly the whole of the "7th Sanitary District" (population 66,197), and portions of the "8th" and "5th" Sanitary Districts. The population lying within half a mile of the hospital has not been separately estimated, but it may be noted that the area within half a mile is relatively less densely populated than that between half and one mile.

The massed figures just given, however, do not suffice to indicate the nature of the exceptionally heavy incidence of small-pox on the East Division. To appreciate this it is necessary to consult Dr. Chalmers' Tables of fortnightly incidence during the two years in question. These Tables show, as regards the pre-epidemic and epidemic periods, that small-pox was set going at the end of April, 1900, in the Central Division. During the first eight weeks, in which a total of 72 cases occurred, the disease became distributed throughout six of the seven Divisions of the city, without any specially noteworthy incidence on the East Division. After June 16th, however, the number of cases from the East Division began to be disproportionately large. The excess in this Division was maintained in almost every fortnight throughout the pre-epidemic period. It was augmented during the epidemic period which followed, from January to June, 1901. And, practically speaking, it continued until the epidemic ceased. After July, 1901, small-

GLASGOW

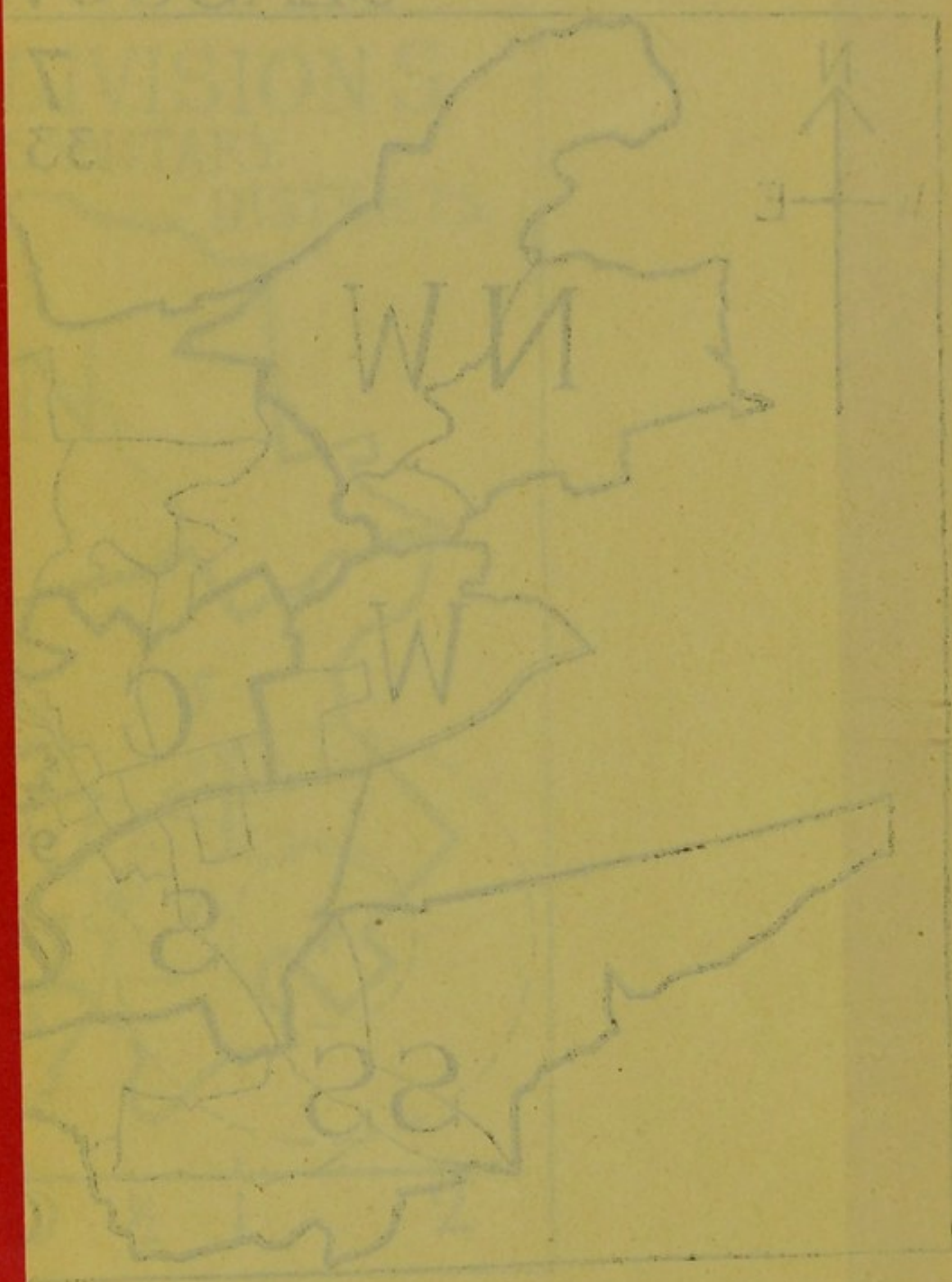
7 DIVISIONS
33 SANITARY
DISTRICTS



GLASGOW

SECTION 7

33



pox disappeared from Glasgow; the hospital emptied. In November there was a recrudescence of the disease. This recrudescence occurred in the North Division of the city, two miles or more away from the hospital. After a period of eight weeks, other Divisions—the Central, South, and West—were affected, while a few cases also occurred in the East Division. At the end of twelve weeks, by which time 84 cases had occurred in the city and some 50 were in hospital, came an outburst in the East Division; and thereafter this Division continued to show an exceptional number of cases, fortnight by fortnight, until the end of the period of recrudescence. During this recrudescence period the proportion of susceptible persons in the whole city had become enormously reduced by the vaccination and re-vaccination performed in 1900 and 1901. It does not appear from Dr. Chalmers' Report that the proportion of inhabitants which had obtained protection during this time in the Eastern Division was any lower than in other divisions of the city.

Dr. Chalmers writes to me:—

"It should be made quite clear that in the Eastern Division of the City it was always easier to push re-vaccination than in other districts where public apprehension was less, so that the greater incidence of cases in the Eastern Division, especially in later stages of the epidemic period as well as during the recrudescence, and again during the return last year (1903-4), is all the more striking, because the excessive number of cases occurred, in reality, among a much smaller number of susceptible people than the relative populations would indicate."

In regard to the number of its vaccinated children at the beginning of the epidemic, the Eastern Division compared favourably with the rest of the city.

Dr. Chalmers tells me that he is about to publish further records of small-pox in Glasgow, which show, as I understand, that the experience of the recrudescence of 1902 has since been repeated. This time, small-pox again broke out in parts of the city remote from the hospital—away to the west. After an interval of several weeks, in which the hospital had again been taken into use, small-pox reappeared and persisted in the East Division; and in the result the East Division again contributed a larger proportion of cases than any other part of the city.

Now, as in Purfleet, all this had occurred before. Many years ago, during prevalence of small-pox in the early 'seventies, an aggregation of cases had occurred in the north of the city, around Parliamentary Road Hospital, which was then used for the isolation of small-pox. But from 1892 to 1897, years in which small-pox was present in Glasgow, and in which small-pox cases were admitted to

the Belvedere Hospital on the east, the proportion of cases contributed by the Eastern Division has been uniformly in excess of the proportion of population residing there. This Dr. Chalmers has shown by the following Table:—

Glasgow Small-pox.—Proportion from each Administrative District (or Division) of the Total Cases occurring in Several Years.

Year.	Total Cases.	Percentage of Total Cases.						
		East.	Central.	South.	North.	West.	South Sub-urban.	North-West.
1900-1	1,759	60.4	12.3	13.6	7.0	3.0	3.1	0.6
1892	78	28.2	19.2	26.9	3.9	20.5	1.3	—
1893	386	45.9	24.7	9.7	12.8	3.8	0.8	2.3
1894	49	32.7	36.7	16.3	3.2	2.0	4.1	—
1895	243	33.4	14.0	9.0	5.8	36.2	0.8	0.8
1896	5	—	—	—	—	—	—	—
1897	59	57.6	3.4	32.2	5.1	—	1.7	—
Percentage Population, 1901.		23	15	17	22	8	8	7

It should be added that for the pre-epidemic and epidemic period 1900-1901, Dr. Chalmers studied the incidence of small-pox not only on the seven Divisions of the city, but also on thirty-three smaller "sanitary districts" into which the city is also subdivided. The same result was again apparent. The attack rate of the whole city was 2.3 per thousand, that of the "7th Sanitary District," which includes most of Glasgow within a mile of the hospital, was 9.97 per thousand. None of the other thirty-two sanitary districts had an attack rate exceeding three per thousand, except the 8th Sanitary District, in which the rate was 6.46 per thousand, and the 11th, which had a rate of 4.2 per thousand. The 11th District was that in which the epidemic had begun. Part of the 8th District lies within one mile, and the whole of it within one and a-quarter miles from Belvedere Hospital.

Looking to all the circumstances of Glasgow, as set out by Dr. Chalmers, it is difficult to resist the conclusion that the operations of the hospital have influenced the distribution of small-pox in Glasgow to a very marked extent during recent epidemic years; and that the magnitude and persistence of the epidemics which have occurred during these years are to a large extent attributable directly and indirectly to hospital influence.

GATESHEAD AND FELLING, 1903-1904.

Sheriff Hill Hospital.

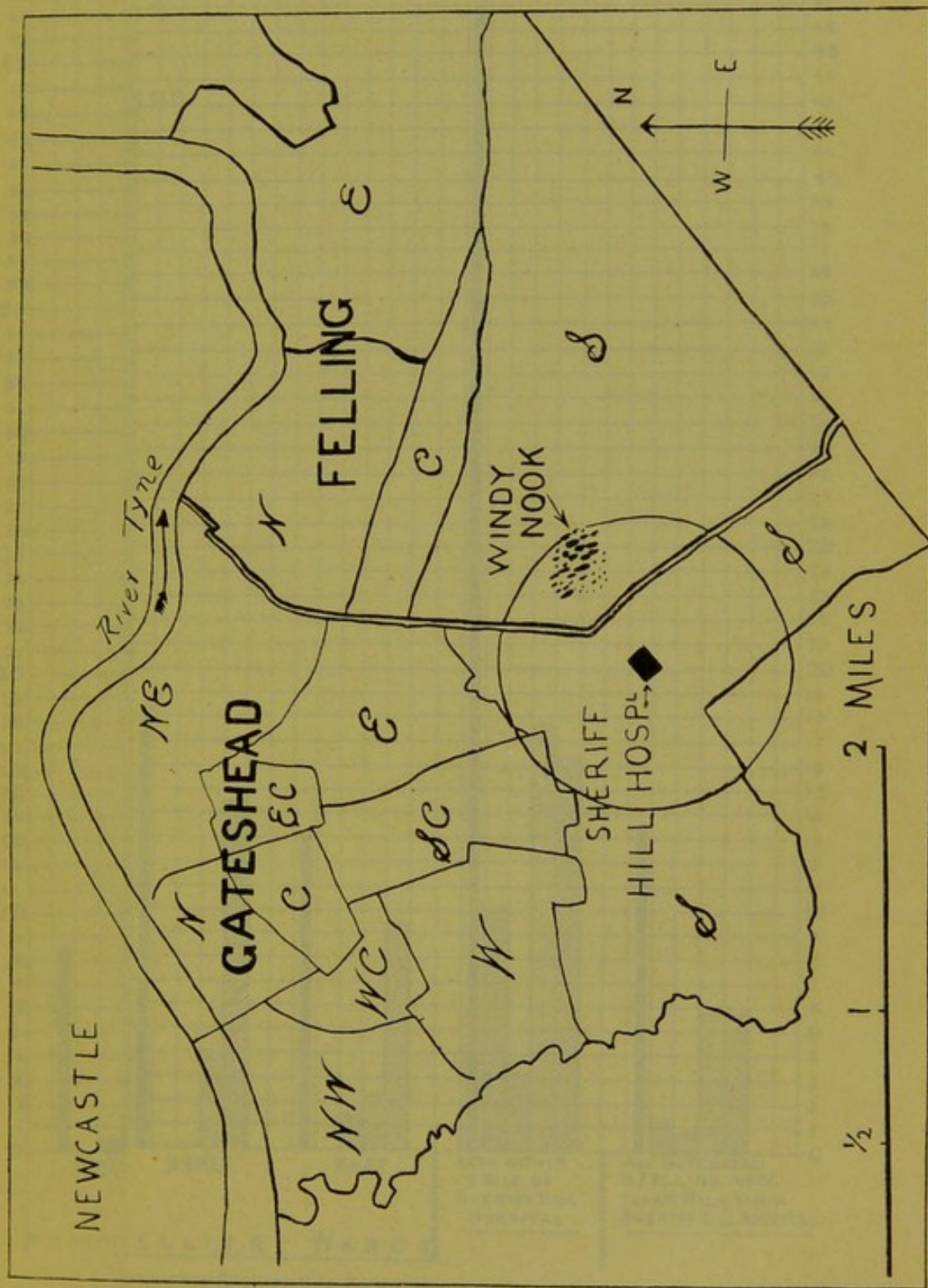
During 1903-1904 small-pox was prevalent, to a greater or less degree, in various towns at the mouth of the Tyne, including Newcastle, which itself contributed a considerable total of cases during these two years. The Borough of Gateshead, on the opposite side of the Tyne to Newcastle, sustained during this period what may be considered a pronounced epidemic (Gateshead, population 110,000; total cases, April, 1903, to May, 1904, 510; total dwellings invaded by small-pox, 316).

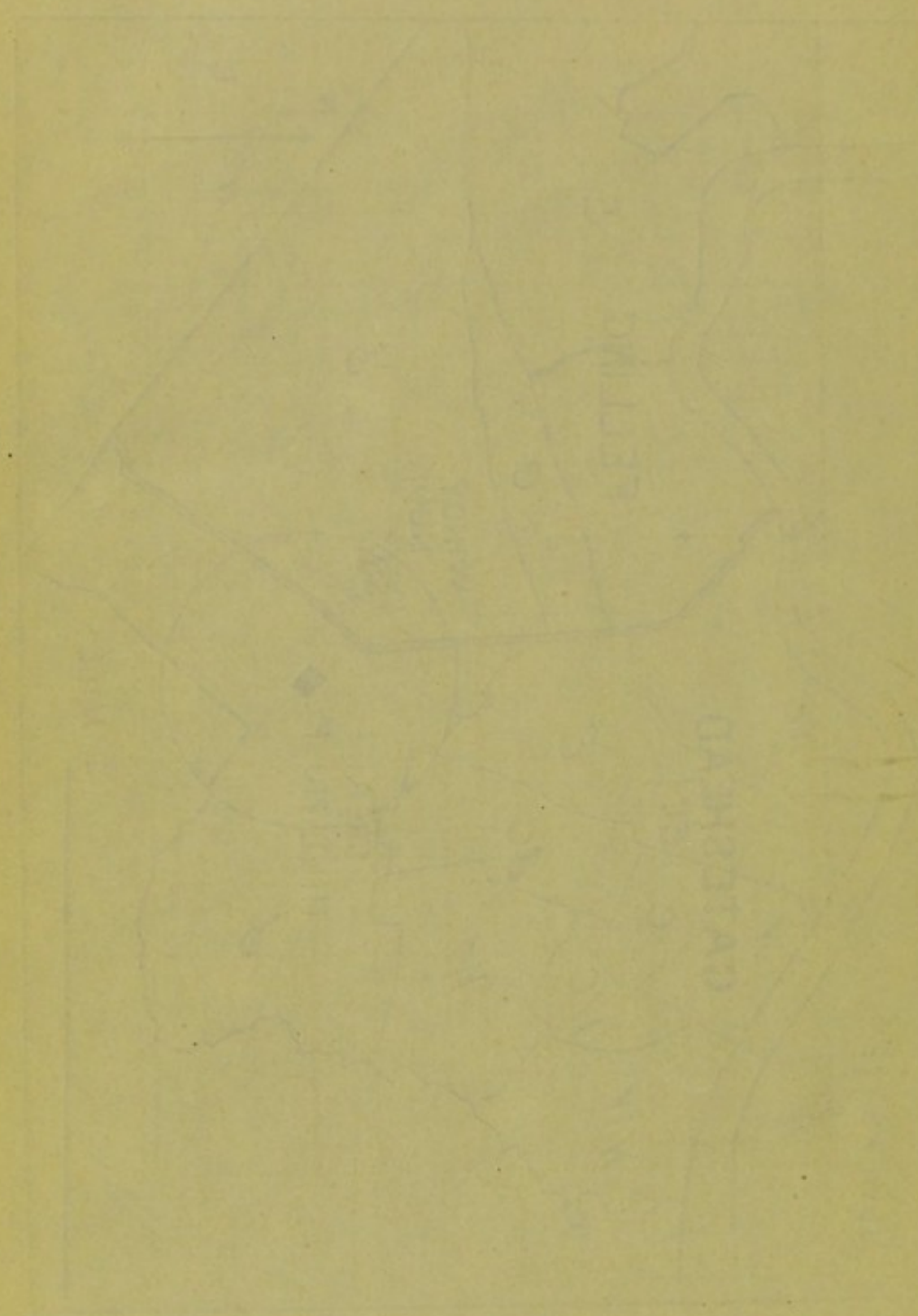
Of other boroughs and urban districts in Tyneside, that which showed the highest attack rate was the urban district of Felling, adjoining Gateshead, and to the east of it (Felling, population 23,000; total cases in the same period, 155; total dwellings invaded by small-pox, 106). The relative position of the two districts will be seen from the rough map exhibited. Early in 1904, Dr. Eustace Hill drew attention to the circumstance that the small-pox epidemic in Felling was mainly affecting, not its most populous part near the Tyne, but dwellings in the higher and comparatively open country to the south—in particular a village called Windy Nook, about two miles south of the Tyne, and close to the Gateshead - Felling boundary. The area specially affected was mainly that within a mile of Sheriff Hill Hospital, which was receiving all the Gateshead small-pox cases. The village of Windy Nook is between a quarter and half a mile from, and north-east of, this hospital. This, together with similar observations by Dr. Peacock, Medical Officer of Health of Felling, led to a detailed inquiry which occupied me for many weeks in 1904, into the local distribution of small-pox in both Gateshead and Felling. In Gateshead the distribution was not unlike that in Felling. Small-pox had been prevalent in various parts of Gateshead, at first (April to November, 1903), with comparatively slight intensity, later (December, 1903, to May, 1904), to a conspicuous and serious extent. But it was the more sparsely populated part of Gateshead—the higher and comparatively open ground near its southern boundary, comprising dwellings near Sheriff Hill Hospital—that had been affected to the greatest extent. By a special enumeration I ascertained that there were 1,297 dwellings (corresponding approximately to a population of 6,200) in Gateshead

and Felling, which are within half a mile of the hospital. The figures as to dwellings, together with fortnightly returns for each district, enabled a statistical comparison to be made between the incidence of small-pox on houses within half a mile of the hospital on the one hand, and on those in Gateshead and Felling, which are more than half a mile away on the other. The results were as follows:—

Dwellings newly invaded by small-pox within half a mile of the hospital...	...	47 per thousand.
Dwellings newly invaded by small-pox more than half a mile from the hospital	...	13.7 „

I applied also a more searching test, by ascertaining the incidence of small-pox on the several *wards* of Gateshead and Felling. In this way it was possible to compare the incidence on the half-mile area with that on twelve other areas, each more than half a mile from the hospital, and each containing somewhere about the same population as the half-mile area. The result is shown in the diagram given in my report and here exhibited. The incidence on dwellings in the half-mile area (47 per thousand) was greater than the corresponding incidence in any ward of Gateshead or Felling more than half a mile from the hospital. The ward more than half a mile from the hospital which had the greatest incidence (25 per thousand) was the East Ward of Gateshead, many dwellings of which are within one mile of the hospital. The excess in the half mile area so conspicuously shown on comparison with the other twelve areas was not the result of a single special outbreak near the hospital during a limited time. It was found to exist in each of three periods of three months, and in the remaining period of four months and a half, into which the epidemic was divided; and on two or three occasions it was conspicuous within a few weeks of the hospital receiving an augmentation of acute cases. Moreover, the incidence on dwellings within a quarter of a mile of the hospital (82 per thousand) was greater than that on dwellings within the quarter to half-mile zone (35 per thousand), which again was greater than that on dwellings in the rest of Felling and Gateshead (13.7 per thousand). It should be added that the facts as to vaccination and re-vaccination gave no evidence of any special susceptibility of the population of the half-mile area as compared with the rest of Gateshead and Felling. As the epidemic progressed it was rather the other way—the half-mile area came to contain a larger proportion of insusceptible people.





CHILLICOTT

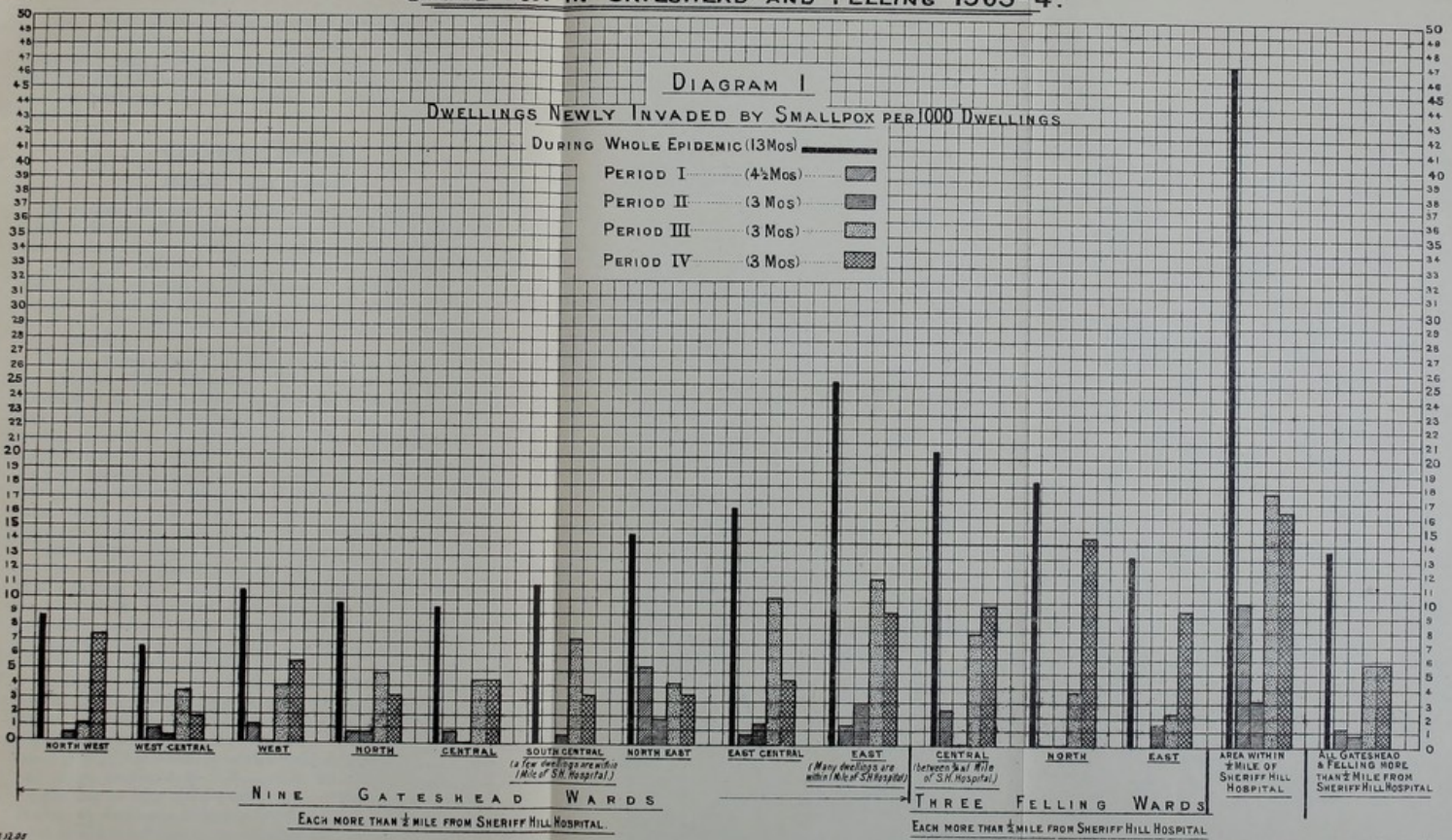
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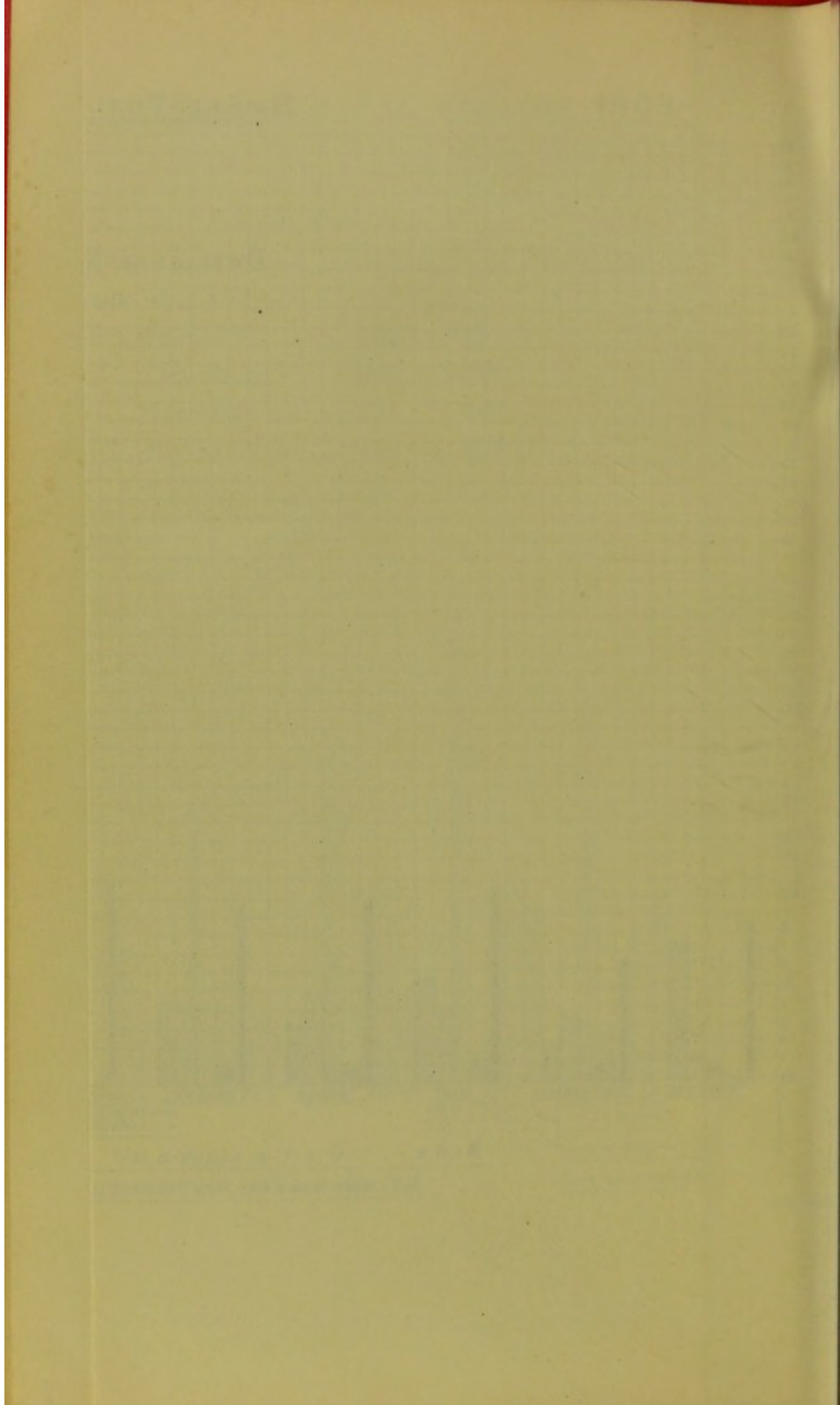
SMALL-POX IN GATESHEAD AND FELLING 1903-4.

DIAGRAM I
DWELLINGS NEWLY INVADDED BY SMALLPOX PER 1000 DWELLINGS

DURING WHOLE EPIDEMIC (13 Mos)

- PERIOD I (4½ Mos)
- PERIOD II (3 Mos)
- PERIOD III (3 Mos)
- PERIOD IV (3 Mos)





These facts, together with study of fortnightly returns and maps showing invaded dwellings in the whole of Gateshead and Felling, indicated that this hospital exercised during 1903-1904 a very material effect in enhancing and maintaining the prevalence of small-pox in both districts.

MANCHESTER, 1902-1904.

Clayton Vale Hospital.

Dr. Niven has put on record the facts regarding the local incidence of small-pox in this city during 1902-03, and he has kindly furnished me with corresponding data in respect of 1903-04. From December, 1902, to June, 1903, there occurred in Manchester (population, roughly, 550,000) a total of 383 reported cases of small-pox; from June, 1903, to November, 1904, the total number of such cases was 219.

During these years the cases in question were isolated in Clayton Vale Hospital. The largest number of admissions to this hospital in any one fortnight in 1903 was about 60. Clayton Vale Hospital lies in the eastern outskirts of Manchester. The population in its neighbourhood is small. In 1900 it was ascertained that only 42 persons lived within a quarter of a mile of it, and 606 persons within half a mile. As regards the half-mile area, there are thus very few data to go upon in considering hospital influence: the conditions are very different from those of Gateshead. But I have been led to include Manchester in my list, for the reason that Clayton Vale Hospital has a considerable population resident between half and one mile from the hospital, namely, a large portion of the Newton Division (population 37,143), and nearly the whole of the Clayton Division (population, 9,795).

Dr. Niven's maps show that during 1902-03 only eight, and in 1903-04 only thirteen, cases of small-pox were reported to have occurred in dwellings situated between half a mile and a mile from this hospital. The cases among the small population resident within half a mile were only one and three respectively. Moreover, Dr. Niven notes that "a number of men were employed near the hospital laying drains, etc., but none of them developed small-pox, although most of them refused to be vaccinated."

So far, then, as conclusions as to hospital influence can be drawn from the behaviour of small-pox in inhabited

areas more than half a mile away from Clayton Vale Hospital during these years, and from the facts as to the small number of people living or working within one half-mile of the hospital, the results for Manchester are plainly negative. Contrast is at once suggested between this negative result and the positive result which was obtained in the case of inhabited areas more than half a mile from the Hospital Ships in Long Reach. To this I propose to revert later; but meanwhile it should be noted that Clayton Vale Hospital at no time during 1902-1904 contained any concentration of recent and severe cases of small-pox of the kind which were present on the Hospital Ships in 1901-02.

LIVERPOOL, 1902-03.

Priory Road, Park Hill, and Fazakerly Hospitals.

One of the most important contributions to the present subject is a report by Dr. R. J. Reece to the Local Government Board, issued within the last few weeks, which embodies the results of a laborious study of the question of Small-pox and Small-pox Hospitals in Liverpool, during the two years 1902-03. In this period, Liverpool (population, roughly, 720,000) suffered far more severely from small-pox than did Manchester. From December, 1901, to the end of 1902, the total number of cases reported (after deduction of certain cases brought by shipping) is given as 552. At the end of 1902 small-pox was increasing, and at the beginning of 1903 it assumed the proportions of a considerable epidemic, which continued until the end of June. 1,585 cases of small-pox were notified during the first half of 1903. Some 141 others occurred in the later months of 1903. Small-pox disappeared from Liverpool at the end of that year. During the whole period of two years, December, 1901, to December, 1903, the total reported cases are given as 2,278, and the total number of dwellings invaded as 1,632. The situation, as regards populous areas of two out of the three hospitals which were used for the isolation of small-pox during this period, was exceptional, and was favourable to the investigation of hospital influence. These hospitals are those at Priory Road and Park Hill.

Priory Road is a small hospital on the north-east of the city, with a total accommodation ordinarily reckoned as suitable for some forty small-pox patients. The maximum number of cases admitted thereto in any one fortnight of

the epidemic was sixty-five. This hospital has a considerable population living within half a mile of it, and a large population living within one mile.

Park Hill, a larger hospital than Priory Road, with a total accommodation of about 350 beds, is in the south of the city, about four miles from the Priory Road Hospital. The two hospitals are separated almost by the whole length of Liverpool. Park Hill also has a considerable population in its neighbourhood, the figures as regards houses being :—

		Total Houses distant from the Hospital.			
		0— $\frac{1}{4}$ Mile.	$\frac{1}{4}$ — $\frac{1}{2}$ Mile.	$\frac{1}{2}$ — $\frac{3}{4}$ Mile.	$\frac{3}{4}$ —1 Mile.
Priory Road Hospital	...	85	1970	7257	12412
Park Hill Hospital	...	171	2259	4563	5617
Fazakerly Hospital	...	2	166	867	1423

To these I have added the figures for the third hospital—Fazakerly—a large hospital outside the city boundary, which had accommodation for 160 small-pox patients. Fazakerly Hospital, unlike the other two, is in a comparatively sparsely-populated district. The situation of these Liverpool hospitals is shown on the rough map exhibited.

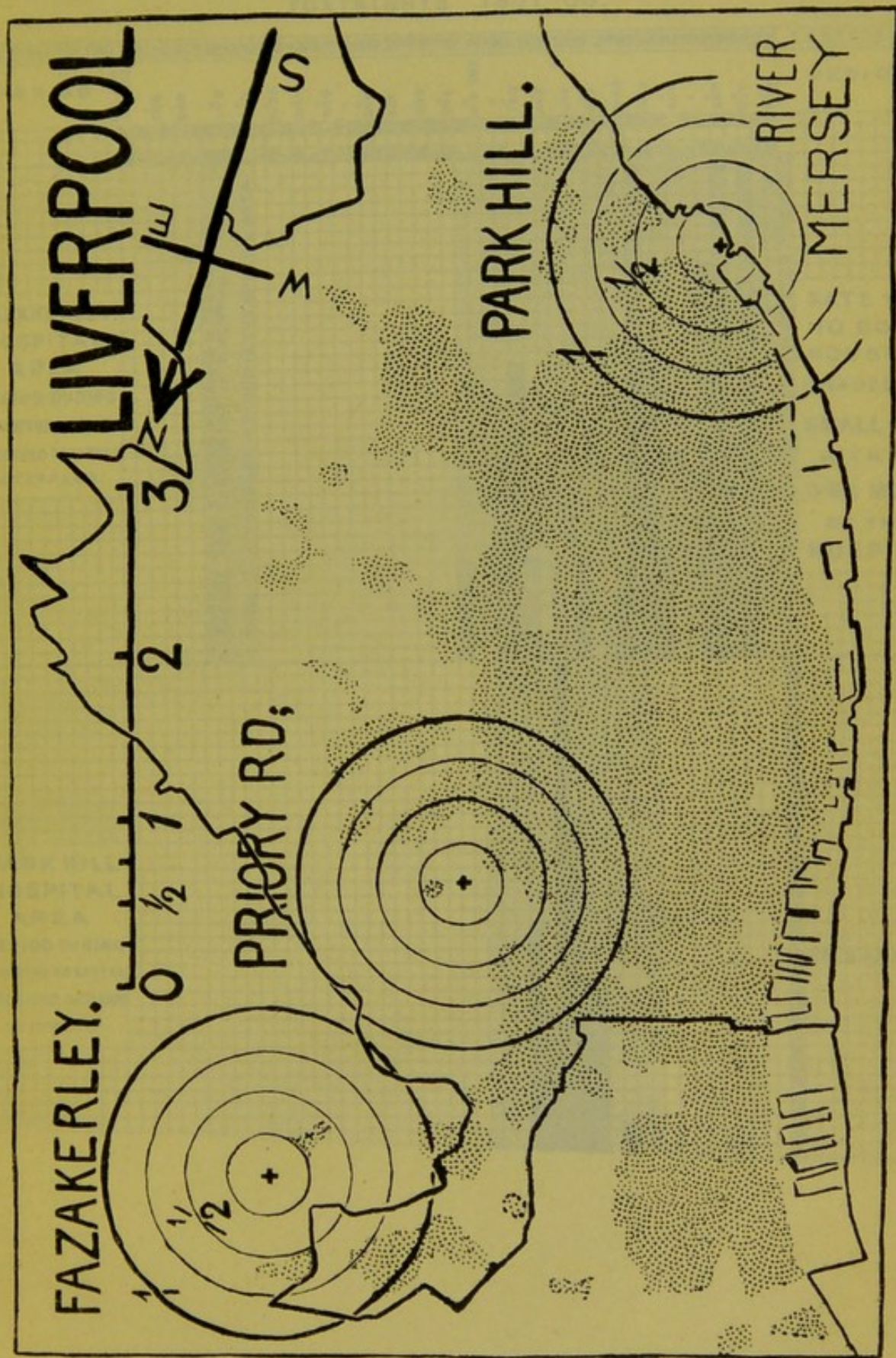
Liverpool did not have all three of its hospitals open for small-pox cases throughout the epidemic. The periods during which the several hospitals were open to receive cases were as follows :—

Period.	Approximate Number of Weeks in Period.	Hospital or Hospitals to which Cases of Small-pox were Admitted.
6th December, 1901, to 10th December, 1902	53	Priory Road.
10th December, 1902, to 12th January, 1903	5	Priory Road, Fazakerly.
12th January, 1903, to 26th May, 1903	19	Priory Road, Fazakerly, Park Hill.
26th May, 1903, to 1st July, 1903	5	Fazakerly, Park Hill.
1st July, 1903, to November, 1903	21	Fazakerly.

The extent to which cases of small-pox were admitted to the several hospitals during each of the above periods is shown by the diagram given in Dr. Reece's Report. The rapid admission of large numbers of small-pox cases into Park Hill Hospital in January, 1903, is well illustrated by this diagram.

Now, anyone knowing the facts only thus far, but acquainted with Mr. Power's researches on the behaviour of small-pox in London before 1885, would at once fasten upon this sudden opening of Park Hill Hospital in January, 1903, as the most obvious point for inquiry. What was the history as regards small-pox of the populous areas near this hospital before it was opened, and what happened afterwards? Dr. Reece's report gives a clear answer. Before January, 1903, the central part of the city, and also—and especially—its north-eastern part near to Priory Road Hospital, had been principally affected. Cases had occurred from time to time in the area within a mile of the (as regards small-pox) empty Park Hill Hospital; but relatively to the rest of the city, the incidence of small-pox on this area had been slight. Park Hill Hospital was opened on January 12th, to meet a sudden demand for extra isolation, entailed by a localised outbreak in the central part of the city, due to an unnotified and severe case which was moribund when discovered. The admissions to Park Hill Hospital in successive fortnights ended January 17th and 31st, February 14th and 28th, were respectively 7, 88, 55, and 97. The houses newly invaded in the inhabited area within a mile of the hospital, in the fortnights ended January 3rd, 17th, 31st, and February 14th, were respectively 4, 3, 2, and 3. Then, in the fortnight ended February 28th, the number of invaded dwellings in the mile area rose to 31; and in the three following fortnights 23, 55, and 40 houses in this area were newly invaded by the disease. And until Park Hill Hospital ceased to receive cases of small-pox, in June, this area continued to suffer to a far greater extent, relatively, than the rest of Liverpool. This is shown graphically in Dr. Reece's diagram, exhibited.

In the matter of graduation of incidence of small-pox, again, the familiar experience was repeated. The following are the figures; the rates of invasion at different distances from the hospital are also shown on Diagram II, extracted from Dr. Reece's Report.



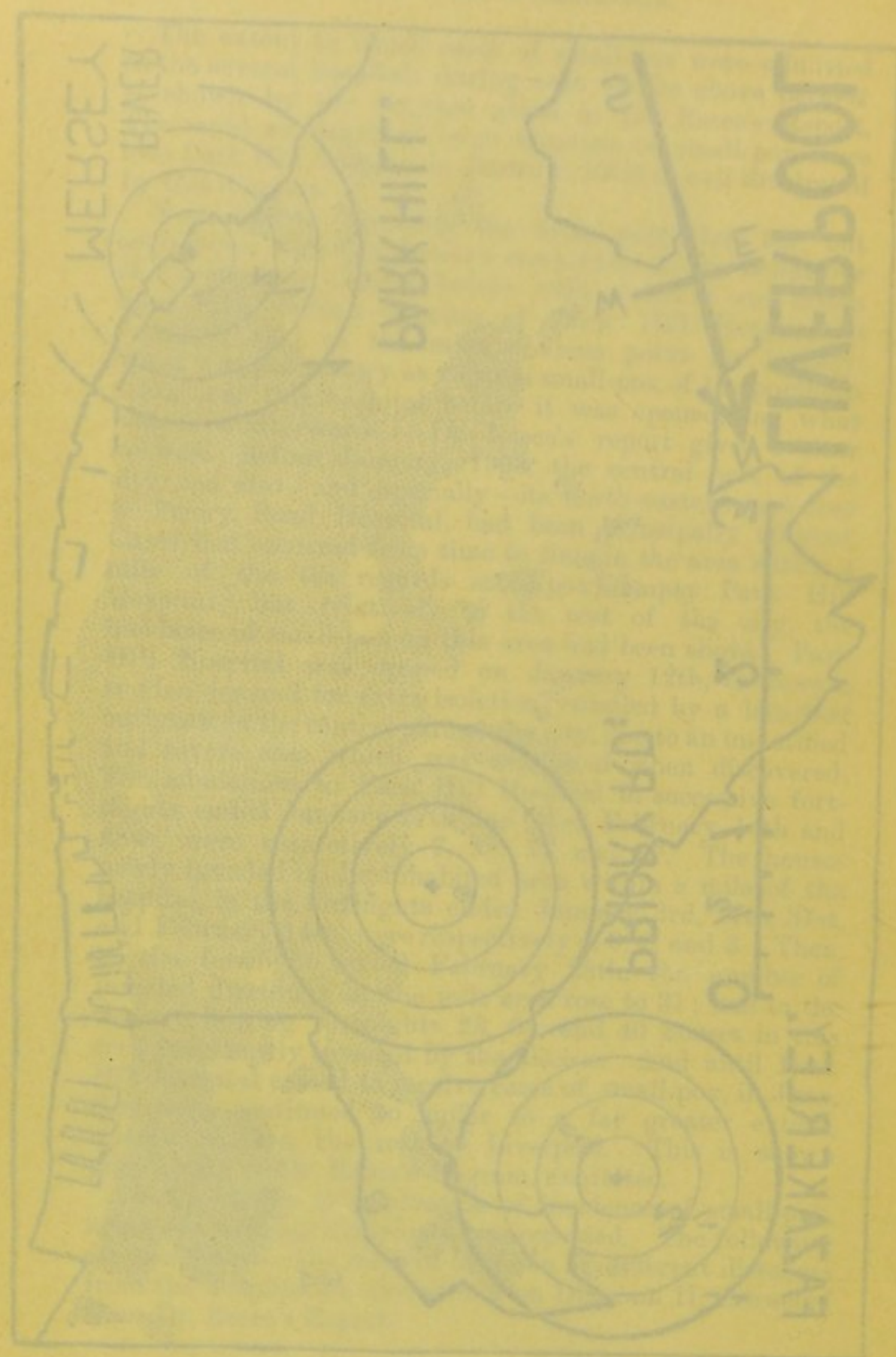
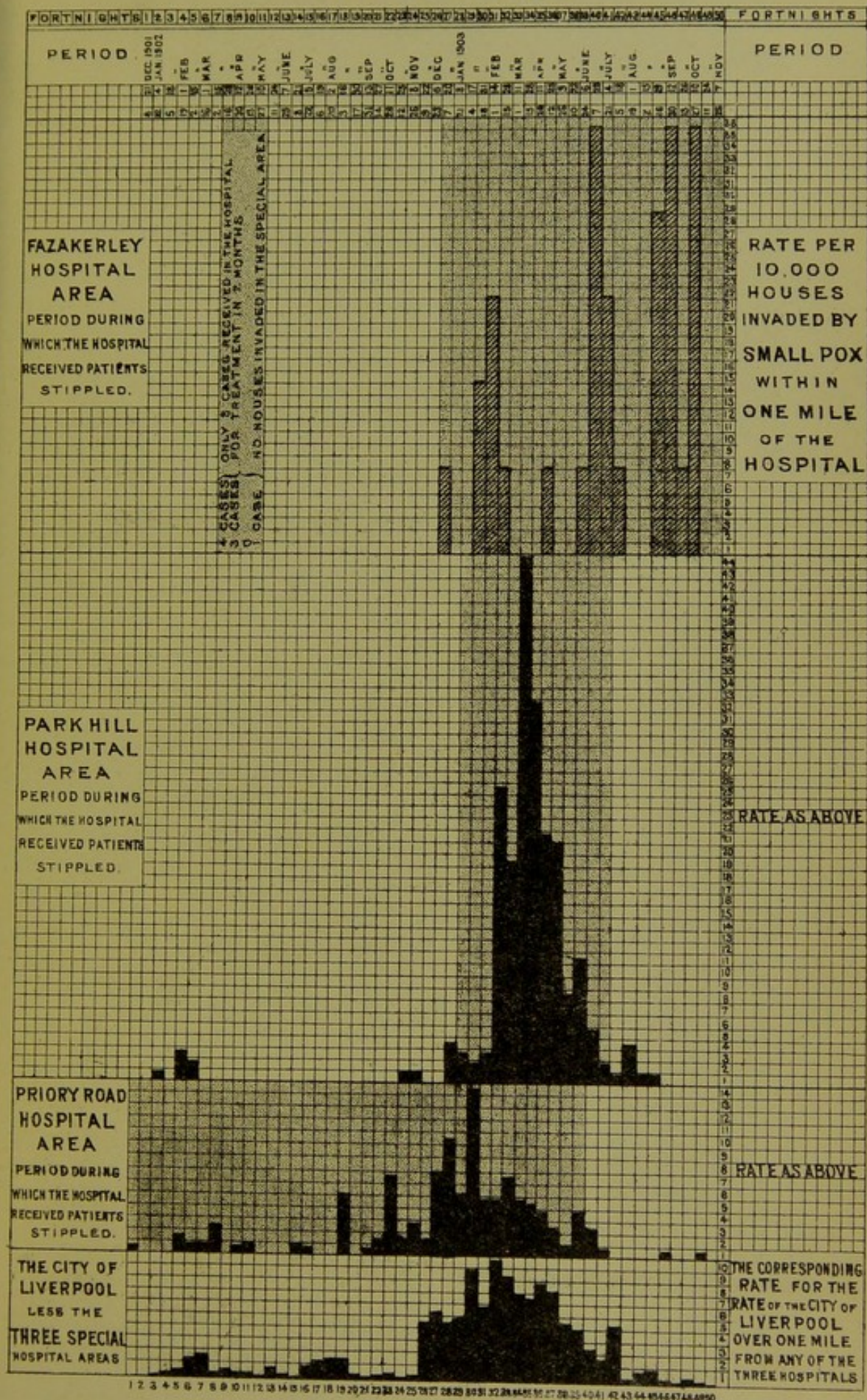


DIAGRAM I. SHOWING THE INCIDENCE PER 10,000 HOUSES IN EACH INSTANCE OF HOUSE INVASION BY SMALL-POX ON AREAS OF LIVERPOOL SEVERALLY WITHIN ONE MILE OF A SMALL POX HOSPITAL AND IN THE REST OF THE CITY IN FIFTY SUCCESSIVE FORTNIGHTS 1901-03.



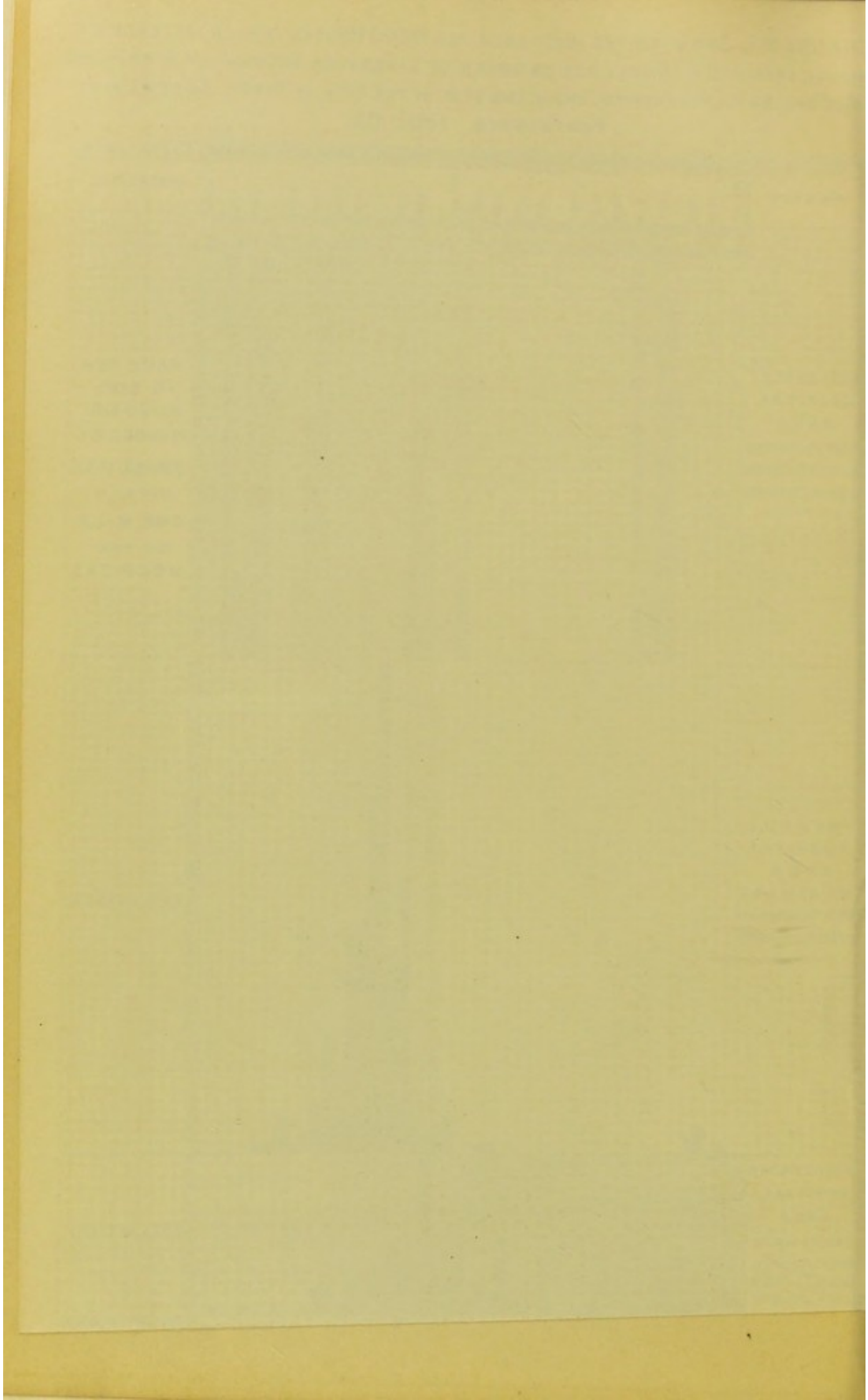
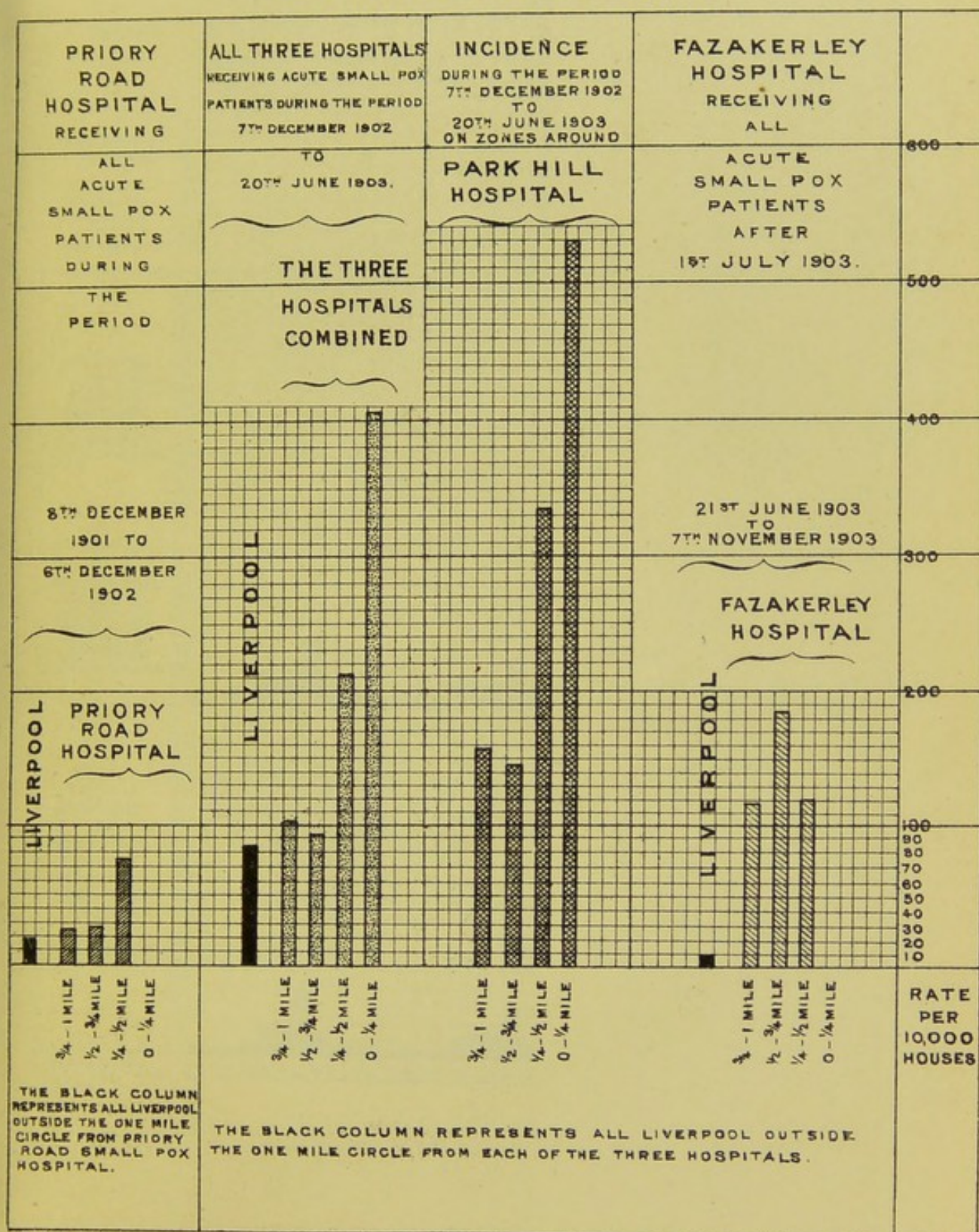


DIAGRAM II.

SHOWING IN SUCCESSIVE PERIODS, DECEMBER 1901 TO NOVEMBER 1903, THE INCIDENCE OF SMALL POX INVASION, PER 10,000 HOUSES IN EACH INSTANCE, IN THE SEVERAL AREAS WITHIN ONE MILE OF LIVERPOOL SMALL POX HOSPITALS, AND ON CERTAIN SUB-DIVISIONS OF THOSE AREAS. FOR PURPOSES OF COMPARISON THE "INVASION RATE" IN EACH PERIOD OF THE HOUSES OF LIVERPOOL AS A WHOLE IS SHOWN IN EACH INSTANCE BY A BLACK COLUMN.



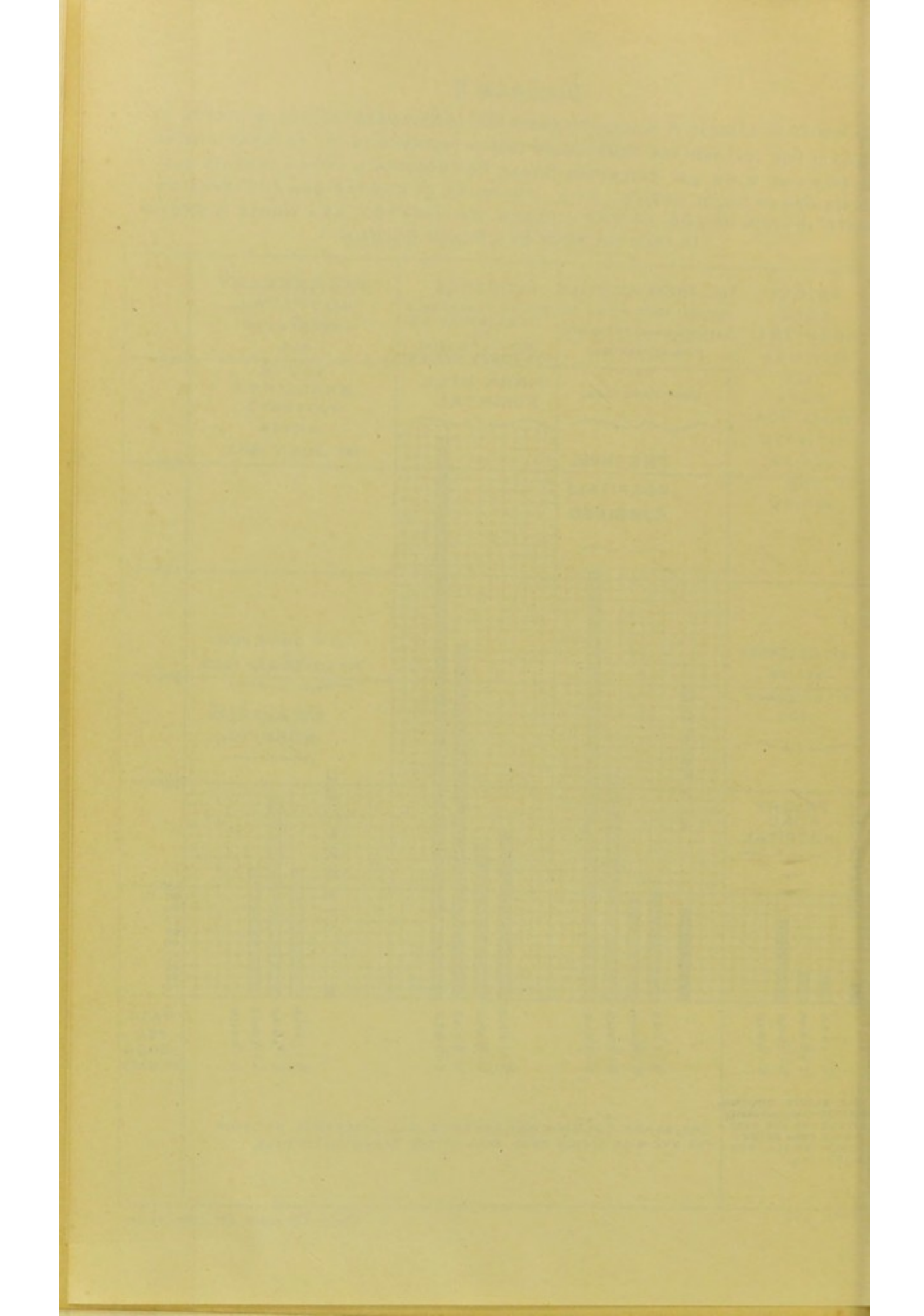
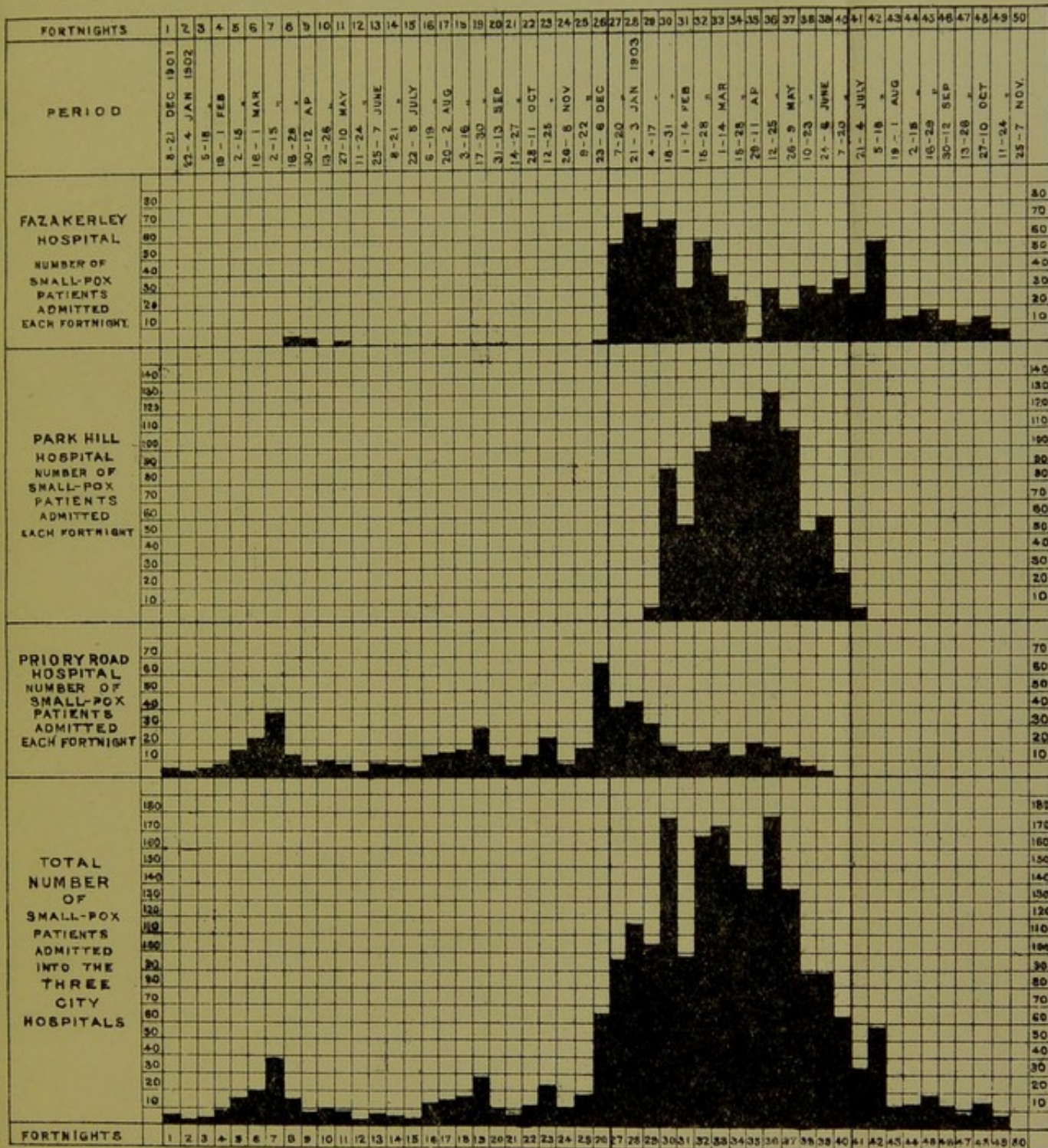
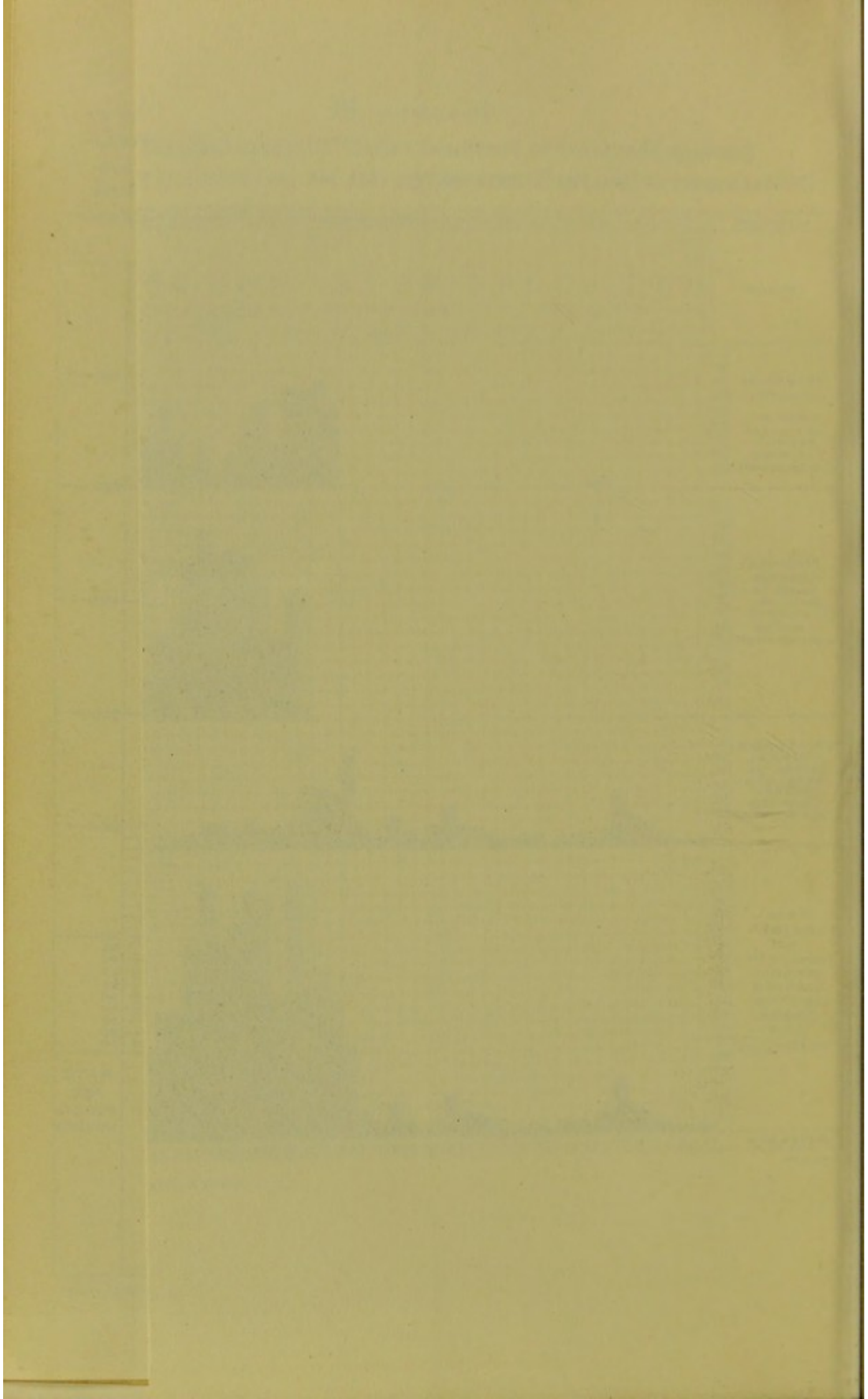


DIAGRAM III

SHOWING FORTNIGHT BY FORTNIGHT FROM 8TH DECEMBER 1901 TO 7TH NOVEMBER 1903 THE NUMBER OF SMALLPOX PATIENTS RECEIVED INTO THE LIVERPOOL CITY HOSPITALS.





December 7th, 1902, to June 20th, 1903.

	Distant from Park Hill Hospital.				Liverpool more than one Mile from any Small-Pox Hospital.
	0 - $\frac{1}{4}$ Mile.	$\frac{1}{4}$ - $\frac{1}{2}$ Mile.	$\frac{1}{2}$ - $\frac{3}{4}$ Mile.	$\frac{3}{4}$ - 1 Mile.	
Total houses ...	171	2259	4563	5617	100,969
Number of houses newly invaded by small-pox ...	9	75	67	89	868*
Houses newly invaded, per 1,000 of total houses ...	53	33	15	16	8.6

As regards the other two hospitals, it will be seen from Dr. Reece's tables and diagrams that, during the first year, 1902, when the smaller Priory Road Hospital sufficed for Liverpool cases, the incidence upon dwellings within a mile of that hospital exceeded that on the rest of Liverpool, including that on the mile area round the two hospitals, Fazakerley and Park Hill, which then were not receiving any small-pox cases. From December 8th, 1901, to December 6th, 1902, the invasion rate was only 1.6 per thousand houses in the city more than one mile away from Priory Road Hospital; while it was 2.76 on houses situated within a mile of the hospital, 3.33 on houses within three-quarters of a mile of the hospital, and 6.81 on houses within half a mile of the hospital.

I may refer to two other points which come out from study of the fortnightly maps given in this Report. The first is, that in the early part of the second year, 1903, after both Park Hill and Priory Hospitals had become full of small-pox cases, the bulk of the city, which is more than one mile from either hospital, is seen to have been affected to a much less extent than either of the areas within a mile of these hospitals. The second is, that towards the end of the epidemic, in the second half of 1903, small-pox, which was dying out in the city, and dying out of the Park Hill and Priory Road Hospital areas, lingered to a very noticeable degree in the comparatively sparsely-populated areas

* Two hundred and ninety-six of these houses were invaded between 7th December and 14th February, during which time only twelve houses in the mile area were invaded. The hospital did not receive any small-pox cases before 12th January, 1903.

round Fazakerly Hospital, which during that time had been receiving the whole of Liverpool's small-pox cases.

The history of these two epidemic years in Liverpool, as given in Dr. Reece's Report, affords a striking and recent illustration of the way in which, in epidemic years, the local distribution of small-pox in a large inhabited area may be determined by the operations of small-pox hospitals situated within or on the outskirts of that area.

GENERAL REVIEW OF SMALL-POX HOSPITAL INFLUENCE, 1900-4, IN THE ABOVE CASES.

Let us consider the facts above summarised regarding the epidemic period 1900-4. In one instance, Manchester, which must be considered with the reservations above noted, the evidence of hospital influence is negative. But in the other four cases which I have reviewed, definite positive evidence of hospital influence has been forthcoming. Clearly, the Hospital Ships in Long Reach, the Hospitals of Belvedere, Sheriff Hill, Park Hill, and Priory Road, have exerted a very conspicuous influence in enhancing the prevalence of small-pox respectively in the Orsett Union, in Glasgow, in Gateshead and Felling, and in Liverpool.

Besides Manchester, there may have been other towns in the kingdom during 1900-4 in which there has been a notable prevalence of small-pox, in which small-pox cases have been received in a hospital or hospitals so situated in regard to populous areas as to permit the matter to be tested, in which it has actually been tested by careful study, in which the facts have been exactly recorded, and have proved negative as regards hospital influence. If such cases exist, I regret that I have been unable to find them, and, consequently, have not included them in my paper.

Obviously, the Manchester case, together with other recorded instances (if such exist) that are negative in the above sense, demand careful consideration and comparison with the numerous positive instances. But they do not in least take away from the importance of these positive instances. No one would claim that contamination by typhoid excrement of a stream on a gathering-ground must always be followed by an epidemic of typhoid among the water consumers. Conversely, no one would claim the escape of the consumers from such an epidemic

as evidence that water contaminated by typhoid excrement cannot cause typhoid outbreaks. Whatever explanation we give of it, we are bound to accept the fact that when hospitals used for acute cases of small-pox have been situated in or near to populous areas, they have frequently had the effect of starting, augmenting, or maintaining the prevalence of small-pox in those areas. The areas in question have been influenced in a special fashion, characterised by an incidence of small-pox on their dwellings, which diminishes in intensity with the distance from the hospital, and has been traced as far as a mile away. The influence exerted in the above circumstances has again and again been one of the principal facts, if not the dominating fact, of a small-pox epidemic, and thus has entailed very serious consequences to the public health.

We cannot get away from these facts; they are as definite as any known to epidemiology. When Dr. McVail read the paper to which I have alluded, in 1894, they had already been ascertained by a multiplicity of careful and detailed observations, in respect of many hospitals, in different epidemics, in London and the provinces. Absence of small-pox, and the general removal of small-pox hospitals to sparsely-populated areas, led, perhaps, to this having been to some extent forgotten a few years ago. Recent epidemics have now enabled the question to be tested afresh, with results entirely confirming those of former years. "Hospital influence," and its explanation, must remain one of the most important chapters in the natural history of small-pox.

EXPLANATION OF "HOSPITAL INFLUENCE."

To come now to the explanation of the facts, and their bearing on the thesis of aerial convection. Any one who studies Mr. Power's writings of twenty years ago on this subject, will find that the principal basis of his thesis of aerial convection consists, not—as is sometimes wrongly stated—of an elimination of every other possible cause of spread of small-pox in the neighbourhood of the hospital, but in the explanation which it afforded of one of the principal manifestations of this hospital influence—the characteristic graduation in the incidence of small-pox on the neighbourhood of the hospital. Now, in 1900-4 we have again had striking evidence of the existence of this graduation occurring in different epidemics round a plurality of hospitals. Again it has to be accounted for, and the

conditions of inquiry demand that the explanation should be consistent, and one which is applicable to each of these hospitals in each of these epidemics.

Graduated intensity of small-pox incidence has never been explained by traffic of vehicles and ambulances to and from the hospital, by communications of patients in the hospital with other persons outside, or by incomings and outgoings of staff. Nor do such explanations assist in regard to the hospitals of 1900-4. In none of these instances is there any evidence that the staff was recruited from the immediate locality of the hospital: we all know that a hospital staff is not obtained in this way. Nor is there any evidence that the visits of the staff to shops or to houses of friends were graduated in frequency according to the distance of these houses and shops from the hospital. In Gateshead I obtained direct evidence in the contrary sense: when the staff left the hospital (which happened comparatively rarely), their custom was to go straight down to the busy centres and shops of the town, or over to Newcastle. Similarly, it is not the custom of municipalities to select the contractors who supply the hospital with food or stores, according to the proximity of their houses to the hospital. So, too, with regard to visitors or friends of patients, coming to the hospital, looking over the wall, through the gates or railings, and the like. In the 1900-4 cases, where such authorised or unauthorised visits were made, I can find no evidence that those who came most frequently were persons dwelling within a quarter of a mile; next, those who dwelt between a quarter and half-mile from the hospital, and so on. Consider, for example, the case of Belvedere Hospital in the two periods of recrudescence. Here, on each occasion, the small-pox cases first admitted to the hospital came from a part of Glasgow some miles from the hospital; and it is impossible to account for the outbreak which in each case followed in the hospital neighbourhood on any assumption of visits of friends or relatives. The suggestion has been made that mere curiosity of the inhabitants living near to a small-pox hospital would encourage them to try to communicate with patients. Possibly, for a time, or on some special occasion; but on what grounds can we assume such persistent and graduated curiosity continuing month after month and year after year? Gross maladministration, a staff incompetent or improperly supervised, patients getting out of hospital grounds inadequately enclosed, might entail local spread of small-pox due to personal communication among persons

living nearest to the hospital. Such opportunities were afforded the other day in the case of Dewsbury Hospital. But who can suppose such gross maladministration at Glasgow, or at Liverpool? In such cities we have not gone back in our hospital organisation or in our precautions during the last twenty years; we have advanced.

Ambulance traffic has specially to be considered. It has been suggested that the small-pox patient in an ambulance gives off particulate infection of small-pox into the air, and that aerial convection for short distances of these infective particles, operating on a susceptible individual during a very short period of time, would be more likely to give him small-pox than would infective particles derived from an aggregation of acute small-pox cases, and conveyed through the atmosphere from a greater distance. On this assumption, the concentration of ambulance traffic towards a given hospital might be expected to occasion an exceptional invasion of dwellings near to the hospital, and also might, in some measure, explain graduation of incidence. But, as Mr. Power showed, we should expect, if this were so, to find the invaded houses grouped along, or near to, main lines of communication and traffic to the hospital, and this he found not to be the case. I cannot do better, also, than quote Dr. McVail's observations on this point: I am unable to find any evidence from the experiences of 1900-1904 which points to a different conclusion:—

"It is true that people living in one street might encounter the ambulance in another, and go home and lie down with the disease a fortnight later. But it is equally true, that on any sufficient basis of facts, a larger proportion of people belonging to the streets traversed by the vans would be exposed to the ambulance influence, than of people belonging to other streets. And, in regard to those who met the van in one street and lived elsewhere, it would be a very extraordinary thing if the houses to which these people returned happened to be arranged in numbers diminishing regularly according to the distance from the hospital to which the van was travelling. It would be still more extraordinary if this same regularity of arrangement were found to repeat itself around the same hospital in epidemic after epidemic."

We must admit, then, as regards the 1900-1904 experience, that we cannot fix responsibility on to any single component of "hospital communication and traffic," any more than we have been able to do so in the case of Fulham in 1885, or in other subsequent instances. We cannot say it is the ambulances, or the staff, or the patients' friends, or the tradesmen. But it is worth considering whether the sum of all these possibilities of infection can account for the influence of the hospital on its neighbourhood—direct personal communication with a patient here, indirect infection by a nurse there; an ambulance on this occasion; a

wall-climbing patient on that, a porter's visit without disinfection on a third. True, the graduation of incidence is not explained in this way, but may it be that in some manner or other not yet understood the total effect of all these things may give us graduation?

My own observations as regards two of the hospitals which spread small-pox in 1900-4 point to a negative answer to this question. The hospitals are Sheriff Hill and the Hospital Ships. Sheriff Hill Hospital is close to the boundary of Gateshead and Felling. During the epidemic of 1900-4 all the small-pox cases in this hospital came from Gateshead. Felling had its own hospital more than two miles away. No ambulance came to the hospital from or through Felling; the friends of patients and others who came to the hospital, came, not from Felling but from Gateshead. The staff of the hospital had nothing to do with Felling, and never went there. The Felling people at Windy Nook, who suffered severely and persistently during the epidemic, had no curiosity as regards the Gateshead patients in the hospital on the hill above them; it was true that many of their friends had small-pox, but these were at Felling Hospital. Windy Nook people, going into Felling, or to Gateshead, or to Newcastle, would have to go out of their way and up-hill in order to pass the Sheriff Hill Hospital. In short, the sum of "hospital communication and traffic," in regard to Felling, was practically nil. Nevertheless, as my report shows, the exceptional incidence of small-pox was more conspicuous on the Felling side of Sheriff Hill Hospital than that on the dwellings in Gateshead, at corresponding distances from the hospital.

The Hospital Ships in Long Reach during 1901-2 had abundant communication on the Kent shore with Dartford. They are moored off the Kent shore, and all their land traffic and communications are with this side. Neither ambulance steamers, patients, nor staff had anything to do with Purfleet, half a mile away across the water. Hospital communication and traffic in this case was non-existent. Anyone acquainted with Purfleet, and who has also lived on the ships, as I have done, knows that there is not the least attraction in that place, and that the staff has nothing to gain by rowing across there, even if the ships' rules allowed it. I have been told that the statement has been made that in 1895 members of the staff did sometimes row across to Purfleet, and that it has hence been inferred that in 1901-2 there may again have been such communication. I claim, however, in this case to speak with the authority

of most careful local inquiry on many occasions throughout the latter epidemic. Moreover, if any such visits occurred, they could afford satisfactory explanation of the Purfleet epidemic only on the assumption that they were made systematically week by week, or month by month, throughout that epidemic; that members of the staff were able to get boats for this purpose, and so systematically to transgress the hospital rules; that on each occasion they further transgressed the hospital rules by neglecting to change their clothing and carry out the prescribed rules for disinfection; and that on each occasion they landed at Purfleet, left their boat and mixed with its inhabitants without it being known who they were, or whence they had come. Such assumptions have no basis in fact.

Nor does the negative experience of Manchester help us in this connection. The factors which I have enumerated existed in Manchester as elsewhere. Ambulances converged to Clayton Vale Hospital from all parts of the city; the staff was not kept for two years within the walls of the hospital; Dr. Niven specially notes that people came to the hospital on Sundays, looked into the grounds, and all the rest of it. No doubt, as a whole, the administrative precautions taken by Manchester against spread of infection by hospital communications and traffic were excellent—this may be expected of Manchester. But what is the evidence that they differed, to any material degree, from those of Liverpool or Glasgow? It is hard to say which of these three great cities we should select if we required to show instances of excellent modern administration, in the matter of ambulance service, hospital precautions, and the like.

We have now to consider the recognised explanation of "hospital influence"—the hypothesis of aerial convection—in relation to the behaviour of small-pox during the 1900-4 period in dwellings in the neighbourhood of the several hospitals under consideration.

CONSIDERATIONS AS TO "AERIAL CONVECTION."

Aerial convection, as an explanation of "hospital influence," requires it to be granted, in the first instance, that the infective material by which small-pox infection is transmitted is particulate; that the air of hospital wards in which acute cases of small-pox are isolated contains, often or always, an abundance of infectious particulate matter; and that these particles may be carried up into

the outer atmosphere by currents of warm air rising from the hospital.

In these circumstances, the fate of the floating particles will be determined by the prevalent conditions of the atmosphere: wind, temperature, moisture, and the like. It may be presumed that when (as will most usually happen) they meet with a definite wind of moderate or strong velocity, they will thereby, in ordinary cases, become spread out and scattered, so that the individual particles, by the time they have ultimately fallen, are, so to speak, anywhere; the greater part of them, most likely, having been carried far away from the neighbourhood of the hospital. Again, if the escaping particles are carried from the hospital into a shower of rain, they will presumably be cleared out of the air and washed to the earth. The theory of aerial convection, as applied to explain the characteristic incidence of small-pox round the hospital, requires that from time to time meteorological conditions will enable the floating particles to be carried by light air currents for variable distances, in such a way that the floating matter undergoes no great degree of dispersion, and tends to settle over comparatively limited areas in the neighbourhood of the hospital. The situation of these areas in regard to the hospital will in such circumstances be governed by the character and direction of the prevailing convection currents. The result is that, now and again, and here and there, persons living in the vicinity of the hospital become exposed, it may be for a few minutes or for a few hours, to the inhalation of air which, in regard of the floating particles it contains, approximates in infective ability to that escaping from the acute small-pox ward of the hospital.

It will, I think, be readily admitted that a hypothesis of aerial convection operating in some such manner as this affords (what we cannot otherwise obtain) a consistent explanation of the facts of 1900-4 with regard to all the hospitals which spread small-pox in their neighbourhood. If accepted, it accounts for Purfleet and Felling as well as for Liverpool and Glasgow. It explains the transference of the disease from one quarter of the city to another, as a result of the hospital operations of Glasgow and Liverpool. It explains the persistence and recurrence of "hospital influence" during period after period of small-pox prevalence. It enables us to understand how it is that this hospital influence has continued to be exerted in large cities, notwithstanding modern sanitary organisation and

modern improvements in hospital administration. Especially, also, it enables us to comprehend the occurrence of graduation in intensity of small-pox incidence. And, finally, this hypothesis is the only consistent explanation of the instances of "hospital influence" in former years; and thus we have a means of reconciling present and past experience in this respect.

It should be added, also, that the hypothesis of aerial convection gives us a suggestion of the reason for the escape of Manchester. Dr. Niven (who some years ago had experience of spread of small-pox round the Westhulme Hospital of Oldham, and also, I understand, at the Monsall Hospital of Manchester) adopted at Clayton Vale Hospital Manchester, both in 1902-3 and in 1903-4, methods specially designed to reduce the access of particulate infectious matter to the air of the hospital. Great care was taken in the cleansing of the wards, so that the amount of dust was reduced to a minimum. Every case of small-pox in the acute stage was oiled, twice daily, with a 2.5 per cent. solution of carbolic oil. In special and severe cases, extensive portions of the body, or even the whole body, were kept swathed in lint wrung out of boric solution and enclosed by gutta-percha tissue, while a mask of the same kind was placed over the face—other parts of the body not so dressed were kept oiled. It should be noted that this system, the persistent performance of which is, in Dr. Niven's words, an unpleasant and tedious work, requiring both energy and determination, was much more than the mere limited application of carbolic oil or dressings to certain cases, which is practised in many small-pox hospitals. As I have said, the smallness of the population living within half a mile of Clayton Vale Hospital, and the comparatively limited proportions of the epidemics in Manchester, makes caution necessary in drawing conclusions from the absence of "hospital influence" there. But it is at least very suggestive that the negative results of Manchester are met with in the case of a hospital where special care was taken to prevent aerial convection of particulate matter.

During the many years in which the thesis of aerial convection has been available, and has been discussed as an explanation of "hospital influence," it has not been objected to, so far as I am aware, on the ground that it involves unlikely or extravagant assumptions. At the same time, I have occasionally found colleagues in the public health service who are not only sceptical as to the

existence of aerial convection, but who also go so far as to prefer leaving hospital influence unexplained to accepting aerial convection as the explanation. For example, they will refuse to consider the possibility that a given case of small-pox in the neighbourhood of a small-pox hospital has been caused by aerial convection so long as there is any chance, however remote, that the patient contracted his small-pox (apart from any relations with the hospital) by "direct" or by "mediate" infection, or by exposure to a hypothetical "missed case." On these lines, particularly if the term "mediate" is made sufficiently elastic, they can without much difficulty dispose of nearly every case in the hospital neighbourhood. But, having done so, they leave us in a worse position than before: a position in which it is necessary to assume that the causes of direct or "mediate" infection—the tramp, the contact of the contact, the "missed case," and the rest, are more dangerous to people who happen to live in the neighbourhood of the hospital than to those who live elsewhere.

This untenable position no doubt in part is merely the result of their having been unable to see the wood for the trees. But the basis of it is, probably, an impression that the suppositions involved in aerial convection are, *a priori*, improbable. It may be useful, therefore, briefly to inquire: Do the assumptions of the aerial hypothesis conflict with present-day knowledge as to the nature of small-pox infection, or as to the behaviour of floating particles in the air?

The infection of small-pox is certainly particulate. We do not know much regarding the micro-organism of variola, but there is no question that it must be present in abundance in the secretions and exudations of the mouth and air-passages, and in the vesicles on the skin, of an acute case of small-pox. Dr. Mervyn Gordon's recent work on salivary bacteria helps us to realise the enormous number of minute particles of infectious saliva and mucus which must be projected into the air when a patient who has abundant eruption, accompanied by copious secretion, in his mouth and throat, shouts, sneezes, or coughs. It is inevitable, also, that minute particles of epidermis, fresh from contact with virulent small-pox lymph, must be shed in immense quantity into the atmosphere of a small-pox ward, particularly when patients are restless or struggling. There is an obvious difference between acute cases of small-pox and acute cases of scarlet fever in the degree of their ability to cast infective particles into the air. If asked as

to the relative danger incurred in exposing susceptible persons to the atmosphere of a ward containing acute small-pox and one containing acute scarlet fever, general experience of both diseases would justify us in attributing the greater danger to small-pox. It should be added also that there is ample evidence that the infectivity of the small-pox virus is not destroyed by dessication.

An abundance of floating infectious particles in air escaping from a small-pox ward may then reasonably be assumed. It is needless to dwell on the ability of fine particulate matter to float in the air and to be carried by air-currents, or upon its tendency to fall. We need not go for illustration to Tyndall's experiments, or to Krakatoa, or to Mont Pelée: observation of the household chimney will suffice. If biological illustrations are desired, there is the experiment of Hutchison, who sprayed *B. prodigiosus* into the open air of the parade-ground at Göttingen, and recovered the bacilli on plates 170 and 600 metres away. Dr. Gordon informs me that he has found evidence of the convection of microscopic droplets of saliva to at least 150 feet from the mouth of the speaker in a large assembly-room.

The important question is, of course, the ability of these floating particles, in suitable conditions, to settle in the neighbourhood of the hospital without a great degree of dispersion. Here, again, we can form some judgment of the probability of this occurrence from familiar observations. Instances are known in which weighable quantities of metallic deposit, consisting of fine solid particles derived from zinc- or copper-smelting works, have been obtained from leaves and foliage collected many miles away from the chimney responsible. Again, in the case of alkali works, minute floating particles of moisture, saturated with acid gases, are carried long distances in the air before they fall; and it frequently happens (particularly on cold nights, accompanied by very gentle breezes) that the air-current containing these acid particles pitches, as it were, on a limited area a mile or two away from the works, producing a strictly localised destruction of vegetation. In such cases, the alkali company does not question aerial convection: it pays compensation. A more familiar instance, and perhaps a better analogy, is a factory chimney giving out smoke on a comparatively calm day. The eye can trace the smoke particles, along lines of steady air-currents, in their ascent, in their deflection from the chimney, and in their descent. The smoke can be watched as it nears the ground; it may

be close to the chimney or a mile away from it, spreading out and depositing its particles over a comparatively small area. It may be remembered, in connection with this analogy of a chimney, that the volume of air escaping from the wards of a small-pox hospital is usually large, and that the difference in temperature between the hospital and the outer air is often considerable. The effect of this temperature difference, on a calm day, would be to carry floating particles in the first instance to a considerable height above the hospital.

The chimney analogy is also useful in considering the limitations which the facts require us to place on the operation of aerial infectivity. Individual dwellings, groups of dwellings, or institutions, may be in the neighbourhood of the factory chimney, even close up to it, and yet the accidents of air-currents have never, throughout several months, brought them at any one time more than a few unnoticed soot particles. In other dwellings, or groups of dwellings, it may happen perhaps twice in one day that the inhabitants have cause to ask "where these blacks are coming from?"

Much more could be said on this subject, but the above considerations, I think, sufficiently indicate that the assumption that, on occasion, particulate matter from the hospital descends in the neighbourhood without a great degree of dispersion is in no way exacting. There would, indeed, be strong grounds for disputing a contrary proposition.

From *a priori* considerations, however, we should expect these occasions to be relatively infrequent. There are many obvious reasons which make it impossible to believe that they continually arise, hour by hour and day by day, in all states of wind, temperature and moisture. This point, though self-evident, is important: the theory of aerial convection, as an explanation of the known facts of small-pox hospital influence, requires that the occasions on which infection can be carried aerially should be relatively infrequent. This consideration, though fully dealt with by Mr. Power, appears to have been missed by some writers on the subject, who seem to regard acceptance of aerial convection as implying that at all times, when a hospital is receiving acute cases of small-pox, infection is being conveyed aerially to all parts of the hospital neighbourhood. For these writers, acceptance of aerial convection would apparently require us to believe, contrary to experience, that a susceptible person merely passing a small-pox

hospital, in a vehicle or on foot, thereby incurs something worth calling a "risk" of contracting small-pox. In the same way, they appear to claim that aerial convection, if it occurs at all, must, during an epidemic, affect every house and every institution near the hospital. They are not content with a house-invasion rate of 5 per cent.; they demand that it should be 100 per cent. Having made that demand, they usually point to the escape of the 95 per cent. as evidence that aerial convection has not been operative. And, in conclusion, they say that Mr. Power's theory "has been greatly exaggerated." The exaggeration is all their own.

If, on the theory of aerial convection, the infectious ability of a hospital in regard to its neighbourhood occurs only from time to time, and in suitable meteorological conditions, we have still to ask, in order to complete our inquiry, whether such suitable conditions did obtain on occasions when aerial convection was operative round the hospitals of the period 1900-4. Unfortunately, this question cannot be answered with any certainty. The difficulties in ascertaining, on the one hand, the facts regarding the date and hour of infection of a given case; and, on the other hand, the meteorological conditions of that date and hour, are insuperable. All that can be done is, where possible, to fix upon certain days on which aerial convection appears to have been specially manifested, and then to ascertain the general weather conditions of those days, as recorded at the nearest observatory. Some facts are recorded which enable this to be done in the case of Belvedere Hospital in 1900 and 1901, and in the case of the Hospital Ships at Long Reach in 1901-2. Examination in the above sense of the limited data available indicates, in both instances, that when aerial convection appears to have been infecting persons living near the hospital, the general type of weather was consistent with periods of calm or light winds, sometimes accompanied by fog. I have not met with any instance in which the days coming specially under suspicion were characterised, according to the records available, by persistent strong winds or by practically continuous rainfall.

The question can also be approached in another way, by inquiring whether the direction of prevalent light winds accords with the position, as regards the hospital, of neighbourhoods which have been specially affected. In this connection I would note that in Long Reach light winds blowing from west-south-west to east-north-east, *i.e.*, from the Hospital Ships towards Purfleet, are apparently fre-

quent. In the case of Sheriff Hill Hospital, also, it was noteworthy that Windy Nook and other parts of Felling which were specially affected by small-pox, lie in the path of south and south-west winds, blowing from the hospital; and that winds from this quarter are here frequent and are commonly light.

In brief, the conclusion of this paper is, that the experience of the recent epidemic period, 1900-4, has again afforded us a remarkable demonstration of "small-pox hospital influence"—a demonstration wholly in accord with the experience of previous epidemics. For the consistent explanation of this "influence," we cannot, in our present knowledge, do without the thesis of aerial convection. On the contrary, the case for accepting this hypothesis has been strengthened. We have reached a position, indeed, in which we cannot be content with any mere negation of aerial convection, and cannot pass the matter by with the convenient phrase, "Not proven." Epidemiology, as a science, requires us to continue to accept it, and to act upon it, as a satisfactory working hypothesis, until we can be shown some other and more probable explanation, which is applicable with equal consistency to the numerous and varied occasions on which hospital influence has been manifested during the past five-and-twenty years.

DISCUSSION ON DR. BUCHANAN'S PAPER.

DR. C. B. KER: I should like, in the first place, to say with how much admiration I have listened to Dr. Buchanan's Paper. I am bound to admit that I have listened to him to-night feeling much more predisposed in favour of aerial convection than hitherto. I own frankly that my views have always before been rather contrary to this hypothesis. But I do not wish to make too many criticisms: I thought it would be interesting to give you just the personal experience we have had in Edinburgh during the small outbreak we had last year.

The Edinburgh Hospital is, I regret to say—and I say it with all due deference—deliberately situated in defiance of every recommendation of the Local Government Board. It marches with the ordinary Fever Hospital for Edinburgh—that is to say, the grounds of the two hospitals adjoin. This hospital has a population, including staff and patients,

of about 450 people. On the other side is one of the poor-houses for the city of Edinburgh, which has a population of about 1000 people; while to the back of the hospital is a hydropathic establishment, which is a place of large resort, and has a population of about 200 people. Now, all these places have their boundaries practically adjoining. The distance from pavilion to pavilion of the Small-pox Hospital and the Fever Hospital is a matter of 72 yards; the nearest point of the actual buildings of the poorhouse to the nearest pavilion of the Small-pox Hospital is about 325 yards. The paupers in the poorhouse, many of whom are employed in agricultural work, work right up to the boundary of the Small-pox Hospital, which is quite close to the Small-pox pavilions. During the time of the outbreak a large number of these paupers were engaged close up to the hospital boundary, and a large number of general patients in the Fever Hospital were equally close on the other side.

In addition to this, if you take a larger area, you come to some other institutions. There is the Craig House Asylum, with 325 people. Within the mile area, and in the direction to which the prevailing wind blows, there is a population of 3000 people. In the other direction there are two villages, one with a population of 520 people, and the other with a population of 730.

The interesting point is that, with the exception of the poorhouse, there were no cases in this area. Dr. Harvey Littlejohn investigated three cases in the poorhouse, and he assures me that these were quite satisfactorily explained, two of them coming from infected lodging-houses. The other poorhouse, moreover, four miles away, had the same number of cases. With the exception of those cases, there was only one case within a mile of the hospital. The hospital was open for some time, but it had not a very large population. The average number of small-pox patients for a period of two months was about fifty-five. But Dr. Buchanan has pointed out that, with an average no larger than that, the influence was felt around Belvedere. Again, I gather that there were only sixty patients in Priory Road Hospital, in Liverpool, when the influence began to be felt. Therefore, I assume that we must allow that fifty patients can be dangerous; and if that is the case, it is an interesting fact that we had this population of fifty-five for two months consecutively (and if you take the three months, the average was forty-seven patients), and yet there was no case of small-pox within a mile, even with this considerable population.

A MEMBER: What about vaccination?

Of course, the Fever Hospital staff is adequately vaccinated, and most of the patients, being children, recently vaccinated. But the pavilion that stands within 72 yards of the small-pox pavilion is the erysipelas pavilion, which contains nothing but adults; there are no children in it. It also contains a class of patients of the lowest type, and the least likely to be re-vaccinated. I don't think anyone would say that you could safely vaccinate an erysipelas patient. Not a single precaution was taken during these three months, and the erysipelas wards were full all the time.

The next nearest were the typhoid wards; we took no special precautions, except as regards the staff. In the same way, there were no special precautions taken in the poorhouse, except in the case of the persons who had been in contact with the cases that occurred.

The population towards Edinburgh in the direction of the prevailing winds is about 3000 people, and embraces a pretty good residential district. Of course, we pride ourselves on being better vaccinated than most large English towns, but that is a sufficient population to contain many people not sufficiently re-vaccinated. I say that is an interesting negative case. The question naturally arises: Is there any reason at all why this should be? Is there any difference in the way, for instance, in which the Edinburgh Hospital is managed, or anything to account for this in any way? The only thing that I do differently from other people is this: we have one rule with regard to the nursing staff which we keep absolutely rigidly. The nurses in the Small-pox Hospital are not allowed to go beyond the hospital grounds, in any circumstances whatever. Every nurse in the hospital volunteers for small-pox service, and she remains for a period of six weeks. She knows when she goes to the Small-pox Hospital that she will not be allowed to leave it until the six weeks are up, unless she breaks down. Only the best nurses are allowed to go—the most capable and trustworthy—and it is considered an honour to be allowed to go. No one goes in and out of that hospital except the medical officer. I am the only person who enters and leaves the hospital, except the ambulance attendants. This probably does not amount to very much, but that is the only thing in which we differ from the majority of other small-pox hospitals. That is the advantage of having your small-pox hospital near to your ordinary fever hospital; you can select your nurses in

this way. We do not get together a haphazard staff for an epidemic, as is often done; it is a privilege which only the best nurses are allowed to partake of. Of course, there is always a chance of leakage. I do not think in other ways we manage better than other people; but we put nurses whom we know are, at any rate, particularly conscientious and suitable people to put there. In that way, we have perhaps a better chance of preventing leakage. Of course, one has seen dreadful things in different hospitals. In the old hospital it was, on one occasion, necessary to have a cordon of police to prevent people shaking hands over the walls. In our present hospital they cannot possibly do that. We have more control than we used to have.

Just outside the mile limit there were about three or four persons infected, and in three out of the four cases there was what I might call satisfactory exposure; in the fourth case we could not trace it in the least. I do not know how it came, but in any case that house was just over a mile away; it was a very difficult case to explain. If the infection was aerial, it had passed harmlessly over the heads of 4000 persons to reach the infected house.

The only other point that I will trouble you with—or, perhaps, I should say the only deduction I should like to draw—is: Are not the regulations of the Local Government Board a little vexatious? I think they actually mention poorhouses and fever hospitals, but here we are actually wedged in in a strategical position between these three public institutions; and yet, as far as we can make out, with an average of fifty patients in our hospital, there has been no spread in its neighbourhood. I am prepared to admit that if we ran up to a couple of hundred, as at the hospital ships, it might be different. But why should we prepare for a large epidemic? If you isolate rapidly, and vaccinate fully, surely fifty is enough to start. Why should unfortunate municipalities have small-pox hospitals at the end of the world, where they cannot be properly supervised? You cannot get supervision with a temporary staff, and temporary doctors, and everything else temporary. You can for 150 patients, because you have then to get a big staff; but I am thinking of the chance of leakage in a small hospital of ten, or fifteen, or twenty cases, put right out into the country. It seems to me that it is much better to have them under the eye of some competent person nearer to the town.

I have brought a map with me, because I thought it might interest some members of the Society, as showing,

the locality in which the hospital is situated. It is an ordnance map of the city of Edinburgh, on which the old and the new hospital are both shown. I was formerly in charge of these buildings of the old hospital, which was in the centre of the city of Edinburgh.

We used to be abused for causing small-pox in this neighbourhood where our old hospital stood; but note that the cases are in the same neighbourhood now, and that they decrease in numbers the further we go from this hospital, empty though it was. There were no special antiseptic precautions taken. I do not think we took any special measures in that way. The dust is destroyed, but, of course, a great deal must escape.

In conclusion, I do not in any way wish to deny the possibility of air-borne infection. My contention is that, for practical purposes, it may be neglected. It is not the best way to secure efficient administration of small-pox hospitals to allow those in charge of them to plead aerial convection, if the hospital influence is at all felt. The acceptance of the theory is bound to weaken the sense of responsibility in even the most conscientious of us.

DR. NEWSHOLME: I am sure that, in the first place, we shall agree in heartily expressing our appreciation of Dr. Buchanan's Paper: which shows that he has, to a very considerable extent, inherited the happiness of phrase and lucidity of expression which always distinguished the writings of the late Sir George Buchanan.

My own position in the matter of distal aerial convection of small-pox is that of an agnostic. I have no prejudice against it; I do not, however, regard it as proved. I wait for confirmatory evidence, and I incline to think that such evidence of a satisfactory character will not be forthcoming. I believe in the occasional operation of "hospital influence." I agree that there have been instances in which there has been excessive incidence of small-pox around small-pox hospitals; but I believe that when this has occurred, and is not due to the accidental sweep of the epidemic, it is most likely to have been caused, not by distal convection, but by the same cause as has produced epidemics of small-pox remote from such hospitals, viz., personal contact.

My beliefs are open to correction by evidence; but we are asked by Dr. Buchanan not merely to modify the strength of our beliefs, but to admit that

"We have reached a position, indeed, in which we cannot be content with any mere negation of aerial convection, and cannot pass the matter by with the convenient phrase 'not proven.'"

To such an admission a stricter examination of the evidence is necessary than would be required for determining a question of mere increase or decrease of probability; and the matter is further complicated by the association of Mr. Power's name with this hypothesis of aerial convection. Had Mr. Power time to attend these meetings, he would be made to realise the depth of respect in which his work, as well as he himself, is held. It is extremely difficult to disagree with a hypothesis which has received his endorsement, and, at least in its scientific form, is his own creation. That an infected house is a potential source of infection, that a hospital is a house predestined to infection, must be admitted as facts justifying all practicable means for separating the hospital from the town, even though we may recognise that a hospital should have means of controlling its infectivity which a private house cannot enjoy. The question would therefore be scarcely more than academic, were it not for the facts that precautions such as would be necessary on a hypothesis of distal aerial convection may cost a price which, if the hypothesis were incorrect, and hospital infectivity were controllable by improved administration, might be spent to better advantage to the public health, which constantly calls for more sacrifices than an ordinary community is able or willing to make.

I think we shall take a clearer view of the relative probabilities of distal and proximal convection, if we look at the history of the theory of aerial convection as an explanation of the spread of disease. I need not do more than refer to the miasmatic theories formerly held with regard to cholera and malaria. The theory of aerial convection was held even with regard to syphilis; and so late as 1881 a similar hypothesis was put forward by the late Dr. Airy for diphtheria; and he suggested that this disease could travel aerially three, six, twelve, eighteen, and even possibly thirty miles, and brought forward a number of instances in which coincidences were more numerous than doubtful cases. Dr. Airy's summing-up is very interesting. While admitting that his arguments and instances do not suffice to establish the theory of aerial spread of diphtheria, he adds:—

"I think they are sufficient, in conjunction with earlier observations, to give the theory a right to be disproved, and pending disproof, to be provisionally accepted."*

* *Trans. Internat. Med. Congress, London, 1881; Dis. of Children* p. 69.

I place this transference of the burden of confirmation or disproof of an important induction to those who oppose his views, in conjunction with a similar remark by Dr. Buchanan.* He says:—

“I am struck by the absence of evidence that the problem has been carefully studied, in instances comparable to those I have mentioned, with the result that no graduated incidence has been observed.”

While Dr. Airy's hypothesis has never been subjected to the process of disproof which he challenged, it may be taken as common ground that it has not received the acceptance of epidemiologists; and I think that both writers have underestimated the order of probability which should be established in favour of a hypothesis, before a burden of disproof can be held to rest on those who are unable to accept it. In the case of a distal aerial convection, of which the operation is conditioned as described by those who accept it for small-pox, there is a special objection to the demand for disproof. Taking the hypothesis as stated by Mr. Power and those who have followed him, it requires for its operation the simultaneous presence of many conditions, some of which are even now imperfectly specified and inadequately recorded; and as a matter of historical fact in the criticism of this hypothesis, any attempt to lay stress on epidemics of which the figures have not conformed to its requirements has been met by the reply that some essential condition must have been absent.

Distal aerial convection has, in point of fact, been a favourite preliminary hypothesis in the ætiological discussion of many diseases; but with extension of our knowledge it has been discarded successively in regard to ague, cholera, diphtheria, and influenza. The instance of influenza is the more striking, because the hypothesis of aerial convection had held the field for several centuries, until the investigations of Dr. Franklin Parsons disproved it. Distal aerial convection to-day remains as the occasional explanation of the spread of one disease only—small-pox.

It is common ground that personal infection is the most important and most frequent means of spreading small-pox. The balance of probability compels us to believe that in the instances in which personal infection cannot be traced, it does nevertheless operate; unless we find, after the most rigid investigation, that there is proof to the contrary. If circles and zones showed graduated incidence of small-pox

* *Journal Roy. San. Instit.*, vol. xxvi, No. 4, May 1905, p. 205.

in the population around a small-pox hospital, and if the hospital could be shown to be strictly self-contained, without traffic in or out, the case for aerial convection would be demonstrated, assuming that the circum-hospital infection were shown not to have occurred from other external sources. Unless there are strong practical reasons to the contrary, we are bound in the absence of proof to accept the conclusion to which the greater weight of probability attaches. In a completely self-contained hospital circumstanced like the Small-pox hospital at Edinburgh, as described by Dr. Ker, we should under the circumstances named above have no hesitation in accepting aerial convection as the method of spread, if spread occurred. But it did not occur; and at other hospitals from which spread apparently has occurred we have the choice between three suppositions:—(1) Aerial infection; (2) personal infection by means of patients or the staff of the hospital and the traffic to and from it; and (3) personal infection from others, absolutely independent of the hospital, and in the normal course of an increasing epidemic. I propose to confine my further remarks to the additional evidence which Dr. Buchanan has adduced from the experience of recent years.

As to the second means of spread, we know that its operation can seldom be excluded without difficulty, even in the best-administered hospitals. Dr. Buchanan commits himself to the statement that no surreptitious visits were made at Purfleet from the hospital ships, and he claims to speak with the authority of most careful local inquiry. I do not think that this opinion of Dr. Buchanan's, formed as it was on the basis of an inquiry made some time after the event, can prevail against the considerable body of independent evidence, including that of Dr. Thresh's report, that such visits occurred; and it must be remembered that those who broke the regulations in this manner would not be likely to have given to an inquirer information which would convict themselves. In a large hospital it is very difficult to prevent such offences. But even if the isolation of the ships at Purfleet had been complete, it must be remembered that the epidemic was in vigorous progress at Erith and other places having traffic with Purfleet before it declared itself in the Orsett Union; and it not only would be exceedingly difficult to show that the Purfleet infection was not originally imported by personal infection from surrounding districts, but no serious attempt appears to have been made to do so. As an explanation of the

Purfleet (Orsett Union) outbreak, which consisted from first to last of only 52 cases, the amount of infected traffic, whether from the ships or from other districts, need only have been small. Probably a dozen primary cases would have sufficed; and it seems to me impossible to say that the presence of infection imported to that small extent is excluded by any of the facts published as to this epidemic. The effect of such importation would have been the more powerful if some of the earlier cases had been—as were the first case at Grays and others at Erith—"missed" cases. The Purfleet experience is, in fact, an example of the manner in which an explanation of the facts is possible without recourse to the hypothesis of aerial convection: and it seems to me to illustrate fairly the view that, in the absence of an exact history of each individual case of small-pox, such as can rarely be secured by an investigation late in the outbreak or after its close, the question is one of choice between two conjectural methods of convection: of which one is known to be potent in the diffusion of small-pox, and solely responsible for the diffusion of many other diseases; while the other has been displaced even as a working hypothesis from the ætiology of all diseases except small-pox, and is supported largely in respect of that disease by the allegation—in the case of Purfleet seen to be ill-founded—that no other explanation will cover all the facts.

In one respect the case of Purfleet lacks one element of the reasoning by which the hypothesis of distal convection is supported. It is alleged that hospitals which are surrounded by inhabited houses exhibit their infectivity by determining an excess of cases as compared with other parts of the infected district, and that this excess shows a characteristic graduation in the number of cases within distances varying from $\frac{1}{4}$ mile to 1 mile from the centre. Purfleet has no such complete inhabited environment; but in other recent cases, of which Liverpool is the most conspicuous, it is suggested that this characteristic graduation of an excess of cases is shown as clearly as can reasonably be expected in a phenomenon in the production of which there are admittedly other contributory factors. In some of the cases on which stress is laid, I do not think that, even after making allowance for the interference of other factors, the correspondence is close enough to justify the description of the graduation as characteristic. It should be added that, *even if such a graduation were closely made out, it would give no help in determining whether the*

infection was conveyed aeriaily or by personal traffic. A group of infective particles despatched from a centre would fall with similar distribution, whether they were shed by a man walking along streets and calling at houses, or by a man in a balloon, or by a current of air passing over the same path; and the density of infection on any area would in each case vary directly with the distance from the centre of infection, and inversely with the superficial area and with the number of houses. It would, therefore, be necessary to eliminate the effects of hospital traffic as a contributory cause before any excess-incidence in the circum-hospital area could show the operation of distal convection. This elimination is the more difficult because, although the precautions in some cases may leave it possible for distal convection to occur, they may, on the other hand, be carried far enough (as apparently in the case of Manchester) to oppose a notable hindrance to the diffusion of infection from the hospital by any means of convection. The question, therefore, to which an answer can be sought with most probability of success is whether, in fact, any such excess has been shown; and it must be remembered that if such an excess is shown, it still remains possible that it is due to hospital traffic, and capable of being controlled by improved administrative precautions. I do not propose to examine all the figures quoted by Dr. Buchanan, because the consideration of those of Liverpool, on which he perhaps lays most stress, will indicate the difficulty which I find in accepting such statistics as warranting the inference that hospitals determine an excess of small-pox in their neighbourhood. Dr. Reece's report on small-pox in Liverpool presents twenty-three spot maps, showing a density of infection round the hospitals which is undoubtedly high as compared with that of Liverpool as a whole. Such maps are obviously not an accurate measure of relative infectivity, because the density of infection in an element of a district depends on the number of houses as well as on the superficial area. Taking these spot maps, however, as being intended to give a rough-and-ready clue to the distribution of the disease, it is necessary, in the first instance, in order to determine whether there is an excess incidence of cases in the circum-hospital area, to ascertain whether similar densities can be observed in other areas of those dimensions. Both results would, for accuracy, need to be corrected for density of habitations; but the use of spot maps assumes that the absence of such corrections does not prevent the drawing

of valid and useful inferences from the density per unit of superficial area. I think, myself, that they may properly be used in this way for preliminary inferences, subject to a reservation to which I shall refer later; and I have in like manner made a study of Dr. Reece's maps, with the object of seeing whether the circum-hospital density, as shown on the maps, was, in fact, beyond what existed elsewhere. I found that the infection-densities in the mile radius in regard to each of the two large small-pox hospitals were notably less than those within circles drawn from centres over a mile distant from each hospital; and I append the figures which are thus disclosed, compared with the circum-hospital figures.

					New Houses Invaded.				
Miles	0 to $\frac{1}{4}$	$\frac{1}{4}$ to $\frac{1}{2}$	$\frac{1}{2}$ to $\frac{3}{4}$	$\frac{3}{4}$ to 1	Total.
<i>Priory Road Hospital.</i>									
1.	In circles and zones from the	hospital as a centre	1	30	61	121	213
2.	Ditto, from a point over a mile	W.S.W. of the hospital	44	138	215	182	579
<i>Park Hill Hospital.</i>									
1.	In circles and zones from the	hospital as a centre	9	75	64	88	236
2.	Ditto, from a point over a mile	N.N.W. of the hospital	47	124	132	190	493

(Dr. Hope informs me that when circles are drawn around the empty Netherfield-road Hospital, circles and zones showing graduated incidence are obtained.)*

It appears, therefore, that in Liverpool the area on which, as shown by the test furnished by spot maps, the maximum density of infection—or, at least, a density enormously larger than round either of the hospitals—occurred centred round points which were over a mile away from any small-pox hospital.

At first sight this result is surprising when we consider the enormous differences in density of infection shown in Dr. Reece's table, comparing the circum-hospital areas with the rest of Liverpool. This discrepancy arises from the fact that the density of infection over the whole of Liverpool, excluding the hospital areas, cannot legitimately, in my opinion, be compared with the density of infection over

* It is perhaps scarcely necessary to repeat that the figures in the above Table, for which corrections for house-density cannot be made, are only used like spot-maps as a preliminary test.

any of the special areas; because, as a matter of fact, and as the spot maps clearly show, the infection did not at any time attack more than a relatively small part of Liverpool. The comparison instituted in Dr. Reece's figures is thus between districts which admittedly were at substantial risk from the infection, and districts which were at little or no risk. For the densities of infection of different areas to be comparable, it is obvious that whatever may be the intensity of the infection at work in each area, as measured by its absolute density in that area, the infection must in each case be so distributed that the maximum deviation of density in any element of one area from the mean density over the whole area is the same as the corresponding maximum deviation in the other area. A reference to Dr. Reece's spot maps will show that this test, or any test which does not propose to include districts which were free from infection in the areas chosen for comparison with the hospital areas, will exclude so much of Liverpool from the area with which comparison can properly be made as to deprive the figures relating to the "rest of Liverpool" of any sort of significance in the comparison.

I may point out in passing a somewhat similar—though less wholesale—source of error. This occurs when the density of infection in a small area into which a highly-infectious contagium is introduced is compared with that in a larger area. In Purfleet, for instance, the introduction of a few cases, particularly if "missed," would naturally cause a sufficient number of secondary cases to make a formidable density of infection when calculated on the minute population of the village; in the same way as one or two "missed" cases in a common lodging-house in a large town may determine an outbreak of as many dozen cases, which, if reckoned on the population of the immediate environment, would give a wholly misleading picture of the morbidity of the entire town, or even of one of its wards.

I am bound, therefore, to submit to you as the result of the considerations which I have outlined, that the positive evidence derived from recent experience in support of the hypothesis of distal convection adds little, if anything, to the strength of the case so brilliantly advanced by Mr. Power. And when account is taken of the increased facilities given by compulsory notification, as well as by the mere weight of years, this result can scarcely be interpreted otherwise than as a distinct weakening of the probability that the hypothesis is true.

I am not prepared to urge that a method of convection

which, it is suggested, requires special adjuvant circumstances should appear in every epidemic; but the high proportion of epidemics which have failed to show evidence of aerial convection is a fact of unquestionable significance. When one recollects the considerable periods over which small-pox epidemics commonly last, and the great variety of meteorological and other conditions which occur during this period, the number of negative cases seems wholly disproportionate to what would be expected were the hypothesis correct. My difficulty is in nowise diminished by Dr. Buchanan's observations on this point.

No valid comparison can be made between the failure of a high percentage of small-pox epidemics to disclose evidence of aerial convection, and the fact that in a scarlet-fever epidemic due to milk or some other single infection, perhaps only 10 out of every 100 persons are infected. The 90 persons in the single epidemic escape by chance, or through insusceptibility, or for some other reason; and as these circumstances will probably appear in different combinations in different individuals, the infection attacks a widely heterogeneous body, and will naturally give widely varying results. In a group of 100 epidemics, on the other hand, we have by hypothesis 100 suitable populations, of which the susceptibility and fitness for the disease are shown by the occurrence of the epidemic. The units are not individuals but populations, and in the absence of overwhelming evidence to the contrary, they must be regarded as—and almost certainly are—homogeneous. The variations which occur in this case cannot, therefore, be explained by assuming individual variation among these units. They can only be explained, consistently with the hypothesis of aerial convection, by assuming that in the percentage of failures the necessary combination of meteorological conditions has been absent throughout all those epidemics which have failed to conform to the hypothesis. Whether this assumption is justified or not is a matter on which no great experience of the British climate is required for forming an opinion; but it is a totally different assumption to that on which an infection would be expected to attack all or none of the individuals of a community; and I think that the evidence in favour of the latter needs considerable amplification before the assumption can safely be made.

DR. NIVEN: I shall endeavour to go very straight to the point in what I say. The influence of small-pox hospitals

in producing an increase of small-pox around the hospital appears to me beyond the need of proof; and I believe that the small-pox hospital placed in the midst of dwellings does, in the absence of special precautions such as I shall briefly mention, cause an increase of small-pox in its neighbourhood. It is no answer to take a centre, perhaps in some area of diffusion, place your circles around that, and so obtain artificially-graded figures; that is mere juggling—it really will not in any way meet the question.

In 1893, however, as it happens, I did apply a test, which may be considered a fairly just one, to a diffusion of small-pox which occurred around the Westhulme Hospital, Oldham. I took—not an accidental centre such as I have just referred to, but a common lodging-house in the centre of the town, and drew quarter-mile circles round this common lodging-house. I also took the small-pox hospital as the centre of another set of quarter-mile circles. The percentage of houses invaded by small-pox in an untraced case during 1892 and up to March, 1903, was:—

Within $\frac{1}{4}$ mile of the hospital	4.7
" $\frac{1}{4}$ to $\frac{1}{2}$ mile "	1.5
" $\frac{1}{2}$ to $\frac{3}{4}$ mile "	0.45
" $\frac{3}{4}$ to 1 mile "	0.42

Round the common lodging-houses, the percentage of houses invaded by untraced cases for the same period was:—

Within $\frac{1}{4}$ mile of the lodging-house42
" $\frac{1}{4}$ to $\frac{1}{2}$ mile "43
" $\frac{1}{2}$ to $\frac{3}{4}$ mile "53
And from $\frac{3}{4}$ to 1 mile "22

The same relation in the incidences round the hospital and round this centre held in the previous outburst of 1888.

I consider that to be a fair comparison, and it is obvious that, taking the part of the town which would be most likely to be affected by small-pox, you get no such graduated increase on approaching this point as you get around the hospital.

I suppose no one would seriously dispute that small-pox hospitals do cause small-pox around them. I think it is necessary to have some experience of the working of small-pox hospitals in order to form a just judgment on that question. I should like very briefly to state precisely what happened in Oldham in 1893, there being then a number of acute cases in the hospital. There were two bursts of

untraced cases which occurred, one about January 9th, 10th, and 11th, and the other from January 29th to February 8th. The first burst comprised six cases, and the second burst 21 cases. A fortnight before the first occasion the wind was variable, but embraced the district affected by the cases; on the second occasion, however, it blew steadily for some days, a fortnight before the outburst, towards the quarter of the town which was particularly affected. All these cases were carefully investigated, and could not be traced to previous cases. Now, I should just like to say a word or two on this subject. In speaking of the different causes to which the cases in the neighbourhood of the hospital may be traced, it has been justly said by Dr. Newsholme that the influence of the hospital, in so far as carelessness of the staff was concerned, would produce precisely the same effect on the graduation of incidence around the hospital as a diffusion of small-pox infection through the air. That is quite true if the movements of the staff are capable of producing the effect: I do not believe that they are. We find numerous contacts from cases of small-pox going to work, and no infection appears to result. It is incredible that the staff of the hospital should produce effects which are not produced by contacts from private houses. These contacts with private cases are subjected to precisely the same kind of infection as the members of the staff; their garments are probably more infected than those of the latter, especially as the staff are obliged to change their clothes before leaving the hospital. Nevertheless, it is rare that you get any cases following the visits of these contacts to works. Although we cannot absolutely exclude contacts as a cause of small-pox, it is so small that it may be disregarded in this connection, and I do not think we can seriously think of that as a possible cause of small-pox outbursts around the hospitals. I should like to press this point a little further. Had small-pox been carried by the staff to cases outside, it is not to be believed that the sanitary inspectors would have failed to produce evidence of contact between members of the staff and at least one case. Yet no such evidence is forthcoming.

Now Dr. Newsholme says that you cannot get an absolute demonstration of aerial diffusion from a small-pox hospital; but I think that what I am about to show you comes as near to being an absolute demonstration as it is possible to get in matters of this description.

I have here a series of maps showing, as from the

1st of January, 1894, the incidence of small-pox in Manchester week by week, from which it is seen that there were three cases—perhaps none, perhaps one, two, three, and so on—down to April, and then they begin to get a little more frequent; very few of them indeed, however, occurring in the district where the hospital was situated. Some of them undoubtedly did; and I do not see how you are to exclude these as not having been caused by the hospital. I will read a few particulars, and then show the maps.

Week ending	April 7th	...	3 cases in the town.
"	" 14th	...	6 " "
"	" 21st	...	5 " "
"	" 28th	...	16 " "
"	May 5th	...	4 " "
"	" 12th	...	6 " "
"	" 19th	...	2 " "
"	" 26th	...	5 " "

And then this happened. (In the week ending June 2nd, there were 39 cases, 19 of which were scattered round the hospital.) Here is the Small-pox Hospital (indicated on map), and here are the various fever pavilions. The fever hospital is separated from the adjoining district of Harpurhey by a brook, which was practically never crossed, over the greater part of its course. There is but little commerce in this direction. The wind on the 11th, 12th, 13th, and 14th May was variable, and probably boxed the compass. (The direction and force are taken only at 9 o'clock in the morning.) On the other hand, it was blowing on some days under investigation in this direction, and this, and this. You will perceive that, coincident with these outbursts all round the hospital, you have these five cases occurring in four wards, widely separated, three in the scarlet fever wards, and one in the enteric ward. Now, the two sides of the hospital—the fever and small-pox—were kept absolutely separate, except for the visits of the medical superintendent and matron, who went first into the fever wards. No small-pox had occurred in 1894 in the fever wards previous to these outbursts, so far as I can ascertain; and here you have all these cases occurring at one time, and coincident with the outbursts around the hospital outside, part of which external outburst lies external to, but on lines connecting, the small-pox hospital and the fever wards. The five internal cases occurred, two on the 26th, two on the 28th, and one on the 30th May. In the district round the hospital there occurred on three days, May 26th, 27th, and 28th, fifteen fresh cases of small-

pox, only one of which could be traced to a previous case. Of these fifteen persons, six were housewives, three others were women at home, one aged seventy-three, one a dress-maker, and the third a newsagent. Only five of the fifteen were males. The houses in which they occurred were, many of them, quite out of the beaten track, and away from the approaches to the hospital. The staff of the hospital would generally come down the Oldham Road (in this direction). They might cross the fields, and pass down (in this other direction), but there is no reason to suppose that any of them had small-pox; and it is altogether out of my experience that persons not suffering from small-pox should cause any appreciable amount of small-pox.

Now, you have to afford an explanation of all this, and I say that absolutely no explanation can be given unless you accept aerial diffusion, which is as near as possible to an exact explanation as you can have.

A MEMBER: How near were those wards to one another?

DR. NIVEN: I am afraid I cannot tell exactly, but I can give you a map.

A MEMBER: Can you tell us within a quarter of a mile?

DR. NIVEN: Oh, they were quite close; the grounds being simply separated by a hoarding. The distance of the invaded wards from the small-pox hospital would be about 500 ft. If you consider that you have two such coincident occurrences, you have to find some cause for these cases outside which will also explain the simultaneous cases inside, and I submit that absolutely nothing will account for such an occurrence as this except the carriage of small-pox in the air. Of course, if you could explain these cases by direct infection, it would be another matter, though, even then, the double occurrence outside and inside the hospital walls would cast doubt on any history of direct personal infection cases. As I have said already, I think that the occurrence at Monsall Hospital in 1894 is as near a positive demonstration as it is ever possible to get on such a matter. Personally, I should regard my experience as conclusive for practical purposes.

I came to the same conclusion with regard to the outburst of small-pox round the small-pox hospital at Oldham, which occurred in 1893. I had had previous experience of diffusion in 1888, when Westhulme Hospital was accused of radiating small-pox, and in my capacity of Medical Officer of Health I repudiated that view. I went to every

house in which small-pox had occurred, and I certainly picked a good many holes in the statement which had been put forward, so much so that the Local Government Board considered the charge "not proven." But although that was so, there remained a doubt upon my mind whether, taking all the facts into consideration, these cases might not after all have been due to the hospital. Then, when we got such a sweep of small-pox as that occurring in Oldham in 1893, which we could not trace to previous infection, or find any explanation of, I felt that I must give up the battle. We, therefore, moved all the recent and fresh cases out into the country, still retaining Westhulme for convalescents. The result was that there was practically no small-pox around Westhulme Hospital in 1894. The same thing happened in Manchester in 1894. We moved all the acute cases from Monsall, still using Monsall for convalescents. The cases then ceased to occur in the district of Newton Heath, in which Monsall Hospital is situated.

I had previously been persuaded—perhaps too easily—that patients could not conveniently be oiled: that it made them sick, and that the other inconveniences were very serious. But I considered that, as we had to treat the acute cases at Clayton, we ought not to run any risk of having small-pox diffused; and from the beginning of treatment of acute cases there, in 1894, we had all the patients in the acute stage oiled daily. For the last two or three years they have been oiled twice daily. I have recently been making inquiries as to this practice, and I think perhaps I may have laid too much stress upon the difficulties. I am told that the patients who are not too seriously ill oil themselves, and that they like it; in fact, they find the irritation of the skin reduced by the $2\frac{1}{2}$ per cent. carbolic oil. Of course, oiling of the very bad cases is rather unpleasant for the nurses. I have no doubt that the disease has been prevented from spreading to the houses in the vicinity by this means, and I have also no doubt that the same mode of treatment would again suffice to prevent the diffusion of small-pox. It is, at all events, a refuge for people in difficulties, who cannot get a hospital sufficiently removed from centres of population.

DR. HOPE: Unfortunately, I was not able to be present when Dr. Buchanan's Paper was read, but he has been good enough to send me a copy, and I have given it very careful study.

I should like to say that nobody doubts the ability of small-pox hospitals in this country to cause the spread of small-pox in their vicinity—there is no question about that, I take it. The question at issue is whether the presence of small-pox in the vicinity of hospitals is to be ascribed to aerial convection, or whether it is due to contact with infected persons or things either from the hospital or from other infected sources: whether, in fact, its presence in the vicinity of the hospital is not due to precisely the same causes that give rise to it in parts remote from the hospital.

At the outset of his Paper, Dr. Buchanan mentions the part played by acute cases of small-pox. Now it is to acute cases of small-pox that the mischief is ascribed, and I should like Dr. Buchanan to give us a definition of an "acute case." All small-pox at the outset is acute, as everybody knows; but when modified by vaccination the acute symptoms pass away soon, and the patient becomes convalescent. The risk of infection in these cases is inconsiderable, unless the patient is in close and intimate contact with susceptible persons; in such ways, for example, as living in the same house, sleeping in the same bed, travelling in the same carriage, or working in the same room.

On the other hand, in unmodified cases of small-pox, long after the acute stages have passed by, the patient in hospital is eminently infectious; indeed, the ancient Chinese method of inoculation was by means of powdered scales or scabs from an infected person. So, when patients are transferred from one hospital to another, they may be in an infectious condition or they may not. The term "acute cases" is, therefore, an unfortunate one, and conveys very little meaning.

One point in Dr. Buchanan's Paper—a feature also which is conspicuously noticeable in all his Reports—is the eminent fairness with which he gave consideration, not only to the facts which tend to support his views, but those which appear to have an opposite tendency. There is, in fact, in his official Reports not less recognition and prominence given to the causes of spread of small-pox other than aerial convection, than to those circumstances to which the explanation of aerial convection alone appears to him to be applicable. In this respect he has followed the essentials of scientific investigation, as well as the methods to which the Local Government Board Reports owe so much of their value.

The conclusions to which Dr. Buchanan's experience and investigations have led him are, in one important respect

similar to those of Mr. Power, viz., the importance of the peculiar atmospheric conditions which are likely to be conducive to the distribution aerially of infective matter, and to the fact that these conditions are not uniform nor always active; but that they may, from time to time, so combine as to render the transference of particulate infective material as easy as the transference of dust.

With the other outbreaks to which the writer of the Paper refers, viz., those at Orsett, Glasgow, and Manchester, I do not propose to deal, as the short time at my disposal will be fully occupied with the outbreak at Liverpool, and to the Report of Dr. Reece upon it, which is so largely drawn from in Dr. Buchanan's Paper. That Report, upon the face of it, would almost convince any person unfamiliar with the subject, that the Liverpool hospitals had proved a grave source of public danger.

In commenting upon Dr. Reece's Report, I, of course, confine my remarks to those aspects of it to which the writer of the Paper has alluded. There are other features in that Report which, in due course, and in the proper place, will be dealt with.

One of the most remarkable features in connection with Dr. Reece's Report is, that from start to finish there is no reference whatever to any single one of the considerations upon which the Reports of Mr. Power and others, and of Dr. Buchanan himself, laid so much emphasis. There is no suggestion of any other possible cause of the spread of small-pox except aerial convection; and this he assumes to take place without any of the meteorological conditions upon which Dr. Buchanan lays so much stress.

Mr. Power was careful to point out that—

"Small-pox is a disease infectious beyond all others of its class. Not only does it spread with greater facility than, for instance, scarlatina or typhus, but the measures of isolation and other precautions against dissemination which suffice with those diseases are, as regards small-pox, altogether futile."

He then goes on to speak of the spread of small-pox by means of bedding, etc. He then adds:—

"Cases of small-pox, themselves so little serious as to be mistaken for 'chicken pox,' have in our ordinary experience the power of producing in unprotected persons severe attacks of the disease. And other slight cases of small-pox, not mistaken but purposely concealed, do much in all experience to spread the disease in an epidemic form."

Clearly, in investigating the causes and progress of the extension of an outbreak of small-pox, no circumstance should be rejected which has an obvious and distinct bearing upon the outbreak; and in preparing a statement

for the judgment of others, it is well, in matters of doubt, to state the facts on both sides of the question.

I would criticise Dr. Buchanan's Paper so far as it rests upon this report of Dr. Reece's.

I would point out that Dr. Reece, before entering upon his investigation at all, states distinctly that there is only one explanation of small-pox incidence around hospitals, viz., dissemination of infection by aerial convection, and that that explanation is absolutely satisfactory to him. This is his substitute for the temperate and extended view of the question expressed by other writers. So reluctant is he to disturb his satisfaction, that he rejects all the considerations upon which so much stress is laid by Mr. Power and other observers, and makes no reference whatever to the volume of proof laid before him as to other definite sources of infection.

It is clear that Dr. Reece bases his case against Liverpool and his conclusions, upon this assumption and upon maps, carefully prepared, the accuracy of which I fully admit. So confident is he that this view comprehends the entire situation, that no other aspect of it is deemed worthy of consideration. The question may, in the first place, be considered from Dr. Reece's standpoint, and the further evidence considered subsequently.

In the city of Liverpool there are three other hospitals, besides those under discussion, used for the various infectious diseases other than small-pox. I have taken one of these, viz., the Netherfield Road Hospital, and put rings round it in the same manner as Dr. Reece has done round the small-pox hospitals. The concentric zones have been placed round it, and the map has been prepared by the same people who prepared Dr. Reece's maps. And what we find is as described in Tables on opposite page.

These are the zones which have suffered all through the outbreak, and they show more marked gradation than in the case of any of the other hospitals. What would have been said if we had used that hospital for small-pox cases? We might have done so, and if we had, this would undoubtedly have been held up as conclusive evidence—evidence beyond question—of the results of aerial convection. But these cases, which occurred in the neighbourhood of Netherfield Road Hospital, were, in a very large proportion of cases, clearly traceable to other sources, which I have defined and described elsewhere. It has also been proved upon similar evidence, and in the same way, and in an equally large proportion of cases, that the presence of

TABLE showing per 10,000 Houses, and for the period December 8th, 1901, to November 9th, 1903, Houses invaded by small-pox in areas respectively within one mile, within three-quarters of a mile, within half a mile, and within quarter of a mile of the Hospital at Netherfield Road (which did not receive cases of small-pox). The corresponding rate for the rest of the city (including the three hospital areas of the Parkhill, Fazakerley, and Priory Road Hospitals) is given for comparison :—

Period of the Outbreak of Small-pox in the City of Liverpool.	Netherfield Road Hospital Area : House Invasion per 10,000 Houses, within—				House Invasion per 10,000 Houses in the rest of the City of Liverpool, including the Hospital Areas of Parkhill, Fazakerley, and Priory Road.
	1 Mile of the Hospital.	$\frac{3}{4}$ Mile of the Hospital.	$\frac{1}{2}$ Mile of the Hospital.	$\frac{1}{4}$ Mile of the Hospital.	
Dec. 8th, 1901, to Nov. 9th, 1903 (23 months).	163.8	176.7	199.8	311.7	96.1

Netherfield Road Hospital (which was not used for Small-pox).
Houses invaded per 10,000 Houses in each Zone.

Zones.	0 to $\frac{1}{4}$ Mile Houses in the Zone, 4,716.	$\frac{1}{4}$ to $\frac{1}{2}$ Mile Houses in the Zone, 11,944.	$\frac{1}{2}$ to $\frac{3}{4}$ Mile Houses in the Zone, 17,692.	$\frac{3}{4}$ to 1 Mile Houses in the Zone, 14,354.	The rest of the City.
Invaded houses .	311.7	155.7	154.8.	133.0.	96.1

small-pox in the vicinity of the small-pox hospitals had a precisely similar origin; whilst there is no shadow of proof to connect any of them with any other source of infection.

Information regarding small-pox is more easily obtainable than formerly, and especially so in Liverpool. There has been important and special legislation in recent years, and this legislation is not designed merely to supply information, but to aid sanitary authorities in safeguarding the public in the prevention of disease. We have, of course, the Notification Act, which is applicable throughout the country. We have also chicken-pox scheduled as a compulsorily-notifiable disease: and this is a very important thing, because in the recent outbreak fifty-four persons supposed to be suffering from chicken-pox, and who were being treated for that disease, were found to be suffering from small-pox.

But even this legislation is not sufficient to meet all the necessities of the case. Many inquiries have to be made, and it is not uncommon for relatives and friends to give untrue replies to those inquiries. Hence it was that the Liverpool Corporation, in 1902, obtained Parliamentary powers to deal with persons guilty of wilful suppression of the truth, or of giving false information with regard to small-pox; and they obtained important penal clauses, which dealt sufficiently with these offences.

The valuable assistance in tracing disease and verifying the sources of infection which each and all of the foregoing Acts of Parliament now afford, were, of course, not available until the respective dates at which they were passed; but they were all, especially the 1902 clause, of great assistance in tracing sources of infection during the outbreak we are considering. But there is no reference to these aids to investigation, nor to their important results, in Dr. Reece's Paper, although they were placed before him. I shall, therefore, ask for five minutes' indulgence to supplement the omission; but I wish first to refer very briefly to the hospitals themselves.

Taking them in the order in which they have been dealt with in the Paper:—

The Fazakerley Hospital is a new hospital for small-pox, erected within the last few years by the Liverpool Corporation upon a site which was very carefully considered. The Corporation were desirous to place their hospitals in such a position that they should not be encroached upon by buildings in the future, and that patients should have such aids to recovery as fresh air would afford them. The amount paid for the site, which consists of 130 acres, was close upon £40,000. The Corporation were aided in their selection of a site by an experienced inspector of the Local Government Board, who, in 1898, gave it his warm approval. Subsequent to that, a small additional quantity of adjoining land came into the market; and in order to borrow the money for its purchase, as well as to borrow £20,000 for the hospital itself, another medical inspector of the Local Government Board, of great experience, investigated the subject, and he also expressed his approval of the site in question. Finally, a third application for more money for hospital purposes was made, and a third inspector of the Local Government Board, after investigation, approved of the site. It will be seen, therefore, that the site was the best which foresight could suggest, and which money could procure. Until Dr. Reece's report

appeared, there was no suggestion that this institution had been other than a benefit to the city. It might be, of course, that the three inspectors of the Local Government Board, as well as the medical advisers of the Corporation, are wholly and entirely wrong, and that Dr. Reece is right; but it cannot be expected that his views will be accepted without very rigid inquiry. Clearly, there was more at stake than the mere reputations of those who gave an opinion in favour of the site. If it were unsuitable, there can be but few sites in the kingdom which are suitable for small-pox hospitals at all, for not many Corporations would be in a position to pay so large a sum as £40,000 for a site.

According to Dr. Reece's Table, on page 14 of his Report, the house-to-house invasion within a quarter to half-a-mile zone of the Fazakerley Hospital, was four times as great relative to the city invasion as it was in the quarter to half-mile zone at Priory Road Hospital. What is still more remarkable is that the house invasion in the half- to three-quarter-mile zone at Fazakerley is, according to Dr. Reece's Table, eighteen times as great relative to the city invasion as it is in the corresponding zone of the Priory Road Hospital; further, in the three-quarter to mile zone it is twelve times as great. Yet the Priory Road Hospital falls short of modern requirements: it is not one which comes up to our standards, and is, moreover, in a situation altogether unusual in regard to the population around it. Priory Road Hospital, nevertheless, is less dangerous than Fazakerley Hospital.

It must be mentioned that Fazakerley Hospital has only got nine houses within the quarter-mile zone, and a total of one hundred and seventy-five houses within a half-mile zone of the hospital. Similarly, the number of houses within a mile is relatively very trifling, as compared with the other two hospitals. There were only two houses invaded within the entire half-mile circle, and it is a wilful misuse of figures to prepare Tables which suggest an enormous house invasion upon facts such as these.

Furthermore, the great majority of the few cases within the mile zone occurred, not at the time when the hospital was at its fullest use, but on the contrary at a time when the hospital was least used; whilst they dwindled away to a minimum at a time when the hospital was at its fullest use.

I notice that Dr. Buchanan (very wisely and properly) attaches so little importance to Dr. Reece's Table relating to Fazakerley, as to take no notice of it. I do not know whether this is out of regard for the appreciation which his colleagues have for the site; but it is a wise course,

for, upon reasons which will shortly be more apparent the Table is valueless and misleading, and it is appropriately discarded. I do not say this because a large sum of money was spent upon the site, or because the site was approved of by others besides myself. All of us may have been absolutely mistaken; and, if so, the Corporation would at once cease to use the hospital, and, with the sanction of the Local Government Board, buy another site more suitable, the first object being, of course, to advance the public health and the public safety.

But Dr. Buchanan assigns no reason for his rejection of the Table relative to the Fazakerley Hospital, and his acceptance of those relating to the other two hospitals.

With regard to Priory Road Hospital, which it is alleged has done so much damage, there is one other striking fact, and that is: that the population living within a quarter-mile of it, though not large, is about three times the number which has been regarded as the margin of safety; yet this population of five hundred persons of all conditions, living within the quarter-mile zone of the hospital, has, throughout the entire eighteen months that the hospital was used for acute cases of small-pox, been absolutely free: only one case having appeared within the quarter-mile zone throughout the whole of the period.

Now turn to Park Hill Hospital. Figures are given suggesting an enormously greater incidence on the quarter-mile radius of this hospital than the other one, or, indeed, anywhere else; namely, 526 per 10,000, as against 85 for the rest of the city, excluding the hospital areas. Now 526 is a very large figure to use, and it is a relief to know that it means the invasion of only nine houses during the time the hospital was in use. But prior to the use of the Park Hill Hospital for the treatment of small-pox, the disease had repeatedly been present in its vicinity: in January, 1902, a house within the three-quarter-mile zone was invaded by small-pox; in February, 1902, one house was invaded within the quarter-mile zone, and two houses within the half to three-quarter-mile zone; in March, 1902, a house within the half to three-quarter-mile zone was invaded, and one between the three-quarter and mile zone; in November, 1902, one house in the three-quarter to mile zone was invaded; in January, 1903, three houses were invaded within the half and three-quarter-mile zone, and one house between the three-quarter and mile zone. So that there were twelve separate and distinct invasions of

houses within the alleged zone of influence before the hospital received its first case of small-pox.

During the use of the Park Hill Hospital for small-pox, nine houses were invaded within the quarter-mile zone of the hospital. These nine houses are represented by the figure 526.3, quoted in Dr. Reece's Table, as the instance in which the influence of the hospital is most pronounced. Now, what probability is there of these nine invasions having an origin analogous in character to the preceding twelve: which occurred in the vicinity before this hospital was used for small-pox at all? And if any of them should be proved to be infected in that way, what becomes of the Table and the inferences drawn from it? Four of the nine patients had, it was known, been exposed to infection elsewhere—did they get their infection from this source, or did they get it from the hospital? I will not waste time by quoting these cases: they are in print, and they will be duly put before the Society.

Let us now turn for a few moments to the study of the actual proofs of the sources of infection, both within and without the various zones; for, believe me, the behaviour of small-pox was absolutely identical in both. The Acts of Parliament which I have quoted, in the hands of a staff of adequate size, training, and intelligence, proved what was the actual source of infection in about 1,000 out of 2,082 cases; and out of these 490 were actually found by watching those who were known to have been exposed to infection, before any medical man had been called in. The staff was very much augmented for these purposes. Any person who had been exposed to infection in the centre of the city or elsewhere was kept under observation, and if he exhibited any symptoms he was noticed at once and visited medically. In addition to these, and to the 54 cases which were treated as chicken-pox, a large number of mild and unrecognised cases had been roaming about town within and without the zones, whose illness was only recognised by the fact that they infected members of their families or friends. Were these aerially infected from the hospital? Their infection has been ascribed to it, notwithstanding that their source of infection was the same as that of patients living miles away from the hospital.

I should like to quote from a Report on this particular matter, which I published in 1903:—

"Doubt as to the nature of the illness proved another fruitful source of dissemination, owing to the fact that a large number of cases, modified by vaccination, were regarded by the medical attendant as chicken-pox; it

became necessary to schedule chicken-pox as a notifiable disease, in order that doubtful cases might be visited by a medical officer experienced in small-pox. The value of this measure is confirmed by the fact that fifty-four cases of small-pox, which were under treatment as chicken-pox, were discovered and dealt with. Unfortunately, however, there were still many cases of so mild a type that no medical man was called in; and it was only after the severe infection of others by these cases that the real nature of the disease was evident. A further mischief connected with this mild form of the disease arose from the patients going in public conveyances to places of public resort, or to places of business, laundries, tobacco works, tailors' shops, etc., not only infecting those with whom they associated, but infecting articles which they handled. Children and other inmates of their houses were also going to and fro to their daily duties. In several cases the patient, in doubt as to the nature of his illness, had sought advice at the out-patient department of the charitable medical institutions, and had sat amongst the crowded occupants of the waiting-room until his turn came to see the doctor. Re-introductions of the disease were frequent, by tramps, labourers, and so on."

I should also like to quote the case of Messrs. G. W. & Co., six of whose employées fell ill at the same time at their homes in different parts of the city, pointing clearly to one infection and at the same time. Of these six cases, one lived within the zone of Priory Road Hospital, one in the zone of Fazakerley Hospital and one of Park Hill Hospital, the remaining three in parts all outside the hospital zones, whilst they all worked in the centre of the city. Now, what was the source of infection of these men? Was it that aerial infection from three different hospitals picked out three of them, and was, in fact, the cause of their illness, whilst the infection of the other three was from other sources?

I will just mention two other cases, and quote no more at present. At Everton Brow a servant-girl in a public-house was affected with small-pox so slightly that it was at first unrecognised. The nature of her illness was verified when the manager and his daughter had been infected. Her relatives were infected in another house, and they in turn passed on the infection to fellow-workmen residing severally in three or four other different parts of the city, all of which were outside the mile zone of any hospital excepting one, which is within the mile zone. Now, that one is said to be due to aerial convection from the hospital. If it is not, what becomes of the maps and dots? There is no reference to any other causes; that is what one—I will not say complains of—but surely all this evidence ought not to have been suppressed altogether.

Henry S. Morris and his father, of No. 83, Admiral Street, were being treated as cases of chicken-pox.

On March 7th, 1902, the doctor communicated with the Medical Officer of Health that he had suspicions regarding

the illness, and the Assistant Medical Officer of Health forthwith visited the patients, found them to be suffering from small-pox, and had them removed to hospital.

A relative of the above persons, named Annie Robinson, living at No. 14, Balkan Street (which is within fifty yards of Parkhill Hospital walls), was notified by a doctor on March 7th to be suffering from small-pox. She had been ill since February 27th, and had been in the habit of visiting her relatives at No. 83, Admiral Street, where small-pox existed. The dates of illness confirmed this source of infection.

There were no cases of small-pox in Parkhill Hospital at this time.

Now, Dr. Reece's attention has been called to whole series of such instances; instances, in fact, so numerous that, if he were willing to admit the possibility of such infection, the whole of his spot-maps would be wasted labour. These facts have been brought under his notice; but he prefers to abide by his conclusion that all these cases are due to aerial convection: thus claiming for aerial convection selective qualities which nobody else has claimed for it, and for which no explanation is offered. Dr. Reece may disclaim this view; but if he disclaims it he must modify his spot-maps, and if he modifies his spot-maps, he acknowledges that his labour has been in vain, and his conclusions fall to the ground.

There is only one other point. You will recollect what has been said about the situation of the Fazakerley Hospital, and these maps and charts show you plainly enough. If you look at the maps, you will find that there was the maximum number of invaded houses in the vicinity at the end of about the forty-eighth fortnight, at a time of minimum use of the hospital, whereas the maximum use of the hospital was made about the twenty-seventh to the thirty-first fortnight, when there was a minimum of house invasion. There can be no question of incubation that can spread over all that time.

I should like to say just one word with regard to the main question treated in Dr. Buchanan's Paper. I do not suggest for a moment that Dr. Buchanan's conclusions are not correct. But he weakens his case by bringing in instances of which he himself has no knowledge, and which depend altogether on dots and maps, and the sweeping aside of every other consideration. If an investigation into the causes of accidents were ever made, and the investigator contented himself by going to a hospital for

accidents, drawing rings round that hospital and putting dots for the accidents, and then drawing the conclusion that all these accidents are to be ascribed to the influence of the hospital, his conclusions would be obviously wrong.

DR. BUCHANAN: I should like, Sir, to ask Dr. Hope: Can he say if there is anything in Dr. Reece's Report—and, if so, where—which gave him the impression that Dr. Reece stated that all these cases were due to aerial convection? I have studied the Report from cover to cover, and I do not find anything in it which leads me to suppose that he says that every dot that appears in the maps is a case due to aerial convection.

DR. HOPE: He does not say so in so many words; but his conclusions point to that conclusively from the fact that he excludes all other possible sources than aerial convection. In your own Reports we find due prominence given to other sources of infection, and due allowance made for them. In Dr. Reece's Reports there are not any. If he were willing to admit that patients within a mile of the hospitals were infected through the usual channels, his conclusions would fall to the ground.

DR. MCVAIL: I have read Dr. Buchanan's Paper with very great interest, but would not have troubled coming from Glasgow to hear it, because I so heartily concur with his conclusions. But when Dr. Newsholme agreed to open the discussion on the other side, I was anxious to learn what could be said against aerial convection. I have heard the theory denied, sniffed at, called ridiculous; but none of these attacks satisfied me as sound argument. I was therefore very glad to have the opportunity of hearing Dr. Newsholme, who is always able to say everything that can be said in support of his case.

It is very pleasant to learn two things as to Dr. Newsholme's position. First of all, that he is merely an agnostic, taking no side for or against aerial convection. He is not convinced—he does not deny the theory, but he simply does not know.

Secondly, he thinks it of no practical consequence whether aerial convection occurs or not. He agrees that small-pox hospitals are centres of infection, and consequently that such hospitals should be situated at a distance from populous places. If that is the general feeling throughout the public health service, there must have been

a greater advance of opinion than I have been aware of. It was stated, not long ago, by a highly respected authority, that aerialists are the opponents of hospitals for small-pox; but it appears now that non-aerialists, or agnostics, are opponents of small-pox hospitals in exactly the same sense as aerialists—both holding that these hospitals should not be situated in populous places.

I did not clearly make out whether Dr. Newsholme is of opinion that these Liverpool hospitals, into whose figures he went so minutely, had any relation to the small-pox around them; whether the fact that when the one hospital only was in use, that hospital became surrounded by small-pox, and the subsequent fact that when all three hospitals were in use, all three became surrounded by small-pox; whether Dr. Newsholme came to the conclusion that these were mere coincidences, and that there was no causal connection at all.

DR. NEWSHOLME: Mr. President, I must leave the general trend of my remarks as I have already made them. I may simply say that my own opinion is, that there was no proof of hospital influence.

DR. MCVAIL: Dr. Newsholme is of opinion that quite possibly neither aerial convection nor intercourse with the hospital had anything to do with the excessive prevalence of small-pox: first of all, around the one hospital when it was alone in use; and, secondly, around the three hospitals when they were all in use simultaneously.

DR. NEWSHOLME: I believe that small-pox is always liable to be spread by intercourse between the hospital and its surroundings; and if there was such intercourse, that may have increased the prevalence of small-pox in the neighbourhood. Whether there was such intercourse or not I cannot say.

DR. MCVAIL: At the Congress of the Sanitary Institute in Glasgow—at a meeting at which I was not present—I noticed afterwards that an opinion had been expressed, to the effect that the decision of Mr. Justice Farwell in the Nottingham case had settled the question of aerial convection. That view seems to have got some hold, and, as I have said elsewhere, it is therefore desirable to note what really appears to be the relation of the Nottingham case to the theory of aerial convection.

Mr. Justice Farwell—as reported in the medical press—

stated that, in order to obtain an injunction in such a case, it was necessary to establish "a strong probability, almost amounting to a moral certainty," of risk or danger following from the hospital. He stated that to be the law of England, and it is also the law of Ireland, for he was quoting from the opinion of an Irish judge. That being the law, the decision in the Nottingham case could not be other than as given by Mr. Justice Farwell. No one who appeared in that case—and perhaps no one who supports the theory—would make the statement that, with regard to every such small-pox hospital there is "a strong probability, almost amounting to moral certainty," that small-pox will spread from it. The view of the aerialist—as I understand it—is, that it requires a concurrence of favourable conditions to produce this result, and that concurrence cannot be expected with anything approaching to certainty. Mr. Justice Farwell's decision does not in any way invalidate the theory of aerial convection. He expounded an important point in the law of England, but the state of the law does not affect the medical theory.

But Mr. Justice Farwell went on to give his legal view as to the nature of the evidence which would be required in a matter of the sort, and that view is of the very greatest interest. He held, with reference to Dr. Thresh's evidence, that "the plaintiff's case depended on the inference to be drawn from an unbroken series of facts—in all cases where A has occurred B has followed, therefore A causes B. But the conclusion depends on the universality of the premiss, and a negative instance unexplained spoils the chain."

Now, does that formula apply in the field of medical science, where we deal not with abstract problems in which the factors are assumed to be unchanging and unchangeable, but with a variable and as yet undiscovered micro-organism, conveyed by a variable medium to a variable human subject? Suppose we have a case of scarlet fever which is not followed by a second case, and that there is no explanation of the failure to infect. It appears, then, that because B did not follow A, scarlet fever is not, at common law, to be regarded as an infectious disease. Fortunately, disease prevention is carried on under statute law, not under common law.

In September, 1902, in *Public Health*, I read this:—

"Two cases of virulent small-pox occurred in a crowded slum neighbourhood, and were not removed to hospital; and, although the neighbourhood included a number of unvaccinated persons, no spread of infection occurred."

Here there was opportunity of infection by contact; but B did not follow A; and if "a negative instance unexplained spoils the chain," then small-pox cannot be legally regarded as infectious by personal intercourse, any more than by aerial convection. I do think that the Nottingham decision does not take us very much further than we were before.

What attitude should be adopted with regard to evidence on the question of aerial convection? If the attitude be this—that aerial convection can be admitted only when the possibility of all other sources of infection is absolutely excluded—then there comes the further point: Is it not also held to be invariably impossible to exclude absolutely all other such sources? If it be required of believers in aerial convection that they shall prove a negative (namely, that no other possible cause of infection did exist), at the same time that it is pointed out to them that a negative is unproveable, obviously the requirement cannot be fulfilled. Surely, the same rules of evidence should be applied to aerial convection as to other questions in the domain of medical science—rules, for example, which take cognisance of the fact that we cannot actually see the virus of infectious disease—of scarlet fever, or typhus, or whooping-cough, as it passes from one individual to another, but have to form our opinions on a basis which must fall short of actual demonstration.

I noticed, some time ago, a paper on this subject by a very competent medical man. He had to do with a small-pox hospital, and he said that he treated small-pox in the same hospital with other diseases; but that the measures that he was taking with regard to small-pox were quite sufficient to prevent its spreading, and it had not done so. And then he went on:—"I must also add that if the use of this ward for small-pox is found to affect the other pavilions, I shall be quite satisfied that the regulations have not been carried out." That is an example of a mental attitude which I would venture to deprecate in this discussion.

In *Public Health* for September, 1902, there was a leading article dealing critically with the theory of aerial convection. In the course of that article this passage appears:—

"Another experiment has been tried in London at the South Wharf Shelters, to which at least eight thousand cases have been conveyed *en route* for the hospital ships. The shelters hold twenty-five patients, and on one occasion as many as 120 patients have been detained there and on the steam-boat when the traffic on the river was stopped by fog; and yet, as Dr. T. Orme Dudfield has pointed out, without ill effect on the locality. There has

been no spread of the disease in the neighbourhood of the shelters; and, in fact, their immediate vicinity has been freer from small-pox than many other parts of the borough."

That seems a pretty clear case of absence of aerial diffusion; but, in the January following, this letter appears:—

"In the September issue of *Public Health* a statement was made to the effect that there was immunity from small-pox in the vicinity of South Wharf Shelters; and this is again mentioned by Dr. Malet, in his letter in the December issue. It arose from a statement made by myself to the Medical Officer of Health for Kensington, early in the late epidemic, and when many cases had not occurred in the vicinity of the place in question. Subsequent events, however, proved that this immunity did not continue, and there was a much greater incidence in this region than in any other part of the borough.

"In my special report, published early in October, you will find a full statement of the facts in regard to the Wharf, as far as could be ascertained, under the head of 'Aerial Diffusion.' This statement ought to be corrected, as it is being constantly re-quoted, and is quite erroneous. Taking the area round the Wharf, there are about 2,560 inhabitants cut off from the rest of the borough by docks, with an attack-rate of 10.55 per 1,000 inhabitants, as compared with 5.35 in the next most infected area of the borough, and 2.30 for the whole borough. I have discussed the ways in which the Wharf has influenced this incidence, and I certainly think that danger is to be looked for in the direction of intercommunication between persons employed in the hospital and the surrounding inhabitants, and not through aerial diffusion of the infection."

Thus we see that in September the supposed absence of small-pox is at once accepted and cited as evidence against aerial convection; but the subsequent discovery that small-pox had had a fourfold prevalence in the neighbourhood of the wharves is not admitted to yield any presumption in favour of such convection.

Dr. Louis Parkes, who was in the chair at the Newcastle Meeting of the Royal Sanitary Institute this year, made a very shrewd remark in closing the discussion. He said that he thought all negative cases should be investigated equally with all positive cases. I do think that is a most valuable suggestion. The incidence around small-pox hospitals in populous places should be gone into, and the whole facts stated. It is quite possible that what may appear due to aerial convection at one time may subsequently prove to be in a large measure due to other causes. I think that Dr. Savill did something to indicate that in the case of Warrington. On the other side, there is the supposed negative case of the hospital wharves which I have just mentioned. Also, it has been most fortunate that Dr. Hope's views as to the absence of hospital influence in Liverpool have been traversed by Dr. Reece; and when one gets time to look into Dr. Hope's remarks, it will be possible more thoroughly to weigh up the facts. But I

confess I was surprised at Dr. Hope assuming that the fact that Dr. Reece, in his report, has made no reference to the possibility of other causes of small-pox in the quarter and half-mile circles meant that Dr. Reece denied any such other causes. It is manifest that there must be other causes, but what we want is to explain the *difference* between the larger incidence round hospitals and the smaller incidence in the rest of the community. It is the difference that is in question. Obviously, the existence of a small-pox hospital does not prevent ordinary means of infection around it. That goes without saying.

There was one group of experiences mentioned in the course of the Nottingham case that I have been hoping to know more about. Dr. George Reid gave weighty evidence referring to seventeen hospitals in Staffordshire, from not one of which had there been any evidence of spread of small-pox. It is most important that the whole of the facts regarding these seventeen hospitals should be published. It is impossible for a witness in the box to arrange and elaborate his evidence as he can do in a Paper on the subject; and just as the Liverpool hospitals' experiences can be best studied not solely from Dr. Hope's evidence in the Nottingham case, but also from his Annual Report, read alongside of Dr. Reece's Report to the Local Government Board, and from Dr. Hope's contribution to the present discussion, so one would desire to have Dr. Reid's valuable experience recorded in detail, giving the full facts regarding every one of the seventeen hospitals: the population around them, the times when they were used for small-pox, the numbers of acute cases, and the precautions against personal conveyance of the disease. Then we could weigh the evidence along with Dr. Hope's and Dr. Thresh's, and the Glasgow, Gateshead, Essex and other cases, and consider the whole volume of such evidence at our leisure.

I am quite at a loss to discover where the difficulty lies in accepting the possibility of aerial convection of small-pox from hospitals. We agree that small-pox is conveyable for at least some distance through the air. We agree also that small-pox and scarlet fever and diphtheria may be spread directly or mediately from any case or cases of the disease; and so theoretically all these diseases may be spread by hospital intercourse. But is it not possible that prevalent views of the power of small-pox to spread by intercourse are exaggerated? Already I have referred to two London small-pox cases which caused no spread of infection. At Fulham Hospital, when small-pox prevailed there,

nine non-resident servants did not carry the disease to their houses in nine different streets, but so soon as a hospital dustman himself caught small-pox, he infected his own family. We never see scarlet fever or diphtheria spreading from a hospital as small-pox does. Why, then, should the excessive prevalence of small-pox be always and wholly attributed to intercourse? Why especially should this be so when we know that a hospital like Darenth may contain hundreds of convalescent small-pox patients in all stages of desquamation, with crusts and scabs abundantly shed from their whole integument day after day, and yet without any extension of infection from the hospital; whilst on the other hand hospitals containing acute cases closely confined to bed, and previous to dessication of the pustules, cause prevalence of small-pox all around them? If the cause were intercourse or mediate infection from small-pox dust, surely a convalescent hospital should be a prime source of danger, instead of being found practically harmless; and, per contra, hospitals with acute cases confined to bed, and previous to drying of the eruption, should be much less effective as centres of infection. But experience shows that small-pox has, over and over again, spread from hospitals containing a collection of acute cases. These considerations are the very basis of the aerial theory. It is simply an endeavour to explain known facts: to construct a working hypothesis which shall adequately account for the observed phenomena of small-pox hospital influence.

Indeed, I do not myself see why, when there is exceptional prevalence of small-pox round a hospital, every case regarding which even the most far-fetched and improbable suggestion of mediate infection can be made, should at once be dogmatically set down to that imagined cause, rather than to atmospheric diffusion.

The evidence is not confined to this country. Aerial convection was discussed in America before the end of the eighteenth century; and Sir John Rose Cormack, about a quarter of a century ago, noticed small-pox being spread from hospitals in Paris, and formed the opinion that it was due to what he called epithelial drift. He also referred to one reported case, where it was believed to have spread specially from that side of a small-pox hospital which had open windows.

After all, it seems to me that the greatest difficulty in accepting the theory of aerial convection is to be found in the statements that have been repeatedly made regarding

the absence of small-pox in certain public institutions within a quarter and a half mile of small-pox hospitals. That was what concerned me a dozen years ago in looking into the matter, and I am struck by the recurrence of such evidence in the present discussion. I would like, therefore, to say a word regarding it. Last week you had Dr. Ker, from Edinburgh, who gave you some account of what has taken place there. What I understand is this—that there is practically no population, excepting in certain institutions, within half a mile or more of the Edinburgh Hospital.

DR. BUCHANAN: Except a small group of dwellings at this end.

DR. M'VAIL: But if within the half mile there is no general population, I think it is necessary to point out that this was not a control experiment. The nature of a control experiment is this: that the two cases shall resemble each other in every point, excepting the one on which the control experiment is made; that is, we should have within a half-mile of the hospital institutions that escape small-pox, at the same time that we have a surrounding population which does not escape. With regard to Edinburgh, these conditions seem not to have been fulfilled. If there was no population in this radius round the hospital except in the institutions, then one does not know whether there would have been any spread of small-pox if there had been a population. The value, in the meantime, of the Edinburgh case seems simply this: that there was a population within that distance of the hospital which did not take small-pox; but whether that was due to its being an institutional population or not, is not in the least proved. In so far as the patients in the fever hospital were confined to bed, or to the wards, they would be much less exposed to aerial infection than persons moving about outside; and that remark would perhaps apply even to the erysipelas cases who were nearest to the small-pox wards. The case has its own value, but its value is limited; and I confess that those of us who know Edinburgh, and the standing of its medical profession, and how the vaccination laws are obeyed in that city, can have little difficulty in believing that you might have institutions there whose population was practically insusceptible owing to vaccination.

In order that such institutional cases should yield a lesson of any real value, it is necessary to know quite definitely the vaccinal condition of the inmates. When, as has

repeatedly occurred, I have been compelled to send small-pox to an ordinary fever hospital, I have examined the vaccinal conditions of every patient in the hospital; and the number who were found not to require vaccination owing to recency of primary vaccination, or to re-vaccination, or occasionally to previous small-pox, has been surprisingly great. For the rest, I have never failed to persuade those who really needed renewal of protection to submit to the operation. In consequence, the institution has been simply insusceptible to small-pox. It is not enough to know that, under risk of small-pox, re-vaccination was not performed on every inmate. The question is, how many who needed vaccination or re-vaccination were left unprotected.

Another essential is to know the age-distribution of these populations. In workhouses, a large number of the inmates are past the age at which small-pox attacks are likely. For example, only one-sixth of the general population reaches the age of sixty; but in workhouses the proportion of old persons is very much greater, and small-pox attack is much less likely at or over sixty than at earlier periods of life. Then, again, with regard to inmates at other ages, they must largely consist of persons who have often been in common lodging-houses, where general vaccination is attended to whenever small-pox appears, or in prisons where the operation may be a matter of routine in cases requiring it, whilst the vaccination of children born in workhouses is not likely to be neglected. These considerations make it necessary that we should know the actual facts regarding each institution cited as evidence against aerial convection.

In the absence of details, the argument from institutions does not appear to me to disprove the power of small-pox to spread by diffusion through the atmosphere.

Those who regard aerial convection as the best explanation of some of the facts we are discussing to-night may perhaps be accused of scientific credulity. But scientific incredulity may be far more dangerous. A man is apt to form very definite conclusions as to the means by which a given infectious disease may alone be conveyed; and in that case, though the disease may be spreading all around him by a different method, he, being hidebound by his preconceived opinions, will fail to recognise the agency at work. Having determined that aerial convection is impossible, or that hospitals are harmless, he may go right into the midst of an outbreak so caused, but his eyes and his mind will be closed to the facts that are before him.

THE PRESIDENT: Both sides of the question before us have been well and fully stated, and most of the points I had intended to bring forward have vanished one by one as I have listened to Dr. McVail, who has put them infinitely better than I could have done. There are still, however, one or two on which I should like to make a few remarks, and first as to the antecedent probability of aerial convection.

We are probably all agreed that particulate infective matter passes from a small-pox patient into the air of the room in which he lies, and can effect susceptible persons at short ranges, say within the room; and our faith in this would not be shaken by finding that sometimes in such circumstances actual infection did not follow. From the once frequent records of infection from rags, it may be inferred that the poison does not readily become inert, but can retain its virulence for long periods. Particulate matter of other kinds is carried far by air, in a direction determined by that of the air-currents for the time being, to settle, sooner or later, more thinly as the radius increases; and this without the aid of flies or other insects which may, nevertheless, be given. As regards smoke particles, metallic and other dust, and *bacillus prodigiosus*, these results are demonstrable. The suggestion is that small-pox particles under the same conditions behave in the same way, and like *bacillus prodigiosus* remain infective at the end of their transit. The infected air of the sick room must pass into the open air, although it is not—as smoke is—usually delivered at a high level in a hot current, with considerable upward initial velocity; and any particles it contains may therefore be expected to settle all the sooner on that account, especially in the absence of high winds, which seems to have coincided with experience of aerial convection.

It seems to be generally agreed that whatever the explanation may be, there is occasionally some "hospital influence" to be recognised: a graduated incidence of small-pox around hospitals, graduated much as the deposit of smoke would be. It might well happen, on the hypothesis of aerial convection, that owing to the direction of air-currents at critical times, the diffusion would be mainly in one direction, and that this might coincide with that of traffic to and from the hospital. But at Fulham, at all events, the larger incidence was in another direction. Dr. McVail has referred to the difficulties which attend any attempt to explain this incidence, and especially its

graduation, on the basis of personal contact or traffic. No one disputes the danger which must arise from such sources, if there be laxity of administration of hospitals and ambulances. The question is, whether the most liberal estimate of carelessness in those respects can explain the facts observed at Fulham and elsewhere, as to the amount and peculiar distribution of small-pox in the vicinity.

Why it should be only small-pox which is liable to exert this "hospital influence" is less clear on either view. As far as I know, nothing of the kind has been brought home to scarlet fever, even when isolated on a large scale in hospitals surrounded by dwellings. Measles, which has many points of analogy with small-pox, has not yet been concentrated in hospitals in the same way.

Possibly the infective matter of some of these diseases is readily destroyed by dryness, or by exposure to air, or becomes ineffective when diluted beyond a certain point. This, however, is not the only problem of the kind. Why is small-pox not conveyed by milk or water, as certain other infectious diseases are?

At all events, small-pox, and so far small-pox alone, has been found now and then to occur around hospitals in a characteristic way, the details of which would be *à priori* probable on the hypothesis of aerial convection, but could hardly have been predicted on any other view.

Negative experiences are valuable as grounds for the conclusion that aerial convection is, at all events, exceptional, which is not denied by those who accept Mr. Power's suggestion; but they cannot in themselves disprove it, any more than the escape from enteric fever epidemics of communities who derive their water supplies from polluted sources, or the escape of individuals during such an epidemic, disproves the reality of water-borne infection.

It is not a hospital question alone, though the facts can be better studied, on a large scale, when the possible focus is single, and infected in a greater degree and for a longer time. I believe that if circumstances permitted the same close observation of small-pox in private houses, we should find here and there examples of aerial convection far beyond the sick room. One suggestive instance occurred in my own experience, years ago. A case of small-pox was brought into a house in a crowded part of Nottingham, at a time when the town had long been free from the disease. It remained unreported for some days; and after the usual incubative period other cases occurred in houses near, which had, as far as could be ascertained, no direct

communication of any kind with that first infected. The secondary houses were in the same block—only a few yards away—but in courts approached only by passages from other streets. The wind was light, and blowing in the direction which the hypothesis of aerial convection would require.

We are greatly indebted to Dr. Buchanan for his excellent Paper. The discussion has been so full that he cannot speak at length to-night; but I will ask him to say a few words now, reserving for publication in the *Transactions* his reply on the discussion generally, including the observations which we hope to receive in writing from those members who have not been able to address us to-night.

The following contributions, including Dr. Buchanan's Reply on the whole Debate, have since been received:—

Dr. R. J. REECE: Although the President was so good as to invite me to speak at the adjourned discussion on Dr. Buchanan's Paper, I thought it best, in view of the lateness of the hour, and of the many references which had been made to my recent Liverpool Report, to avail myself of the Society's decision to receive additional contributions to the discussion in writing.

It may be well to point out that this Liverpool Report, as its title sets forth, is a report to the Local Government Board on Small-pox and Small-pox Hospitals in Liverpool, 1902-3, and not a thesis on aerial convection. Dr. Buchanan, in his Paper, and other speakers in the discussion, have carefully distinguished between the occurrence of what is conveniently termed "small-pox hospital influence," and the various explanations of this influence. In Liverpool the principal matter which I had to ascertain for the purposes of the Local Government Board was whether or not hospital influence had been manifested. The importance of ascertaining this is at once evident, on consideration of the situation as regards populous neighbourhoods of the three hospitals in which the acute cases were received, and of the magnitude of the Liverpool epidemic during the two years in question. Dr. Buchanan has given an outline of the principal facts as to this; fuller details can be found in my Report itself. It must also be remembered that, before I undertook my inquiries, it had been asserted that the use of these several hospitals in Liverpool had not been attended by exceptional prevalence of small-pox in their vicinity.

Thus Dr. Hope, in a Report to the Liverpool Corporation "On the Recent Outbreak of Small-pox," dated 31st December, 1903, had already said (p. 28):—

"Until within recent years there was an impression that a small-pox hospital, however well conducted, must necessarily be a source of infection to the district in which it is situated, owing to what is known as aerial convection, *i.e.*, conveyance of infection for prolonged distances through the atmosphere. It is important, therefore, that the experience of the Liverpool hospitals in this respect should be borne in mind, because it shows that in strictly-disciplined institutions, placed as these hospitals are, no danger arises from this source."

But although much importance naturally attached to opinions thus expressed, it did not appear from the data given by Dr. Hope in the Report above quoted, that they rested upon a thorough and systematic study of the local incidence of small-pox at different periods, and in relation to the operations of different hospitals: a work which necessarily would involve such enormous labour that the most energetic medical officer of health—even Dr. Hope himself—might reasonably be reluctant to undertake it. Dr. McVail has referred to a desire which has been expressed that "negative instances"—cases in which a hospital or hospitals have been stated on good authority to have exerted no adverse influence on populous areas in their vicinity—should be investigated by the Medical Department of the Local Government Board. Liverpool afforded excellent opportunities for an investigation of the kind desired. As I said in my Report (p. 10):—

"A demonstration, therefore, of absence of spread of small-pox from hospital, in the case of Liverpool, where three hospitals, two of them in the city, were receiving acute small-pox cases, and where in particular one of the hospitals (Park Hill), for a period of some twenty-four weeks at the height of the epidemic, received acute small-pox patients in large numbers, would, if established, be not only particularly interesting to epidemiologists, but in its administrative aspects would be reassuring to small-pox hospital authorities."

My main work, therefore, was to get out the data; to apply all available means of checking and correcting the collected facts as to the occurrence of cases and the invasion of houses; to determine populations and numbers of dwellings in different areas; to spot maps so that they showed, fortnight by fortnight, all the newly-invaded houses in the city; and, subsequently, to combine these various data as to local incidence of small-pox in relation to the hospital operations. In my Report I summarise the questions with which I had to deal as follows:—

"(1) Has the inhabited area within a mile in each instance of a hospital suffered more severely than the rest of Liverpool? And if so—

"(2) Has exceptional incidence within that area corresponded in point of

time (having regard, of course, to the period of incubation of small-pox) to the use of the hospital for the treatment of acute small-pox cases? And—

“(3) Is there evidence that within the several “one-mile areas,” as they may be termed, dwellings nearer to hospital sustained a heavier incidence of small-pox than those further away?”

And my conclusions, based solely upon data upon the accuracy of which Dr. Hope and I are completely in accord—for obtaining many of them I am of course indebted to him and to his staff—were:—

“(1) Inhabited areas within a mile of each of the three Liverpool small-pox hospitals have suffered more severely from small-pox than the city as a whole.

“(2) Exceptional incidence of small-pox within these areas has corresponded in point of time with the use of these hospitals for the treatment of acute small-pox cases.

“(3) Broadly speaking, within these hospital areas the dwellings nearer to hospital have sustained a far heavier incidence of small-pox than those further away.”

Dr. Buchanan, in his Paper, has referred to the way in which the Liverpool experience, summarised in the above conclusions, is parallel to that met with in London before 1886; and subsequently in a plurality of other instances of provincial hospitals which have had considerable populations in their neighbourhood; and with regard to Liverpool he draws the inference which I have done, namely, that we have there a notable and striking example of the characteristic “small-pox hospital influence.” It is now necessary, however, to consider Dr. Hope’s view of the facts brought out by my inquiries. I understand him to urge that these are not instances of true “hospital influence,” but of some kind of spurious imitation of the real article; that the excessive incidence of small-pox round these hospitals, the correspondence of such incidence in point of time with hospital operations, and the graduations observed have had no relation other than fortuitous to the hospitals themselves: that, indeed, he advances a kind of “Theory of Fortuity.”

I do not think Dr. Hope has assisted this contention materially by instances such as—in one series of cases—a woman, “Annie Robinson, living at No. 14, Balkan Street (which is within fifty yards of Park Hill Hospital walls),” where the source of infection was traced by his staff; for, as he himself points out, “there were no cases of small-pox in Park Hill Hospital at this time.” Or in another case, which he has placed on record elsewhere, of a patient who died suddenly within the $\frac{3}{4}$ -1 mile zone of the Priory Road Hospital, and to whom he traces the infection of certain other persons, for he fails to trace the infection of this primary case, and the hospital

was at the time receiving acute small-pox patients. Neither do I consider that anything is gained by the cases he quotes of Messrs. G. W. and Co., or the servant-girl at Everton Brow. Of course, scores of instances could be brought forward in which persons living near a small-pox hospital were in all probability infected independently of it. Living close by a small-pox hospital does not confer immunity from small-pox attack by direct infection—the point is too elementary to need stating. The question at issue is not whether *all* cases living within a mile of a hospital contracted small-pox from the hospital, but whether the *excessive* incidence round the hospital is attributable to the hospital.

I may add one other point. Dr. Hope succeeded—through the efforts of his staff and with the aid of the special powers to which he has referred—in tracing a probable source of infection in about 1,000 out of the 2,000 cases which occurred in Liverpool during 1902-3. Incidentally, I may note that there seems to be an ample margin for cases due to aerial convection from the hospitals. But there is also some question with regard to the 1,000 cases which are accounted for. In some of these cases, occurring in the hospital areas, it appears to me, after study of detailed lists, that after all the recorded evidence of contact with small-pox cases was slight; and that it might at least be equally probable in certain cases that aerial convection from the hospital was the real cause. This, however, can only be a matter for speculation. A further point is the possibility that certain of the undetected cases which remained at their houses (in any part of the city) caused infection in their neighbourhood as a result of aerial convection, not of direct or mediate infection. Dr. Hope has given an interesting case in point in a diagram attached to his 1903 Report on the Liverpool outbreak: where a woman named Powell, who, while suffering from small-pox, remained at a house in Lansdowne Street for three weeks before removal to hospital and is believed to have caused the infection of as many as 29 cases in the neighbourhood. Now, in accounting for these 29 cases, Dr. Hope and his staff have been content in several instances with recorded explanations, such as that the patient lived: "in the neighbourhood of Lansdowne Street," "close proximity to Lansdowne Street;" "a shop in Lansdowne Street," and so on.

This method of accounting for the infection of cases can hardly be regarded as complete. It might, perhaps, be

extended in the following manner: "Walked down a street in which the Powells lived;" "Belonged to the same Band of Hope as the Powells," "Received a telegram from the Powells."

I do not, however, seriously question that these patients did, in some way or other, receive infection from the woman Powell. But, in view of our experience of small-pox hospitals, and of cases such as that at Nottingham, to which the President has referred, it seems to me that aerial convection of infection over comparatively short distances from the Powells' house should be included among the conditions, such as *fomites* or mediate infection, which in this instance may be supposed to have favoured the local spread of the disease.

I find it somewhat difficult to follow Dr. Hope's criticism in regard to rates. The statistical part of my Report was intended for readers who could distinguish between the actual figures given and the rates calculated on those figures, and throughout that Report I have given the actual figures side by side with the rates. In dealing with the case of Fazakerley Hospital, I have in three separate places drawn attention to the necessity of caution in drawing inferences from rates based upon small numbers. The fact is, of course, that some of the rates are calculated on large figures, *e.g.*, the rates for the larger areas round Park Hill Hospital during the period 7th December, 1902, to 20th June, 1903, when that hospital was receiving cases on a large scale at the height of an epidemic, while others are calculated on small figures, *e.g.*, the fortnight-by-fortnight rates, or the rates on the small area within $\frac{1}{4}$ mile of Park Hill, which contains only 171 dwellings, nine of which were attacked, giving the rate of 526 per 10,000 referred to by Dr. Hope. The smallness of the number of houses in such an area as this, or as in the Fazakerley neighbourhood, is a matter which the epidemiologist must take as he finds. The point which comes out in dealing with the mile areas round these several hospitals, and the subdivision of those areas, is that whether you take the large figures for the large hospital at the height of the epidemic, or the smaller figures for the smaller hospital in the early portion of the epidemic, or the figures for the outside hospital with its comparatively few houses round it, each in its degree, and on the invasion rates, indicates the same characteristic excessive incidence corresponding in point of time to the hospital operations; and when the figures are largest, and the conclusions to be drawn from the rates are consequently the most definite,

the indication of the excessive incidence, and the graduation of incidence, are clearly manifest.

With regard to Dr. Hope's observations on the danger which he represents me to have attributed to Fazakerley Hospital, as compared with small-pox hospitals situated within the city, it is hardly necessary to point out that a house invasion rate of 3 per cent. in the case of a hospital like Fazakerley, which almost fulfils the Local Government Board requirements as to population, is a trifling matter administratively, by contrast with the same invasion rate in populous areas such as those round Priory Road or Park Hill.

The Netherfield Road story does not seem to me to lend much weight to the argument that the occurrences round hospitals were merely fortuitous. The analogy is hopelessly incomplete. If it is desired to show by analogy that the occurrences round the small-pox hospitals were accidents, and nothing more, we want something of this kind—a series of facts as to the incidence of small-pox round three establishments which are the only three of their kind in or near the city—say, three skin hospitals, three piano factories, or three of anything else you please, provided they are the only three, and are placed in different parts of Liverpool, and outside the small-pox hospital areas. If the analogy is to be of value, these facts must be found to show an excessive incidence of small-pox round each establishment, and a graduation of incidence round each establishment. And it would not be enough to find that this excessive incidence and this graduation occurred when the whole epidemic period was taken. In the case of establishment A, they must be found during the period in which Park Hill Hospital was receiving cases, and not at other times; in the case of establishment B, solely while Priory Road was receiving cases; and in the case of establishment C, solely when Fazakerley was receiving cases. In the absence of some such demonstration, I do not see how to attach weight to the comparison which Dr. Hope invites us to make.

What Dr. Hope appears to advocate, and what I have termed a theory of fortuity, is an explanation that the excessive and characteristic incidence of small-pox within the hospital areas was due to accident; that undetected cases, or other causes of spread of small-pox from person to person, occurred in exceptional numbers, as a result of mere chance, in the areas in question at the time when the hospitals were open. On this explanation, the apparent

relationship between the excessive incidence of small-pox on the hospital areas and the activity of the hospitals, the gradation in intensity of incidence within the hospital areas and the rest, are mere matters of coincidence.

The theory might be seriously entertained if the experience of Liverpool in 1902-3 stood by itself. But when the notorious occurrence of similar phenomena round other hospitals in other epidemics is borne in mind, it may be dismissed as altogether inadequate.

As to the explanation, my own belief is, that aerial convection probably operated in the case of each hospital. Naturally, I have carefully considered the alternative explanation: that of direct and mediate infection due to hospital communications and traffic. The data placed at my disposal by Dr. Hope give no suggestion of support to the latter explanation, in the case of any one of those hospitals. The assumptions required to explain the excessive incidence, and the gradation of incidence in each instance, by a prolonged series of unsuspected communications between the hospital and its neighbourhood, undetected by Dr. Hope's staff, seems to me to be so improbable that it may safely be dismissed. I realise, however, that in this contention I am not likely to receive the support of Dr. Newsholme.

Looking to the whole case, therefore, I remain of opinion that in Liverpool, during 1902-3, we had to deal with a remarkable series of instances of true hospital influence; the excess of small-pox in the neighbourhood of the hospitals standing to the operations of these hospitals, as Dr. Buchanan has put it, in the relation of effect and cause.

DR. PHILIP BOOBBYER: Owing to the interest I feel in the subject of Dr. Buchanan's Paper, and my inability to attend either of the recent meetings of the Epidemiological Society at which it was discussed, I am specially glad to avail myself of the opportunity now kindly offered me of sending in writing a contribution to the Society's *Transactions* under this heading.

At the outset I wish to say that I entirely agree with the President and other speakers, that there can be no doubt of the capacity of small-pox infection—as of other particulate virus—to spread for short distances through the air. And, given the possibility of such extension without intermediate vehicle, it is obviously difficult to limit the distance over which it can possibly take place. It is surely

a mistake, however, to consider that a virus of such exceptional—if short-lived—activity should spread almost exclusively through one only of the available media. We hear comparatively little nowadays of indirect agencies of infection, but I have known the disease undoubtedly carried on many occasions by such agencies. In the neighbourhoods immediately and remotely surrounding small-pox hospitals we have the hospital ambulances and other conveyances, and the clothing and persons of hospital servants, nurses, and medical men, continually moving about, and constituting a serious menace to unprotected persons with whom they may come into contact or proximity. This is no fancy picture of remote possibilities, but a statement of fact based upon experience. I have known small-pox carried by a coat sent for repair, after having been worn by the owner in paying one casual visit to a small-pox hospital ward, in which also he was careful to avoid actual contact with the patients and furniture. The difficulty of tracing infective contact is well known to all who have had much personal experience of small-pox outbreaks; it is also a matter of common knowledge with such persons, that the more careful and detailed the investigation the greater the proportion of cases in which such contact can be clearly traced.

Where special inspectors are employed to deal with small-pox contacts, it is often found that sources of infection are ultimately brought to light which have been missed and undreamed-of at the first inquiry, although perhaps such inquiry may have been most carefully and systematically conducted by an experienced medical man. The inspectors frequently gain the entire confidence of the people among whom they work, and thus obtain clues which might otherwise escape the most vigilant search. No one, I think, will call in question the *bona fides* or scientific expertness of Local Government Board inspectors like Dr. Buchanan and Dr. Reece, or some other recent aerialists; but all Medical Officers of Health who have had much experience in the investigation of local small-pox outbreaks, will, I think, agree with me that many important facts must almost necessarily be hidden from these gentlemen which would, in all probability, come readily to the knowledge of the local sanitary inspectors. Even two medical experts agreed upon the main issues like Drs. Buchanan and Thresh, making almost simultaneous investigations in the Essex-shore outbreak of 1901-1902, arrived at different conclusions, so far as their estimate of the possibilities of personal contact was concerned. In a recent case, too, tried

in the High Court, one medical witness adduced as evidence of air-carried infection certain cases of small-pox which were shown by another to have arisen by contact with previously known cases.

It is, I think, extremely probable that much of the infection in the Essex-shore outbreak of 1901 and 1902 was carried out of East London by dock and other labourers, travelling by road, rail, and river, to and from their work. I may instance an occurrence in my own neighbourhood, which illustrates how readily infection may thus be spread without necessarily attracting attention in the process. At the end of March, 1904, an outbreak of small-pox occurred at Arley, near Nuneaton, in Warwickshire, among some pit-sinkers there. The disease was not recognised at Arley, and several of the patients whose homes were in the neighbourhood of Nottingham returned there in a highly infectious condition to be nursed. We were fortunate enough to discover and secure the isolation of these patients; but, had we not done so, an extensive further outbreak might have occurred, and in the absence of information about the Arley cases, the source of infection in the Nottingham district would have been open to various explanations.

In looking over the zone maps, we find that on the prevailing-light-wind theory of spread, such wind must have been of very variable direction in most recorded cases. In two of Dr. Buchanan's instances, an east and a west wind are respectively necessary to explain the special incidence observed on this hypothesis. Much has been said on the subject of so-called negative evidence, with special reference to the question of its significance, but no agreement seems to have been arrived at. Now, without going the length of Haygarth in his discussion with Waterhouse, and saying that negative is practically destructive of affirmative evidence, we must all agree that it serves *pro rata* as a diluent of it; and if no evidence of aerial convection could be found by the Local Government Board in the case of four-fifths of the small-pox hospitals in use in this country; and if, as was certainly the case, the general circumstances of many of these were apparently identical with those having a positive record, then surely we are justified by such facts in asking whether it is not probably some factor more variable than the atmosphere surrounding the hospitals which is productive of the difference.

A distinguished judge to whom the problem was incidentally submitted, in a recent case tried in the High Court, insisted upon taking a broader issue, ignoring the

atmospheric question. Looking at the matter logically, he said:—"The conclusion that all hospitals are sources of danger does not necessarily follow from the premise that some are such"—with which proposition we must all agree—and then went on to add that, apart from the logical question here involved, so-called negative evidence was available to more than outweigh the positive statements on the other side. So much for a layman's view of the case.

While speaking of negative evidence, I cannot forbear giving a recent example of it from my own experience. During the past two years, it has been my practice to nurse all severe cases of small-pox occurring in Nottingham, in the open air, or rather, in bell-tents with open sides. About twenty of these cases have been so treated at our Bulwell Forest Hospital during this time. The patients have been placed within some 85 ft. of a road along which about 1000 coal-and-iron workers, and other persons, pass daily on their way to and from their work. The direction of the prevailing wind (south-west) is from the tents across the road at an angle of about 45 deg. One only of these 1000 habitual road-passengers contracted the disease, and he apparently became infected by contact with a case near his own house, at a distance from the hospital.

The actual *modus operandi* of hospital influence in propagating small-pox—when it occurs—is often stated to be immaterial; so far, at least, as administrative interests are concerned. This, however, is obviously incorrect, for if the risk of aerial spread is real and serious, then small-pox hospitals must be remotely isolated; whereas, if the principal risk is from lax administration and the like, all that is necessary for reasonable safety is to secure a site in a sparsely-inhabited district, and look carefully to the subsequent management of the institution and its inmates.

DR. J. THRESH: The subject of the aerial conveyance of the infection of small-pox has so frequently been discussed, that it is practically impossible to advance any fresh arguments either for or against; but there is no doubt in my mind, that the recent inquiries made by Drs. Reece and Buchanan, with reference to the Gateshead and Liverpool small-pox hospitals, have strengthened the evidence in favour of air-spread infection. At the same time, I am also convinced that those who refuse to accept the evidence in connection with the outbreaks which occurred in London, and, more recently, in Essex, when the London small-pox hospitals were moored in the Thames, will not have their

opinions altered by the experiences of Liverpool and Gateshead. I regret exceedingly that those who seek to explain the unusual incidence of small-pox, around large hospitals receiving acute cases, rarely produce evidence showing that they have thoroughly investigated the incidence of the disease in such cases, and have found that there was no excessive prevalence near the hospitals. This is the only kind of evidence which, in my opinion, can have any weight. Statements to the effect that no excessive incidence has been noted are freely made, but rarely supported by evidence. For example, it has often been stated that the hospitals at Liverpool had no effect upon the residents around; yet, when carefully investigated by Dr. Reece, he had no difficulty in showing that they were *foci* from which the disease was spread.

My further experience in connection with this disease, and further inquiries with reference to the effect of the hospital ships, only tend to confirm the opinion that the *materies morbi* of small-pox is conveyed in some way through the air, and may be so conveyed for considerable distances. At the time I communicated my Paper on the Essex epidemic to this Society, I mentioned that I was watching with interest the effect of the West Ham Hospital, situated in Dagenham parish in the Romford Rural District, which was then being filled with cases from West Ham and the surrounding towns. I had not long to wait, as cases speedily cropped up in the village and amongst the more scattered population around; and, although little difficulty was experienced in stamping out the disease in such densely-populated localities as West and East Ham, it could not be stamped out in this rural area, until the epidemic had practically ceased in the towns sending cases to the hospital. Myself and assistants inquired into the origin of all the cases around, and found a much larger proportion which could not be attributed to contact than was the case elsewhere. I kept a spot map, and at the termination of the outbreak found that the area round the hospital had suffered far more severely than any other part of Essex, save that near the hospital ships. In Dagenham, the parish surrounding the hospital, the attack-rate was 8 per 1,000 population; in two parishes to the south-east, lying between the small-pox ships and the West Ham Hospital, it was 25 per 1,000; in the parish adjoining Dagenham (Hornchurch), it was 3.75 per 1,000; and, in the remaining parishes of the Romford District only two cases occurred. The two parishes with the largest

proportion of cases were between two fires: exposed to the virus from the ships with the wind in one direction, and to that from the land hospital with the wind in another. The outbreak in Dagenham and Hornchurch, the parishes surrounding the West Ham Hospital, commenced when some forty to fifty cases had been received in the hospital, and continued for a month after the rapid subsidence of the epidemic in West Ham. Altogether, 123 cases occurred in the Romford Rural District, distributed as under:—

In Rainham and Warmington, between the London and West Ham Hospitals	48 cases = 25 per 1,000 population.
In Dagenham (where the West Ham Hospital is situated)...	...	49	" = 8. " "
In Hornchurch (the next parish)	...	24	" = 3.75 " "
In Havering, Upminster, Canham, and Corbetley	...	2	" = .8 " "

I could find nothing which could possibly explain this distribution, save the position of the small-pox hospitals.

DR. G. REID: That a small-pox hospital situated in a populous locality is, to some extent, a source of danger to the inhabitants is generally admitted, although all are not agreed as to the cause of the danger or its extent. In dealing with this question in his Paper, Dr. Buchanan fails to convey the impression that he has approached the subject with an unbiassed mind; in fact, he passes over in a few sentences the opinions of those who do not accept the theory of aerial convection as being proved. Indeed, in reading his Paper (I was not present at the meeting at which it was read), one can hardly avoid the impression that bias causes him to treat the opinions of those who differ from him rather with ridicule. The following quotation, having reference to the evidence which, according to Dr. Buchanan, some observers require in proof of the theory, I think indicates that this is so:— "... they appear to claim that aerial convection, if it occurs to all, must, during an epidemic, affect every house and every institution near the hospital. They are not content with a house-visitation rate of 5 per cent.; they demand that it should be 100 per cent. Having made that demand, they usually point to the escape of the 95 per cent. as evidence that aerial convection has not being operative."

While not accepting the aerial convection theory as being proved, my opinion is not based upon any such ridiculous proposition, but is the outcome of my own ex-

perience, and the experience of many others whose views are entirely disregarded in the Paper in question. Dr. Buchanan admits that there may have been examples of towns suffering from small-pox during 1900-04, where the facts proved to be negative as regards "hospital influence;" but, at the same time, he expresses his regret that he was unable to find them. It would be interesting to know what steps he took to discover such cases. Can it be possible that he had not heard of the Nottingham Small-pox Hospital action, which occupied the attention of a High Court Judge for five or six days, and in which numerous experts gave evidence both for and against the aerial convection theory, based upon previous as well as recent experience? Further, among the experts on that occasion whose evidence negatived the theory was Dr. Hope: who, in support of his opinion, instanced his recent experience in Liverpool during the same epidemic as that upon which Dr. Reece formed a diametrically opposite opinion regarding "hospital influence" and the aerial spread of the disease. Why should Dr. Reece's report receive full recognition and acceptance, and Dr. Hope's opinions be entirely disregarded, in Dr. Buchanan's Paper?

The subject is admittedly a complex one; expert opinion at the present time is probably equally divided regarding it; and the solution of the question will not be arrived at by discarding all arguments which do not fit in with one's preconceived theory, whatever it may be.

Personally, having regard to the nature of the contagion, I should have no difficulty in accepting the aerial convection theory, were it not that my own experience does not substantiate it; and when I find that the opinions of others who have had quite exceptional opportunities of judging as to the truth of the theory agree with mine, naturally I feel that my opinion is strengthened. The position as it presents itself to me is, that, while one must accept the fact that there are instances of "hospital influence," they probably can all be accounted for otherwise than by the aerial convection theory. On the other hand, there are many instances on record in which, while the circumstances have been peculiarly favourable to extension by aerial convection, no such extension has, as a fact, taken place; therefore, one cannot but question the accuracy of the theory. To my mind it is difficult, to say the least of it, to accept the theory in the face of often repeated negative experience in the case of hospitals situated in populous centres, surrounded by imperfectly-vaccinated communities;

and where, for many months at a time, and under all conditions of weather, acute cases of small-pox in considerable numbers have been under treatment.

I differ from Dr. Buchanan, and some others, regarding the comparative value of positive and negative evidence of "hospital influence" in such circumstances, as bearing on the question of aerial convection. By negative evidence we can, at any rate, prove that under conditions said to be favourable to aerial convection, such, as a fact, has not taken place; whereas, while positive evidence of "hospital influence" is no doubt met with in some cases, there are causes other than aerial convection to account for this; and because we cannot always prove that such causes have been in operation, this does not necessarily entail the acceptance of the aerial convection theory.

Like many others who now are sceptical regarding the theory, at one time I was a believer in it; but, as I have said, experience has caused me to doubt its accuracy. Without going minutely into detail, I would mention shortly what that experience has been.

We have seventeen small-pox hospitals in Staffordshire, with a total of 309 beds, serving a population of about 1,000,000; and I have records of fifty-seven distinct occasions on which the hospitals have been in use, the total number of cases isolated being 1,225.

Most of these hospitals are by no means well adapted for their purpose; and, from the point of view of aerial convection, they may be said, as a rule, to be dangerously situated. Only one complies with the Local Government Board's requirements as to distance from populations.

The following are the special features of the hospitals in this respect:—

In four cases the hospitals actually adjoin scarlet-fever hospitals; in two cases the hospitals are within 100 and 150 yards respectively of workhouses; in eight cases the hospitals are within 100 to 300 yards of populous centres; in eight cases the hospitals may be said to be actually surrounded by groups of houses.

Notwithstanding the above facts, I can confidently state that on none of the fifty-seven distinct occasions on which these hospitals were in use has a single case of small-pox occurred which, by any possibility, could be attributed to aerial convection from the hospitals. In one instance (the only one which occurred) an attack of small-pox was traced to one of the hospitals; but the patient, who was a road man, had been working for a week or two on the road

adjoining the hospital; and it was conclusively proved that he had frequent conversations with the patients, and had been in direct contact with some of them. This occurred at a time when the hospital was greatly overcrowded and understaffed, and the single protecting fence did not constitute a sufficient barrier against communication with the hospital from the outside.

I may mention that from this case others arose before the disease was recognised; and, had the origin of the first case not been traced to direct contact, aerial convection would no doubt have been credited with the occurrence.

Possibly it may be suggested that some of the hospitals referred to may have had too small a number of beds to justify much weight being attached to the absence of evidence of aerial convection; and, as I have no desire to attach more weight to my argument than the facts warrant, I have grouped the hospitals as follows, to show the actual number of beds. It must be remembered, however, that overcrowding frequently happened, and larger numbers of cases were admitted than the number of beds warranted: temporary provision being made for their accommodation:—

No. of Beds.			No. of Hospitals.	
5 to 10	7
12 to 16	4
17 to 22	4
60	2
			<hr/>	
			17	

DR. R. D. SWEETING: Whilst generally agreeing with Dr. Buchanan in his conclusions, I think that there is danger of overstating the case for aerial convection, by assuming that graduated intensity of infection around a small-pox hospital *per se* necessarily connotes aerial convection. In this connection, I cannot do better than reproduce the conclusion of the Royal Hospitals Commissioners of 1882 on this point. They say, on p. 26 of that Report:—

“One point appears to have made so much impression on several important witnesses that it should be specially noticed. Mr. Power himself clearly perceives, and fully admits, that the graduated distribution of disease around the hospitals, which is so remarkably illustrated in his report, is as explicable by personal communication as by atmospheric dissemination. *With such a hypothesis*, he says—meaning that of dissemination—*equally as with a hypothesis of conveyance by human movements, the gradation of hospital influence from centre to periphery would be in complete accordance.* The admission is so material that it ought to be developed. Let us replace the image of a wave, which is suggestive of one hypothesis, by an image equally appropriate, but

adjusted to the other. Instead of an expanding wave, let us imagine a number of converging and diverging lines of infection. It is plain, in the first place, that along these lines—the lines of hospital communication—the incoming and outgoing of persons and things would offer more occasion of disease wherever these persons or things were likely to pause in their route and by consequence more in the immediate neighbourhood of the hospital than farther off. But disregarding this effect of propinquity, and supposing that one quarter of a mile is as fruitful in occasions of contact with the carrier of infection as another, let us imagine that, on the different lines of communication which radiate to and from the hospital, infection is shed equably along the whole of their courses, and is represented on a diagram by black lines, varying in breadth according to the amount of infection which is dispersed, and in length according to the distance along which it is carried. It does not need the actual inspection of such a diagram to perceive that the effect of these lines in blackening each successive hospital ring will constantly increase as they close in on the centre; or, in other words, that the converging incomings and outgoings of the hospital would produce exactly the 'graduated intensity of infection,' from which, prior to reflection, we are tempted to infer an expanding wave. The argument is capable of arithmetical statement, and it would be easy, if it were worth while, so to exhibit it."

It is, therefore, clear to me, following this able and lucid statement of the Commissioners, that all that graduated intensity from centre to periphery of the mile circle around a small-pox hospital shows is *hospital influence*. But this may be aerial or administrative; and, therefore, it comes to this: that every instance of alleged spread from a small-pox hospital must be taken on its merits and fully investigated, before it can be said that aerial convection is fully established. Mr. Power did this in his 1884 Fulham Inquiry, when he conclusively eliminated administration. Dr. Buchanan has done this also to a large extent in regard to Gateshead and Purfleet; whilst in the Liverpool case of Dr. Reece, though administration is not formally excluded in the Report, this mode of spread was expressly eliminated by Dr. Hope, the Medical Officer of Health, whose position in this respect was taken as a postulate at the outset of the investigation. In brief, where you have graduated incidence from centre to circumference around a small-pox hospital, you must, by inquiry and reasoning, eliminate administration before you can prove—or even suggest with any force—aerial transmission. The graduated intensity of itself does not prove conveyance of small-pox infection through the air.

I cannot accept the view that the immunity of the district around Clayton Vale Hospital was due to the carbolic oiling of patients: for this reason that, at Fulham in the 1880-1881 epidemic, this was carried out to a large extent as matter of routine; whilst in 1884-1885, at the same hospital, when the numbers were expressly limited to a dozen or so, the oiling process was carried out

very fully and thoroughly. Masks, too, were often used, *e.g.*, of various substances, intended to prevent pitting. I cannot but think that the non-spread of small-pox from this hospital was rather due to non-aggregation of acute cases and paucity of susceptible material around the hospital. At any rate, it is a negative instance only, and of little value one way or another.

Finally, my experience at Fulham in 1884-85, when small-pox and fever were treated at one and the same time, on opposite sides of the hospital, and separately administered, is opposed to Dr. Ker's at Edinburgh. Cases of small-pox repeatedly cropped up in the scarlet-fever wards amongst imperfectly-vaccinated children; and it was only by re-vaccinating promptly all scarlatinal cases directly their temperatures dropped, that control was enabled to be exercised over these manifestations. Here, again, though aerial convection was highly probable, administration could not be altogether eliminated; for, in spite of elaborate precautions, I was accused of introducing small-pox into the fever wards, being the only person who visited both parts of the hospital.

PROFESSOR KENWOOD: The impression left upon my mind after the perusal of Dr. Buchanan's Paper was one of appreciation of the able manner in which he had handled certain facts, in an endeavour to make them prove his case for the distal aerial convection of small-pox. He even went to the extent of using the negative instance of hospital influence at Manchester as an argument in favour of this theory, without stopping to consider whether the precautions adopted in that hospital did not also operate against the dissemination of the disease by human agency. All must acknowledge that, in some recorded instances, a small-pox hospital has been the centre of a zone of exceptional incidence, and that this incidence has in some cases been graduated. The real point at issue is as to how the graduated zone of incidence, which is *sometimes* observed, is best accounted for. In the opinion of many of us, the manifold channels of direct and mediate infection through human agency offer the most acceptable explanation of all the hitherto-recorded facts.

In seeking for the true explanation of the nature and extent of hospital influence, it is necessary to keep in mind the facts that it frequently does not exist at all; that small-pox epidemics are generally characterised by the establishment of series of zones with graduated incidence, quite

irrespective of the locality of small-pox hospitals; and that the zone near the hospital is generally inhabited by the poorer section of the community. Small-pox hospitals are not placed in good-class residential districts; and the conditions of life, of housing, and of vaccination, among the poorer people, lend themselves to an increased incidence of the disease. It is the experience everywhere that the poorer part of the community suffers from small-pox generally two or three times more than the better-class part; therefore, is it fair to compare the incidence upon a broad hospital zone with a one-mile radius (and, therefore, over three square miles in area) with that of the rest of the town, which includes the better-class residences and residents?

- Although there is a tendency to exaggerate the true extent of hospital influence, I have no doubt that it has been in existence in some cases. But the fact remains that for every instance of alleged hospital influence it is easy to produce at least one instance (I believe I could produce two) where no such influences existed. That circumstance seems to me to tell most strongly against the theory of distal aerial convection. The aerial convectionists appreciate this difficulty, and they meet it by surmising that the favourable conditions for aerial convection are rarely in operation. Well, from the scant meteorological information to be gleaned in different epidemics, I have observed that at the time when aerial infection is claimed to be in operation, the most diverse atmospheric conditions have prevailed, such as high winds, low winds and calm; clear, blue sky and fog; keen frost and high mean temperature. Dr. Savill has shown that at Warrington the incidence of the disease was not greatest in the direction to which the prevailing winds blew; and Dr. Newsholme, in criticising the Liverpool experience, points out that the prevailing winds must have been blowing in opposite directions at the same time, if they were instrumental in carrying small-pox infection to two of the hospitals which are alleged to have shown hospital influence. It is to my mind preposterous to tell us that the infection can be carried for a mile, but that the necessary atmospheric conditions in our ever-changing climate may be unfavourable for the period of nine months after acute cases are admitted to the hospital (as at Liverpool); and that it may not come into operation at all throughout the whole epidemic, in at least as many instances as it does do so, even where (to adopt Dr. Buchanan's favourite formula) "there is no evidence" of any difference as to

vaccination among those who occupy the mile-zone and those who live beyond it. Surely, if the theory is true, the operation of cause and effect should be observable in every case where a fair opportunity is presented for the experiment. Dr. Buchanan's travesty of our position in the matter has been referred to by Dr. Newsholme. Nobody in his senses maintains that every individual within the mile-zone should catch small-pox; but we do say that *some* of the susceptible people should do so in *every* instance where the hospital is occupied by many acute cases for many months. This is what I understood Mr. Justice Farwell argued in the Nottingham case; and, if so, Dr. McVail's criticism is unjustified, because it is based on the assumption that the learned Judge's argument that a negative instance unexplained spoilt the chain, had reference to infection from an individual, instead of to a negative instance of distal aerial convection from a small-pox hospital.

The operations of human intercourse are often untraceable, and they necessarily vary with the efficiency of the hospital administration. It is never practicable to exclude all the possibilities of mediate infection due to human intercourse; and those of us who have had much experience of small-pox appreciate how difficult it is to define the set of conditions which may determine the spread of infection; and we have had many opportunities of noting how the element of chance appears to come in: so that on two occasions, which appear to be comparable in every respect, the infection gets a hold in one case and fails to do so in the other. Some of the arguments advanced appear to suggest that because you cannot trace the human agency (direct or mediate) in the spread of infection, aerial spread is the only possible explanation. Well, during my thirteen years' experience as a health officer, I have not succeeded in tracing the channel of infection in quite one-fourth of the cases of scarlet fever, diphtheria, and other communicable diseases that I have investigated. Am I to console myself for my failure by such a theory as that of distal aerial convection? Faulty administration, the wilful withholding of information, accidental contact with unrecognised (and, therefore, never known) cases, imported infection, and the many circumstances favouring spread which obtain most among the poorer classes, all combine to account for small-pox hospital influence. When I hear anyone denying the possibility of leakages and of breaches of hospital regulations, I envy him his faith in things human;

and I think of the simple faith that once was mine, and which has been so rudely shaken by my subsequent experiences of hospital administration.

Dr. Buchanan asserts that there is "no evidence" that personal communication between the hospital and the quarter of a mile zone is greater than that between the hospital and the more distant zone. It is difficult to believe that he states this seriously. The hospital is the centre, during an epidemic of a large amount of traffic (ambulances, visitors, staff, workmen, tradesmen, etc.), which converges, and therefore becomes more and more intensified in each successive zone as the hospital is approached. I see no difficulty in accepting this circumstance as the explanation of the graduated incidence which is sometimes observed; especially when it is borne in mind that the communication with the hospital staff is always greatest nearest the hospital. Despite what Dr. Buchanan asserts, we generally, in my experience, draw some of our staff from very near the hospital; and, in respect to this point, Dr. Goodall has forwarded me the following statement:—

"I think it is somewhat strong to make a general statement that hospital staffs are not recruited from the neighbourhood of the hospitals. I am sure that this cannot be said of the fever hospitals of London, except with respect to the nursing staff. At Homerton, at any rate, a considerable number of the ward-maids, laundry-maids, porters, etc., come from the immediate vicinity of the hospital. Even if they have not relatives, they have friends quite close to it. And these classes of staff all come in close contact with infection in the hospital.

"Of recent years the Asylums Board, in order not to maintain a too numerous staff, have systematically transferred members of the staff from one hospital to another. I have had many such at Homerton from other hospitals of the Board. Now, it is quite common for these people to resign after being with us for a few weeks; and a frequent reason given for resignation is: 'This hospital is too far from my home; and I want to go back, if possible, to — Hospital, because my friends live near it.'

"In epidemic times a staff usually has to be got together in a hurry; and it would be extremely inconvenient, if not difficult, with respect to these lower grades of the staff, to make sure that applicants from the immediate vicinity of the hospital were excluded."

Before the theory of aerial convection is accepted there should be unequivocal evidence of its soundness, because of the momentous issues which are involved in its acceptance. It should be demonstrated that the usual methods of spread of infection can be excluded (if that is possible); that the number of homes in which the infection cannot be traced to the usual channels diminishes with the distance from the hospital; and that the localised outbursts within the hospital mile-zone are *always* to be observed when there are many susceptible persons living for several months near

a hospital containing many acute cases. It is not permissible to lump together all the infected houses, whether they have been infected by human intercourse or not, and claim that the results prove aerial convection, when they at most only indicate the origin and spread of the infection. How such a practice may mislead has been alluded to by Dr. Hope; and Dr. Clayton (the Medical Officer of Health of Gateshead) claims that of the 56 cases within half-a-mile of the small-pox hospital, the infection was traced to human agency in all but four cases.

Dr. Reece's excellent slide demonstration showed us a long latent period of some nine months, during which patients were admitted to the Priory Road Hospital at Liverpool before any marked incidence occurred in the neighbourhood. Prior to this, there was a localised outbreak in a crowded area in the heart of the city, and from this centre the infection spread towards the hospital zone; and there, among "a slum population," it lingered and persisted. Moreover, the increased incidence in the hospital zone was synchronous with excessive incidence in the other hospital areas, *and in Liverpool outside the hospital areas*; and the incidence of small-pox upon the Parkhill Hospital area had set in shortly before the hospital commenced to receive patients. This does not appear to me to be very convincing evidence even of hospital influence, much less of aerial convection.

An argument adduced by Dr. Niven against human agency being the explanation of the mode of conveyance of infection from a small-pox hospital is, that he regards the infection of contacts as unusual and difficult. My experience on the whole supports this, although I have known several instances which point to the opposite conclusion; but the patient, before he gets into the hospital, has often the acute stage well developed; and the circumstance that so many of those who come in contact with him escape infection, though not protected by vaccination or re-vaccination, appears to me to tell against the theory of distal aerial convection. If the striking power of the infection is limited so near its origin, is it likely to be operative when more diluted and at the distance of a mile?

Hutchison's experiments with *B. prodigiosus* sprayed into the air of the parade ground at Göttingen, and recovered nearly 2,000 ft. away, is a remarkable one, but I do not see that it is any argument in favour of the distal aerial convection of small-pox. Assuming the observation to have been made with all the necessary control experiments (for

prodigiosus is commonly found in air), how long did it take to cover the distance of one-third of a mile? Under an hour, I believe: *not nine months!*

It is contended that because you do not get hospital influence in scarlet fever, and you do in small-pox, there must be some material difference in the quality of the infection of the two diseases. The contention is reasonable, but it tells no more in favour of distal aerial convection than of the alternative view.

A most interesting discussion has resulted in some new light being shed, and some fresh arguments adduced, on both sides. It is sincerely to be hoped that when further opportunities of testing this matter occur, the fullest advantage will be taken of them, and that the inquiry will be conducted in a scientific spirit and devoid of all bias. Meanwhile, I contend that the theory of distal aerial convection has never been justified by the data which have hitherto been adduced in support of it; and that is the reason why it does not appeal to the general body of Medical Officers of Health or to His Majesty's Judges.

DR. WELLESLEY HARRIS: I listened with considerable interest to the discussion raised by Dr. Buchanan's Paper on this important subject; but could not find anything in the statements of the speakers who so forcibly ridiculed its possibilities to, in any way, explain how various extensions of the disease which had come under my observation could have occurred except by aerial convection.

The examples to which I would refer occurred during the period—nearly twelve years—I was Medical Officer for the borough and port of Southampton.

For several years the borough relied upon two private houses situated in the poorest quarter of the town, which they had converted into what they were pleased to term a Small-pox Hospital; and it was found that when four or five cases only were isolated the disease did not spread in the neighbourhood, but whenever a large number of cases were housed in the hospital at the same time, the disease quickly showed itself among the residents of the streets in the immediate vicinity.

Now it is an important fact that in the years 1891, 1892, 1893, and 1894, the first cases of small-pox in Southampton were all imported by ships, most frequently from the West Indies and the River Plate; and in those instances where cases occurred in the town they were subsequent to the isolation of a number of cases from ships in the Hospital.

The question of contact by outside people with the staff can be absolutely disregarded. Special arrangements were made for delivery of food, by which tradesmen were prevented from coming into contact with the staff. The nurses and the other staff consented to forego "leave," and refrained from entering the town while cases were in the Hospital, special arrangements being made for their recreation by means of boating, access to which was gained by a private landing-stage at the rear of the Hospital.

So strong was the opinion that the Hospital was responsible for the repeated extension to the immediate neighbourhood, that a floating hospital was provided for the isolation of small-pox cases, and the land hospital abandoned.

It is strange if it should be by mere coincidence, after the establishment of the floating hospital on the broad estuary of Southampton Water, some two miles from the town, that no further extension of small-pox to the town occurred from imported cases; and in one instance as many as sixteen cases of a severe variety were removed from one ship.

I think the arguments used by one speaker, Dr. Hope, and the spot maps entirely fallacious. To merely spot out positions in which cases of small-pox have occurred in a town, and to draw circles and say that if small-pox hospitals had existed in those districts they would have been blamed, seems to me an unreasonable argument.

I also fail to see the force of a statement which was made by one speaker that, in regard to the aerial convection of small-pox he took up the position of an agnostic, and would not believe it until it was proved. Surely, rather the reverse position should be taken, and aerial convection believed until disproved. This is the view I think the speaker would adopt with regard to other principles.

Personally, I still believe that the aggregation of a number of cases, or severe cases, of small-pox in badly-situated hospitals leads to the spread of the disease by aerial convection to persons residing in the neighbourhood. Whether this be right or wrong, I don't think there can be any doubt that it must be an undesirable practice to establish small-pox hospitals near populous districts.

If this is agreed, what does it matter whether those who believe in aerial convection are right or wrong, as long as the public are safeguarded from the dangers which follow from what, I venture to think, is an improper form of isolation. As far as I am personally concerned, far weightier

evidence than that which has been offered up to the present must be produced before I can alter the opinion which is ingrained in me by the experience I have had.

LIEUTENANT-COLONEL A. M. DAVIES : There seems to be a reluctance to accept the theory of aerial convection ; but, with a movement of air which is only just perceptible, a distance of one mile would be traversed in half an hour. Dr. Thresh showed that there had been a decided wind-prevalence, blowing from the Hospital Ships in the direction of Purfleet : to have been noticeable, this velocity must have been at least eight miles an hour, at which the distance (three-quarters of a mile) would have been traversed in less than six minutes. If the air of a sick-room in which is lying a small-pox patient (or 100 such patients) contains infective material—as to which, I suppose, there is no doubt—why should we conclude that the infectivity of the particles in that air is lost six minutes, or even half an hour later, merely because it has been carried a few hundred yards, or even a mile or two ? With a distinct but not strong wind blowing in the given direction, the air, with its contained infected particles, would traverse the three-quarters of a mile in less than three minutes. An outbreak of enteric fever at Quetta, India, in 1898, was almost conclusively traced to air-borne infection, conveyed a distance of about 2000 yards by prevalent winds ; the *contagium* of small-pox is probably more resistant than that of enteric fever to sunlight and desiccation.

The chief argument against aerial convection, to my mind, is that adduced by Dr. Newsholme in reference to the Stockwell Hospital, where a spot map showed the usual concentration of cases towards it ; but only *on one side*, a railway on the other presenting no obstacle to aerial convection, but an insuperable barrier to human intercourse. In the Essex outbreak the Thames, through a barrier to human intercourse, did not impede the carriage of infection. Are there any other examples to corroborate that of Stockwell ?

If Mr. Henman's device of filtering the air leaving a hospital ward, in connection with the Plenum system of ventilation, were carried out, small-pox hospitals would presumably be deprived of their dangerous characteristics, so far as aerial convection is concerned. Has this been practically tried anywhere ? It would surely be worth while to make an experimental installation, which might settle the question.

DR. J. T. C. NASH: I regret that the exigencies of time have prevented me from hearing any part of the discussion on Dr. Buchanan's able Paper. The general facts adduced by Dr. Buchanan have, to my mind, materially strengthened the "aerial convection" theory so ably advanced by Mr. Power.

So eminent, however, are the authorities behind this theory, that there is a danger of its becoming blindly accepted as proven; but the true scientific spirit recognises that the greater the authority the more necessary is it to subject all the particulates to keen criticism and unbiassed judgment.

To criticise Dr. Buchanan's data in some detail:—

Surely, the excessive incidence in the very large district of Orsett should hardly be ascribed to the influence of the Hospital Ships. Yet Dr. Buchanan compares the attack rate in the twelve rural parishes comprising the very extensive rural area of the Orsett Union, which was 18.5, and that of the urban district of Grays (a riverside town no less than three miles below the ships), which was 16, with the attack-rate in London, which was only 2 per 1000. Are the districts or conditions really comparable? Dr. Buchanan and Dr. Thresh agree that the districts concerned were well organised to deal with small-pox in the way of isolation and disinfection; but how about that most important of all measures in the *prevention* of small-pox, viz., antecedent vaccination? In the vaccination returns for the county of Essex, recorded in the Thirty-First Annual Report of the Local Government Board, 1901-2, we find that nearly 50 per cent. of the children born since 1893 were unvaccinated in 1901. Anti-vaccination is notorious in this district, fostered by an anti-vaccination press. The number of re-vaccinated persons in the district must have been very small. Want of vaccination was, in my humble opinion, the chief reason why there was an exceptional incidence of small-pox in the Orsett district in 1901-2.

With respect to the cases occurring among workmen employed in erecting a temporary hospital on the flats on the Kent shore, it is admitted that personal communication between members of the staff of the hospital ships and their workmen occurred. No further comment is therefore necessary.

With respect to cases of small-pox occurring among the crews of vessels which anchored in Long Reach, surely these could be more easily explained by surreptitious visits

ashore at Purfleet, Grays, Aveley, etc., than by the proximity of the hospital ships. In my own district a case of small-pox occurred in December, 1901, which I traced to a visit to a house in Aveley, in the Orsett Union, from which two cases of small-pox had recently been removed to hospital.

In 1902, again, I had a case occurring in an unvaccinated girl, whose father, I ascertained on inquiry, had recently been working on the Kent flats, near Dartford. The father was vaccinated, but had carried home infection to his unvaccinated child. The father was a disbeliever in vaccination, and also seemed to have a contempt for common precautions. Judging by his statements, a fairly free communication took place between the workmen and small pox contacts.

Finally, it is notorious that the Metropolitan Asylums Board permitted friends to visit patients on board these Hospital Ships with a freedom not above criticism.

In view of the foregoing statements, I cannot agree with Dr. Buchanan that the incidence of small-pox in the Orsett Union, on the Kent shore, and on the crews of vessels anchored near the Hospital Ships, affords so much support to the aerial convection of small-pox as he and Dr. Thresh assume.

On the other hand, in view of the undoubtedly greater contagiousness of small-pox and chicken-pox, as contrasted with scarlet fever, I am quite prepared to admit that, occasionally, a small-pox hospital may be responsible, to some extent, for aerial convection: more particularly when aided by those notorious germ-carriers—flies.

Glancing at the tabular statement of Glasgow small-pox in Dr. Buchanan's Paper, one cannot but notice that in 1894 the largest percentage of cases came from the central administrative district, and in 1895 from the west district; so that the greatest incidence in two out of the seven years was not in the immediate proximity of the Small-pox Hospital in the east district.

Dr. Clayton, Medical Officer of Health for Gateshead, has shown* that the great majority of cases occurring there could be traced to direct contact, and, therefore, these cases fail to support the theory of aerial convection.

The Manchester evidence is directly negative.

After reviewing Dr. Reece's Report on the behaviour of small-pox in Liverpool, Dr. Buchanan speaks of "hospital

* *Journal of the Royal Sanitary Institute*, vol. xxvi, No. 4.

influence," a much safer term than aerial convection; but unfortunately, he swings back to the hypothesis of aerial convection as the explanation of hospital influence. For myself, apart from the agency of flies, I think the thesis of aerial convection is still not proven.

Insect agency, human imperfections, and errors of administration, no doubt account for some instances of undoubted "hospital influence"; and for these reasons I think the Local Government Board is wise in insisting on the isolation of a small-pox hospital.

Further, there is reason for thinking that the infective agent of small-pox is a spore-forming protozoon, and therefore capable of resisting adverse influences, such as fresh air and sunlight, to a greater extent than—for instance—the typhoid bacillus or the diphtheria bacillus, and the probably non-sporing bacterium of scarlet fever.

Therefore, one would expect more frequent instances of the aerial convection of small-pox than of the other chief infective diseases; but while I am quite prepared to admit, on the grounds I have just named, that "hospital influence" is more likely to be met with in cases of small-pox than with the other infectious diseases, I still think that the majority of cases, occurring outside a quarter-mile or half-mile radius of any small-pox hospital, arise quite independently of any hospital influence.

DR. HAMER: Dr. Buchanan has pointed out that at Felling and Purfleet "hospital communication and traffic" were practically non-existent; and that, none the less, exceptional incidence of small-pox in the hospital neighbourhood occurred. Local peculiarities render these two instances particularly instructive; but it may be observed generally, apart from special circumstances, that the fact that hospital influence continues to manifest itself with apparently unabated vigour under twentieth-century conditions, is in itself an important one. "Hospital communication and traffic" at the present time, and in the 'seventies and early 'eighties of the last century, are not one and the same thing. The mischief, if not altogether done away with, has at least been reduced within very narrow proportions by precautions such as are nowadays taken. Mr. Power, at Fulham in 1881, was able to set on one side this source of risk, as not in itself sufficient to explain the phenomena observed; and in comparison with the difficulties presented then, those met with in the modern instances must be regarded as insignificant.

Another aspect of the Purfleet case, which was referred to by Dr. Reece, is deserving of study. The "hospital communication and traffic," which can be excluded from consideration in Purfleet itself, were operative, of course, mainly upon the population of London. Indeed, an experiment on a large scale has been in effect carried out since 1885 upon this population; for in London the influence of hospital communication and traffic, exerted during the last twenty years, can be studied almost entirely uncomplicated by any influences exercised by aerial convection.

Sir Shirley Murphy's diagram, exhibited by Dr. Reece, shows that in precise and abrupt correspondence with the inauguration of the new departure of removing small-pox cases to Long Reach, small-pox ceased to prevail in London on the scale exhibited when the hospitals were within the metropolitan area.

DR. BULSTRODE: Certain of the speakers have expressed the view that the remarkable graduation in the incidence of small-pox which has been observed on so many occasions around small-pox hospitals could on general grounds be as well explained upon a thesis of personal infection as upon a thesis of aerial convection; the implication presumably being that the history of each case *quâ* exposure to infection, etc., must be carefully inquired into before any conclusion can be arrived at. Although this statement is in some degree true, it needs, I would suggest, material modification before it can be accepted in its entirety. The statement as it stands can only apply to those instances in which the topographical and social conditions around the hospital are such as to allow equal opportunities for personal infection in all directions: a circumstance which is but rarely found. A graduated intensity might perhaps be expected as the result of personal infection on the main road of ingress and egress from the hospital, but not, I think, in quadrants; wherein the streets could only be reached by a long and circuitous route from the hospital gates. It would not, I expect, be seriously contended that in the repeated outbursts around the Fulham Hospital the distribution of the cases could be, *à priori*, as well explained by personal infection as by aerial means.

Other speakers have implied their intention of not accepting aerial convection as a *working hypothesis*, basis and type, until it can be definitely proved. These are the agnostics, and doubtless the proof with which alone they will be satisfied may be long in coming. Logically,

however, these speakers are also agnostics with reference to the nebulous hypothesis, the atomic theory, and the undulatory theory of light; and to be consistent they should refuse to take note of these theories in their endeavours to explain natural phenomena, to which, in the opinion of most physicists, these theories afford a key. None of these theories have yet been, or are likely to be, proved; but those who refuse to accept them must surely find themselves in a somewhat difficult position.

The question with regard to aerial convection is not as to whether it has been proved, but as to whether, in the present state of our knowledge, it is the best working explanation of the peculiar phenomena which have been repeatedly observed around hospitals devoted to the isolation of *small-pox*, but not around hospitals devoted to the isolation of *other infectious diseases*. No one has, so far as I am aware, offered any explanation of the fact that no special incidence of infectious disease has been observed around hospitals devoted to infectious diseases other than small-pox. Surely, if the behaviour of small-pox around small-pox hospitals can be in all instances explained by personal infection, it would be reasonable to anticipate that some such manifestation would be sometimes observed around hospitals devoted to other infectious diseases.

Some of those who have been the most strenuous opponents of aerial convection would appear, at any rate until quite recently, to have regarded this theory not only as improbable, but even as largely inconceivable, and they have lost no opportunity of casting ridicule upon it. To my mind, there are numerous facts which suggest not only its possibility but its probability. Indeed, I personally see nothing contrary to expectation (to quote Paley) in the theory, either on zoological, meteorological, or statistical grounds. To ask for proof is to place oneself beyond the range of criticism. What outbreaks have been conclusively *proved* to have been caused by any definite agency? Surely, it is almost always a question of degrees of probability.

There would seem to be every reason, arguing on analogy, for assuming that the virus of small-pox is a micro-organism of more than ordinary resistance. Its high infectivity, and its admittedly long striking distance, would suggest that on these points this disease should be placed almost in a category of its own. Assuming, therefore (upon evidence which surely most epidemiologists would accept), the special resistance of this organism, what is there to prevent its carriage by the wind, when mixed

with other particulate matter, for distances with which the ordinary demands of the aerialist are but trifling.

Darwin, in the *Voyage of the "Beagle,"* refers to the falling of fine dust upon the decks of ships far out in the Atlantic; and some of the dust, which he himself collected, was found to consist, in great part, of infusoria, with siliceous shells, and of the siliceous tissue of plants.

In five little packets which Darwin sent to Professor Ehrenberg, no less than sixty-seven different organic forms were found. Darwin adds:—

"The infusoria, with the exception of two marine species, are all inhabitants of fresh water. I have found no less than fifteen accounts of dust having fallen on vessels when far out in the Atlantic. From the direction of the wind, whenever it has fallen, and from its having always fallen during those months when the Harmattan is known to raise clouds of dust high into the atmosphere, we may be sure that it all comes from Africa."

As regards the retention of the vitality of seeds when carried by one or another agency from one land to another, suggestive references will be found in the chapter on the geographical distribution of species, which is contained in Darwin's *Origin of Species*. Alfred Russell Wallace, in his *Island Life*, when writing on the dispersal of seeds, says;—

"The seeds of plants are liable to be dispersed by a greater variety of agents than any other organisms; while the tenacity of life under varying conditions of heat and cold, drought and moisture, is also exceptionally great.

"An immense number are specially adapted to be carried by the wind, through the possession of down or hairs on membranous wings or processes; while others are so minute, and produced in such profusion, that it is difficult to place a limit to the distance they might be carried by gales of wind or hurricanes."

In the *Quarterly Journal* of the Royal Meteorological Society, January, 1904, vol. xxx, No. 129, there is an account by Mr. H. R. Mill, D.Sc., and Mr. R. G. K. Lempfert, M.A., of the great dustfall of February, 1902, the evidence pointing to the conclusion that the dust which was deposited so widely over the South of England was brought by the winds from the North-west coast of Africa.

The nature of this dust was investigated by Mr. J. S. Flett, M.A., D.Sc., and it was found that—

"Plant *débris* was very abundant: Vegetable hairs occurred everywhere often in great quantity; fragments of epidermis and of cuticle, siliceous skeletons of the epidermis of grasses spores of cryptogams (either singly or in masses), spore cases, resting spores of fungi, hyphæ and various unicellular or filiform, algæ, were all to be found. Diatoms were not frequent—in fact, they were comparatively rare, as were also desmids. Many of the samples sent in a moist condition were full of bacteria."

I have given these extracts, not because I wish to imply that small-pox can be carried over the long distances above referred to, but in order to furnish evidence pointing to the probability of the resistant seeds (vegetable organisms) of disease being carried with other particulate matter over, at any rate, a mile or so from hospitals.

I doubt, however, after Dr. Buchanan's admirable Paper, whether much will be heard as to the *impossibility* of aerial convection.

The following extract from the *Lancet* of July 23rd, 1904, is also of interest in relation to the carriage of soot in the direction of the prevailing wind:—

"Dr. William Butler, the Medical Officer of Health of Willesden, has made some interesting observations upon the smoke and dust nuisances in this district. Allegations were made as to the dust which was stated to come from the Metropolitan Electric Supply Works; and, with the view of throwing light upon this question, Dr. Butler procured twelve boxes, each of the superficial area of one square yard and of a depth of six inches. The bottoms of the boxes were covered with glazed American cloth, to facilitate the removal of accumulations. These boxes were placed in the back gardens at twelve different sites, and in such a position that whatever was deposited in them from the atmosphere would be retained. This dust was collected every twenty-four hours and placed in labelled pill-boxes, and upon examination it was found to consist of black carbonaceous particles distributed among a much finer black powder. These particles consisted of 'miniature cinders,' or the incomplete combustion of fine coal. Extraneous particles were, so far as practicable, separated and the coal dust was weighed, and it was found that substantial deposits were only to be found in those places which lay from the chimney in the direction of the prevailing wind, and the nearer the chimney was approached the larger was the quantity of dust collected."

There is one instance illustrating graduated incidence around a small-pox hospital which appears to have escaped notice by many of us; and it was only quite recently, while looking through some old Reports of medical officers of health, that I discovered in the Annual Report for 1899 of Dr. Wright Mason, of Hull, a diagram which illustrates the point in question. It will obviate any misunderstanding as to the purport of the diagram, if I abstract in full Dr. Mason's observations with regard to it.

Referring to the Garrison Hospital in which the small-pox cases arising in Hull were treated, he says:—

The accompanying diagram shows the percentage of houses of which one or more of the inmates were attacked by small-pox in the north-east, north-west, and south-west quarters, and in the various zones, taking the hospital as a centre. It will be observed that in the south-east quarter no cases are shown. The hospital was situated in close proximity to the river Humber, and in this quarter no houses intervened between the hospital and the river. Within the quarter-zone there were only some seven houses, and of these one was invaded. On account of the numbers being so small, it is not thought advisable to show the percentages and the zone.

It will be noticed that in each quarter the percentage of houses invaded

becomes progressively less as the distance from the hospital increases, with the exception of the "three-quarter mile" zone in the south-western quarter, which district is almost entirely occupied by docks, warehouses, and railway goods-stations.

It may be pointed out that of the 305 days from March 2nd to December 31st, the Meteorological Report shows the direction of the wind to have been from the south-west on 84 days; and it will be noticed that the percentage of invaded houses was much greater in the north-east quarter than in others, this being the direction in which the south-westerly wind would blow after passing over the hospital. The next more-prevailing winds were north-westerly, which occurred on 53 days, so that the number of days on which a south-westerly wind prevailed exceeded by 31 days the next most prevalent.

Dr. CHALMERS (Glasgow) has forwarded advance pages of his forthcoming Annual Report to the Corporation of Glasgow, and he has marked two paragraphs among those which deal with the recurrence of small-pox in Glasgow during 1903-4 as a contribution to this discussion.

Interval of Freedom from the Disease.—Between August, 1902, and September, 1903, only one case of indigenous small-pox occurred in Glasgow. This occurred in May, 1903, in the wife of a seaman who himself had developed the disease a few days after his return from a voyage to Spain, and was in the eighth day of his illness before its nature was recognised, and removal to hospital took place. Two other cases, also in seamen, had occurred in January and February, 1903, but; with these exceptions, Glasgow was free from small-pox during the period already indicated.

In September, 1903, however, it was introduced among the model lodging-house population on the south side of the river by a worker from Talla Water-works, and for several weeks at the beginning of the outbreak no cases occurred among the general population; but, during November, and more particularly as December advanced, the general population of certain districts were invaded, and, as the outbreak extended, its direction followed the main line of preceding prevalences.

* * * * *

TABLE III.—In this table the cases and deaths in each ward are shown, together with the attack- and death-rates. The ward populations stated are the means of the annual estimates, which are based on the number of inhabited houses in 1903 and 1904. It will be remembered that in the 1900-2 outbreak, the attack-rate for the city was 2.3 per 1,000; on the present occasion it was 1.5 per 1,000. In the earlier outbreak the attack-rate in the London Road Sanitary District was 9.97; in Barrowfield, 6.46; and in Calton, 4.24 per 1,000. By the adoption of the wards as sanitary districts, those of Dalmarnock, Mile-end, and

Calton now represent generally the above-named districts, and here the attack-rate again is considerably in excess of that for the wards further distant from the hospital, or for the whole city, the figures for the wards just named being respectively 4.1, 3.7, and 1.7. Only one other ward presents an incidence corresponding to the higher rates here shown, viz, Broomielaw, where 25 cases occurred out of a population of 8,000, giving a rate of 3.04. But it should be explained that 12 of these cases were removed to hospital within one fortnight, and 10 at least were known to have been in association with a case of hæmorrhagic small-pox, which had proved fatal, and had not been recognised as small-pox until the occurrence of the disease among the patient's friends.

It must be remembered, however, that while the attack-

TABLE III.—GLASGOW: SMALL-POX, 1903 AND 1904.

Return of Cases and Deaths in each Municipal Ward.

Municipal Wards.	Mean Population.	Cases.		Deaths.	
		Number.	Rate per Million.	Number.	Rate per Million.
1. Dalmarnock, Hospital in this Ward	50,757	208	4,098	20	394
2. Calton ...	38,463	66	1,716	4	104
3. Mile End ...	43,174	160	3,706	11	255
4. Whitevale ...	33,432	57	1,705	5	150
5. Dennistoun ...	33,378	15	449	—	—
6. Springburn ...	41,823	37	885	3	72
7. Cowlairs ...	25,059	7	279	2	80
8. Townhead ...	39,671	33	832	5	126
9. Blackfriars ...	22,792	25	1,097	2	88
10. Exchange ...	2,235	2	895	—	—
11. Blythswood ...	3,540	3	847	—	—
12. Broomielaw ...	8,214	25	3,044	2	243
13. Anderston ...	29,297	16	546	—	—
14. Sandyford ...	26,216	31	1,182	4	153
15. Park ...	25,087	3	120	—	—
16. Cowcaddens ...	39,075	32	819	1	26
17. Woodside ...	45,356	26	573	2	44
18. Hutchesontown ...	41,719	73	1,750	7	168
19. Gorbals ...	36,550	53	1,450	4	109
20. Kingston ...	34,859	53	1,520	3	86
21. Govanhill ...	33,835	34	1,005	2	59
22. Langside ...	31,142	7	225	—	—
23. Pollokshields ...	17,358	2	115	—	—
24. Kelvinside ...	19,371	2	103	—	—
25. Maryhill ...	36,973	3	81	—	—
Institutions ...	18,509	180	9,725	14	756
Harbour ...	1,241	1	806	—	—
No. Residence ...	—	4	—	—	—
City ...	779,126	1,158	1,486	91	117

rates are here calculated over the whole population, the incidence of the disease on the section among whom the disease occurred, viz., the proportion unprotected by recent vaccination, would be considerably greater, because a very large number of those (over 400,000) who were revaccinated during 1900-2 will still form part of the present population. If, instead of calculating the attack-rate over the whole population, it had been possible to calculate it on the susceptible proportion only, it is doubtful whether there would have been any reduction in the rate at all. It is also worth remembering that the proportion revaccinated in 1901 in the districts immediately surrounding the hospital was greater than in the other districts where the disease was less prevalent; so that the greater incidence in these districts on the present occasion is all the more striking.

DR. BUCHANAN: I am under great obligation to all those who have contributed to the Discussion on my Paper, not least to those who have disputed my conclusions; and I welcome this opportunity to express my thanks.

If, with the benefit of this Discussion, I were now to re-write my Paper on the spread of small-pox by small-pox hospitals during the epidemic period, 1900-4, there would be some new matter to add, and some directions in which it would be desirable to develop its argument. I do not think, however, that I should have cause to modify what I have written. In dealing with the new matter, and with observations which have been by various contributors, I am, fortunately, able to be brief. The facts reported speak for themselves; while on the argumentative side, the contentions raised by some contributors have been in many cases replied to by others.

The introductory paragraphs of my Paper, I think, make sufficiently clear my reasons for selecting for inquiry the experience during the recent epidemic period of London, Glasgow, Liverpool and Gateshead: to which towns, with some hesitation for reasons stated, I also added Manchester. After giving the main facts as to hospital influence for each of these towns, I went on to say (p. 162):—

“Besides Manchester, there may have been other towns in the Kingdom during 1900-4 in which there has been a notable prevalence of small-pox, in which small-pox cases have been received in a hospital or hospitals so situated in regard to populous areas as to permit the matter to be tested, in which it has actually been tested by careful study, in which the facts have been exactly recorded, and

have proved negative as regards hospital influence. If such cases exist, I regret that I have not been able to find them, and consequently have not included them in my Paper."

This statement was made in all good faith, and in the hope that in the Discussion other instances, positive and negative as regard hospital influence, would be brought out, which satisfied the requirements which I had formulated, or could be utilised for scientific examination of the subject in hand. This has been the case. On the one hand, Dr. Ker has given us an interesting account of the Edinburgh small-pox hospital during 1904: negative as to hospital influence; on the other, Dr. Wellesley Harris has narrated his experience of hospital influence at Southampton; and Dr. Thresh has told us of recent distribution of small-pox in Essex in relation to the Dagenham hospital. In addition, although before the period dealt with in the Paper, we have heard from Dr. Niven the story of the relation of small-pox and the Westhulme (Oldham) Hospital in 1892-3, and the very instructive instance of hospital influence round Monsall Hospital (Manchester) in 1894.

Negative instances, in the sense of the quoted paragraph, are of the greatest value. They should be looked for, examined in detail, and considered in all their bearings along with positive instances. I need not add to the observations of Dr. McVail on this point. But I am disposed to agree with Dr. Thresh when he draws attention to the paucity of "negative instances," which can be regarded as helpful to a consideration of the "frequency" with which hospital influence has been exerted. There is no value in the negative instance if the hospital is in a desert; or if the hospital stands in the centre of a town, but has contained only an insignificant number of acute cases of small-pox at any one time; or if the facts as to house-invasion round the hospital have not been ascertained. These are extreme instances, of course, but the same considerations apply in other cases. I have been taken to task for a sentence in which I pointed out that, in several epidemics where hospital influence has been clearly manifested, the proportion of dwellings near to the hospital which have been invaded by small-pox has not been 100 per cent., but usually something much nearer 5 per cent. The reminder, however, was not wholly unnecessary. Suppose a small-pox hospital, in use during an epidemic, and containing sometimes 20 or 30 acute cases at one time, had within half a mile of it 200 dwellings and 1,000 population. I take this number so as to make the hospital

transgress the Local Government Board standards, to which so many references have been made. If during the epidemic these 200 dwellings showed a house-invasion rate of 5 per cent., or 10 houses, and this were a higher rate than elsewhere in the district concerned, there might be reason to suspect "hospital influence." The case would be worth detailed inquiry. But unless the circumstances were very exceptional, I would hesitate to conclude that hospital influence had been exerted when the figures to go upon were so small. On the other hand, if none of the 200 dwellings were invaded by small-pox during the epidemic, there would be reason to suspect a useful "negative instance." Here would be a case in which, *prima facie*, it would be reasonable to expect to have got some evidence, however slight, of hospital influence. In this case, also, the circumstances would repay full inquiry. But the hesitation in drawing definite conclusions, which would apply if the positive result had been obtained, should also apply to the negative.

I have laboured this point, because it seems to me that misuse of the term "negative instance" lies at the root of much misunderstanding on the subject: illustrated, in the Discussion, by the objection taken by Dr. Reid and Professor Kenwood to my "selection" of cases, and by Dr. News-holme's observations (p. 186), on "high percentage of failure."

If any small-pox hospital near which there has been no small-pox is accepted as a negative instance, it is not at all surprising that Dr. Kenwood can produce one such instance to set against every case in which hospital influence has been exerted; while none will question Dr. Boobyer's statement that the Local Government Board (who, by the way, have never attempted such an inquiry) have found no evidence of hospital influence "in the case of four-fifths of the hospitals in the country." Like Dr. McVail, I hoped that Dr. Reid (of whose evidence in the "Nottingham case"* I was aware) would give the Society some valuable data regarding his seventeen small-pox hospitals in Staffordshire. But I am still unable to put the Staffordshire experience, as narrated by him, into line with the negative instances of Manchester and Edinburgh. For an inquiry such as we have been engaged in, I do not see how to draw conclusions from aggregates of beds in hospital, from aggregates of patients in all stages of small-pox in all these hospitals together during uncertain periods, or from

* The point at issue in this case was not whether the Nottingham Hospital had or had not spread smallpox, but whether it should be closed to prevent the possibility of its doing so.

general statements that so many of the hospitals had failed to comply, as regards surrounding population, with the Local Government Board standards. I hope, however, that Dr. Reid will at a future date record in detail all the facts available with regard to some one—or still better, every one—of the hospitals in question.

Among the cases of hospital influence during 1900-4 which have been debated is that of Liverpool. It has been urged that Liverpool should be detached from the series of "positive instances" of hospital influence. The argument is that it was mere accident—no more—that there was in Liverpool an excessive prevalence of small-pox round each hospital. The fact is not disputed. But in view of the further fact that this excessive prevalence was manifested in each case in the particular period during which that hospital was receiving acute small-pox cases, of past experience of other hospitals in other epidemics, of the facts as to graduation, and of other points brought out in my Paper, I am unable to follow Dr. Hope in the argument which he presented to the Society in support of this contention. Dr. Hope's attitude in the matter is, I think, to a large extent explained by the assumption which runs through his whole argument, that Dr. Reece contended that in *every one of the houses* which were invaded by small-pox within a mile of each of the hospitals, the infection of the first case had been contracted from the hospital. I do not find in Dr. Reece's report warrant for this assumption. It has not been made, so far as I am aware, in the case of other small-pox hospitals in other epidemics, and Dr. Reece in his contribution has expressly repudiated it. But it is not surprising that Dr. Hope, by looking at the matter from this point of view, while knowing that his staff had ascertained that personal communication, independent of hospital operations, had been responsible for the infection of many persons who dwelt near the hospitals (as elsewhere in the city), came to take strong objection to Dr. Reece's conclusions.

It is unnecessary for me further to comment upon Dr. Hope's argument, or on Dr. Reece's reply: each has gone fully into the matter at issue. I would like, however, to clear up two points in my Paper regarding Liverpool. Dr. Reid drew attention to the disregard in my Paper of the opinions which had been expressed by Dr. Hope antecedent to the publication of Dr. Reece's report. But these opinions, important as we may regard them, were given in advance of the collection of facts which alone could enable us to see whether, when the Liverpool epidemic was looked

at as a whole, in relation to the operations of its several hospitals, indications of hospital influence were forthcoming. To give these facts in my Paper, I was obliged to draw solely on Dr. Reece's publication. The other point is, that by the briefness of my allusion to the figures for the Fazakerley Hospital, I have unwittingly led Dr. Hope to infer that I attach little importance to indications of hospital influence in that case. I do not think that the experience of Fazakerley, if it stood by itself, could be said to permit positive conclusions on the matter; and I note that Dr. Reece has been careful in his report to warn us of the smallness of the figures available. But the Fazakerley facts, put with the Priory Road and Park Hill facts, seem to me to be very significant.

I should add, too, that as Dr. Newsholme also expressed doubt of the reality of small-pox hospital influence in Liverpool, I have re-read his observations in the endeavour to ascertain what evidence would have satisfied him that hospital influence had been exerted there. If I interpret his meaning correctly, Dr. Newsholme would require for this purpose, that throughout the whole of this city every portion or "element" of area should have been attacked by small-pox with the same intensity as every other portion or "element;" the only exceptions being the areas round the hospitals, where the invasion rates would have evenly and progressively to increase. But this is to ask for a miracle: a method of controversy which, though not without precedent among agnostics, is hardly helpful to epidemiology. Dr. Newsholme, moreover, makes what appears to me an unreasonable demand of the spot maps given in Dr. Reece's Liverpool report. These, like any other spot maps, were published as a convenient record of facts. But they do not give all the facts which were collected and exhibited by Dr. Reece. Merely to count dots on these maps, regardless of the number of dwellings or populations dealt with, is a proceeding which may easily mislead, as Dr. Newsholme himself realises, and cannot be expected to give accurate indications, even when the results of counting are dignified by the name of "infection densities."

Notwithstanding doubts which have been expressed by some speakers as to the significance of hospital influence in particular cases, and of "negative instances" in others, it appears to be common ground that small-pox hospital influence is a definite phenomenon, causally related to the hospital, and requiring explanation. It is something apart from our experience of other infectious diseases which we ordinarily aggregate in isolation hospitals. The explana-

tion must be special to small-pox, and to cases in the acute stage of that disease. Small-pox hospital influence is not an affair which was restricted to Fulham, or to other small-pox hospitals in London, in days gone by. It has since come up again and again, in connection with many different hospitals in different places and in different epidemics. It has been manifested in various ways, but its principal characteristic has been the excessive incidence of the disease on persons dwelling in the neighbourhood of the hospital while a collection of acute cases is under treatment there, evidenced on many occasions by the existence of graduation.

Reasons are given in my Paper for maintaining that Mr. Power's theory of aerial convection not only best fits the facts brought out by recent experience, but involves assumptions which in themselves are *a priori* probable. And it will be noted that in the Discussion there has been general agreement with—at any rate, little dissent from—the proposition that the air escaping from the wards of a small-pox hospital is liable to contain an abundance of floating particulate matter; that, on occasion, and in suitable conditions of atmosphere, such floating matter may be expected to be carried for considerable distances—a mile or more—and ultimately to settle without having undergone any great degree of dispersion; and that there are reasons for believing a greater potentiality of infection to susceptible persons of particulate matter in the air of a ward containing acute small-pox cases than where the disease isolated is scarlet fever, diphtheria, or enteric fever. Dr. Bulstrode, Colonel Davies, and others have made valuable observations on several of these points. Another matter about which there appears to be little dispute is, that the theory of aerial convection affords a consistent explanation of certain ascertained characteristics of hospital influence, especially of the graduation in intensity of small-pox incidence on areas in proximity to the hospital, which has been ascertained in so many cases; and also of the manifestation of hospital influence in the case of inhabited areas which are shut off from intercourse and traffic with the hospitals by barriers of one or another kind. The main criticism which the aerial theory has received in the discussion is not that it explains too little, but that its acceptance would lead us to expect more than we know to have been the case. Dr. Newsholme, Dr. Reid, Professor Kenwood and Dr. Boobbyer, in one form or another put forward such questions as these: Why, if small-pox is conveyed aurally, do we not have more instances of it?

Every hospital in use during an epidemic of small-pox ought to show hospital influence; if aerial convection be true, a "negative instance" should never occur. If hospital influence results from particular atmospheric conditions, these, whatever they are, must, in our ever-varying climate, frequently be present in the case of any hospital receiving patients during any epidemic which lasts for many months. Then, again, what are these atmospheric conditions? Let us have a clear case. If it is wind, produce a hospital area and meteorological record, from which we can see that a S.-W. wind entails an outbreak N.-E., and so on. If it is calm, produce instances where every spell of calm has been accompanied by an outbreak round the hospital.

So far as these questions involve consideration of the frequency and significance of negative instances, I do not think I can usefully add to Dr. McVail's observations, or to what I have said above. Hospitals containing throughout an epidemic considerable numbers of acute small-pox cases, and surrounded in all quarters by inhabited dwellings, are hard to come by. Still more rare is a great concentration of acute cases, such as occurred at the M. A. B. Hospital Ships in 1901-2, and, on the aerial hypothesis, might be expected to cause spread of small-pox among persons dwelling in the vicinity even though the total population in the neighbourhood was small, and the houses limited to a small portion of the area near the hospital.

I do not see how to maintain that meteorological conditions should be the only variable quantity in these cases.* The aerial hypothesis would to me be unintelligible if it was inconsistent with the operation of numerous other factors tending, in one case to enhance, in another to diminish, or in a third to annul "hospital influence." The vaccinal condition of the population concerned is, of course, all-important. Some observations recorded in my report on the Orsett epidemic may be referred to in this connection. Dr. McVail has given considerable attention to this point, as also to the question of age, in considering the frequent escapes of inmates of institutions placed near to a small-pox hospital, of which Dr. Ker gave us a striking example in the case of Edinburgh Fever Hospital. The type of the epidemic again may be expected to have a considerable effect. Small-pox in inter-epidemic years, from some unknown circumstances affecting the life-history of the micro-organism of the disease, may be very different in

* In this matter I would invite Dr. Kenwood to apply his own contention (p. 231), "those of us.....other," to the aerial theory, instead of limiting it to the theory of undetected personal communications.

its infective ability to small-pox in epidemic times. Then, again, the physical conditions may be expected to exert a marked effect. Other things being supposed equal, we should anticipate a difference in the effects of aerial convection, between a hospital in the centre of a plain and one on the edge of a high cliff. The escape of the Hydro-pathic and of the Craig House Asylum during the Edinburgh epidemic may, perhaps, have been related to the fact that each of these institutions is on the opposite side of a hill to the small-pox hospital. Other conditions may easily be imagined which would modify the intensity of hospital influence, or prevent such influence from being exerted: notwithstanding the occurrence of aerial convection of small-pox matter from the hospitals.

It is easy to demand, as does Professor Kenwood, proof of exact correspondence between weather conditions and hospital influence. If such proof were forthcoming, we should be saved the necessity of debating the subject. But, as I have pointed out in my Paper, it is practically impossible to ascertain the precise conditions corresponding to a given infection which has apparently been caused by aerial convection. You cannot date back a man's infection to a particular hour; and, as a rule, the most you can get in the way of meteorological conditions is some daily record kept in the neighbourhood. All the same, certain broad conclusions of the kind which I have stated, as to conditions which appear to favour aerial convection, may be drawn from a careful study of the records available. The way to get more information on the matter would be to take a hospital suitably situated in regard to surrounding population, and then, during a small-pox epidemic, to appoint a special investigator to give his whole time to inquiring into each case of small-pox near to the hospital, as it arises—after the plan adopted by Mr. Power at Fulham—and to make simultaneously, on a pre-arranged system, a series of special meteorological observations. I wish that this could be done for as many hospitals and in as many epidemics as possible. Dr. Hope—if Park Hill Hospital is again opened for small-pox in the middle of a small-pox epidemic—or Dr. Newsholme, should the Town Council of Brighton elect to isolate small-pox in the centre of that town, might be able to give valuable assistance by arranging for some such inquiries.

Like other scientific theories which we accept and make use of, that of aerial convection has its uncertainties and ambiguities. We should like to test it further, by differential methods, in a variety of conditions, which are easy

to formulate, but seldom possible to obtain in practice. For this reason I have avoided the term "proof," as applied to aerial convention, preferring the position stated in the last paragraph of my Paper. To be adopted as a working hypothesis, aerial convection must, of course, successfully compete with other current explanations of hospital influence. Our Discussion seems to me to show that it does so. I may dismiss at once Dr. Kenwood's hypothesis that people living near a small-pox hospital are, owing to poverty and bad vaccinal conditions, especially susceptible to small-pox infection. The facts are dead against him. One of the elementary facts of "hospital influence" is the frequent persistence and repeated occurrence of excessive rates of invasion in areas near to the hospital; notwithstanding that, owing to the special local prevalence of the disease, the majority of the population of these areas has become protected from attack to a far greater degree than people living outside those areas. Dr. Chalmers emphasises this point for the neighbourhood of Belvedere Hospital. I have recorded similar occurrences at Purfleet, and in Gateshead and Felling; and there are many other instances, notably Fulham. As regards the social condition of the people living near the hospital, I may recall the inquiry which I made at Gatehead and Felling: where I got out the facts separately for each of twelve different wards of Felling and Gateshead, for comparison with the hospital area, with the results mentioned in my Paper.

It is natural to consider, as does Dr. Nash, whether flies are concerned with hospital influence. There is no doubt about the attraction which small-pox patients seem to have for house-flies. Any addition to knowledge concerning the ordinary range of flight from a dwelling of the individual house-fly, and similar matters, deserves to be considered carefully from this point of view. It must be remembered, however, that hospital influence has frequently been well marked in mid-winter, and in times of frost, and in connection with well-ordered and cleanly hospitals; in other words, at times when flies are either very scarce, or absent altogether.

There remains the explanation of small-pox hospital influence by the theory of undetected communications, which has been strongly advocated by Dr. Newsholme and Professor Kenwood. To what I have said on this subject in my Paper, I would add now a few notes.

Dr. Hamer's observation regarding small-pox ambulance traffic in London is important as a complement to instances such as the Orsett and Felling outbreaks, where ambulance

traffic as a factor in hospital influence could be altogether put out of question.

The undetected communications which Dr. Newsholme and Dr. Kenwood have in mind appear to be in large measure those between members of the hospital staff and people living in the neighbourhood of the hospital, though other communications are also assumed. Now, anyone coming with a fresh mind to this hypothesis, as an explanation of the recent cases of hospital influence referred to in my Paper, or of such cases as those reported by Dr. Wellesley Harris and Dr. Niven, would at once ask for the facts which form the basis of the assumption. We are all on common ground in knowing that person-to-person infection (in which the infectious matter has only a short journey through the air) is the common method by which small-pox is spread. We all agree with Dr. Boobbyer that the source of infection of a series of connected cases of small-pox is not infrequently missed; and that in individual small-pox cases, where there is no question of hospital influence, it is sometimes impossible to trace the origin of infection. Still, we know the other side, which is, that under modern conditions, during an epidemic of small-pox, it is usual, where there is no question of hospital influence, to be able to refer any given case back to an antecedent case. The epidemics now in question have been the subject of searching inquiries by many observers: County and Borough Medical Officers of Health and their staffs, Medical Inspectors of the Local Government Board, and others. What instances have there been in these cases where people living in the neighbourhood of the hospital have been ascertained to have, in all probability, come by their infection as a result of personal relations with hospital patients or hospital staff? Practically speaking, the answer is—None.* Dr. Newsholme's explanation of hospital influence by such communications is thus hypothesis, every bit as much as the aerial theory is hypothesis. And when Dr. Newsholme's hypothesis comes to be applied to any one of these more recent cases of hospital influence—still more to the whole evidence on the subject—the numerous and varied assumptions, which are required to fit it in with the facts known, seem to me far more improbable than any involved in the theory of aerial convection.

The nature of the difficulties to which I refer (in parti-

* I except the workmen at the temporary small-pox hospital on the Kent shore of Long Reach, 1901-1902, to whom I have referred in my Paper. Here the occurrence of personal communication with members of the hospital staff has to be considered as well as the operation of aerial convection.

cular to the assumptions needed before "graduation" can be explained) will be gathered from the observations of Dr. McVail, Dr. Niven, Dr. Bulstrode, and others, as also from my Paper. Nothing would be gained by their recapitulation, though I may perhaps draw special attention to the point raised by Dr. Niven, that these explanations involve an assumption of infectivity of "contacts," which is contrary to usual experience.

It is desirable, however, that I should comment on Dr. Newsholme's contention in regard to the Orsett outbreak and the M. A. B. Hospital Ships, and incidentally this will illustrate some of the difficulties which arise in adopting his alternative theory. First, on certain questions of fact. My inquiries in Purfleet and the Orsett Union began at a time when no more than nine cases of small-pox had occurred in Purfleet. They were continued at frequent intervals throughout the epidemic. During the whole period, I was in touch with the medical practitioner who attended practically all sick persons in the village, with the Medical Officer of Health, Inspector of Nuisances, and others, who made frequent visits to the place, and I made numerous house-to-house inquiries. As a fact, I did, with the assistance of Dr. Corbett and others, make very careful inquiry as to importation of infection from other parts of Essex or from London—and this not only in regard of the first cases in Purfleet, but also as to other cases occurring later on during the epidemic. Then, as to the alleged surreptitious visits to Purfleet of members of the staff of the Hospital Ships. Lest Dr. Newsholme's remark that "a considerable body of independent evidence exists" that such communications took place should be applied to the epidemic of 1901-2, it is well that I should be definite. From first to last, from the beginning to the end of my local inquiries, after the publication of my Report, after subsequent correspondence, after reading the evidence given by Dr. Thresh, Dr. Newsholme, Dr. Collingridge, and others in the "Nottingham case," where this outbreak was made the subject of prolonged examination and cross-examination, and at the present day, the only suggestion of such visit known to me is that referred to by Dr. Thresh (*Epidem. Soc. Trans.*, vol. xxi., p. 103): "I am convinced that the Asylums Board have fulfilled their promise, and that there has been no personal communication between the ships and the County of Essex—save, probably, the surreptitious visit of a man to his sweetheart. The only case of this kind I can hear of has been denied by the parties concerned." There was no allegation here that small-pox in Purfleet resulted from this visit.

Leaving these matters, we may consider Dr. Newsholme's hypothesis in relation to the epidemic at Purfleet. The points to be explained there have been dealt with in my Paper. It is not so much the magnitude of the epidemic in proportion to the population—though this is important—but the continued invasion of fresh dwellings by small-pox, week after week and month after month, notwithstanding all the administrative action taken, the prompt recognition and isolation of cases, and the increasing immunity which the population of the village obtained in consequence of recent vaccination or recent small-pox. Now, suppose a series of visits to Purfleet by subordinate male members of the hospital staff, managing to get a boat and rowing nearly half a mile across the water. I imagine that even Dr. Newsholme would exclude patients, medical officers, and nurses. The first assumption necessary is that no knowledge of these visits should have been obtained by various inquirers who, unlike Dr. Newsholme, were on the spot. The next assumption relates to the way in which these visits must have been contrived. Each visitor must have selected from among the houses in Purfleet one which had hitherto escaped small-pox or general vaccination and re-vaccination. Visits of this kind must have been spread over some ten months. Towards the end of that period, when not more than forty people were left in Purfleet who could be regarded as susceptible to small-pox, the unknown visitor must have selected the dwellings of the susceptible people. The next assumption is that these visitors (who were not themselves suffering from small-pox) possessed on each occasion sufficient infective ability to occasion small-pox among those whom they visited. I note, in passing, that any ship's porter paying a visit to Purfleet in 1901 would have been wise, in consideration of his personal safety, to exchange his uniform for his outdoor clothing.

Dr. Newsholme, however, has a second string to his bow regarding Purfleet, in the suggestion that, after all, the hypothetical visitors bringing small-pox to Purfleet did not come from the Hospital Ships, but from other places in which small-pox was prevalent. But this is to demand more remarkable coincidences than ever. These hypothetical visitors must have had some reason for thus bringing small-pox to Newcastle. What was it? Why should they have come to the Orsett Union, to West Thurrock, and so particularly to Purfleet? Has there been another village near London, or in the whole country, as to which there has been no question of hospital influence, where small-pox

has been dealt with after the manner practised in Purfleet, where yet small-pox continued month after month to behave in this way? Moreover, this suggestion requires either the dissociation of the Purfleet facts from the circumstances of other areas on both sides of the river, and from the occurrence of small-pox on vessels moored in Long Reach, or else the assumption of a still greater series of coincidences. The demand on our imagination is not lessened if Dr. Newsholme pulls both strings at once, and tells us that part of the Purfleet outbreak was due to unknown visitors from the ships and part to unknown visitors from elsewhere.

In the other case which is intimately known to me, that of Gateshead and Felling, the assumptions necessary for explanation of the facts by Dr. Newsholme's theory are similar as regards their magnitude and diversity, though they differ in character. So would it be with the Monsall outbreak in 1893 to which Dr. Niven referred, and other cases.

Apart from the consideration that its assumptions are much less improbable, the theory of aerial convection has the great advantage of supplying a common and consistent factor. In any case, I think the debate has sufficiently disposed of the contention that aerial convection cannot be considered as a cause of hospital influence so long as there is also a possibility that the disease may have been spread by hospital communications. I do not for a moment question that such communications may on occasion produce small-pox among persons dwelling near the hospital. But that such communications, if they occur, should be given as a reason for refusing to consider aerial convection as a simultaneous—and possibly a major—cause of the local spread of infection, appears to me to be an untenable position in the light of our present knowledge.

In conclusion, I may, perhaps, express the hope that the papers and discussion will indirectly be fruitful by encouraging investigation of further problems of aerial infection, which are now becoming of increased importance. The application of the lessons learnt from small-pox hospitals to the spread of infection caused by individual cases of the disease, to which the President and others have directed our attention, is of obvious importance, in connection with inquiries as to methods by which measles, phthisis, and infectious catarrhal conditions are spread.

N.B.—The four diagrams relating to Liverpool and Gateshead which illustrate Dr. Buchanan's Paper are reproduced from the Official Reports by consent of the Stationery Office.

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