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Summer Diarrhœa and Enteric Fever.

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THIS paper is of the nature of an intensive study. For many years I have published in the annual reports on the health of Manchester and elsewhere, going back so far as 1883, facts and interpretations of facts in relation to enteric fever, and more recently in regard to diarrhœa. It is proposed to lay some of these before you, with a view to see whether we cannot penetrate the mystery a little further, or at all events see our way to undertake further inquiries.

There are, I think, good reasons why we should at times restrict ourselves, in the main, to facts which we observe at home. In instituting a comparison between different centres, there is, to start with, the difficulty that under summer diarrhœa we may not be including the same disease or diseases in different localities. The conditions are so widely different socially that any conclusions as to the effects of other phenomena must be greatly weakened, notwithstanding that each community constitutes a complete population. Then, again, some towns are placed at a considerable elevation, on the Pennine range. Not only are the temperatures of air and soil thereby lowered, but there is a greater rainfall than in lower localities. The impression is liable to be thus produced or, at least, intensified that a high rainfall goes along with a low diarrhœal rate. Even where the system of removal of excreta is a conservancy one, and similar in character in two places, the structural and other arrangements may be so much better in one place as greatly to diminish the evils incidental to such a system in another. Then, again, the soil of elevated situations is likely to be hard rock, and quite impermeable, so that an undue impression is created that diarrhœa is associated with a porous subsoil. From the data, however, obtained for Manchester no such difference of incidence in fatality of diarrhœa, as between one kind of soil and another, can be deduced. It is not denied that such a relation may exist, but the evidence does not appear sufficient to establish the thesis.

For such reasons as these it is a great advantage, in forming conclusions as to the explanation of facts, to deal with areas all the characters of which are quite familiar to the reasoner. The atmospheric temperature, the temperature of the soil, and the rainfall are approximately the same throughout.

There have been not a few inquiries into this subject, some of which, like Dr. Ballard's and Dr. Newsholme's, have dealt with all the aspects of the subject. Others, again, like Professor Delépine, have concerned themselves rather with special aspects of it. Professor Delépine adduces evidence showing that milk may have much to do with the spread of the disease, as it arrives from a distance in towns. Dr. Newsholme, on the other hand, from the observed facts in connexion with breast-fed children, with infants fed on condensed milk, and from comparison of the fatality experienced by infants fed on milk brought from a distance with that experienced by those fed on milk produced near the town, came to the conclusion that the infection experienced was domestic. Notwithstanding the striking facts adduced on the other side, I now feel obliged to concur in the latter view, in support of which it may be pointed out that diarrhœal mortality does not rise in June, notwithstanding the exposure of outside milks to high temperatures—before a certain period is reached.

Summer or epidemic diarrhœa is a term applied to an affection marked by a somewhat definite group of symptoms, in which vomiting sickness, copious diarrhœa, rice-watery and green stools, and finally convulsions play a conspicuous part. This condition is not rarely somewhat prolonged, and is often attended with some degree of fever. On the one hand it shades into typhoid and paratyphoid fevers, and on the other it is not rarely the termination of a tuberculous enteritis or some wasting affection. From the study of a large number of histories, Ballard concluded that he was dealing with a definite disease, and that is also my opinion from the histories of illness which have been obtained for me. Yet I have no doubt that enteric fever in infants may be, and not rarely is, mistaken for diarrhœa. The grounds for this opinion will be readily seen on referring to the histories of enteric infection given in the annual reports on the health of Manchester.

Whether summer diarrhœa is produced by one definite micro-organism or is an illness conditioned by several allied bacilli, it is a clinical entity pursuing a very definite course, and as such is susceptible of study. One of the advantages of an intensive study is the uniformity secured in the deaths classed as "summer diarrhœa." In Manchester, since Dr. Tatham held office, in any case where doubt could exist the certifying practitioner has been asked by letter whether in his opinion the death should be regarded as due to epidemic diarrhœa, and it has been entered accordingly. There have been several claimants for the post of director of the fatal summer outbreak, of which the chief are Gaertner's bacillus, a similar organism isolated by Professor Delépine, and the *Bacillus enteritidis sporogenes.* I am not in a position to pass a judgment on this question. It appears not improbable that the most malignant forms of the disease are those which survive the winter and spring, and serve as foci from which smaller doses of infection are distributed, a process which leads to attenuation of the severity, and also to prolongation of the disease. Ballard adduced abundant proof that the few fatal cases which occur week by week before the summer ascent represent a large number of attacks at different ages, so that at the commencement of the annual ascent there are abundant foci from which the disease can be propagated. Between infancy and old age, however, the fatality is slight, so that its widespread distribution escapes attention. As a rule, also, the sufferer is able to go out to the closet, a fact which may prove to have considerable significance.

Epidemic diarrhœa is infectious. If the history of institutions were faithfully recorded, it would probably be found that it not infrequently spreads in these. The course of its summer fatality is that of a rapidly spreading disease, transmitted by some specific agency. When, as in Manchester in 1904 and 1905, a number of individual cases are investigated, infection from person to person becomes highly probable in a considerable proportion of the cases, the agency being often left vague. It is true this is associated with a low fatality in the winter, spring, and early summer. Nevertheless diarrhœa is kept well alive through great part of the year, in spite of the circumstance that there appears to be no agent capable of conveying it from one house to another. From the annual report for 1908 I insert a table (p. 135) showing the distribution of mortality in the various sanitary districts of the city for a period of thirteen years.

It will be seen that these districts frequently change the order of their infantile rates of mortality, that some of them are subject to great fluctuations, and that the privy-midden districts, like Bradford and West Gorton, and Clayton, are apt to predominate in seasons of high diarrhœal mortality. All these features mark out the disease as being of an infectious character. The further discussion of the facts will tend to lay stress on the influence of the house fly, and it will be found that the course of enteric fever has an important bearing on the subject. But it will be convenient to give at this point a brief account of the efforts which have been made in successive years to elucidate the course of the disease.

From the inquiries of Boobbyer, Scurfield, Tattersall and others, we may take it as proved that there is an intimate relation between the storage of excreta in privy-middens and a high diarrhœal mortality. The materials at my disposal have not permitted me to adduce precise evidence on this point. It suffices, however, to note that diarrhœal mortality is excessively high in all those sanitary districts of Manchester in which the privy-midden has prevailed, as in Bradford, Clayton, Openshaw, and West Gorton. This is also the case in the newly-added district of Gorton. Moreover, the mortality in these districts increases relatively in years of high diarrhœal fatality. On the other hand, in the more central portions of Manchester in which pails have long replaced middens, owing to defects in the structural and other arrangements, there has been much nuisance from the recesses in which the pails stand. Thus, although the contents of the pails are taken away once a week, and no generation of flies can occur in their interior, I am informed that the pupæ of the smaller house fly have been found in these recesses. It is probable that much improvement in this respect has taken place in recent years. But there exist at present quite sufficient materials in collections of horse manure, and of other organic matters, to provide for the continued production of flies, even when all the pail-closets have become innocuous.

The social condition of the population has also much influence on the diarrhœal death-rate. Where the people are very poor, the children are also disproportionately unhealthy at the outset, and the disproportion rapidly increases after birth, owing chiefly to ignorance or carelessness on the part of many mothers.

If reference be made to the table (p. 135) it will be seen that the death-rate is persistently highest in the central and poorest sanitary division, and then in South Manchester, the next poorest. It is true, the differences between the death-rates in different parts of Manchester are not so great as in the case of phthisis, and it is manifest that other

TABLE I.—DEATHS AND DEATH-RATES FROM DIARRHŒAL DISEASES IN THE VARIOUS DIVISIONS OF THE CITY, WITH DEATH-RATES UNDER ONE YEAR PER 1,000 BIRTHS, FOR 1896 TO 1908, AND AVERAGE FOR TEN YEARS, 1898 TO 1907.

	1908			Death-rates under one year per 1,000 births													
	Estimated population	Deaths	Death-rates	1896	1897	1898	1899	1900	1901	1902	1903	1904	1905	1906	1907	Average 10 years	1908
City of Manchester ...	648,846	591	0·90	24·8	39·1	46·3	63·7	35·6	47·5	13·0	22·1	34·1	30·8	39·8	12·2	34·5	24·0
(1) Manchester township	125,197	201	1·58	30·0	45·4	54·6	78·5	47·5	61·6	16·6	31·3	40·9	47·7	58·5	18·0	45·5	37·8
(2) Northern districts ...	197,527	134	0·67	19·6	34·8	37·3	57·4	24·4	42·3	10·7	15·0	30·1	31·3	30·8	10·3	29·0	17·8
(3) Southern districts ...	326,122	256	0·77	24·3	37·7	46·7	58·7	36·1	43·2	12·4	22·0	33·0	22·5	36·7	10·7	32·2	21·5
Manchester township:																	
Ancoats	43,206	77	1·76	30·7	58·3	45·3	85·0	48·6	57·4	17·1	30·2	35·4	50·9	54·9	21·0	44·6	40·8
Central	24,922	41	1·62	46·6	45·2	75·2	71·1	55·0	66·1	15·0	48·5	51·0	52·4	54·9	19·1	50·8	41·3
St. George's ...	57,069	83	1·43	20·8	36·0	52·8	76·7	43·4	63·3	16·9	25·5	41·1	43·3	62·8	15·3	44·1	34·3
Northern districts:																	
Cheetham	42,376	16	0·37	20·1	22·5	22·6	36·0	18·0	27·3	9·4	10·1	10·7	18·8	19·6	7·5	18·0	10·9
Crumpsall	9,430	3	0·31	16·1	26·3	20·4	60·9	14·6	23·2	14·9	9·8	15·2	23·9	20·7	—	20·4	9·3
Blackley	9,830	3	0·30	4·7	4·4	9·8	44·2	4·4	9·2	4·0	12·1	23·5	3·8	3·5	3·7	11·8	6·9
Harpurhey	23,318	7	0·30	24·7	42·6	44·7	72·6	11·3	36·5	1·8	15·9	13·1	21·7	40·1	13·7	27·1	11·7
Moston	20,826	1	0·05	9·1	17·5	51·0	29·1	2·8	19·0	11·7	8·8	25·5	8·4	13·2	2·0	17·2	1·9
Newton Heath ...	39,153	38	0·96	15·7	32·9	37·8	57·9	25·4	49·7	12·3	15·6	31·4	43·3	36·0	16·8	32·6	22·0
Bradford	25,355	40	1·55	24·9	49·8	58·1	93·3	43·3	62·7	13·2	26·9	52·5	47·7	53·6	8·7	46·0	34·6
Beswick	12,588	20	1·56	20·3	52·5	23·6	46·4	40·6	50·0	20·6	8·4	60·8	43·2	23·0	19·0	33·6	37·6
Clayton	14,651	6	0·40	41·4	55·2	61·6	66·7	36·4	94·9	6·2	20·2	38·4	40·1	41·2	7·8	41·4	9·8
Southern districts:																	
Ardwick	45,324	42	0·91	19·2	27·7	50·2	61·7	43·7	48·4	11·5	20·0	38·5	28·7	42·8	12·8	35·8	29·0
Openshaw	29,040	35	1·19	25·7	42·6	58·5	64·3	44·7	48·1	14·2	27·4	28·0	23·9	50·0	8·2	36·7	24·7
West Gorton ...	32,316	24	0·73	21·4	42·7	69·3	85·7	52·6	58·4	21·2	31·1	47·2	35·1	60·0	28·5	49·0	19·6
Rusholme and Kirkmanshulme ...	27,007	13	0·47	17·2	26·3	43·7	34·0	15·4	33·3	11·6	14·0	12·7	10·5	21·2	2·4	19·9	11·1
Chorlton-on-Medlock	55,597	42	0·74	28·7	24·2	32·8	58·7	25·1	17·9	7·1	23·9	23·8	29·1	32·8	13·7	27·5	25·9
Hulme	62,629	87	1·37	26·3	50·3	40·6	49·5	33·1	43·6	12·0	18·7	39·2	22·3	33·6	10·8	30·3	28·3
Moss Side	28,522	2	0·07	—	—	—	—	—	—	—	—	—	3·5	29·8	1·7	11·7[1]	1·6
Withington ...	45,687	11	0·24	—	—	—	—	—	—	—	—	—	11·2	19·4	2·2	10·9[1]	10·3

[1] Average for three years.

factors besides the social have an important influence in producing it. Nevertheless, this factor is conspicuous no less in the central than in other districts, such as Bradford, Ardwick, and Openshaw. How great this influence is can only be determined by direct inquiry into the health of fatal cases of diarrhœa prior to the attack. The result of such inquiries is given in the annual reports for 1905 and 1906, and it is shown not only that the great majority of the infants were in poor health before diarrhœa set in, but that the social circumstances of a considerable section were of a miserable kind. This is not an isolated experience. Dr. Hope has similarly found a close association between diarrhœa and dirty home conditions, and has pointed out that fatal diarrhœa tends to recur in the same household. It would, however, be possible to overestimate the significance of this fact, since the absence of breast feeding has much to do with diarrhœal fatality, and this also tends to recur in the same household. Again, a large minority of healthy infants contract fatal summer diarrhœa, and it is this fact, and the nature of the individual occurrences, which convey a direct and strong impression that summer diarrhœa is infectious. Further, in spite of the absence of middens, taking into account the large number of pail-closets, of stables, and of collections of refuse of one kind or another, it is doubtful whether one can say that the central districts are essentially freer from those causes which one believes to be concerned in spreading diarrhœa than were the outlying privy-midden districts. The effect of wretched home conditions is demonstrated more by the persistence of a steady high mortality than by its average magnitude.

Dr. Hope has estimated that artificially-fed infants suffer more from fatal diarrhœa in the first three months of life than do breast-fed infants in the proportion of fifteen to one. The proportion is at least as high as this, according to our inquiries.

The explanation of this disproportion is not quite simple. It is usually taken to mean that infants artificially fed are much more exposed to infection than breast-fed infants, and this, no doubt, is true. But we must, also, believe that breast feeding renders children less susceptible to attack when exposed to infection, and less likely to die when infected. Direct infection may be conveyed in a variety of ways apart from the food, as by rubbing the gums of the infant, the use of comforters, placing the infants on dirty floors and sofas, and in other ways. We are thus able to see how it is that a certain proportion of breast-fed infants may contract diarrhœa directly, even if flies be not reckoned with.

The essential problem in summer diarrhœa, however, is the summer wave, ascending as it does steeply, and descending with but little less of abruptness. It is to this period that the fatality is due. Broadly speaking, it corresponds to a similar upward movement and descent of the temperatures registered at a depth of 4 ft. in the soil. The readings of the air thermometer in the shade and of the 1-ft. earth thermometer do not correspond closely to the course of this wave of deaths, being subject to considerable fluctuations. But it is now generally agreed that the mortality must have closer direct associations with the surface than with the deeper temperatures. Many attempts have been made to penetrate into the nature of the supposed association between the conditions of the soil and the rise of summer diarrhœa, but not with much success. It has been conjectured that under favourable conditions of temperature and moisture growth takes place at a particular period of the year of those micro-organisms which cause diarrhœa. These have been variously supposed to start near the surface, and at a considerable depth. Reaching the surface, they are supposed to be subject to drought, but not killed. Subsequently wafted into the air, these germs, it is supposed, may reach food, which they contaminate.

If the figures be written down showing the number of cases of enteric fever commencing week by week, a curious phenomenon is noticed, which at an early period arrested my attention—viz., that in some one week of the year the number of cases makes a sudden leap upwards, an ascent afterwards maintained, it may be with fluctuations. This fact has not been in evidence in every year, though in most it has. An effort was made as far back as 1898 to furnish an explanation of this rise from the meteorological data, and it was observed that it was associated with a preceding rainfall, lying between two dry, warm and sunny periods, although the critical rainfall, as I called it, did not occur at a fixed interval before the critical rise of cases. Moreover, the rainfall in question was always during ascending temperatures. The conditions in question were such as would be likely to produce a great increase in flies, but no such idea suggested itself to me in the earlier years. Nevertheless this peculiar rise in the enteric curve was pursued through a series of years, although the explanation eluded discovery.[1]

It was felt that this phenomenon was the key to the explanation of the autumnal rise of enteric fever, and such is still my impression. Moreover, it might very well be that the autumnal rise of diarrhœa would be elucidated when the behaviour of enteric fever was explained.

[1] Manchester Annual Reports, 1898, *et seq.*

Flies first came prominently into notice in connexion with enteric fever in the American-Spanish campaign, when they were spoken of confidently as transmitting infection from the excreta of the sick to the healthy. English medical observers in the Boer war were strongly of the same view. Dr. Arthur Newsholme, Dr. Nash, and other observers, amongst whom was my neighbour Dr. Martin, medical officer of health for Gorton, arrested by these observations, directed attention to the agency of flies as possible transmitters of the infection of diarrhœa and other diseases. It was not, however, till 1903 that I began to make observations on the subject, nor until 1904 that systematic captures of flies were made.

Before proceeding to discuss the results of these observations, it is necessary to consider the hypotheses on which such a phenomenon as the course of the diarrhœa annual wave can be explained.

We may first consider the influence of the soil. In favour of the view that under some unknown conditions the soil may have to do with the propagation of diarrhœal diseases, we have such facts as these :—

(1) Dr. John Robertson's investigations, in the course of which he showed that, under special conditions, cultures of the typhoid bacillus planted in soil could survive the winter and be recovered from the ground.

(2) Professor Delépine's demonstration of the persistence of viability of typhoid bacilli in the walls of a privy-midden for a whole year.

(3) Occasional attacks of enteric fever in men engaged in drainage work (Manchester histories).

On the other hand, from the work of Dr. Sidney Martin it is to be inferred that under all ordinary conditions cultures of typhoid bacilli planted in the soil are speedily overgrown, and the bacillus is soon irrecoverable.

Supposing, however, that enteric bacilli and similar micro-organisms can grow in the soil, the application of this circumstance to the course of diarrhœa is by no means easy. This disease pursues its even course independent of drought or rain, unless the latter is so great as to greatly lower the temperature. It is difficult to conceive of the growths in question spreading upwards from a considerable depth; it is more difficult to conceive of them escaping from the surface, wet or fine; it appears certain that, if such a growth did occur, it would be confined to a comparatively short period, corresponding to the periods occupied in the production of laboratory cultures. There is thus an inherent improbability that such growths would occur only at one particular season, especially in the case of the typhoid bacillus (which is not very exacting as to the temperature which it must have), failing with similar conditions of surface temperature outside this special season. It is excessively unlikely that the interruptions to the escape from the soil of bacilli necessarily produced by wet weather would not affect the course of diarrhœa. It is difficult to see how bacilli are to escape from the soil in the well-paved central portions of towns, or to see why they should fail to be propagated and be distributed from the comparatively uncovered parts of outlying districts. If the course of diarrhœa depended on micro-organisms growing in or on the soil, it would participate in the fluctuations of rainfall and temperature, which would certainly affect such growths. This it does not. One is driven, I think, to abandon the idea that the growth of bacteria, whether in or on the soil, has to do with the annual wave of diarrhœa.

Other hypotheses have been put forward. One is that fruit may be responsible. But, in the case of diarrhœa, the disease appears first in the infant in house after house, and it is certain that the infant has no fruit; the effect produced by fruit on the annual course of the disease can therefore only be very partial. Nevertheless, if flies can infect infants' food they can infect other food, and fruit in particular, so that the histories of direct exposure of infants attacked to previous diarrhœal infection becomes important.[1]

Another view is that heat may be itself the cause of the disease, or, if not of the disease, at any rate of the fatality. As against this explanation we must adduce the comparative immunity of the infants of better-off households, the escape of artificially-fed children when sufficient care has been expended on their nurture,[2] and the comparatively slight incidence of fatal diarrhœa in many towns in the south, in spite of high temperatures. Yet it is impossible absolutely to refute the influence of temperature. The above arguments suffice to show that diarrhœa does not arise from the action of heat in furthering some infection pre-existing in the child, but they do not remove the possibility that heat acts injuriously on weakened organisms, predisposing them to the reception of infection. Yet, clearly, heat can in no way account for the rapid spread of the disease among particular classes of the population.

In his Milroy lectures, again, Dr. Waldo suggested that the spread of the disease was due to dust, and was strongly inclined to favour the idea

[1] *See* Manchester Annual Reports for 1904 and 1905 and Dr. Sandiland's inquiry.

[2] Dr. Matthew Hay, Health of Aberdeen, 1908.

that the specific materies morbi came from horse-dung. There does not appear, however, to be much more in favour of one view than of the other. There is practically no doubt that the latent period of fatal diarrhœa does not often exceed a week, and the most common period is probably two or three days. The course of fatal diarrhœa should be seriously affected by heavy rain lasting several days if dried horse-dung were the cause; yet diarrhœa and flies pursue their way with comparatively little disturbance, wet or fine. Dr. A. H. Ainsworth, in a series of curves, shows the enteric waves rising out of the heavy rains of the monsoon at Poona, and sinking back before the rains are over. The excess of moisture is, in that region, doubtless necessary for the development of the house fly, and it breeds accordingly, having its annual ascent in the same period. Unfortunately, there is only one curve of flies given, but one gathers from Dr. Ainsworth's paper that this is the usual course of events. To come nearer home, why should dust effect so much in August, so little in March, April, May, June, and July? There is, in effect, no disturbance in the diarrhœal fatality till flies begin to multiply. We must, I think, set aside transmission by dust as palpably inadequate as an explanation.

What we require for the explanation of the facts of summer diarrhœa is the presence of some transmitting agent rising and falling with the rise and fall of diarrhœa, the features pertaining to which must correspond to and explain the features of the annual wave of diarrhœa.

None of the other factors of which we have cognizance do afford such an explanation, and we come by exclusion to consider the house fly. The process of conveyance of infection is not striking and arresting, as it is in military camps abroad; nor does the number of flies usually approach that observed in tropical and subtropical countries. We are therefore obliged to attack the question *de novo*, and examine such evidence as we possess to see whether we may rest reasonably confident that in flies we have found the transmitting agent sought for.

If the house fly is the transmitting agent in summer diarrhœa, the following conditions should be fulfilled:—

(1) (*a*) There should be evidence that the house fly carries bacteria under the ordinary summer conditions; (*b*) house flies should be present in sufficient numbers in houses invaded by fatal diarrhœa.

(2) There should be a close correspondence between the aggregate number of house flies in houses and the aggregate number of deaths from diarrhœa week by week.

(3) The life-history of the house fly should explain any discrepancy between the observed number of flies and the observed number of deaths.

(4) The minority of breast-fed children not apparently accessible to infection should receive explanation.

(5) There should be a closer correspondence of diarrhœal fatality with the number of flies than with any other varying seasonal fact.

(6) Any other closely corresponding seasonal fact should be capable of interpretation in terms of the number of house flies.

(7) Any variation from district to district in the annual curve of deaths should be accompanied by a similar variation in the curve of flies.

(8) It will be at once manifest when we come to enteric fever that the house fly plays but a minor direct part in the production of the annual wave. Such part, however, should have reference to the number of flies and of pre-existing centres of infection. If it can be shown that that portion of the enteric wave which is connected with flies changes from one period of time to another in such a manner as to be explainable in terms of flies but not of meteorological conditions, the evidence in favour of flies will be greatly strengthened.

(9) No other available hypothesis must be capable of explaining the course of summer diarrhœa.

In working out the above statement it will not be possible to adhere to the order of these propositions, which, however, will be kept steadily in view.

I come now to discuss the observed relation of the house fly to diarrhœa in Manchester. In any consideration of this question the first need is to ascertain that a relation does exist. Accordingly, after a preliminary trial in 1903, we proceeded in 1904 to select a number of houses in which it was thought that the householder could be relied upon to carry out continuous and careful counts day by day of the number of flies caught. These observers were provided with bell-glass traps covered above but accessible from beneath, the lower rim curving inwards to form a semicircular ring, into which was to be poured day by day a thin sweet beer, with which the observers were provided. This beer is very attractive to flies; they enter from below, hasten to partake of the beverage, and get drowned. It might be supposed that they are attracted simply by the moisture. This, however, is not so. I am informed that other alcoholic beverages exercise a similar attraction for them, and I am told by one householder that in the capture of cockroaches his success with the "demon" trap has been much increased by pouring a little whisky into the cup of the trap.

I prefer this trap to stick papers. It is true no distinction between homalomyia and musca can be made out in flies thus captured. The same applies in great measure to stick papers. With balloon wire traps our captures have been scanty and unrepresentative. The beer trap gives a fair measure of numbers, if properly used, and the flies are easily counted. It is possible that the kinds captured by the different traps vary in their numerical proportions, and I regret that, for reasons of expense, we substituted in 1909 stick papers for beer traps. For the purpose of distinction, balloon traps are satisfactory, the flies being preferably killed by chloroform.

The flies captured, musca and homalomyia together, are emptied out at the end of twenty-four hours, and counted. The number is then entered in a book by the observer, the trap cleaned out, and recharged with beer. At the end of the season the individual enumerations are entered in a record book, day by day, and are then collected in weeks for each station and for all stations. In this procedure, during the five years of completed observations, 1904, 1905, 1906, 1908, and 1909, there has been no substantial alteration except the alteration in traps in 1909. In the various years now one, now another station has been added, other than private houses, including the depots of the Cleansing Department, Monsall and Clayton Hospitals, and Mill Street Police Station. The observations made at these, however interesting in themselves, appear to introduce an element of confusion from the excessive numbers liable to be captured at particular times, which do not agree with those of smaller house stations, and which cause the total curve to be unrepresentative and irregular. When the numbers captured at the different stations are added up week by week, the weekly numbers form for each station a series which is subject to considerable fluctuations. These numbers show often two maxima, sometimes three. Even when the numbers are very large the maxima for different stations do not coincide. It is only when the numbers are added up week by week in all the stations that we get a coherent and graduated curve. It is evident that to get a reliable and complete representation of the numbers of flies visiting or present in houses throughout the season we should require a vast number of stations, and in each a reliable observer. Our completed curve, especially with a small number of stations, must therefore be imperfect as a representation of the total distribution of flies. In this respect the observations carried out in London are much more satisfactory, at least in numbers, than ours. The stations are in the London observations grouped round certain centres specially attractive to flies. Some of these, however, attract flies for purposes of feeding, others for the purpose of breeding. The distinction is fundamental. The former will attract the maximum of flies early on before the maturity of the sexual organs, the latter partly no doubt for feeding purposes, but largely also for reproductive reasons. Now, a fly is not sexually mature until three weeks or so after its emergence from the pupal stage. The maxima may therefore well diverge by three weeks or more.

For ordinary average purposes of estimation house observations with but little reference to special centres of attraction will probably give quite useful results. As a matter of fact, of course, owing to the wide distribution of stables, such centres represent the stable interest chiefly; that is to say, they represent flies in their youth, moving from house to house, maturing, feeding, the element with which we are chiefly concerned, as well as the mature flies which have deposited their eggs. A sweet factory does not necessarily augment the numbers in houses near it. It may, and probably often does, deplete them of their migratory elements. The flies come, of course, primarily from collections of horse manure, of domestic refuse, and so forth.

It is probable that flies are musca or homalomyia according as the materials in which they are produced ferment or not, with consequent elevation of temperature. Musca probably is produced chiefly in horse manure, fermenting tips, and in middens when the ground temperature is high. Homalomyia is produced, also, in the recesses of pail-closets, in vegetable refuse, in the shallower parts of tips, and in middens before fermentation sets in—i.e., before the ground temperature rises. There is no reason to suppose that homalomyia exercises any special influence on the spread of disease. Diarrhœal fatality does not ascend in May or June, when homalomyia has already attained considerable numbers. On the other hand, there is no reason for believing that this fly does not take its fair share in the spread of the disease. In any case its relatively small numbers, when diarrhœa is most prevalent, necessarily relegate it to a secondary position.

No substantial error is involved in adding muscæ and homalomyia.

As we have seen, the results obtained by adding the numbers enumerated at a limited number of stations are necessarily imperfect. Fortunately we are able to apply some corrections, partly from the facts themselves, partly by the use of first approximations. To this subject I shall afterwards return.

It scarcely seems necessary at this point to elaborate much the consideration of the course of the numbers of flies captured week by week; it must suffice here to point out that the production of fly swarms on which the numbers in houses must depend is subject to many influences.

If the collection of manure or refuse is covered over, it depends on the retentiveness of moisture of the manure and the speed with which the top and sides are buried. Multitudes of larvæ perish for lack of moisture; many are destroyed by beetles. Often when the pupæ reach maturity the flies produced must be unable to struggle through the overlying material. Small, open collections of horse manure must be easily chilled by rain, and fermentation arrested; the production of flies in these is more difficult than in large collections, and no doubt requires warmth of soil and of atmosphere in excess of what suffices for large collections. The number of horses in proportion to the size of the manure heap must be an important factor; if the number of horses is disproportionately large, the flies will not be able to emerge except at the sides, and so forth. The fertility of flies is by no means much in excess of the requirements; the production of swarms is largely, in fact, the result of happy accident. It may happen, for example, that the distribution of a large manure heap is eminently favourable to the production of a swarm of flies, and that the weather is particularly favourable; meantime the farmer's cart arrives and transfers elsewhere the struggling brood. A spell of wet weather arrives, chills and kills the flies about to emerge in shallow collections of refuse, but supplies much-needed moisture for development and fermentation to large heaps, or keeps alive the larvæ in covered collections. The influence of weather conditions is complicated and not easily calculable.

It will be necessary for a fuller understanding of the facts to consider some details of the life-history of flies, but we may now proceed with the summary of the facts. I give, therefore, for five years the number of deaths week by week, the number of flies captured in our stations in the same periods, the mean atmospheric temperatures, and the mean temperatures at depths of 1 ft. and 4 ft. For three years are given also the numbers of fatal attacks of diarrhœa commencing in those weeks. There are, however, two serious drawbacks to these series of cases arranged in weeks of commencement. Many families in which a fatal attack has occurred had disappeared at the time of inquiry, and in many others the period of commencement could not be accurately ascertained; the series is therefore in each case imperfect. It is scarcely necessary also to renew the caution that the diagnosis is subject to considerable deduction; it is, however, necessary to remember that this deduction is much more serious at the commencement of the curve than at its height, owing to the smallness of the numbers. It is with these imperfect data, however, that we have to work.

Observations on House Flies.

The data for the five years may be presented as shown on p.p. 146-149.

These data are illustrated by charts, which require some explanation. The charts are divided up as usual. In the case of flies, that number is taken as unity at which fatal cases begin to increase. The numbers for 1904, 1905, and 1906 are given on p. 160; and the numbers in other weeks are reckoned as multiples of these. In years in which this is not easily determined, 2,000 flies are taken as unity for fourteen or sixteen bell-traps; ten cases or ten deaths are taken as one for the corresponding curves. Temperatures are reckoned by taking the lowest mean weekly atmospheric temperature, subtracting from the mean weekly temperature under consideration and dividing the difference by two. We note the absence of direct relation between the curves of temperature or rainfall and those of flies or deaths from diarrhœa; also the manner in which the curve of fatal cases or deaths falls away from the curve of flies in the middle of the decline. This is probably due, chiefly, to immobilization of flies from cold and fungus. We note, also, that the number of cases commencing in weeks is much smaller than the number of deaths, only those deaths being included in which the parents could be found, and the date of illness ascertained with tolerable precision. The commencement, summit, and end of the annual wave furnish information as to the average interval between attack and death in infants.

In 1904 and 1906 the maximum of attacks precedes by a fortnight, in 1905 by a week, the maximum of deaths. If, however, we average the attacks and deaths in successive weeks, the interval of the resulting curves becomes a week. We may conclude that the average period is greater than a week and less than a fortnight, being nearer to the former period than to the latter. We may, also, for any year, start from the dates of attack, and record the number of deaths occurring within a week of attack and in successive weeks thereafter. Doing this for the year 1908, we find the following intervals between attack and death:—

Deaths in	Week of attack	Second week	Third week	Fourth week	Fifth week	Sixth week	Seventh week	Eighth week	Ninth week	Tenth week +
First period ...	19	9	5	1	7	—	—	—	—	7
Second ,, ...	73	37	11	9	3	2	1	1	1	2
Third ,, ...	82	73	55	17	18	8	1	3	5	14
Total	174	119	71	27	28	10	2	4	6	23

We thus see that, as the diarrhœa season advances, the length of the fatal illness is increased. In the maximal week of illness of the cases supposed evenly distributed over the week, three-sevenths would die in the following week.

1904

Week ending ..	July 9	July 16	July 23	July 30	Aug. 6	Aug. 13	Aug. 20
Enteric cases	3	3	6	6	9	8	7
Deaths from diarrhœa	5	—	14	48	77	91	107
Fatal cases commencing	14	23	40	70	120	103	53
Mean temperature in shade	59·8	66·6	66·2	64·0	66·5	59·1	57·1
Rainfall in inches	0·235	0·230	0·090	0·420	0·630	1·090	1·015
Underground temperature at 1 ft. ...	59·7	63·3	64·1	64·5	65·0	62·6	60·1
,, ,, 4 ft. ...	55·7	56·9	58·6	59·7	60·2	60·9	60·5
Number of flies caught in 12 bell-traps ...	1498	4039	5234	6699	19081	18440	8537

1905

Week ending ..	July 8	July 15	July 22	July 29	Aug. 5	Aug. 12	Aug. 19
Enteric cases	1	1	—	7	2	4	9
Deaths from diarrhœa	7	7	22	62	81	89	67
Fatal cases commencing	12	39	58	70	72	61	51
Mean temperature in shade	61·3	67·8	63·2	61·8	59·8	58·6	60·1
Rainfall in inches	—	0·830	0·780	0·780	0·970	0·625	1·050
Underground temperature at 1 ft. ...	63·1	65·7	65·4	64·6	61·3	60·6	60·9
,, ,, 4 ft. ...	57·8	58·9	60·2	60·9	61·0	60·1	59·9
Number of flies caught in 12 bell-traps ...	2788	4456	7799	9493	9627	8542	7112

1906 In this year the curve of cases may be neglected owing to faulty investigation at the height of the epidemic. This is clear from the numbers.

Week ending ..	July 7	July 14	July 21	July 28	Aug. 4	Aug. 11	Aug. 18
Enteric cases	9	4	5	3	7	2	6
Deaths from diarrhœa	4	6	6	12	17	34	80
Fatal cases commencing	9	3	20	44	74	81	68
Mean temperature in shade	60·4	57·1	59·1	63·4	64·9	63·0	59·6
Rainfall in inches	0·070	0·440	0·750	0·340	0·270	0·640	1·220
Underground temperature at 1 ft. ...	60·2	60·1	59·8	62·4	63·9	63·6	61·8
,, ,, 4 ft. ...	56·4	57·1	57·2	57·8	58·7	59·7	59·9
Number of flies caught in 17 bell-traps ...	3489	4909	7852	9280	12144	16101	16303

1907

Week ending ..	Aug. 10	Aug. 17	Aug. 24	Aug. 31	Sept. 7	Sept. 14
Enteric cases	2	4	3	2	1	4
Deaths from diarrhœa	8	9	5	3	5	12
Fatal cases commencing	4	10	—	9	9	16
Mean temperature in shade	59·4	59·1	55·8	57·1	54·8	60·9
Rainfall in inches	1·020	1·660	0·760	0·080	0·790	—
Underground temperature at 1 ft. ...	59·4	59·1	57·7	56·8	55·7	57·3
,, ,, 4 ft. ...	57·0	57·2	57·3	57·0	56·6	56·1

Aug. 27	Sept. 3	Sept. 10	Sept. 17	Sept. 24	Oct. 1	Oct. 8	Oct. 15	Oct. 22	Oct. 29	Nov. 5	Nov. 12	Nov. 19
9	6	10	13	7	18	7	7	4	7	9	5	7
85	68	43	36	28	24	23	11	3	3	4	1	—
34	32	18	23	13	10	7	1	—	—	—	—	—
54·7	62·3	57·7	57·7	57·0	53·4	49·8	49·5	55·5	49·1	50·0	46·9	45·8
1·530	0·590	0·350	0·145	—	0·595	0·475	0·125	0·790	0·060	0·095	1·825	0·170
57·0	61·3	59·0	56·9	55·3	53·8	51·3	49·8	51·2	49·8	49·2	48·1	45·6
59·5	58·6	58·6	59·3	57·7	56·9	55·8	54·6	53·5	53·1	52·4	51·9	51·0
6609	9802	6837	5485	5500	3642	2398	2070	2214	—	—	—	—

Aug. 26	Sept. 2	Sept. 9	Sept. 16	Sept. 23	Sept. 30	Oct. 7	Oct. 14	Oct. 21	Oct. 28	Nov. 4	Nov. 11	Nov. 18
8	8	6	7	11	7	13	6	12	9	10	4	10
81	61	61	42	20	15	10	9	5	4	6	3	3
37	21	12	12	8	4	—	—	—	—	—	—	—
59·1	56·1	58·5	53·4	54·1	56·4	48·1	49·7	40·8	42·2	46·4	45·0	39·2
1·580	0·850	1·510	0·330	—	0·140	0·970	0·830	0·510	0·390	0·640	0·922	0·040
59·9	59·0	58·5	55·3	54·1	52·5	50·2	51·8	45·0	41·2	43·9	43·5	42·1
59·6	59·1	58·7	58·0	56·9	55·8	54·8	53·6	52·6	50·4	48·4	48·0	47·4
6112	5309	5138	3815	2978	1380	—	—	—	—	—	—	—

Aug. 25	Sept. 1	Sept. 8	Sept. 15	Sept. 22	Sept. 29	Oct. 6	Oct. 13	Oct. 20	Oct. 27	Nov. 3	Nov. 10	Nov. 17
4	4	13	20	19	14	13	22	16	23	16	7	14
102	104	135	136	86	58	35	28	11	13	16	9	5
88	65	28	20	17	12	4	5	—	—	—	—	—
62·5	66·8	63·9	56·6	56·3	52·9	57·5	56·3	46·6	52·8	45·5	46·2	43·5
0·920	0·010	0·520	0·990	0·050	—	0·790	0·810	2·270	0·780	1·020	0·150	1·030
61·5	62·3	63·6	59·2	56·9	53·6	53·9	55·8	50·7	50·2	45·5	45·5	42·9
59·9	59·9	60·1	60·2	59·3	58·1	56·6	56·0	55·7	54·4	53·1	51·5	50·1
16605	23572	23144	19747	13421	10651	11976	6873	2685	3349	—	—	—

Sept. 21	Sept. 28	Oct. 5	Oct. 12	Oct. 19	Oct. 26	Nov. 2	Nov. 9	Nov. 16	Nov. 23	Nov. 30
2	7	3	10	6	6	6	10	7	13	12
18	21	32	45	28	18	12	4	5	5	1
26	25	41	10	12	5	1	4	2	2	2
58·1	60·7	55·7	52·9	50·6	49·6	49·3	50·1	45·4	42·5	41·2
—	—	0·180	1·175	0·670	0·980	0·345	0·150	0·495	0·935	0·760
57·3	56·2	55·8	53·1	51·2	51·2	49·8	48·0	48·7	46·3	43·5
56·2	56·3	56·1	55·9	55·9	55·0	53·9	52·6	51·7	50·9	49.9

1908

Week ending ..	July 11	July 18	July 25	Aug. 1	Aug. 8	Aug. 15	Aug. 22
Enteric cases	—	1	1	1	2	2	10
Deaths from diarrhœa	6	9	20	22	48	82	70
Fatal cases commencing	11	21	28	54	65	48	45
Mean temperature in shade	5·91	58·2	62·8	60·4	61·6	57·6	58·2
Rainfall in inches	1·985	2·545	0·255	0·010	—	0·240	0·700
Underground temperature at 1 ft. ...	61·0	59·3	61·0	62·3	62·1	60·5	59·2
,, ,, 4 ft. ...	57·3	57·7	57·8	58·4	59·0	59·3	59·1
Number of flies caught in 15 bell-traps, in houses only	6959	6635	9579	13242	14270	13654	13781

1909.—Record of Flies caught week by week in three divisions of the

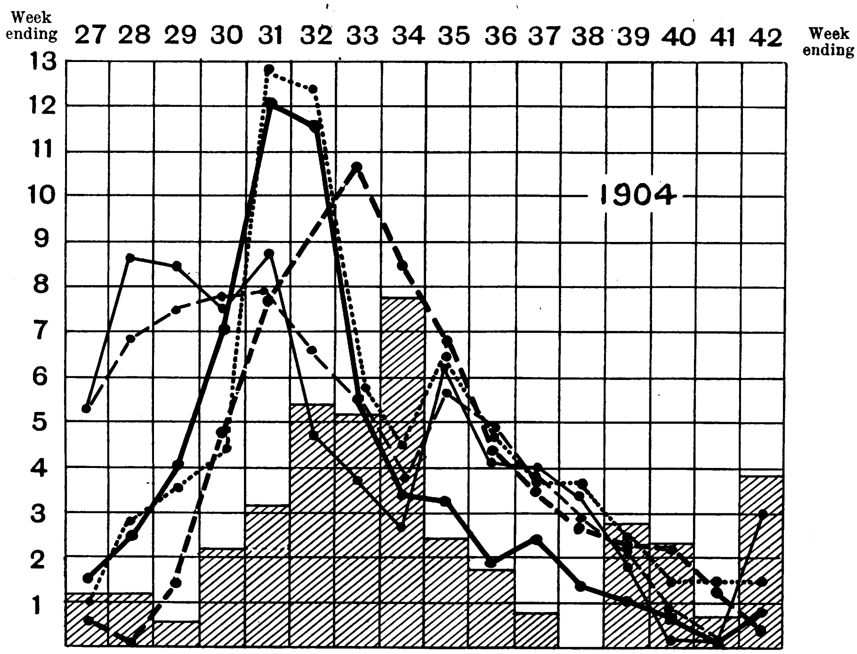
Divisions composed of		21 May 29	22 June 5	23 June 12	24 June 19	25 June 26	26 July 3	27 July 10	28 July 17	29 July 24
Central ... Chorlton - on - Medlock ... Hulme ...	Flies (8 traps) ...	189	645	584	989	757	1487	1869	2489	3816
	Diarrhœa deaths...	—	—	—	1	—	1	—	1	—
	Fatal cases commencing ...	—	—	—	—	—	—	—	1	—
Ancoats ... St. George's ... Harpurhey ... Newton ...	Flies (12 traps) ...	403	1138	2123	4627	4102	6385	7382	10526	9700
	Diarrhœa deaths...	—	3	—	1	2	1	—	4	—
	Fatal cases commencing ...	1	1	1	2	1	—	3	—	5
Bradford ... Beswick ... Clayton ... Ardwick ... Openshaw ... West Gorton...	Flies (14 traps) ...	1953	2893	5550	6020	5926	8236	7947	7013	7597
	Diarrhœa deaths...	2	—	—	—	—	2	1	1	1
	Fatal cases commencing ...	2	—	—	1	1	1	—	4	2
Total number of flies		2545	4676	8257	11636	10785	16108	17198	22028	21113
Total deaths, diarrhœa		2	3	—	2	2	4	1	6	1
Cases commencing		3	1	1	3	2	1	3	5	7
Deaths according to date of onset...		3	1	2	3	2	1	5	6	8
Mean temperature of air		55·8	53·9	51·0	56·1	55·2	57·7	58·6	58·5	58·3
Temperature at 4 ft. underground...		49·6	51·1	52·0	52·4	53·4	54·0	54·9	55·9	56·6
Temperature, 1 ft.		54·0	54·5	53·2	55·4	56·7	55·8	59·3	59·3	59·7
Rainfall		1·150	0·320	0·060	0·285	1·400	0·390	0·780	0·795	0·320

Aug. 29	Sept. 5	Sept. 12	Sept. 19	Sept. 26	Oct. 3	Oct. 10	Oct. 17	Oct. 24	Oct. 31	Nov. 7	Nov. 14	Nov 21
4	10	8	7	8	13	9	8	7	6	13	9	19
54	29	26	21	20	20	11	18	18	17	6	5	1
14	14	13	18	13	13	12	10	5	4	—	—	—
57·9	52·1	54·0	56·7	58·3	65·6	59·3	58·0	49·9	51·5	48·7	45·8	44·4
1·435	0·555	0·690	1·260	1·065	—	0·210	—	0·870	0·370	—	0·720	1·065
57·8	55·7	54·9	53·9	56·3	57·7	57·8	55·4	53·4	50·9	48·7	43·6	43·7
58·6	58·2	57·2	56·2	55·8	56·0	56·5	56·7	56·1	54·8	53·5	51·9	50·2
9436	7856	8832	6896	6439	6777	6635	5845	—	—	—	—	—

CITY, ALSO THE DEATHS FROM DIARRHŒA. STICK PAPERS USED. STATIONS, 34.

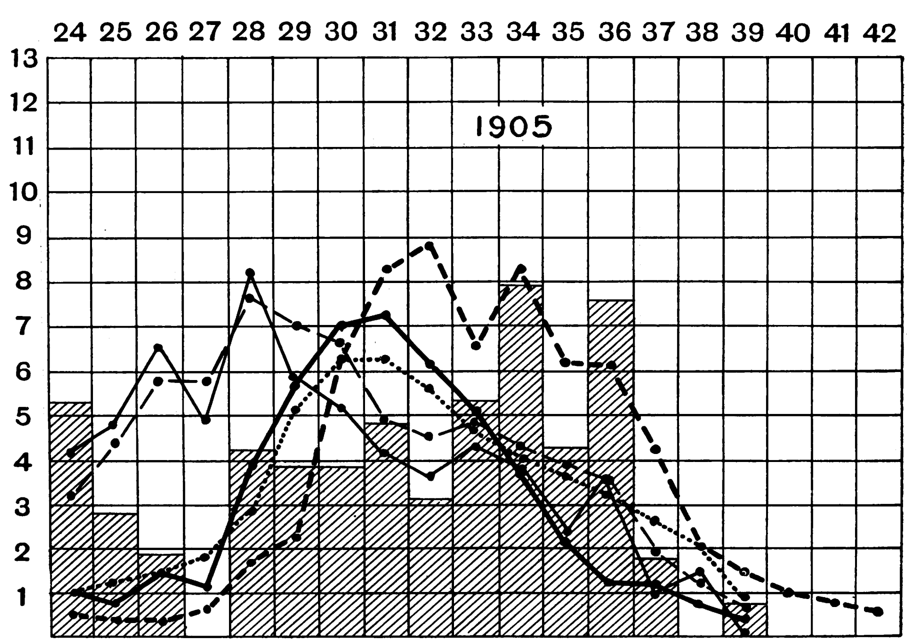
30 July 31	31 Aug. 7	32 Aug. 14	33 Aug. 21	34 Aug. 28	35 Sept. 4	36 Sept. 11	37 Sept. 18	38 Sept. 25	39 Oct. 2	40 Oct. 9	41 Oct. 16	42 Oct. 23	43 Oct. 30	44 Nov. 6
4336	6661	8388	9250	6658	5577	3986	4662	4604	—	—	—	—	—	—
—	2	—	2	3	6	3	4	6	2	6	—	3	1	—
—	3	1	3	5	3	2	2	4	4	—	1	—	1	2
7552	11093	14380	11078	9381	6863	4715	4247	3785	—	—	—	—	—	—
2	5	5	10	16	12	6	6	4	5	5	1	5	—	3
3	5	9	20	9	8	2	3	4	—	2	2	2	—	—
8874	10392	10327	10840	10160	12644	9439	7440	7157	—	—	—	—	—	—
3	4	7	5	6	6	5	5	4	1	1	2	1	—	—
6	4	4	5	6	2	1	2	3	2	1	—	—	—	—
20762	28146	33095	32168	26199	25084	18140	16349	15546	—	—	—	—	—	—
5	11	12	17	25	24	14	15	14	8	12	3	9	1	3
9	12	14	28	20	13	5	7	11	6	3	3	2	1	2
6	13	17	32	21	15	6	7	12	6	5	2	3	2	2
57·8	60·2	64·6	65·6	56·9	54·0	53·3	53·8	55·3	55·0	54·6	54·1	54·2	41·8	48·4
57·0	57·0	57·4	58·6	58·9	58·3	57·4	56·4	55·6	55·1	55·0	54·8	54·0	53·6	51·6
58·5	58·8	61·9	62·2	58·9	56·9	55·3	54·1	53·8	54·5	55·1	53·3	52·6	47·1	45·0
2·525	0·370	0·040	2·035	0·505	0·625	0·850	0·010	0·665	0·840	1·350	1·730	1·705	0·515	0·080

DIARRHŒA.

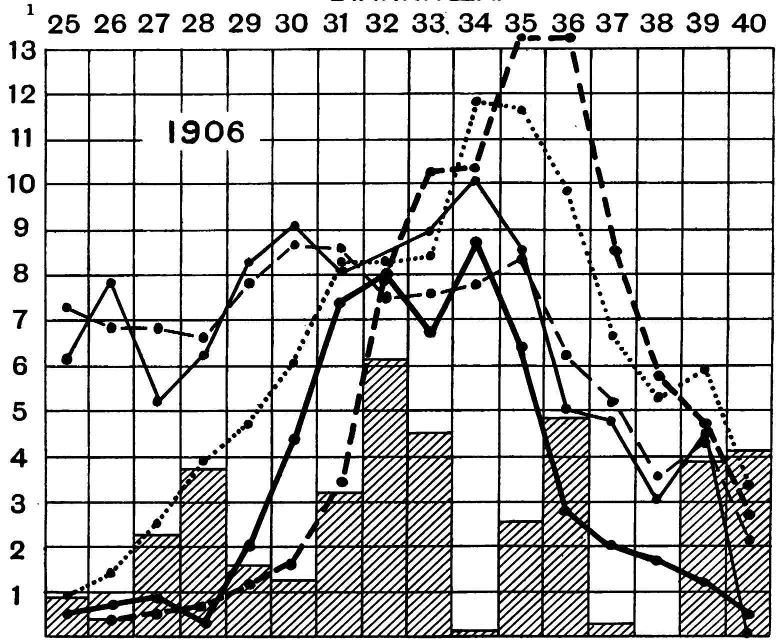


In this and the following four charts the curve ——— indicates fatal cases commencing
...... indicates flies ▬ ▬ ▬ ▬ ,, deaths
—·—·— ,, atmospheric temperature – – – – ,, 1 ft. temperature

DIARRHŒA.

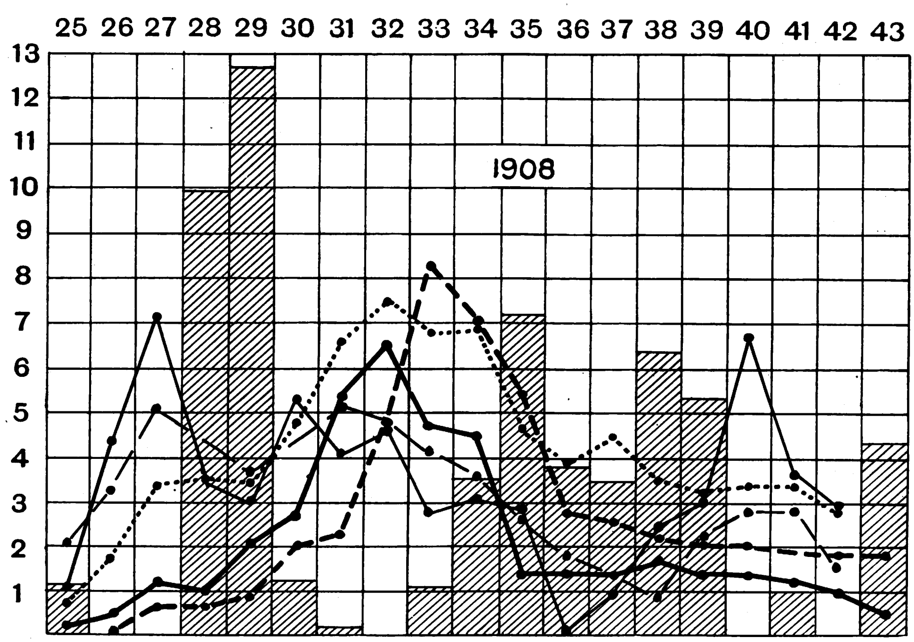


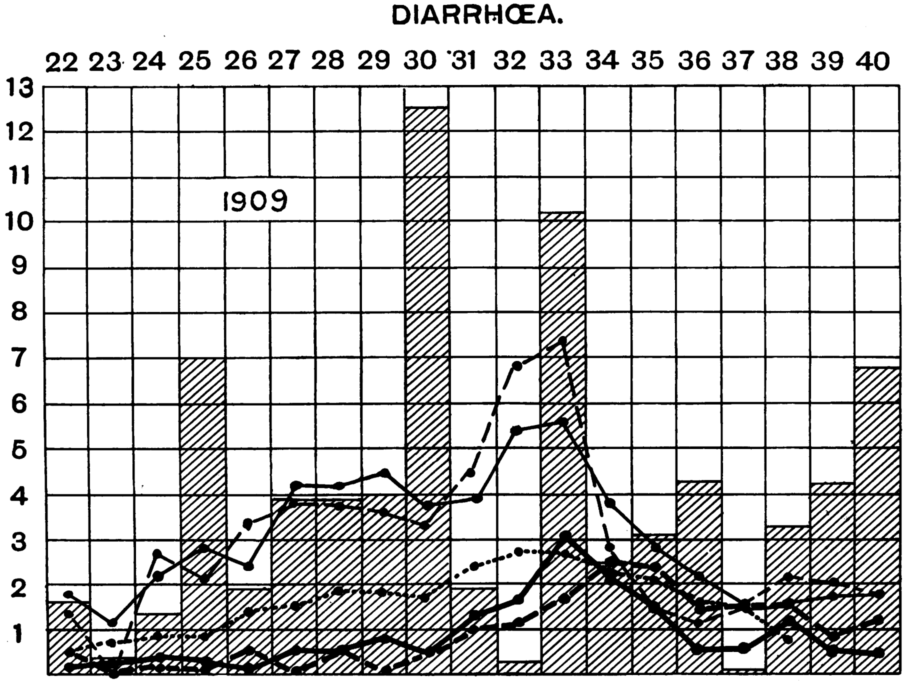
DIARRHŒA.



[1] The number of weeks at the head of this chart should be moved back by one place. Thus, 25 should be 26, and 40 should be 41.

DIARRHŒA.





The distribution of the deaths relative to the cases at the period of maximal fatality is accounted for. It is, however, evident that the interval of maximum attacks and deaths will not much exceed a week. It is probable that with the prolongation of illness the average severity of the attack decreases. This change in the severity of the cases is noted also in the annual report for 1904. We thus see a fairly swift evolution in character of the type of illness. Whether, however, this is owing to the invasion of more resistant individuals or to the dilution of infection in course of transmission we have no means of telling. It is possible, of course, that infection conveyed from without is less virulent than infective matter transferred directly from one person to another. There are, also, a number of cases at the very beginning of the upward movement of diarrhœal deaths which have a long period. In 1904 the cases commencing show an upward movement in the fortnight, and in 1905 and 1906 in the third week preceding the upward movement of the diarrhœal deaths.

RELATION OF CASES AND DEATHS TO THE TEMPERATURES.

We see at once that the course of diarrhœa deaths has no direct correspondence with the course of the atmospheric temperatures or with the temperature of the soil at a depth of 1 ft. On the other hand, a series of high atmospheric and 1 ft. temperatures at a particular period of the year is accompanied by a rush upward of the diarrhœal fatality. The period at which this correspondence is observed coincides with the annual wave of flies. Fluctuations in these temperatures have but little effect on the course of the numbers of flies captured, on diarrhœal cases, or on diarrhœal fatality. Note the temperatures in the following weeks and their relation to diarrhœal fatality: The thirty-fifth in 1904, thirty-second in 1905, twenty-seventh and thirty-second in 1906, twenty-eighth and twenty-ninth in 1908, weeks ending July 24 and 31 and week ending August 28 in 1909. Yet the relation of these temperatures to diarrhœa is undoubted.

It is otherwise with the temperature at a depth of 4 ft. Though the readings at a particular epoch may bear no constant relation to the course of diarrhœa as at the commencement of cases in 1904, 1905, and 1906, yet the course of the 4-ft. temperatures is parallel to that of the course of deaths both in its ascent and in its decline. It reaches its maximum in 1904 in the week preceding the maximum week of deaths, in 1905 in the week preceding, in 1906 in the same week, in 1908 in the same week, and in 1909 in the same week. There is therefore no close correspondence between the maximum 4-ft. temperature and the maximum fatality from diarrhœa, except in the general rise and fall, and Dr. Hamer's observations for 1907 show for that year a complete separation of periods.

With the course of the rainfall the curve of fatality shows no correspondence whatever, and it is evident that any effect exerted by rainfall must be indirect, and must be through temperature, effect on the transmitting agent, or in some other manner.

It is otherwise with the relation of cases and deaths to the number of flies captured week by week. Here the correspondence is close and intimate. The number of cases begins to increase when flies have reached a certain number, and continues to increase until the flies captured attain a maximum. The maximum of cases commencing in the years 1904 and 1905 is in the same week as the maximum of flies. The figures for the deaths are still more striking. The shape of the curves at an interval of a week to a fortnight near the maximum point is practically identical in the two. The errors of the curves will be subjected to examination afterwards. But, even with their manifest and necessary defects, they show a degree of correspondence which creates a high degree of probability that flies are the transmitting agents in summer diarrhœa. In all the curves it will be seen that deaths diminish more rapidly than do flies in the middle part of the decline. For this there are two causes. In years of high diarrhœa-incidence the more susceptible and exposed infants have been killed off or rendered immune. In every year towards the close of the fly season the flies are attacked by *Empusa muscæ*, and are hindered by cold from leaving the house, so that they cease to act as transmitting agents.

At this point it will be convenient to recall certain features of the life of the house fly which have a bearing on the interpretation of the facts. The best accounts of the house fly and of the conditions of its increase which I have come across are to be found in a small book entitled "Our Household Insects," by Mr. E. A. Butler, published in 1893 by Longmans, Green, and Co.; in an article in *Public Health*, May, 1907, by Dr. Griffith, of Hove; and in Mr. Gordon Hewitt's monograph, part 3, to be found in the *Quarterly Journal of Microscopical Sciences*, December, 1909. Many interesting facts are to be found also in Dr. Hamer's contributions, entitled "Nuisance of Flies," reprinted from the annual reports of the Medical Officer of Health to the London County Council, 1907 and 1908. Reference should also be made to the publications of the Local Government Board.

I may say that early in these inquiries I felt the need of fuller information respecting the breeding of flies, and applied for information at the University. Mr. Hardy, of the Museum, gave me some information, *inter alia*, that there are at least two species of beetles which prey on the larvæ of flies, and that the shortest period of development of the house fly is about a week. At that time, however, I could not find accurate information on one of the points which I was hunting up—viz., the period between the deposit of eggs and the emergence of flies. The points which appear to throw light on our present inquiry are the following, taken from Mr. Gordon Hewitt's paper quoted above: Musca prefers to lay its eggs in horse manure, but will also use cow-dung. Gordon Hewitt was successful in rearing larvæ in horse manure, cow dung, fowl dung, and human excrement. Horse manure he found, as I also had done, swarming with larvæ. The larvæ will feed on paper, woollens, cotton garments, &c. They were also reared on decaying vegetables and fruits, bread soaked in milk, and boiled egg. The larvæ were found in privy-middens, and also on a public tip among the warm ashes and clinker.

The shortest period from the laying of the egg to the development of the fly both Griffith and Gordon Hewitt found to be eight to nine days, at temperatures ranging from 95° F. to 71° F. A more common period is from twelve to twenty days. Absence of moisture leads to the production of a small fly. Cold also produces small and imperfect flies. Dryness also delayed development. Deficiency or unsuitability of food naturally impairs development.

In very hot weather the progeny of a fly may be laying eggs in about three weeks after its eggs have been laid. Usually, however, the period is longer than this. Flies are sexually mature in from a fortnight to three weeks.

The larvæ pass through three stages of varying durations. Under favourable conditions the three stages will be completed in less than a fortnight, and the development of the imago will take four or five days longer. The flies lay their eggs, in warm weather, on moist material of the character named. A female fly will lay four to six batches of eggs, each batch 120 to 150 in number.

Flies were shown by Griffith to breed in winter under suitable conditions, and in a good many situations odd ones may be found coming out of their recesses into warm rooms. There is little doubt that they find their way into hollow spaces and cavity walls, as in such situations, they lose a minimum of moisture owing to the stillness of the air surrounding them. Some years ago I observed a considerable number at midwinter in the basement of a common lodging-house, and I have been informed by two medical friends that they have seen odd flies as late as Christmas. Every here and there householders have the same experience. There is little or no doubt, in fact, that the seasons are connected by living flies and not by pupæ. Prolific as flies are, as we have seen, they are subject to numerous drawbacks in breeding: the lack of suitable temperatures, the lack of moisture, heavy rain, overlaying, predatory beetles, and so forth. The larvæ and the pupæ need quite different conditions. The larvæ must have warmth and moisture; the pupæ must have warmth and dryness. These conditions they evidently get *par excellence* in collections of horse manure. But it will also be evident that heavy rains falling on shallow collections will be unfavourable to the larvæ owing to the chilling produced by evaporation, and to the pupæ for the same reason. Such collections will be apt also to dry up too quickly.

Collections of horse manure roofed over will generally be too dry in warm, dry weather, and in various ways the brood may be prevented from developing. Only when the larvæ are deposited in a collection of horse manure, large but presenting fringes not liable to be heaped over, with access of moisture, but dry on the top, will the entire brood be likely to emerge. The same favourable conditions will also be found near the base of large tips. Only a small part of the eggs laid, we may be sure, come to fruition. Hence the numbers recorded, no doubt, represent the main course of the fly numbers, and appear to show that it should not be very difficult to arrest the whole process.

House flies are thirsty creatures, and must have access to moisture. It is doubtless this feature which makes them cluster round the face of the pedestrian in summer, and which causes them to beset the mouth and nose of the infant. They were observed in Manchester to cluster especially about the nose and mouth of infants suffering from diarrhœa. The same feature was observed in South Africa with regard to enteric fever. This I believe to be due to the development of a peculiar ether which I have noted in the breath of many cases of enteric fever. Driven by need of moisture, they betake themselves to cups of tea, bowls of milk, condensed milk, syrups, and beer. But they are also partial to sugar, and therefore visit sugar and breadcrumbs. Warmth also is agreeable to the musca. For all these reasons it prefers the kitchen to the parlour. The need for water is such that we must not accept the dictum that flies are driven indoors by rain. Possibly very heavy rain may have that effect.

It appears needless to trace the early course of the wave of flies, which probably finds its starting place about April in some covered fermenting collections of horse manure. As the ground and atmospheric temperatures rise, the number of swarms increases. I have already given reasons why the numbers do not reach greater dimensions early in the season. The reasons for the annual decline lie partly in the increase of cold, partly in the spread of the *Empusa muscæ*, which begins in July, becomes destructive in the latter part of August, and, as September advances, kills large numbers. Numbers also probably seek shelter. That *Empusa muscæ* is unable to prevent the growth of flies under favourable conditions is seen in the renewed swarms which appear in some seasons late in the autumn, under the influence of high temperatures; but it is also probable that the beetles which prey on the larvæ become more numerous as the season advances. It is possible that the flies which seek shelter are fairly numerous. I have seen spiders in midwinter in very good condition, with not a fly about. Possibly mice do not disdain them. Mr. Butler states that there is no antagonism between flies and cockroaches: it is a point worth determining over again. It would be interesting to know what becomes of the dead flies ; usually they are swept away, but those which are not seem to disappear very promptly.

It is evident that if flies are responsible for the ascent of the diarrhœal curve, it requires a goodly number to start it on its upward course ; in fact, we must imagine to ourselves an infant's food visited by a very large number of flies, some only of which convey infection from the excreta of a previous case. The quantity of infection in a given case will thus at first be small, and a number of slight cases will be produced, which scarcely attract notice ; but as flies multiply, and cases multiply, the amount of infection conveyed to foods will increase. Meanwhile direct infection is going on, and probably the cases thus infected will, in general, be most fatal, those infected by flies becoming more fatal, but still less so than those infected by direct conveyance from person to person. As flies and cases continue to multiply, however, the fly-borne infection preponderates more and more, and we get a massive and fatal infection which quite overshadows the direct process. The average case thus produced remains, however, less fatal, and has a longer course than that owing to direct transmission.

With regard, now, to the manner in which we may suppose flies to carry infection. What we have to explain is why infants continue, in increasing numbers, to become infected without any evidence of exposure, at home or elsewhere. We may fairly assume that the freshly-emerged fly does not usually carry off from his birthplace any great amount of infection ; he is spruce, young, active, and keeps himself clean. Setting out in search of food and drink, he betakes himself, amongst other places, to the most likely house he can find. Perhaps it is baking day, and there is a goodly assemblage of flies. The baby has diarrhœa, and they settle on its lips, or visit its napkin, which is tossed aside. Perhaps they light on some diarrhœal excreta from an older child. The kitchen is cleaned up, and the flies now infected escape, in search, it may be, of moisture, and soon find themselves in another house where there is also an infant. Here they visit the various articles of food, and, if in sufficient numbers, leave a certain amount of infection behind them. The amount which they leave will depend, however, on how many centres of infection there are about ; that is, on how many flies come from infected houses in which they have access to infected excreta, and on how many have visited infected excreta, presumably for the purpose of depositing eggs. The chances will be very much against any one fly carrying infection, and the numbers must therefore be very large before infection is conveyed. That such is usually the case will be gathered from the particulars collected by Inspector Hewitt, and published in my annual reports for 1904 and 1905. It will be seen, too, that so long as the flies are healthy and active their power for mischief increases with their increasing numbers, as a steadily larger number will be visiting pail recesses and middens, and as the numbers of children attacked increases. This process is limited, as we have seen, by the increasing numbers who have recovered, and by the growing sluggishness of the flies.

There is no doubt at all about the extent to which flies move from house to house. This is gathered directly from examination of the records of individual stations, in which the numbers fluctuate remarkably. Equally little doubt is there about their visits to middens, and their return to fresh houses laden with the spoils of these places. If, however, we compare the incidence of diarrhœal mortality on different localities, although the influence of middens is unmistakable, we must conclude that all the phenomena of spread of the disease can take place in their absence. There is some presumption, at all events, that it is the transference of flies from house to house which is the chief factor in conveyance; the circumstances, however, forbid any exact conclusion. The infected flies may give the disease to the infant through its food, by direct contamination of its lips, or indirectly by way of some older person, and the observations published in my annual reports for 1904 and 1905 show that the latter mode of entrance of diarrhœa into a house is probably not infrequent. Nor, in this case, does the infection need to be carried into the house by flies; it may come in syrup, milk, bread, or fruit which they have visited in some shop.

It does not follow because older persons have not had a well-marked attack of diarrhœa that they have not been infected and so introduced the disease. The remarkable series of cases published in the Manchester Annual Reports for 1905, and cases given in other reports, show that enteric fever has been contracted and recovered from without almost any ill-health. No doubt precisely the same is true of summer diarrhœa. Now we have proved that slight cases of overlooked enteric are, for purposes of infection, far the most potent source of direct infection. Nevertheless, direct or contact infection is a small factor in the causation of the annual wave, though not unimportant in determining its magnitude. Nor is the remarkable correspondence between the curves of diarrhœal fatality and the curve of flies confined to our observations in Manchester. Dr. Hamer's curves of mortality for 1907 and 1908 correspond in a remarkable manner to the curve of flies which he has given for the stations surrounding stable centres. Now it is precisely from these centres that we should get swarms of young, active migratory flies. How far do these migrations extend? From observations on marked flies carried out at Monsall Hospital Dr. Miles B. Arnold has shown that they can travel a distance of 190 yards. It is not to be supposed, however, that they do, in fact, in the heart of towns often take such flights; nevertheless, it is a valuable proof of their power and inclination to travel a considerable distance. Dr. Hamer has thrown out the suggestion that the numbers required in different years to cause infection may be different, depending on the initial centres of infection, as it must do. That appears highly probable, especially at the commencement of the outbreak; but the variation is likely to be least where the number of endemic centres is large at all times, as is probably the case in Manchester, and it is not likely to have much effect towards the summit of the curve if diarrhœa reaches any great intensity. The migrations of flies, however, may be quickened by warm, dry weather, and the number of infected flies reaching a given house will certainly be dependent on the total number of flies and on the centres of infection within reach. Hence the fluctuations in diarrhœal mortality in sparsely peopled outlying districts, and in districts with a low average diarrhœa-incidence, are likely to be much greater than in a densely inhabited district with many endemic centres. What determines the number of endemic centres at the commencement of the season we do not yet know, though we can assume it to be a function of the average incidence. It is greatest amongst the poorest classes, who also possess in general the least well-constructed and the least well-kept houses. On reference to the table of district mortalities we do, in fact, find that fluctuations are greatest in the better-off, least densely peopled districts. It is difficult to see how this is to be explained on any theory of soil influence. If, now, the number of flies required to carry infection varies with weather, being least in warm, dry sunny weather, largest in cold, wet weather, we must not expect too close a correspondence between flies caught and fatal cases commencing. The actual degree of correspondence would seem to show that the adverse factors named do not operate as much as we should expect.

The question of the number of flies required to produce an effect on the diarrhœa curve is one of considerable moment, if we assume, as I now propose to do, that the relation is one of cause and effect, a matter to which I will hereafter return. Referring to the records, we find that the number of cases begins to increase in the following weeks: In 1904, in the twenty-seventh, in which the number of flies captured was 1,498 in 11 beer-traps; in 1905, in the twenty-fourth, with 1,527 in 14 beer-traps; in 1906, in the twenty-fifth, with 1,984 flies in 17 beer-traps. The corresponding number of flies per 10 beer-traps is thus shown:—

Year	Week	No. of flies
1904	27	1,362
1905	24	1,090
1906	25	1,170

As regards the end of the curve, in 1904 the last considerable number of cases commencing is in the fortieth week, when the number of flies captured was 2,398; while in 1905 the corresponding week was the thirty-eighth, and the number of flies 2,978. In 1906 the curve of cases does not represent the curve of deaths; if, however, we measure back three weeks from the end of the curve of deaths, we find that the number of flies was 3,349. The number of flies, therefore, at the end of the period was greater than at the beginning. It is doubtful, however, whether these flies at the end of the period had much, if anything, to do with the continuance of the curve of deaths, which, for reasons already given, is more probably dependent on direct infection at this point.

It may here be pointed out that we must not attach too precise a value to numerical comparison of deaths and flies at different points of the curves. Undoubtedly the number of flies effective to produce an effect on the diarrhœal curve at the outset must be much more potent for purposes of infection than the number at the height of the fly season. Many of these are engaged in reproduction, and are not available for transmission of disease; many others are already invaded by empusa and will not be available either for reproduction or for carrying infection. On the other hand, the centres of infection have multiplied to such an extent that the fresh broods coming into play can effect much more mischief than could a similar number of flies at the commencement of the curve. The relations are too complex for the establishment of numerical relations. The numbers are available only for comparison near the points of the curves to which they refer.

RAINFALL AND FLIES.

We may now consider the relations of rainfall and temperature to flies. These are not now under consideration in their bearing on infection, but it appears desirable to see how they affect the curve of flies, bearing in mind that this curve needs correction and that in any attempt to reconstruct the course of the fly curves we should get valuable aid from such a study. We may begin with the relation of rainfall to flies as being the least involved.

The effect of rainfall on flies will be gathered from the summary of fly observations. In 1904 there is a fairly heavy rainfall in the thirty-second, thirty-third, and thirty-fourth weeks. Coincidently with this there is a fall in the number of flies and of deaths, and also in the various temperatures. Nevertheless with diminished rainfall in the thirty-fifth week, and rise in the mean temperature of the atmosphere, there is increase in the number of flies captured and arrest in the decline of cases of fatal diarrhœa. Recovery is thus rapid though imperfect. But the diminution of flies and cases in the three weeks is very marked. In 1905 there is over an inch of rainfall in the twenty-fourth week. This does not prevent considerable rise of all temperatures. Rainfall of nearly an inch in the thirty-first week accompanies the maximum of flies and of fatal diarrhœa commencing. There is, however, fall of the atmospheric and 1-ft. temperatures, the latter marked. Rainfalls of over an inch in the thirty-third and thirty-fourth weeks are accompanied by rise in the atmospheric and 1-ft. temperatures, but accompany a fall in the number of flies and of fatal cases commencing. Rainfall of $1\frac{1}{2}$ in. in the thirty-sixth week is accompanied by rise of atmospheric temperature with fall of the 1-ft. temperature. There is not marked fall in the number of flies, though there is in the number of cases, in the fortnight following. In 1906, in the thirty-third and thirty-fourth weeks, rainfall of about an inch is accompanied by some fall of atmospheric temperature, which, however, rises sharply in the second week. There is stasis in the first week in the number of flies and in the number of fatal cases commencing, and rainfall of about an inch in the thirty-sixth week is accompanied by fall in the number of flies, number of fatal cases commencing, atmospheric and 1-ft. temperatures. In 1908 heavy rainfall in the twenty-eighth and twenty-ninth weeks is accompanied by diminution in the number of flies captured in the second of these weeks, and by falls in the atmospheric and 1-ft. temperatures. The decline of the 1-ft. temperature is most noticeable in the twenty-ninth week, and it is not until the thirtieth week that stasis of the 4-ft. temperature is evident. It is observable that the upward movement of flies remains partially affected in the thirtieth week. In the thirty-fifth week rainfall of about 1½ in. is accompanied by marked diminution in flies and of the 1-ft. temperature. In 1909 rainfall of 1·4 in. in the week ending June 26 is accompanied by fall of atmospheric temperature and stasis of 1-ft. temperature, also by stasis in production of flies. Rainfall of 2 in. in the week ending August 21 is accompanied by stasis in the number of flies, and in deaths a fortnight afterwards. The mean temperature of the air rises, as do the ground temperatures in the same week.

Thus heavy rainfalls tend to lower the temperature of the surface, but have less effect on the atmospheric temperature, which may rise in spite of them. They produce a greater effect on flies than they do on temperature, and this effect on flies is reflected in the number of fatal cases commencing, and in the number of deaths in the week but one following. This is not a quite accurate statement for the end of the curve. But at this period rain will have less effect in reducing the number of flies captured than on deaths, since it is on the production of young active flies that their chief influence is exerted, and the number of these is not exhibited in the numbers now captured. The influence of rainfall when excessive is thus not exerted entirely through its effect on the general temperature. It has a marked influence on flies and deaths apart from this. It is probable, however, that the influence is really on caloric in the first instance. Smaller collections of refuse, privy-middens, and small shallow collections of horse manure get saturated and unfavourable to the pupal stage. The numerous points which they present permit of rapid evaporation exceeding that of surfaces in general, with consequent chilling of the imago. Moreover the surface temperature is different from that of the atmosphere or that at a depth of 1 ft., and is probably lower than either, owing to evaporation. There can be no doubt that heavy rainfall exerts on the whole a disastrous influence on the production of flies. This does not occur at rainfalls of 0·8 in. or under, which appear to have the reverse effect, at all events so long as the atmospheric temperature is rising.

In this nice balance more or less of rainfall over the quarter cannot much matter. In fact, in Manchester no sustained correspondence can be made out between mean rainfall in the third quarter and the number of deaths from diarrhœa. The years of highest rainfall—viz., 1891, 1892, 1893, 1895, and 1903—have all been years of fairly high diarrhœal fatality; 1907, the year of lowest diarrhœal fatality, was not a year of exceptionally low rainfall. Even heavy rainfall, it will be seen, does not necessarily exert any unfavourable influence on the development of flies or the extension of diarrhœa. Its doing so will depend entirely whether it is able to lower the surface temperatures below those which are favourable to the development of the larvæ or the escape of the imago. If it fail to effect this, its influence will probably be in the opposite direction, owing to the great need of moisture for the development of the larvæ and, one may add, for the health of the flies.

It appears desirable that we should now consider how the continuous course of the 4-ft. temperature arises, and how it comes to be related to the continuous course of the fly curve and of the diarrhœal curves.

The Course and Interaction of Temperatures.

From the data for any year it will be seen that the atmospheric temperatures ascend much more rapidly during the summer than do those at the depth of 1 ft., and that these in turn ascend and descend much more rapidly than do those at a depth of 4 ft. The fluctuations of the atmospheric temperatures, however, are generally reflected in the mean temperatures for the same week at a depth of 1 ft., though much reduced in amount. At a depth of 4 ft. the oscillating upward and downward movement of temperatures disappears and is replaced by a fairly steady upward and downward curve. Why is this? We may divide the soil into layers of 1 ft. in thickness, the 1-ft. thermometer being placed at the junction of the fourth and second, the 4-ft. thermometer at the junction of the fourth and fifth. In the long bright days of June radiant heat is absorbed by the surface layer of the soil, and by numerous other surfaces, which in their turn warm the air in contact with them by convection. Part of the radiant heat is absorbed and causes the temperature of the surface layer to rise, according to its specific heat and conductivity. Only a portion of the heat absorbed reaches in, say, two days, the second layer of soil, those portions being lost which are radiated off from the surface, or used up in raising the temperature of the layer, or used to a small extent in supplying chemical energy and energy of growth, or expended in evaporation at the surface. The inward flow of heat is accelerated or retarded by the movements of heat at the surface, which are therefore reproduced on a smaller scale. Exactly the same applies to the flow from the second to the third sheet, from the third to the fourth, and so on; the oscillations in the flow inwards or outwards getting rapidly reduced, until at a depth of 4 ft. the movement is practically continuous. At each successive layer, the first effect of inward flowing heat is to raise the temperature of the layer, a circumstance which rapidly reduces the size of the oscillations. According to Copeman, in Sir Shirley Murphy's "Treatise on Public Health," the annual movement of heat reaches a depth of 60 ft., the movement becoming slow and uniform when we reach some depth. The 4-ft. thermometric readings therefore constitute a fairly uniform register of the excess of heat received at the surface over heat lost by the first 4 ft. of earth from February to August, and of loss over receipt from August to February. Any growth of bacteria, however, must partake of the fluctuations observed at or near the surface, and must be subject to the effects of rain in chills as well as arrest of movement from the surface. The growths and their dispersion would therefore tend to partake of the changes of temperature and dryness at the surface. This opinion is strongly supported by the known rapidity of production of cultures of bacteria. They would almost certainly suffer acutely or flourish exceedingly, the more so that the periods of favourable and unfavourable surface conditions are often of considerable duration. There is, however, no sign of such vicissitudes in the progressive and decided curve of diarrhœal fatality.

It is easy to see, from study of the course of temperatures, that the wave of inflowing heat takes rather over a week to reach the depth of 4 ft. The course of the 4-ft. temperature corresponds generally, therefore, to the excess of heat entering over heat leaving the surface in the week before. But it is a summation, and it does not even follow when the 4-ft. temperature continues to ascend that there has been any heat added in the week previous. There may have been defect, the difference being made good from the absorbed heat stored up in the four top feet of ground in the weeks before. Nevertheless, the ascent of the 4-ft. temperature indicates in general continued gain of heat at the surface over loss a week before, the amount thus stored up flowing back if need be to make good loss from lowering of the surface temperatures.

Now, Dr. Griffith and Mr. Gordon Hewitt both show the need of heat for the development of the larvæ. How is the inflow of heat indicated by the 4-ft. temperatures related to flies? Imagine a collection of horse manure in an open midden rising to a height of 3 ft. or 4 ft. If the bottom of the receptacle is some 2 ft. below the surface, the manure takes at the sides the mean temperature of the surrounding earth, and the larvæ, which prefer the bottom of the heap, are deprived of heat if the surrounding soil is at a comparatively low temperature. With a large heap of fermenting manure in the receptacle, sufficient heat will usually be generated to raise the temperature of the bottom layer. But the temperature of this layer will depend on the difference in level between its temperature and that of the surrounding ground. Now, so long as the surface layer is receiving more heat than it is losing, its temperature rises, and the heat lost from the manure heap is, *pro tanto*, reduced. That is to say, the larvæ feeding at the bottom of the heap are under steadily improving conditions so long as the 4-ft. temperature is rising. A small heap of manure is much more dependent for its heat on the ground than a large one, and is also much more dependent as regards the starting of those internal processes of fermentation which raise its temperature. The same applies to middens.

If from any cause, such as rainfall, the temperature of the manure heap is lowered, inflow of heat takes place from the soil around and beneath. The temperature of the soil at various depths is thus of considerable importance to the growing larvæ, and it is this influence which the 4-ft. temperature integrates and registers. The pupæ, on the other hand, need a warm, dry atmosphere. Much rain, by causing evaporation and chilling, is hostile to the emergence of the fly. Now, the state of the atmosphere may not be determined by the condition of the surface, though it generally is. To some extent there is independence in the conditions determining a favourable surface and a favourable interior of the manure heaps in which flies are generated. In general, however, sunny, warm weather is favourable to both. Thus at the commencement of the season musca probably starts to increase in a few isolated, considerable, fermenting manure heaps, and large tips. As heat flows into the ground more manure heaps start fermenting, and by now there are more muscæ laying eggs; multiplication increases. Towards the height of the season every heap of refuse and excrement receives in a warm year sufficient heat from the soil to support the larvæ. In comparatively cold years it is the smaller heaps of refuse and middens which are cut off from producing. Moreover, the stages of evolution are longer, and a larger number of flies perish from being overlaid. This, however, is less liable to happen in middens and tips than in heaps of horse manure. These considerations explain the marked fluctuations in death-rate from diarrhœa observed in the districts served by privy-middens. Homalomyia, as we have seen, can apparently complete its evolution at a lower temperature than musca. It is probable, therefore, that in comparatively cold years homalomyia will be found in proportions to musca relatively high.

What is it that determines the fall in the curve of flies? As we have seen, great numbers are destroyed by empusa. The fall coincides, however, with the period at which heat begins to be lost from the ground, and the loss is, of course, most rapid at the surface. We are thus, again, reminded of the great importance to the larvæ of a high temperature in the upper layer of the soil. There is still much laying of eggs, but the conditions of ground and atmosphere become, as a rule, steadily less favourable. If a spell of warm weather sets in again, however, flies increase, and diarrhœal fatality may also increase. For this purpose, however, the increase of flies must be considerable, as we are now dependent for carriage of infection on the fresh brood, the older flies having become comparatively inactive, and there may also be comparatively few infants left unattacked. Hence the increase of diarrhœa is usually small compared with the rise in temperatures. Flies, then, in their development and reproduction, respond to all the influences which affect the 4-ft. temperature: rain, sunshine, high absorption of surface heat, warmth and dryness of the atmosphere, and sum up in their annual wave these influences, though not just as the 4-ft. temperature does. Both correspond to the course of gain and loss of heat by the surface.

Flies stand, however, in a more intimate relation to deaths from diarrhœa than do the 4 ft. temperatures. The reaction to their increase probably takes place within two or three days, judging from the daily records. In all cases their increase precedes that of deaths by a week to a fortnight, except where we may confidently reckon on an error in the numbers captured, regarded as representing the average numbers. Yet on this point some caution is necessary; it is easy for such an error to occur. But it is also quite possible that we might have a great increase of flies round a nest of diarrhœa with a general decrease in flies, but with increase in diarrhœa. In other words, it is necessary to bear in mind that the curve of contaminated flies is not necessarily coincident with the curve of all flies. There is, of course, this great difference between flies and the temperature at a depth of 4 ft.—that the former can directly influence the course of diarrhœa, the latter cannot.

The second line of reasoning by which we are brought to the conclusion that the annual rise of diarrhœa is caused by the annual uprush of flies consists, therefore, in the establishment of a close correspondence between the number of flies captured in any one week and the number of cases of diarrhœa occurring in the same week, or the number of deaths occurring about ten days subsequently, coupled with the absence of any other satisfactory cause. The first line of reasoning was deduced from a study of the distribution of diarrhœal mortality in the sanitary districts of the city, based on the table given at p. 135. The behaviour of diarrhœa in this table is the well-known behaviour of an infectious disease. The districts change their order of diarrhœal mortality from one year to another, a circumstance which indicates a change in the proportion of susceptible material, or of centres of infection, or of carrying agents, or of all three. We have seen, however, that in years of comparatively low temperature in the third quarter, middens will probably take a minor part in the production of flies. Now, if flies are the cause of the summer rise of diarrhœa, there should be a comparatively small fatality in midden-privy districts in colder seasons. The mean temperature of the third quarter is, however not the best measure of the heat available for the production of diarrhœa, which may be very unevenly distributed. The best measure, probably, is the rate of mortality from diarrhœa during the quarter. The necessary comparisons, however, cannot be made in the last three years, during which period there has been a rapid reduction in the number of middens. During the period covered by this table there have been but few years of sustained low temperature. The most convenient will be 1896, 1902, and 1903. The total series of mean temperatures and diarrhœal death-rates in the third quarter is as follows :—

Year	Death-rate per thousand living	Mean temperature third quarter. Degrees Fahr.	Rainfall third quarter Inches
1896	2·93	58·5	9·7
1897	6·01	58·9	9·7
1898	6·00	60·1	6·1
1899	6·96	60·8	7·7
1900	4·14	60·3	9·6
1901	6·33	61·9	6·5
1902	0·88	57·6	5·9
1903	2·19	57·8	12·3
1904	4·48	60·2	6·9
1905	3·89	58·9	9·4
1906	4·91	60·8	6·2
1907	0·45	58·5	7·8
1908	2·61	59·2	10·7

Now, the districts which up to recently have been predominantly served by midden-privies, and not by either water-closets or pail-closets, are Bradford, West Gorton, Clayton and Openshaw, while the pail-closet districts were St. George's, Ancoats, Central, and in large measure Hulme. If, therefore, flies are largely concerned in the spread of diarrhœa, we should expect that the diarrhœal mortality would rise in privy-midden districts in seasons of high temperature and low rainfall, and would fall under the reverse conditions. As we have seen, the effect of rainfall on the production of flies will be specially felt by small collections of manure and by middens. We find accordingly that in the season of highest diarrhœal mortality—viz., 1899—West Gorton and Bradford take the premier position, while in the next highest—1901—Clayton takes the first place. In 1898, again, Bradford, West Gorton and Clayton come before Ancoats and St. George's. In 1902 and 1903 Bradford and Clayton fall below the Central districts. Not so West Gorton, which in 1902 stands highest of all. The privy-midden districts in 1902, again, do not fall so low compared with their own mean as do the Central districts, while in 1903 they fall much lower. That has to do, no doubt, with the high rainfall in 1903 as compared with that in 1902. In 1896 their death-rates from diarrhœa are comparatively low in regard to their own means, except Clayton. Thus, while there is a general tendency in the direction which theory would indicate, the facts lend only a general support to the theory. There are clearly other and baffling factors at work. Such factors are the establishment of immunity in persons above the age of one year, the varying facilities for the production of fly swarms, and the varying amounts of infection available as a nucleus. All that we can assert is that the table indicates an infectious disease subject to local factors of the kind indicated, and is not consistent with the view of a general growth of bacteria in or on the soil. It may be pointed out that the quarterly seasonal factors in 1897 and 1908 are similar, with very different mortalities. This may be supposed to be due to the abolition of middens, but it is rather too soon to come to a positive conclusion, although the figures for Bradford, Clayton, West Gorton and Openshaw would seem to justify this conclusion. We have not gained so much from this line of argument as we might have hoped, but nevertheless the table forms an important part of our reasoning as regards infection.

Another promising line of investigation is to take different districts of sufficient size, plant in them a sufficient number of house-fly stations, and compare the incidence of diarrhœa deaths or cases commencing with the number of flies captured. If the course of fatality in the areas chosen in different parts of Manchester varies, then, if house flies are carrying the disease, the numbers of house flies captured in those areas will vary to correspond. That such differences do occur in the course of fatality in the main divisions of the city is clearly shown by the table just considered. Prior to 1909 I was not able to try this plan, and, unfortunately, owing to the small number of flies and the low diarrhœal fatality in this year, the observations are of little value. In 1906, however, the number of deaths was much larger, and the difference in their distribution in different districts was very striking; unfortunately, the number of fly stations in any one district was small, and no decisive conclusions could be drawn, yet the results, as far as they go, support the view that the disease is propagated by flies. They may be stated thus:—

FATAL CASES OF DIARRHŒA.

District	Beginning in June and July	Beginning in Aug. and Sept.	No. of fly stations	Maximum number of flies week ending
Ancoats	49	29	3	July 29, one station August 19, two stations
Newton	22	24	0	—
Bradford	15	14	1	July 29
Ardwick	17	13	1	July 29 and August 12 equal
Chorlton-on-Medlock	18	17	1	July 22 and 29, again an increase August 12 and 19
Central	14	27	1	August 12
St. George's	34	51	1	August 12
Cheetham	3	15	0	August 19
Beswick	4	16	1	August 12
Clayton	2	11	1	August 5
Openshaw	8	15	1	August 12
West Gorton	11	28	1	July 29 and August 19
Hulme[1]	18	26	1	August 19

It will be conceded that these data, scanty and defective as they are, are very suggestive, the more so as Ancoats, Newton, Bradford, Ardwick, and Chorlton-on-Medlock are contiguous districts.

Insufficient as are the data for 1909, they have been given in the collected records, to which I must refer back. In this year stick papers were used, and each stick paper captured more than double the number of flies caught by beer-traps. I neither like the method nor am I sure that the results are quite comparable with those previously obtained. It is possible that homalomyia occurs in much larger proportion in beer-traps than on stick papers, and it is certain that homalomyia is more likely to visit middens to lay its eggs. Be that as it may, we may divide the total numbers of flies at least by six to get figures comparable with those for 1904 and 1905; probably the divisor should be greater. This being premised, it may be stated that three large areas were chosen. In the

[1] The number of flies in this district, however, in the weeks preceding greatly exceeded those in the weeks following.

first were planted eight stations; in the second, twelve; in the third, thirteen. The number of flies in each is insufficient to produce decided effects, the curve of deaths being irregular and indefinite in the first. In the second, however, both curves towards the apex are definite and very similar. Generally speaking, the apices of both curves precede that in the first area by a week. It is certainly interesting to find that the only area which gives a definite peak of flies also gives a definite peak of deaths. In the third area the number of flies is small and the apex not well marked; there is a general rise of deaths corresponding, the summit of which is level and extends over six weeks. The greatest incidence of flies in the third area corresponds to that in the second, but is insufficient to produce a marked curve. The total curves give the usual results, the curve of flies preceding that of diarrhœa deaths by a fortnight. Indeed, the correspondence in the total curves is very striking once these are established. Perhaps the energies of the flies were used up at the commencement of the season in establishing centres of infection.

On the whole, these figures, which go for little, sustain the connexion between flies and fatal diarrhœa.

On Correction of the Curve of Flies.

The enumeration of flies suffers from the small number of stations, and is liable to suffer if these are injudiciously selected. We may therefore occasionally find misfits in the curve of flies. We may, I think, apply corrections by observing the following considerations. Notwithstanding that the number of stations is small, it is probable that the numbers of stations in which there is increase or decrease in any week as compared with the week preceding will show the trend of the fly movement when the number of flies captured has gone astray. Then, moreover, there are certain broad features of the curves: The curve of cases begins to move a fortnight or three weeks before the curve of deaths. The intervals are: 1904, a fortnight; 1905, three weeks; 1906, a fortnight; 1908, a fortnight. The greatest number of fatal cases begins in 1904, in the same week as flies reach a maximum. This is also the case in 1905. In 1908 and 1909 the maximum of cases commencing occurs in the week following the maximum week of flies. At the same time, any stasis in flies at this period is attended with diminution of cases. That is probably dependent largely on the influence of the fly fungus. An upward movement means the coming of a large number of young and active flies. There is thus a presumption that in the higher parts of the curve and up to its summit, in years of moderate and considerable intensity, the number of cases commencing corresponds to the number of flies, so long as that number is rising. The maximum number of deaths follows the maximum number of flies, being sometimes in the week following, sometimes in the week but one following, the interval between deaths and cases being somewhat the shorter. In 1904 the interval is a fortnight, in 1905 a week, and here the two-weekly plateau in both is very striking. In 1906 the interval is a week, and here again the two-weekly plateau is striking. *Cæteris paribus*, up to near the apex, the same number of flies should produce more cases as the weeks advance, owing to the previous increase in infective matter. This is well marked in 1906. In 1908 the interval is a week. In 1909 the small two-weekly maximum follows the small fly maximum at an interval of a fortnight, the same correspondence at the apex being noted. It has already been explained that the actual interval between cases and deaths is somewhat over a week but less than a fortnight, and in the case of flies somewhat greater; but if regard be had to the fact that the maximum of production will fall in different parts of any given week, the uncertainty of the interval becomes matter of course. Moreover, the average interval probably varies from year to year.

Defective as are the numbers of cases with definite dates of commencement, and imperfectly, therefore, as they represent the curve of deaths, we may obtain some light on the distribution of flies from the arrangement of fatal cases commencing in different weeks. These indicate, as do the deaths, that the actual maximum of flies was in the week preceding the maximum of deaths, while the number of flies in the weeks preceding and following this were probably equal.

But there are also other points by which the movements of flies may be fixed. The 4-ft. temperature attains its maximum in the weeks:—

Year	Preceding or following maximum of cases	Preceding or following maximum of deaths	Preceding or following maximum of flies
1904	Week following	Week preceding	Week following
1905	Same week	Week preceding	Same week
1906	Week but one following	Same week	Week but one following
1908	Week following	Same week	Week following
1909	Week but one following	Same week	Week but one following

The movements of the 4-ft. temperature do not therefore correspond closely with those of the curves of deaths, flies, or cases; still less do they do so at the commencement of the curves. These movements therefore cannot be used to correct the curve of flies. We may say, generally, that the number of flies at any period of a curve depends on the number of eggs laid three or four weeks before at the commencement of a curve, or a fortnight or so before in a warm season near the apex of the fly curve. It depends on the presence at that period of bright, sunny weather. It depends, also, on the surface temperature, high temperatures favouring the development of pupæ, especially in sunny, dry weather. It depends on the temperature of the surface of the ground a week before, which in its turn influences the development of larvæ. It depends also on the rainfall, much rainfall lowering the surface temperature and hindering the emergence of flies. The effect of rainfall is, however, much influenced by the atmospheric and ground temperatures, since with moderate rainfall and high temperatures there is increase of fermentation in manure heaps and accelerated development of larvæ.

From a careful consideration of the weekly data we may therefore form a very fair estimate of the probable development of flies. We may, by means of these data, construct fly curves even without reference to the number of deaths from diarrhœa, and, though a rough correspondence is thus obtained, it is by no means so close as that actually observed even with our deficient observations. Our chief object being, however, to correct the curves, we may use all the data available for this purpose, remembering that in the latter part of the fly curve it is impossible to determine how much is young, active carrying flies and how much fungus infested and disabled flies.

We may now apply these considerations to successive years:—

1904.—Applying first the numerical test to the number of flies captured in houses at the apex of the curve, we find that in the thirty-second week there were more stations showing an increase over the thirty-first week than there were showing a decrease. It may be assumed, therefore, that the figures in the thirty-first and thirty-second weeks represent substantially the numbers of flies at this point. As we have seen, the greatest number of deaths is probably about the ninth or tenth day after the greatest number of flies. If now we add the number of deaths occurring in the weeks and fortnights following the maximum number of flies, we get figures which represent nearly this point of time, and the features of the fly curve at this point are reproduced. This is interesting as bearing on the number of flies found in the houses in the thirty-second week. We find throughout that heavy rain does not increase the number of flies in houses. Very often the reverse is the case. Flies, therefore, are not to any great extent driven into houses by rain. But heavy rain may keep a section of them indoors, those, namely, which have come in to feed, and which are not driven out by thirst—a very powerful factor in determining the movements of the house fly. Now, the weather was not very favourable to the production of flies in the thirty-second week. The atmospheric temperature fell considerably, and there was a fairly heavy rainfall. On the other hand, there had been an enormous number of eggs laid during each of the four preceding weeks, so that the impulse of the fly to escape from the pupa was only partially checked. Notwithstanding the unfavourable conditions, a large proportion of the flies captured would still be freshly generated flies which had escaped direct from horse manure into the houses. If we accept the figures, also, the movement of these flies from one house to another was only partially restricted by rain. It would seem from the data given in these tables that an atmospheric mean temperature of 59° F. and upwards is not unfavourable to the production of flies, although the higher the temperature the more favourable it is. This temperature, however, is too low for growing larvæ, which, however, at the beginning of the season receive the artificial heat of large fermenting heaps of horse manure. A great increase in the atmospheric and surface temperatures occurs in the twenty-eighth week and onwards for four weeks, and a sudden increase in flies occurs in the same week. It will be evident that a considerable amount of laying of eggs in favourable spots has occurred prior to this period, especially in the twenty-fifth and twenty-sixth weeks. It is not, however, till the twenty-eighth week that the temperatures are very favourable for the laying of eggs. An enormous deposit of eggs takes place, and a fortnight afterwards occurs the great uprush of flies. In spite, however, of the large number of eggs laid in the thirtieth and thirty-first weeks, there is a great decrease of flies in the thirty-third week under the unfavourable conditions then prevailing. Hence a low surface temperature is able to arrest the production of an enormous number of larvæ. With the advent of high atmospheric and surface temperatures in the thirty-fifth week, we get a second increase of flies and a check in the diminution of fatal diarrhœa cases. This appears to show that as a result of the low temperatures of the air and surface in the two weeks preceding, the development of larvæ had been delayed, but was able to attain completion under more favourable conditions.

1905.—In this year the conditions are favourable to laying of eggs in the twenty-fifth, twenty-sixth, twenty-seventh, twenty-eighth, and twenty-ninth weeks; we therefore expect, if the conditions are favourable, a marked increase of flies in the twenty-eighth, twenty-ninth, thirtieth, thirty-first, and thirty-second weeks. There is, however, a diminution in the thirty-second week corresponding to less favourable atmospheric conditions; nor is improvement manifest in the thirty-third week, despite favourable atmospheric and surface conditions. This is due, no doubt, to the high rainfall causing chilling of the smaller heaps of material; the curves pursue a normal course.

1906.—The conditions for laying of eggs became favourable in the twenty-fourth and twenty-fifth weeks. We expect a considerable increase of flies in the twenty-seventh and twenty-eighth weeks, which occurs notwithstanding unfavourable air and surface temperatures in the earlier periods. The rainfall is, however, not unfavourable. The conditions for laying eggs are favourable in the twenty-seventh week, and we expect a considerable increase of flies in the thirtieth week, the conditions being favourable, which they are. The subsequent course of the flies is such as we should expect.

It seems unnecessary to follow the course of flies, and therefore of deaths, into further years.

We may sum up the results of this analysis as follows: Summer diarrhœa is an infectious illness. This is shown by the course of the annual wave, by the manner of its incidence on the different sanitary districts of Manchester, and by the history of individual cases. The health of infants prior to attack—in other words, the social condition—has much to do with the fatality. The summer wave is not due to dust, nor is it conditioned by any growth of bacteria in or on the soil. There is nothing to support the view that the infective organisms are of animal origin, and the connexion between privy-middens and diarrhœa goes far to prove the contrary. The disease becomes more fatal only after house flies have been prevalent for some time, and its fatality rises as their numbers increase and falls as they fall. The correspondence of diarrhœal fatality is closer with the number of flies in circulation than with any other fact. The next closest connexion is with the readings of the 4-ft. thermometer, with which, however, diarrhœal fatality can have no direct relation. Flies and the readings of the 4-ft. thermometer are both functions of air and surface temperatures and of rainfall.

Certain facts in the life-history of the fly throw light on discrepancies arising in the decline of flies and cases. The close correspondence between flies and cases of fatal diarrhœa receives a general support from the diarrhœa history of sanitary subdivisions of the Manchester district. The few facts available for the study of the correspondence of flies and fatal cases in different subdivisions, in the course of the same year, also lend support to this view. No other explanation even approximately fits the case.

Enteric Fever and Flies.

At this point it will be convenient to give the main table, on which the facts are based. Table A gives for each year since 1891 the number of deaths from diarrhœa in each of the fifty-two weeks of the year, missing out the odd week in leap year. It also gives the cases of enteric fever reported to the Public Health Office, arranged not according to the number of cases notified in each week, but according to the number of cases commencing in that week. The dates of commencement are founded on the reports of the District Sanitary Inspectors, and are therefore to some extent imperfect, except when special skill is employed. The results show, however, that these dates are not far out, and it is to be remembered that the reports are annotated and revised by skilled examiners, while the Inspectors also have been instructed on the principles according to which they are to fix the dates of commencement. These data both for diarrhœa and enteric fever are added up in corresponding weeks of successive years to give a composite series in two periods, 1891 to 1897, and 1898 to 1908. This is done because the notifications, so far as enteric fever is concerned, are checked in the later period by the serum test, and are much more reliable in consequence. But this division has the incidental advantage that it permits comparison of two different periods. The figures for individual years are illustrated by charts. In these the number of deaths from diarrhœa has been divided by five for convenience of representation (*see* pp. 192 to 199).

TABLE A.—DIARRHŒA DEATHS (IN WEEKS).

Year	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26
1891	4	2	7	4	4	2	4	3	3	3	8	1	—	2	3	12	6	4	5	5	1	4	2	5	4	10
1892	2	3	2	2	—	1	3	3	3	5	4	2	2	3	7	1	1	2	3	2	2	1	2	2	2	9
1893	—	2	2	4	9	1	1	4	7	4	3	7	4	2	6	4	2	3	4	7	5	4	5	11	19	58
1894	5	3	5	5	3	1	3	5	1	3	8	—	5	7	8	3	5	3	3	1	6	4	3	7	1	5
1895	2	7	4	4	3	5	3	4	5	4	4	1	5	2	5	1	5	1	7	6	4	1	3	9	5	8
1896	4	3	1	4	6	5	1	1	5	—	2	3	3	3	8	6	3	—	3	4	5	5	7	8	10	16
Total	17	20	21	23	19	15	15	20	24	19	29	14	19	19	37	27	22	13	25	25	23	19	22	42	41	106

Year	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52
1891	7	9	5	13	8	15	19	18	17	33	29	21	23	17	14	14	8	7	4	3	3	4	2	2	1	4
1892	4	7	3	3	15	17	24	46	49	33	35	17	12	14	17	4	4	3	6	3	4	8	3	2	2	4
1893	72	83	92	60	38	31	39	44	51	39	37	30	25	16	22	8	3	9	6	7	3	1	2	—	5	3
1894	9	12	15	16	20	28	31	18	16	14	9	6	7	5	7	9	9	8	3	2	2	3	3	1	7	1
1895	5	9	21	24	29	31	46	62	60	68	74	60	59	55	48	30	28	17	12	5	8	5	3	4	3	3
1896	18	11	42	58	61	47	46	19	27	21	11	8	10	11	2	7	6	7	2	6	5	1	3	8	3	2
Total	115	131	178	174	171	169	205	207	220	208	195	142	136	118	110	72	58	51	33	26	25	22	16	17	21	17

DEATHS FROM DIARRHŒA.

Year	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28
1897	1	7	6	2	7	2	2	3	5	5	1	3	5	7	1	3	1	4	3	4	5	3	5	4	4	5	8	7
1898	5	4	1	3	3	6	6	2	5	3	7	3	6	2	2	3	3	4	2	2	4	6	2	7	6	6	3	9
1899	4	3	2	5	3	2	6	2	8	2	4	2	4	4	1	2	4	1	3	4	5	4	4	9	4	8	5	9
1900	3	3	3	4	3	4	—	7	3	3	3	1	3	4	8	3	4	6	5	7	5	4	1	7	6	7	6	7
1901	2	4	4	6	1	6	1	5	7	3	2	4	—	5	4	3	3	3	3	1	2	3	3	1	3	—	3	12
1902	7	3	2	2	3	2	—	3	1	2	3	1	3	3	3	—	—	3	1	4	5	1	4	5	3	1	1	1
1903	3	4	2	5	8	4	5	2	3	5	4	2	1	1	1	—	2	7	3	4	4	6	2	4	5	9	6	13
1904	5	1	2	4	1	1	2	2	2	5	2	5	2	3	5	4	3	5	3	1	1	3	—	1	4	5	5	—
1905	2	5	3	3	—	—	—	1	1	2	2	1	3	1	1	3	—	6	1	1	1	—	4	5	4	4	7	7
1906	1	2	2	2	3	3	2	5	3	2	3	—	4	3	2	4	2	5	3	3	1	2	5	3	1	3	4	6
Totals	33	36	27	36	32	30	24	32	38	32	31	22	31	33	28	25	22	44	27	31	33	32	30	46	40	48	48	71
	132				118				123				117				124				141				207			

Year	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52
1897	15	33	71	150	167	116	73	65	46	28	18	5	8	10	4	4	4	6	5	4	2	2	1	6
1898	5	20	36	54	46	46	87	111	108	111	121	110	50	40	26	14	14	7	6	6	5	4	3	4
1899	22	42	75	103	107	136	128	129	81	79	32	16	9	4	11	5	2	6	4	4	3	5	4	1
1900	11	13	37	63	50	50	83	78	64	36	58	41	26	19	20	15	4	6	5	5	2	3	4	3
1901	36	79	122	119	107	110	86	68	61	35	26	21	12	7	8	5	4	2	2	5	3	—	2	2
1902	3	4	3	8	2	2	10	14	19	40	15	20	17	14	8	6	14	5	6	4	3	4	1	8
1903	14	14	23	29	39	42	31	21	24	20	26	20	22	11	7	9	6	6	2	4	10	4	1	4
1904	14	48	77	91	107	85	68	43	36	28	24	23	11	3	3	4	1	—	3	3	2	4	3	3
1905	22	62	81	89	67	81	61	61	42	20	15	10	9	5	4	6	3	3	2	3	5	4	1	5
1906	6	12	17	34	80	102	104	135	136	86	58	35	28	11	13	16	9	5	2	2	4	3	1	3
Totals	148	327	542	741	772	770	731	725	617	483	393	301	192	124	104	84	61	46	37	40	39	33	21	39
	1758				2998				1794				504				184				132			

TABLE A (*continued*).—ENTERIC FEVER COMMENCING IN EACH WEEK OF THE YEAR, FOR THE YEARS 1891 TO 1896.

Year	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26
1891	11	10	12	18	9	18	15	8	15	10	9	14	16	15	13	15	16	12	7	8	6	8	8	6	4	1
1892	13	17	15	13	14	12	8	13	14	13	8	10	14	9	3	6	6	17	9	14	6	11	2	6	5	12
1893	12	10	10	13	8	5	6	9	6	9	6	9	5	10	5	8	1	5	2	4	5	3	9	10	11	7
1894	12	6	11	15	7	13	5	11	9	8	3	9	9	12	6	5	10	9	2	4	3	5	2	4	7	6
1895	8	7	12	7	15	11	9	9	10	4	5	9	4	2	4	6	9	8	6	7	10	13	8	6	6	5
1896	11	8	9	17	10	9	8	9	8	12	9	6	2	8	13	17	10	18	7	10	10	4	9	9	11	4
Total	67	58	69	83	63	68	49	59	62	56	40	57	50	56	44	57	52	69	33	47	40	44	38	41	44	35

Year	27	28	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52	
1891	4	7	8	5	8	12	14	25	22	20	18	19	18	32	23	28	17	22	25	26	35	15	20	18	15	13	
1892	4	6	10	12	9	8	9	11	20	13	19	13	22	17	7	11	15	27	19	14	16	23	9	11	10	5	
1893	13	10	10	10	15	14	14	20	19	28	25	27	15	19	16	30	27	18	20	20	14	14	9	9	9	5	
1894	7	6	4	6	11	13	17	14	17	9	6	11	14	9	12	10	10	11	12	11	15	10	10	5	6	11	
1895	3	6	3	4	8	6	8	9	6	8	12	8	19	17	16	12	17	25	21	19	20	9	10	15	6	6	
1896	5	10	7	5	7	8	8	11	6	14	12	4	11	11	18	18	17	13	10	6	9	10	15	6	8	9	7
Total	36	45	42	42	58	61	70	90	90	92	92	82	99	105	92	109	103	116	107	96	109	81	73	64	54	49	7

CASES OF ENTERIC FEVER COMMENCING IN EACH WEEK OF THE YEAR, FOR THE YEARS 1897 TO 1906.

Year	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28
1897	10	10	7	5	10	5	8	7	11	6	8	6	4	5	10	3	2	3	5	3	9	1	4	3	3	6	4	3
1898	13	12	19	14	12	12	9	15	12	9	8	2	6	9	10	14	10	11	4	2	1	3	6	4	1	4	1	7
1899	12	14	16	12	5	9	6	5	3	5	6	3	5	7	6	7	10	3	1	6	4	6	3	4	4	5	2	—
1900	11	8	7	10	2	7	5	7	4	5	3	10	6	12	11	7	8	8	4	3	3	2	7	5	6	2	—	—
1901	4	3	3	3	3	5	4	—	4	2	2	4	6	2	8	2	6	2	4	1	2	1	5	5	3	2	1	2
1902	3	3	3	6	5	7	6	7	2	2	5	7	7	8	9	11	7	2	4	4	4	3	8	4	10	8	7	3
1903	8	3	4	5	8	4	11	7	5	6	9	6	14	10	4	4	3	1	6	7	14	7	3	12	5	6	5	6
1904	3	5	9	11	1	3	5	3	6	8	4	4	7	7	3	10	10	6	5	6	2	4	3	3	7	2	3	3
1905	9	11	7	12	12	3	5	5	9	10	12	9	7	7	11	5	6	8	6	4	4	1	1	3	3	2	1	1
1906	7	14	15	14	7	4	2	6	2	3	4	2	1	3	1	3	8	3	—	3	3	3	2	4	5	3	9	4
Totals	80	83	90	92	65	59	61	62	58	56	61	53	63	70	73	66	70	47	39	39	46	31	42	48	47	40	33	29
	345				247				228				272				195				167				149			

Year	29	30	31	32	33	34	35	36	37	38	39	40	41	42	43	44	45	46	47	48	49	50	51	52
1897	6	5	3	8	7	17	20	12	13	19	20	16	17	11	12	13	18	12	20	21	20	17	23	12
1898	4	4	6	7	8	7	23	15	13	18	21	28	39	31	30	21	10	20	17	20	20	18	14	18
1899	2	5	5	5	15	11	12	10	12	10	15	12	6	9	7	10	8	9	10	9	10	4	7	9
1900	3	6	7	9	9	8	13	10	12	9	17	14	19	11	10	10	5	12	7	8	9	3	1	3
1901	3	7	13	15	13	12	13	22	14	20	14	17	27	11	15	9	9	9	6	7	3	6	2	3
1902	4	9	10	6	5	8	10	7	5	17	20	11	9	12	12	10	3	10	5	14	7	14	5	10
1903	6	6	6	6	6	6	5	4	6	11	14	12	13	9	5	16	15	9	8	9	11	9	7	5
1904	6	6	9	8	7	9	6	10	13	7	18	7	7	4	7	9	5	7	6	6	5	7	12	1
1905	—	7	2	4	9	8	8	6	7	11	7	13	6	12	9	10	4	10	4	6	6	8	6	7
1906	5	3	7	2	6	4	4	13	20	19	14	13	22	16	23	16	7	14	9	8	8	2	6	8
Totals	39	58	68	70	85	90	114	109	115	141	160	143	165	126	130	124	84	112	92	108	99	88	83	76
	235				398				559				645				396				346			

Before entering on a consideration of these data it is necessary to sum up the results of previous work in connexion with enteric fever. This disease has been treated of in the Manchester Annual Reports from 1896 onwards, but the first extensive analysis was made in the years 1904 and 1905, and these, with the subsequent reports, will be found to contain data which will probably be useful to those desirous of tracing the course of an outbreak in an urban community. In 1883 an account of an outbreak in Newton Heath, Manchester, will be found in the *Edinburgh Medical Journal* for May and July,[1] in which special stress is laid on the extent of infection between individuals in contact, and on the considerable proportion of very slight attacks which might easily have been overlooked. In a paper in the *Manchester Medical Chronicle* of 1887, entitled "Remarks on Fever,"[2] in explanation of the comparative severity of incidence observed on new-comers to a district in which typhoid fever is present, it is said: "It is true that the numbers attacked are not a complete proof that the new-comers suffer more severely than the residents, since we know that many cases of typhoid fever, especially in children, escape altogether." The observations made in 1904, 1905, and 1906 confirm these remarks. As in previous and subsequent reports, the histories of what one may call "contact" infection are given very fully. In 1904 between one-fourth and one-fifth of the cases are traced to such infection. In 1905 over one-third are linked together, and in this year the cases thus connected are, by the skill of the inquirers, grouped in nests. In 1906 the cases are not published in detail, but it is stated that 36·8 per cent. are traced to contact with previous cases. In 1905 and 1906 a free interpretation of the word "traced" must be allowed, although the facts are fully displayed in the annual report for the former year. Thus, with sufficient energy and skill, the sources of enteric fever may be much more fully ascertained than they usually are. That we have been able to do so is due chiefly to the acumen of a particular Inspector, Mr. Hewitt. Needless to say, the alleged cases brought to light in the course of the inquiries in 1905 are mostly confirmed by the serum test.

In the annual report for 1904, from an analysis of the periods in the disease at which cases were notified by practitioners, I was able to assert that a large number of cases must escape attention altogether. These inquiries fully prove this directly, as is seen from the following figures for 1905:—

	Aged under 5	Aged 5—9	Aged 10—14	Aged 15 +	Total
Enteric fever, all cases	27	29	38	217	311
Cases discovered by Mr. Hewitt ...	11	5	6	13	35
Cases discovered by other inspectors	8	4	3	16	25

[1] *Edin. Med. Journ.*, 1883-4, xxix, p. 121.

[2] *Med. Chron.*, Manchester, 1887, vii, pp. 1, 199.

Thus, about one-sixth of all cases known to us had been overlooked. These overlooked attacks are, it will be seen, specially liable to occur in children, and, of these, in young children. A number of them are infants apparently suffering from diarrhœa. In the 1904 report it is recorded that of twenty-six overlooked cases causing infection, six were believed to be suffering from diarrhœa. It is evident, then, that there is a considerable amount of cloaking of enteric fever by diarrhœa in the diarrhœa season. Further, these overlooked cases have, as we might expect, a special influence in causing infection. In 1904 there were twenty-six overlooked cases giving rise to subsequent attacks, of which nine caused thirty subsequent. In 1905 there were thirty-six ascertained overlooked attacks, giving rise to subsequent attacks. The whole subject is reviewed in the Manchester Annual Report for 1907, to which I would refer.

In the above statement nothing has been said about the manner in which disease is conveyed from one person to another. This is usually assumed to be through contamination of food, although it is often difficult to see how this could have occurred, unless there are still cases which we have overlooked—cases, in fact, so slight that they elude clinical inquiry altogether. There can be practically no doubt that there are many such cases. Amongst other facts it may be mentioned that at various times we have subjected to the serum test all members of families, and in some instances alike those who have been markedly ill, those who have had slight illnesses, and occasionally those who had apparently not been ill at all gave well-marked Widal reactions. Even in the absence of such cases, however, it does not necessarily follow that infection should be conveyed by handling food. In the season of house flies these may act as carriers, or some other living agents of transmission may be at work. If these are capable of carrying infection from one house to another, they are a priori capable of transmitting it within the house. We are thus led to inquire as to the proportion of "contact" cases occurring in different seasons. This point is investigated for the years 1905 and 1906, in which we find the following proportions :—

1905.

	First quarter	Second quarter	Third quarter	Fourth quarter	Total
Cases traced with precision to direct contact	38	13	10	11	72
Cases associated with the previous consumption of mussels ...	26	20	20	31	97
Untraced	32	22	37	55	146
Total	96	55	67	97	[1]315

[1] Of the 315, 4 are either not enteric fever or are doubtful.

1906.

	First quarter	Second quarter	Third quarter	Fourth quarter	Total
Cases traced to direct contact	38	7	17	64	126
In connexion with shellfish	37	15	17	37	106
Untraced	15	7	35	55	112
Total	90	29	69	156	344

In both years the proportion of cases traced to *direct contact*, which for the most part means occurring in the same household or from visiting an infected house, is low in the third quarter, a circumstance which may be variously interpreted.

The next most important factor in the propagation of enteric fever, at any one time, is the eating of shellfish, and by shellfish in Manchester is meant mussels. Enormous quantities are consumed in the open season, from September 1 to April 30, the largest quantity being used at the commencement of the season in September and October, and up to the close of the Christmas holiday. A fair quantity is also used in the close season, from April 30 to September 1, at which time they are brought from Scotland and Ireland. It is, however, certain layings off our own coast which are more especially associated with the occurrence of enteric fever. They are brought from Liverpool to the wholesale dealers, or large retailers, in bags. By these they are consigned to retail shops, or purchased by hawkers, who, no doubt, buy the poorer qualities. The large quantities sold by hawkers we have no means of tracing. When the mussels, suspected in connexion with a case, have been bought from a retail shop, we can often ascertain the date on which they were purchased, and the wholesale man. From him we get the laying. In this way we have been able by accumulation of facts from year to year to build up a case in respect of the layings from which infected mussels come. Occasionally more than one person contracts enteric fever from eating of the same mussels at the same time. In this and other ways we have reached the conviction that mussels are responsible for a good deal of enteric fever. It does not follow, of course, that because a man who has eaten mussels begins with enteric fever, a week or a fortnight or three weeks afterwards, the mussels have been the source of his attack. It is probable, however, that a considerable number of such connexions are linked as cause and effect. Of the cases arising in connexion with shellfish in 1904, we find that notwithstanding the large number traced to direct contact, scarcely any of those found to be associated with mussels belonged to the groups which had been exposed to direct contact. This is also true of 1905.

Inasmuch as mussels are mostly eaten by adults and adolescents, it is to be expected that the distribution of cases associated with their consumption would be different from the distribution of those traced to contact. The following table from the annual report for 1904 shows the mode of distribution according to sex, age, and employment :—

Occupations	All cases	Traced definitely or probably to contact with a previous case	Having consumed shellfish raw or cooked before attack
Workers in cloth, clothing, &c.	43	5	6
Nurse, laundresses, charwomen	7	1	0
Provision dealers, &c.	18	0	9
Ironworkers and their labourers	23	1	9
Other labourers	21	2	5
Other occupied males	36	3	7
Housewives	45	14	7
Children under 14	94	36	3

The sex and age of cases associated with the consumption of shellfish may be seen also from the following figures for the years 1904 and 1905 :—

	Aged 0—14	Aged 15—24	Aged 25—34	Aged 35—44	Aged 45—64	Aged 65 +	Total
1905.							
Males	5	24	28	10	1	0	68
Females	6	7	13	3	2	0	31
1904.							
Males	4	8	16	7	1	0	36
Females	1	4	2	2	2	0	11

In 1906 there were 106 cases associated with consumption of shellfish; males and females under 15 numbered 10, males aged 15 and upwards 73, females aged 15 and upwards 23. In 1907 there were 67 cases, in 1908 88 cases, 71 of whom were in males and 17 in females. It thus appears that the occurrence of cases due to shellfish is a factor in determining the excess of incidence on males. An analysis of the seasonal incidence of enteric fever in the Manchester Annual Report for 1907 leads to the same conclusion. Yet the excess still remains when the shellfish cases are taken away. The consumption of infected shellfish is not, therefore, the sole cause of the excess of incidence on males.

The above figures, of which those for 1905 and 1906 represent the most complete investigations, give an estimate of the cases which may be due to shellfish. If we assume that the drop in the nineteenth week which lasts during the summer represents the withdrawal of the shellfish influence, we may form some estimate of its amount. Now, the drop on the average of ten years, 1897 to 1906, about the nineteenth week, will be hereafter seen to be 3·3 cases per week. If we may assume the excess to last seventeen weeks, we get 56 cases due to mussels after the new year. The number prior to that date will be somewhere about $\frac{3}{2} \times 56$, or 84. The total average number due to shellfish, among notified cases, would thus be 140. The average number proved to be connected directly with consumption of shellfish in 1904-1908 is

$$\frac{47 + 99 + 106 + 67 + 88}{5} = \frac{407}{5} = 81$$

The difference of 60 could be considered as expressing the cases indirectly due to shellfish.

It is probable that a considerable number of persons eat mussels when they have had too much liquor. These are purchased from hawkers, eaten and forgotten; contaminated mussels, therefore, probably do more harm directly than we can trace. The contaminated batches come at irregular intervals, and the cases arising from them will be liable to rise and fall in an irregular manner. The quantity consumed is, however, much greater in the period before Christmas than from that date to the end of the season. A study of the circumstances will show that the maximum infection from this cause will be prolonged at least into October and November, depending as it does on infection from the riverside towns above the layings. Not a few infections have been traced to a primary mussel-infected case, and infected shellfish thus exert in Manchester a marked influence in sustaining the continuance of enteric fever.

In considering the modes of propagation of enteric fever there are other points of view which we must think of. Attacks in males preponderate considerably over attacks in females. I have given the following figures for 1904. From the census returns we get the numbers of persons living in Manchester at the census of 1901:—

Males aged 14 and upwards (these include unoccupied males) ...	181,172
Occupied females, aged 14 and upwards	86,978
Unoccupied females, aged 14 and upwards	110,878
Children under 14 years of age	164,844

ENTERIC CASES OCCURRING IN MANCHESTER IN 1904.

Workers (male) aged over 14 (these include cases in unoccupied males)	141
Workers (female) aged over 14	47
Women employed in housework, &c.	54
Children up to age of 14	77

The greatest incidence is on males at ages above 14, then on female workers, then on unoccupied females, least on children. The usual explanation is that many cases of enteric are ambulant, and work increases the opportunities of infection; then, of course, workers have money to spend, and can eat raw mussels and other unwholesome dainties as they choose. The inquiry into the influence of specific occupations reveals nothing of importance. As we have seen, there is reason to believe that the incidence on younger ages is quite understated by our public records, if uncorrected. As regards social station, it was shown in the annual report for 1904 that enteric fever is a disease of the artizan and poorer classes. Malnutrition appears decidedly to predispose to it.

The water supply as it arrives in Manchester has been shown by Professor Delépine, and in my annual report for 1904, not to be responsible for the spread of enteric fever. The milk supply must also be exonerated so far as our records of individual cases go. Watercress of the cheaper sorts may produce some effect in March and April, at which season there is a slight swell in the enteric wave. Fruit may be contaminated by flies, as may other articles of consumption, and convey the disease in the fly season.

Further light may be got from tables or spot charts showing the occurrence of cases for each month or week in each of the eighteen sanitary districts of the city. These spot charts will be found in the annual reports for 1904, 1905, and 1906; the corresponding facts will be found for 1900 to 1903 in the annual report for 1904, and may for these years be exhibited as spot charts.

From a study of these facts and charts in the annual report for 1904 the following conclusions were drawn:—

(1) Strictly local rises are apt to occur at all periods of the year.

(2) They generally extend over two or three months, but may be limited to one, in which case infection in one family may be the cause.

(3) Such local rises occur in the first six months of the year 1900 in five districts, in 1901 in one district, in 1902 in seven districts, in 1903 in six districts, and in 1904 in four districts.

The above statement made in 1904 contains much of what there is to be said with reference to the autumnal rise of enteric fever, which is nevertheless greater, as a rule, than these remarks would indicate. In subsequent reports greater inclination exists towards recognition of the action of the house fly as the cause of the autumnal rise than is there shown. The facts for individual years vary greatly. In some years the increase in cases accompanying and following the diarrhœal wave is very striking. The autumnal rise may be divided into two parts—viz., that which precedes and that which follows the thirty-eighth week. In

Manchester it is not possible to separate in this and subsequent weeks the influence of mussels from other factors, and the curve now represents a blend of influences. It is, however, occasionally possible to see the part of the curve subsequent to the primary enteric rise separated off from that which is influenced by mussels. And it then behaves much as if there were no mussels. Now the primary part of the autumnal rise exhibits in most years a very striking phenomenon. In some one week the cases of enteric suddenly increase, and this increase is maintained with subsequent further but very irregular ascents and descents. The tendency of the primary rise is to have a flat top for some weeks, a tendency which comes out very strongly on adding the facts for different years. This tendency to flattening is characteristic of slight effects, the period of occurrence of which cannot be definitely fixed. It is very manifest in the diarrhœa curves in 1909, and in the diarrhœa cases commencing day by day in years of severe incidence. It is dependent on want of precision in dates of onset, in latent periods, and so forth. Nevertheless, though not great, the increase usually occurs in a decisive and striking manner. This first part of the enteric autumnal rise is that which we have to explain, though we cannot overlook the subsequent part. In the valuable contribution made by Dr. Sandilands to the question of the infectiveness of diarrhœa, he adduced evidence to show that in towns in which middens had been converted into water-closets a much greater and a more characteristic fall had occurred in the incidence of enteric fever than was manifest in other towns which retained their midden-privies. On the face of it, this would seem to be due to conversion of these closets into water-closets. Certainly, however, a great change may occur, and has occurred in Manchester without such conversion. In 1891 the death-rate from enteric fever was 0·37 per 1,000 persons living. In 1899 it underwent a sudden fall to 0·12 per 1,000, and fell further with oscillations, till in 1907 it was 0·06 per 1,000. In 1908 it was 0·12 per 1,000. Now, the drop in 1899 was not due to conversions, which are now proceeding rapidly, but were not in progress at that period. It is true, special pails began to be supplied in 1894 to houses in which enteric fever occurred, and a marked fall was witnessed in that year. Further, processes of disinfection of middens and pail sites became more active after that year. Nevertheless, this would not meet the difficulty of overlooked cases. It is also to be remembered that the development of our towns for many years has meant a relative increase of water-closets. At the same time there can be no doubt that conservancy systems, and especially privy-middens, have a decided influence on the continuance of enteric fever. From figures which I prepared for the years 1891 to 1898 it appeared that the proportion of cases occurring in connexion with middens to those occurring in connexion with pails increases in the aggregate of years, though not in every year, in the months of August, September, and October, the season of flies.

At this point we may consider why flies should be thought of in connexion with midden-privies. They are, of course, well known to visit these places and to breed there. But, in addition, as we have seen, enteric fever is more severe on males than on females, and on female workers than on housewives, and (according to the figures) than on children. But the housewife or one of the children empties the ashes, cleans the closet, and performs all those duties which bring members of the household into direct relation with the privy. It seems likely, therefore, that the influence exerted *by* the midden is not exerted *at* the midden, but consists in something which is transferred to the house and affects all the members of the family alike. Such an influence would be the transmission to food of infection from the midden by the house fly or by some other living pest. This follows, unless we regard the influence of home life in determining sex incidence as being an altogether subordinate one, a view which the ascertained facts would seem to contradict. We must, however, take into account also the greater tendency to treat females and children at home, while the breadwinner, when disabled, is to a much greater extent notified and removed to hospital. This tendency is very manifest in the case of phthisis.

Study of the Incidence of Enteric Fever in St. George's Sanitary District in 1904.

A map is given in the above year of the occurrence of cases in this district and in Harpurhey, showing four distinct nests of enteric fever in St. George's and one in Harpurhey. St. George's is served by pail-closets, Harpurhey partly by middens. Many of the cases in St. George's are linked together by histories of contact. The occurrence of these nests in St. George's in place and time excludes house flies and shellfish as main factors. The nests are probably determined in some way by local infections of food, and are probably of the nature of contact infection. To some extent also this is true for Harpurhey. In 1906, however, a fresh and more intense outburst occurred in Harpurhey and Moston further along the Rochdale road, which was in the fly season, and in which midden-privies probably played an important part. This study serves to emphasize the influence of contact infections, not always clear in their character, and shows that our investigations still leave obscurities to be cleared up. These local nests of enteric fever are to be distinguished from the infective nests in the annual report for 1905, in which the cases were often some distance apart.

We may sum up these observations as follows: There are two proved influences operating to produce the enteric curve in Manchester—so-called direct contact and mussels. The influence of both is powerful, that of the second problematical only in amount. It is, however, limited in time, and hence, on the addition of the facts for different years, this influence should be clearly manifested. Owing to the commencement of the close season for mussels on April 30, there should be a fall observed in the added figures on the nineteenth week, a fall maintained during the rest of the year up to about the thirty-eighth week, in which and in the following weeks we may suppose the effect of the mussel season to be first and most markedly manifested. If flies cause the transmission of enteric fever, we may expect that the fall occurring at the close of the mussel season will remain until the fly season has begun. Owing, however, to the comparatively small amount of. enteric infection to which flies have access we may expect that the enteric rise will be later than the diarrhœal. Further, as enteric fever has so greatly declined, we should anticipate that a much larger number of flies would now be required to affect the enteric curve than was the case in 1891. Hence the enteric rise will be shifted back, as we go backward in years, relative to the diarrhœa curve. It will also have a much increased tendency to disappear in years of low diarrhœal fatality, and therefore of low numbers of flies. If this should be found to happen irrespective of conditions of temperature or other states of weather, we shall have strong reason for believing that flies do cause the primary rise of enteric fever in autumn. Then again, as we have seen, there is good reason to believe that many cases of enteric fever are regarded as diarrhœa when the latter disease becomes prevalent. This is more particularly true of years of high diarrhœal death-rates, although, owing to the comparatively low amount of enteric fever present at all times, the amount of this error will not increase in proportion to the severity of diarrhœa.

We may therefore look for two increases in enteric fever following the fly season, a primary increase following close on the increase in diarrhœal fatality, and a subsequent increase due to contact infection from overlooked attacks of enteric caused by flies and recorded as diarrhœa—an increase not necessarily small, in the light of the histories of "direct infection" from overlooked cases which have been accumulated. There is a third source of increase at the later period. The turnover of diarrhœal infection, if one may use the expression, is rapid as the curve of flies ascends. That of enteric fever is much slower. There will, however, in general be enough flies left, when the primary rise has occurred and when the overlooked cases come into play, to produce a secondary direct fly increase. These two secondary fly increases will in general come into collision with the mussel increase and we must disentangle them as best we can.

If now we refer to the figures showing the number of cases of enteric fever added week by week for the years 1897 to 1906, we find that a drop occurs in the nineteenth week, and this fall is sustained up to the thirtieth week, when a slight rise is manifest. The numbers reported week by week then advance irregularly, marked increase occurring in the thirty-third week. A second marked increase occurs in the thirty-fifth week, and thereafter the curve remains level. A third jump upwards, and the greatest, occurs in the thirty-eighth week, and is sustained for three weeks. A marked fall occurs in the forty-second week, the curve remaining at about the same level to the forty-fifth week, after which it declines irregularly up to March. A small rise occurs from the thirteenth to the seventeenth week, after which the numbers fall to their summer level. Now it appears certain that the summer fall in Manchester is conditioned by the withdrawal of contaminated mussels from the market, mainly, though not entirely. The abrupt fall in the beginning of May must certainly be ascribed to this cause. Similarly the rise in the thirty-eighth, thirty-ninth, and fortieth and forty-first weeks is largely due to the commencement of the mussel season. Is the rise from the thirtieth to the thirty-seventh week to be ascribed to flies? Referring to the figures for diarrhœa, we see that in the added series flies must have been at a maximum from the thirtieth to the thirty-fifth week. We may surmise, then, that much of this part of the curve is due to direct infection by flies. By the end of this period, however, the number of cases of enteric fever notified weekly has increased threefold, while the number of flies has by no means diminished proportionately. Not only so, but there are also numerous overlooked centres of infection. It may be, then, that the influence of flies alone would produce a rise in this part of the curve, apart from mussels altogether. This question we are in a position to put to the proof. If this be so, taking into account the tendency of enteric fever to spread by direct contact in nests, having regard also to the probability of overlooked cases increasing *pari passu* with cases due to flies, and taking into account the continuance of flies to a late part of the season, we might suppose that we had explained the enteric curve in Manchester.

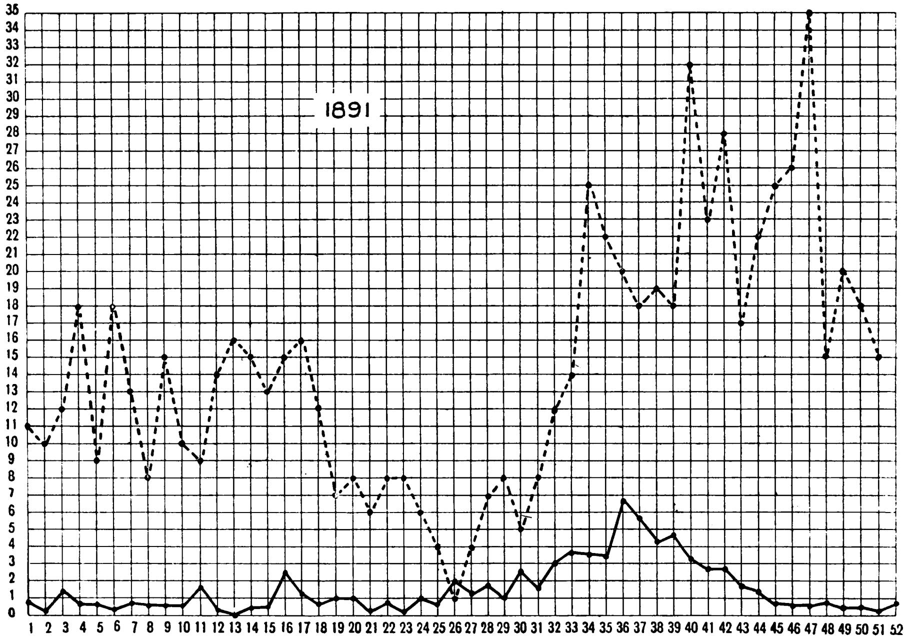
We are in a position to eliminate any influence due to shellfish in the years 1904, 1905, and 1907. We get for these years the following numbers in weeks of cases of enteric commencing, of cases associated with consumption of shellfish, of the numbers found by subtracting these, and for the last half of the year of deaths from diarrhœa :—

Weeks	Number of cases of enteric commencing	Number associated with shellfish	Difference	Diarrhœa deaths
1	33	14	19	—
2	25	12	13	—
3	26	4	22	—
4	33	6	27	—
5	27	4	23	—
6	17	3	14	—
7	27	5	22	—
8	17	7	10	—
9	27	9	18	—
10	32	2	30	—
11	27	10	17	—
12	23	4	19	—
13	34	5	29	—
14	24	3	21	—
15	18	4	14	—
16	19	6	13	—
17	19	7	12	—
18	15	4	11	—
19	17	5	12	—
20	17	2	15	—
21	20	3	17	—
22	12	4	8	—
23	7	2	5	—
24	19	4	15	—
25	15	1	14	—
26	10	—	10	—
27	10	1	9	24
28	10	1	9	26
29	13	1	12	59
30	20	1	19	144
31	18	0	18	203
32	20	2	18	259
33	20	2	18	296
34	33	3	30	281
35	23	2	21	217
36	30	1	29	154
37	34	8	26	128
38	36	6	30	89
39	47	15	32	86
40	45	9	36	73
41	36	6	30	54
42	33	9	24	37
43	28	6	22	32
44	41	10	31	36
45	37	4	33	16
46	35	10	25	14
47	37	8	29	8
48	53	9	44	13
49	53	9	44	19
50	43	12	31	16
51	42	12	30	9
52	24	8	16	12

We thus see that though the rise in the thirty-seventh, thirty-eighth, thirty-ninth, fortieth, and forty-first weeks is much reduced by subtracting the shellfish cases, it is not removed. We must therefore be prepared to consider it an integral part of the enteric curve, apart from shellfish, and as probably associated with flies in the manner indicated. From the forty-fourth week onwards we must regard the influence of flies as at an end, and we expect a decline of incidence. We do not get this, and we are thus inclined to doubt the adequacy of flies to explain the later parts of the curve. In the same way we find that the curve is much levelled about the nineteenth week by removing the influence of shellfish. But some difference remains. The impression remains that direct infection of the kind exhibited in the annual reports on the health of Manchester for 1904 and 1905, taken along with the conveyance of infection by flies and the reinforcement of the disease by contaminated mussels, almost explains the whole phenomena of enteric fever.

We must now consider the individual curves of enteric. The composite curve which we have just considered brings into prominence and magnifies the influence of mussels on the course of enteric fever, but blurs the action of flies which occurs at different periods in different years. We thus get some measure of the amount by which mussels affect the course of the disease. The number of cases in the eighteenth to the twenty-first week of the ten years' series averages 43, in the four weeks preceding 70. There is thus a drop of 33, averaging 33 per week on the ten years. A similar rise occurs in the thirty-eighth, thirty-ninth, fortieth, and forty-first weeks, which, however, is now due in part to flies. We may, however, obtain an idea of the magnitude of this contribution to the curve from the figures already given. It is clearly considerable. It comes in rushes. Like the result of fly infection, each access is no doubt accompanied by overlooked cases. Contact or direct infection also produces nests limited in time. Both influences tend to produce irregularity in the curve. The influence of fly infection will not be marked by such ups and downs, and will proceed more smoothly, as a rule. Hence that part of the curve which is most marked by irregular, abrupt ascents and descents is most likely to be due to mussels or direct infection. If the ascents occupy three weeks or upwards we may usually suspect direct infection or mussels; if one week only, and very steep, we may suspect mussels. This is, however, a very imperfect first approximation.

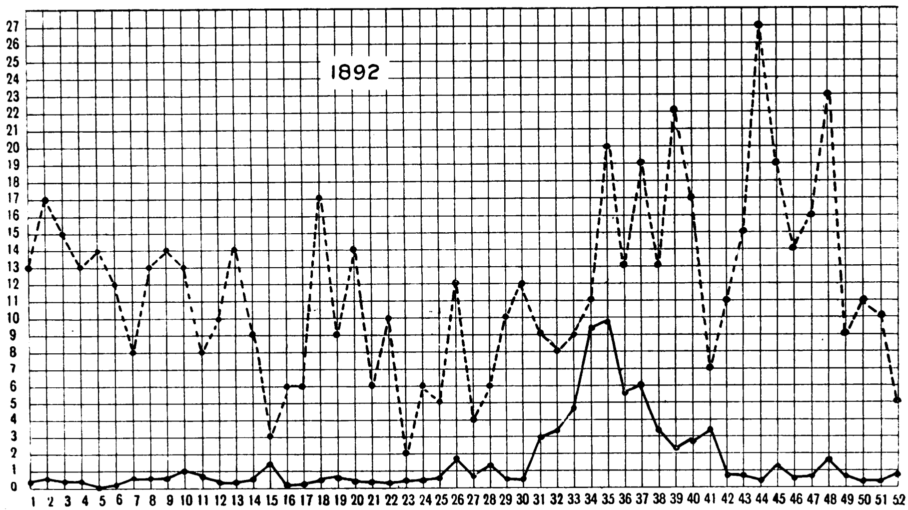
We may next examine the figures and charts for separate years—1891 to 1908. The following observations may be made on the series: Enteric fever cases commencing in weeks are shown as a black dot on the



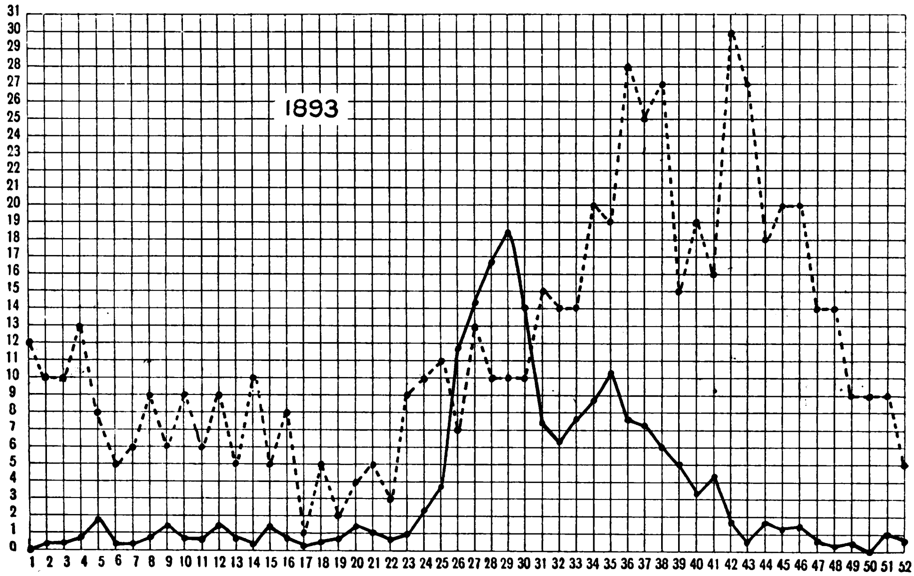
In this and the following seventeen charts

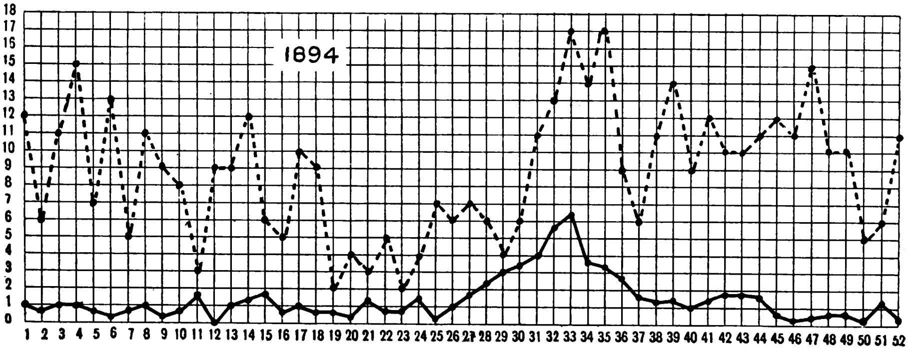
——— indicates diarrhœa deaths, divided by five

. . . . . . ,, enteric cases commencing

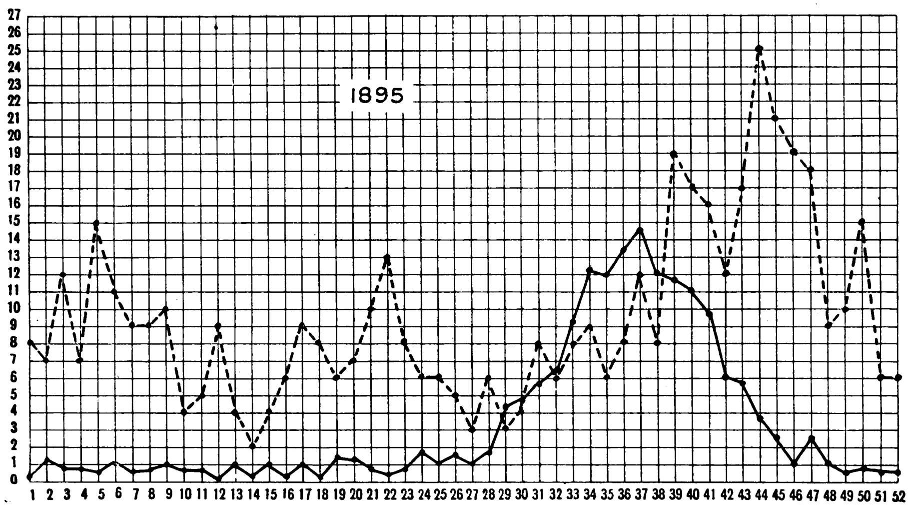


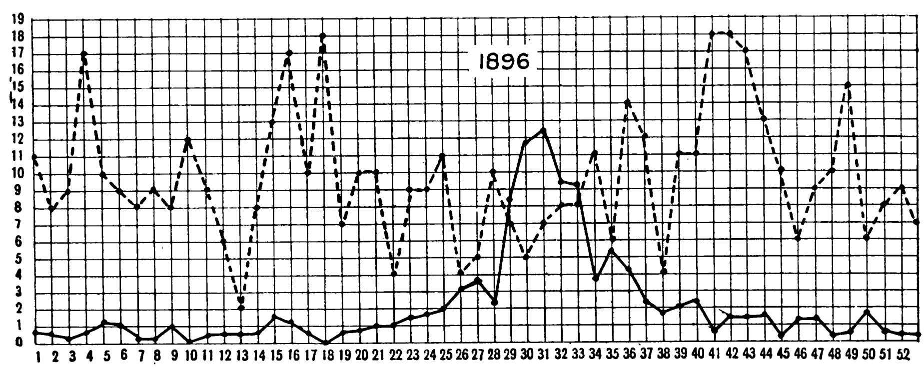
line marking the corresponding week of the year. Deaths from diarrhœa are divided by five, in order to render the curves more manageable, and the corresponding numbers are entered as black dots. This has the effect of flattening the diarrhœa curve, and to some extent obscuring its great



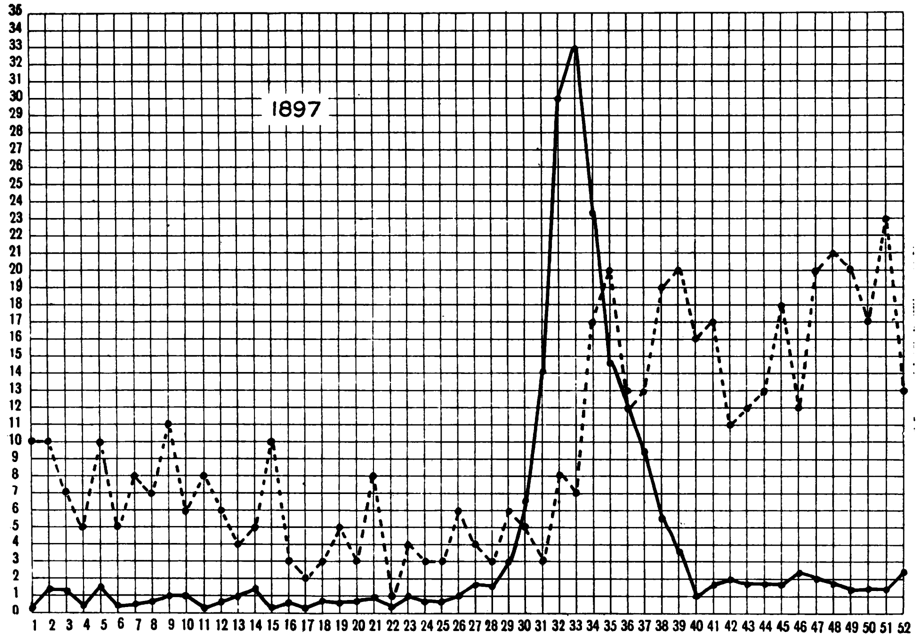


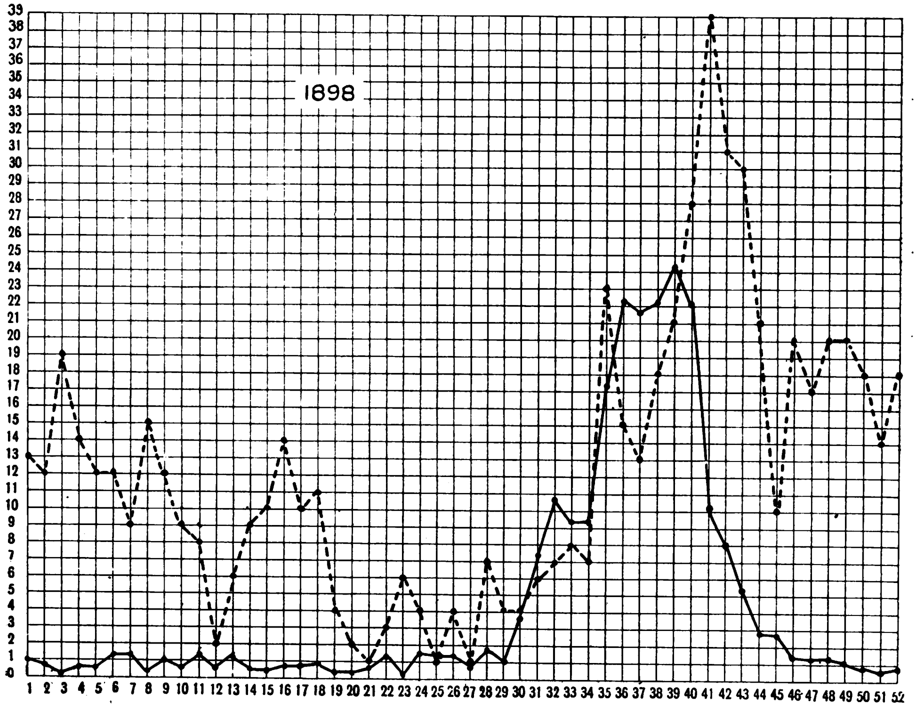
height. The imagination must therefore be here brought into play. The curve of diarrhœa cases will be something like a hundred times as steep as that here shown. In general, we perceive that a high diarrhœa curve is accompanied by a high enteric curve, and an extended diarrhœa wave by an extended enteric wave. The enteric rises are much more considerable, relative to the diarrhœal, in the earlier than they are in the later years. They also begin earlier, relative to the diarrhœal curve.



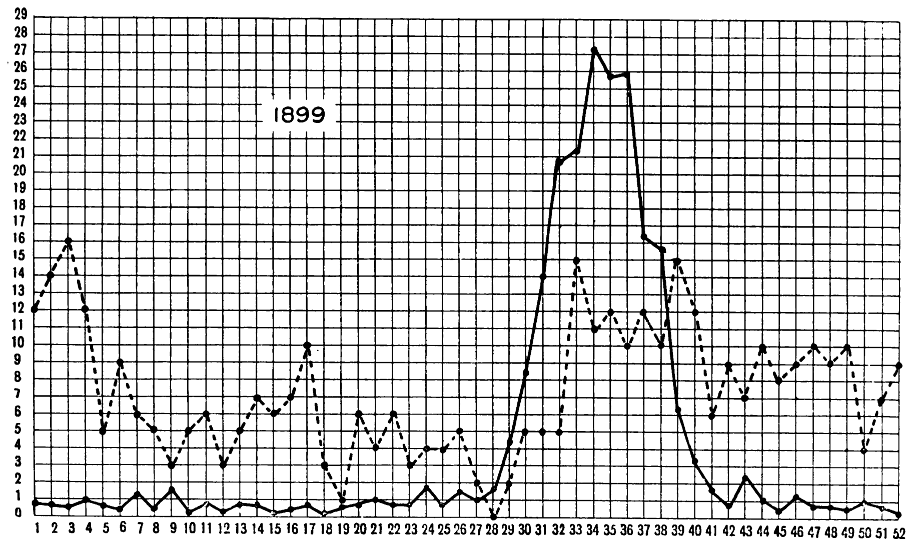


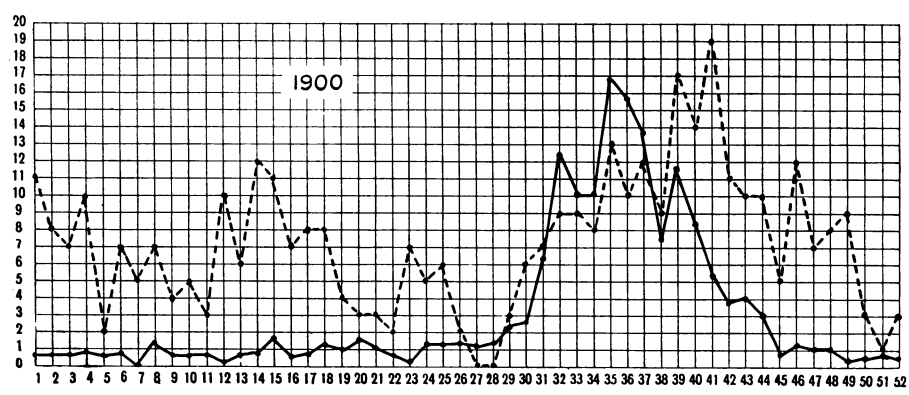
This cannot be connected with any condition of soil or temperature, the more so that the earlier years were years of low mean temperature in the third quarter, but may be explained by the greater prevalence of enteric fever in the earlier years, when a much smaller number of flies


1897

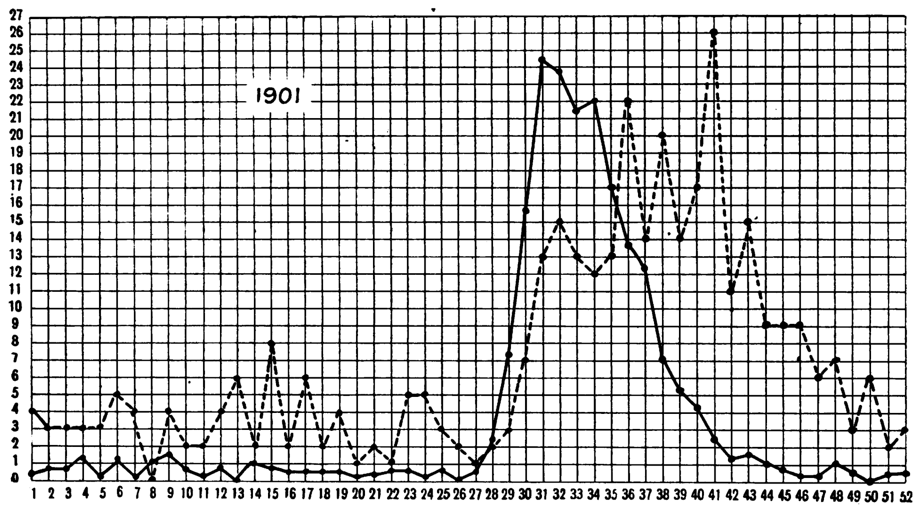

1898

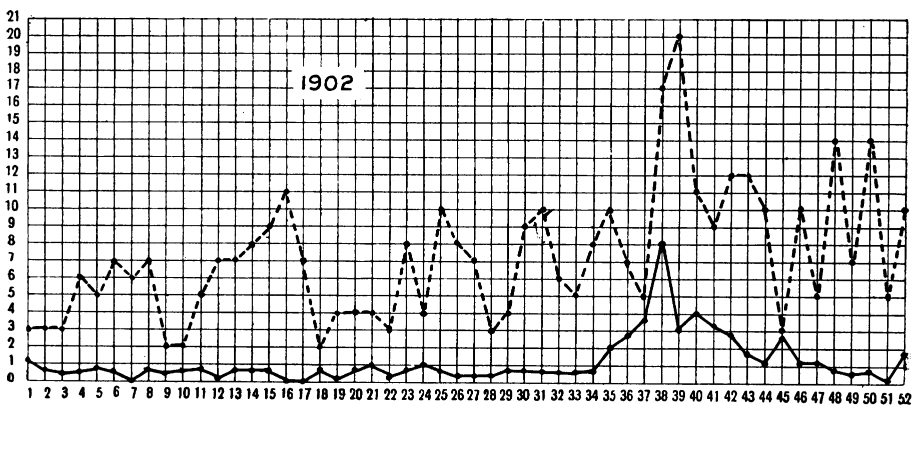
was able to raise the curve. The critical rise of enteric fever in a particular week is much less conspicuous in the curve than in the figures, and, it will be seen, is often attended with a preliminary but much smaller ascent. The autumnal enteric rise is often divided into a portion

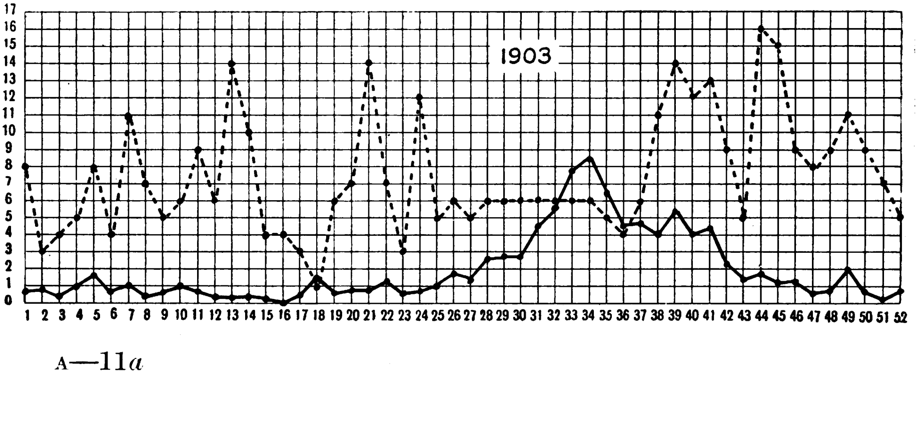


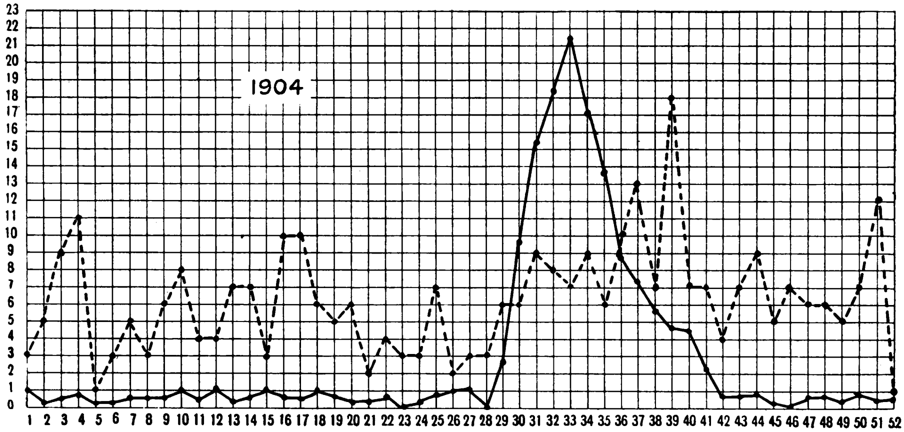


corresponding to the diarrhœal wave—that is, to the wave of flies—about ten days before the wave of deaths from diarrhœa; a second wave, which we have seen to be partly due to flies, operating on infection from overlooked cases, and on the now increased enteric infection; and


1901


1902


1903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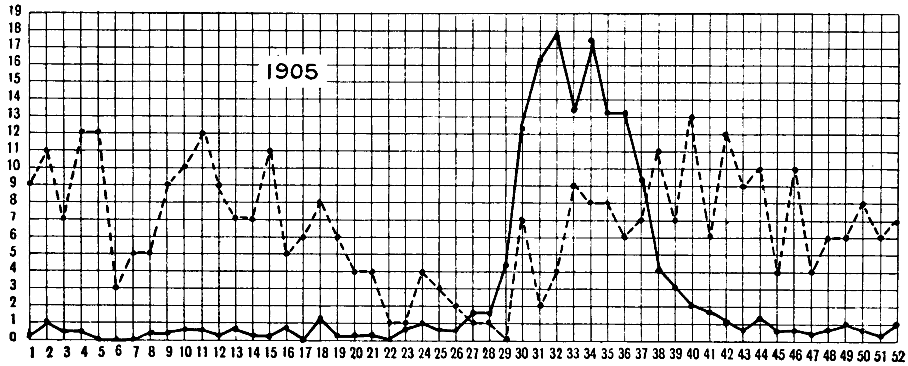
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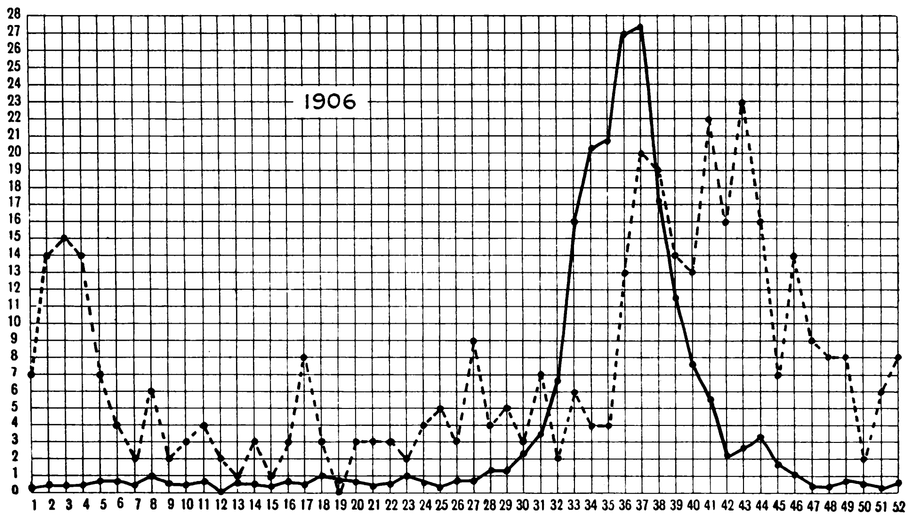
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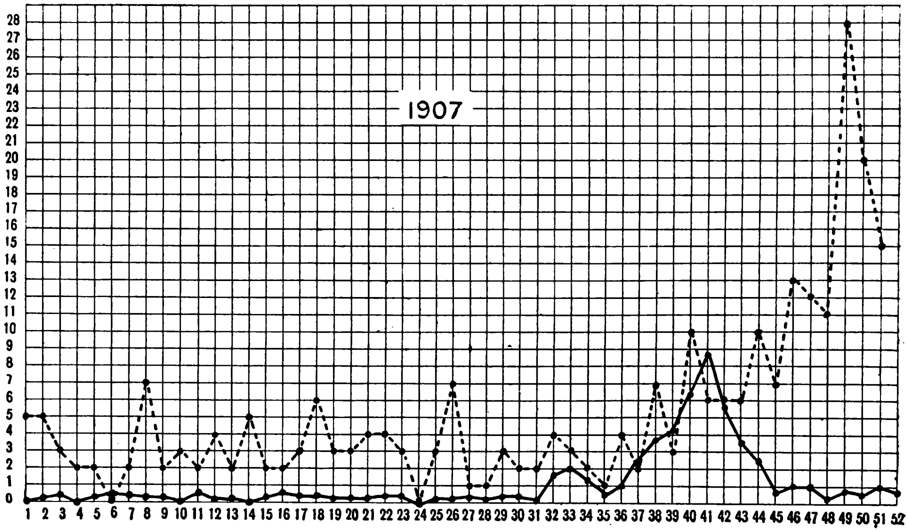
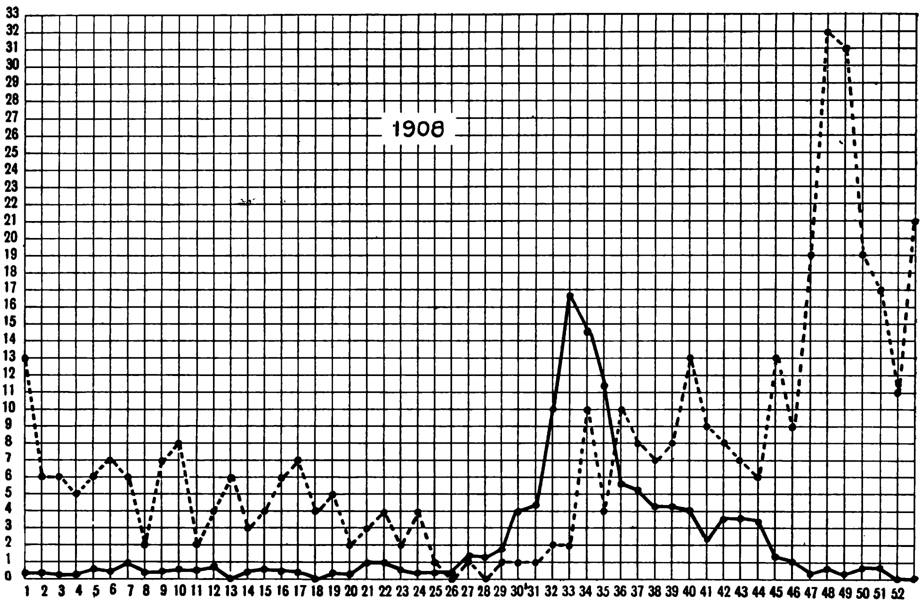
even a third wave when the diarrhœa curve is much prolonged. These secondary and tertiary waves are in part produced by shellfish, especially towards the end of September and through October. We may exhibit the facts of separate years in the table on pp. 200 to 204.





FACTS RELATING TO THE CURVES OF ENTERIC FEVER.

Year	Maximum weeks of flies as determined from the meteorological data and from the course of diarrhœa deaths	Weeks of maxima of diarrhœa deaths, &c.	The so-called critical rise in enteric cases commencing	Weeks of ascent of the enteric wave	Features of the year
1891	Maximal weeks, 34 and 35; minor maxima, 31 and 32	Maxima, 36 and 37; minor maxima, 33 and 34; diarrhœa curve ascends 32 to 36, extends from 32 to 42; diarrhœa incidence low	34	Curve ascends, 32, 33, 34; primary wave, 32 to 39; secondary, 39 to 43; tertiary, 43 to 48; apex of primary wave precedes apex of diarrhœa curve by a fortnight	Primary wave due chiefly to direct influence of flies; secondary partly due to flies directly, partly to infection from overlooked cases, partly to mussels; tertiary wave mainly due to infection from overlooked attacks; mussels in evidence, weeks 1 to 10; nest or nests of infection, 11 to 19
1892	Maxima, 33 and 34	Maxima, 34 and 35; curve extends 31 to 40; diarrhœa incidence not severe	35	35; primary wave 35 to 41, secondary 41 to 49; 1	Preliminary slight rise in the enteric curve 29 to 34 probably due to flies; the primary wave partly due to infection from overlooked cases; the secondary wave more so; from weeks 1 to 11 the curve indicates much direct infection; from 34 to 41 mussels are in evidence
1893	27 (and 28?) secondary maxima 33 and 34	29, and minor maxima in 35; the curve of deaths extends from the 24th to the 42nd week; incidence severe	None distinct; the curve rises by a series of steps	The curve begins to rise in the 23rd week and ascends to the 31st week; there is a secondary increase in flies, and a fresh wave takes its start on the top of the previous one, 33 to 39; the tertiary wave extends from the 41st to the 44th week	The primary and secondary waves are merged 31 to 39; a tertiary wave is added 41 to 44, due in part to infection from overlooked cases; an extended entire enteric wave results corresponding to an extended entire diarrhœa wave; enteric wave large
1894	30 and 31	33; wave extends from 26th to 40th and from 40th to 44th weeks; incidence low	31	Primary curve 30 to 37; secondary 37 to 50	The primary curve of enteric corresponds to the main diarrhœa wave; the secondary wave is not high, and is probably due mainly to direct infection from previous cases, overlooked and otherwise; total incidence moderate

1895	32, 33, 34, 35, 36	37; flat, broad-shouldered top; *considerable incidence*	37 and 39	The enteric curve ascends in steps; the primary wave from 31 to 42; the secondary from 42 to 43	The long primary wave passes at its highest point into the mussel rise; up to the 38th week it corresponds well with the diarrhœa curve; the second step of the primary wave takes both direct infection by flies and indirect infection from overlooked cases; the secondary wave is largely due to the latter cause, but also to flies: from 4 to 9 and from 14 to 26 we have extension of local infection; considerable curve
1896	First maxima 28, 29, 30, 31; small rise 33	30 and 31; diarrhœa wave, 23 to 40; main portion, 28 to 34; incidence moderate to low	No critical rise	Primary wave slight, 31 to 38; secondary wave considerable and broad, 38 to 46; tertiary small wave 46 to 50	The secondary wave is large only by comparison with the slight primary wave; mussel infection appears to be indicated in the 4th, 16th, 18th, and 49th weeks
1897	31 and 32	33 (32); diarrhœa severe, sharp peak	34; primary enteric curve peaked; secondary curve prolonged and sustained	Primary wave, 32 to 36; this enteric wave bears a striking resemblance to the diarrhœa curve of deaths except in its termination; secondary wave 36 to 42; interval, tertiary wave 46 to 52	It looks as if the secondary wave terminates the influence of the fly period; if so, we may have to look for something special to explain the tertiary wave; the earlier features of the curve suggest mussel infection, but slight sustained high enteric
1898	Two maxima 34 and 35, 37 and 38	Prolonged maximum, 36 to 40; incidence very severe	35	Primary wave rises at first by steps, 31 to 45; this wave has two peaks, 35 and 41, corresponding to the two fly maxima; there is a correspondence generally with the diarrhœa curve; secondary wave, 45 to 52	The effects of a secondary rise of flies, coupled with infection from cases overlooked in the first part of the curve, result in a large outburst; the secondary wave is due to infection from overlooked cases? Incidence of enteric severe
1899	32, 33, 34, 35	34 to 36; the diarrhœa curve ascends steeply, has a flat top, and descends steeply, 29 to 41; incidence severe	33	33; the primary wave lasts from 32 to 41; it has a flat top from 33 to 39; the secondary wave is smaller than the primary	The secondary wave of 1898 continues up to the fifth week of this year. A minor wave 12 to 18 is due probably to some extension of direct infection, possibly to some extent to watercress. Incidence of enteric not severe in comparison of diarrhœa, due to failure of the secondary wave

Year	Maximum weeks of flies as determined from the meteorological data and from the course of diarrhœa deaths	Weeks of maxima of diarrhœa deaths, &c.	The so-called critical rise in enteric cases commencing	Weeks of ascent of the enteric wave	Features of the year
1900	Smaller maxima in 30, 31, a larger maxima 33, 34; the second maxima show no relation to the four-foot temperature	32 and 35, 36; incidence moderate; curve of diarrhœa, 29 to 44	35 ?	31 to 33; the primary wave lasts from 30 to 45, corresponding to the wide diarrhœa curve; it contains three successive ascents corresponding to the two ascents of the diarrhœa curve and the superposed secondary infections. The secondary wave is small	There is a general correspondence between the primary enteric wave and the diarrhœa curve. The successive reinforcements of infection have time to come into play during the fly season. The secondary wave of 1899 lasts to the fifth week of this year; rises of the curve occur weeks 11 to 18, 22 to 25, due probably to extensions of contact infection, possibly to some extent to watercress; incidence moderate
1901	29, 30, 31, 32	31, 32, 33, 34: steep ascent and descent, rounded peak; *incidence severe;* curve 29 to 42	31	Ascends 30, 31, 32; primary wave blended with secondary to make one wave; 29 to 46	The primary wave shows a peak corresponding to that of the fly curve, then a superposed portion due to the descending fly curve, which rises above the first part of the wave; its character is observed by the zigzagging of the lines, indicating influence of mussels. Incidence of enteric fairly severe
1902	36	38; death-rate slight; extent of curve, 34 to 48	38	38, 39; primary wave, 37 to 41; secondary, 41 to 44; the later part of the curve zigzags, possibly due to influence of mussels	The enteric primary wave is high in proportion to diarrhœa, but closely agrees with it in time and character; the secondary wave is largely due probably to secondary infection from overlooked cases; the earlier portions on the curve suggest several extensions of local or direct infection, 3 to 9, 10 to 18, 24 to 27, 29 to 32
1903	31, 32; smaller second maximum, 38	33, 34; death-rate small; extent of curve, 28 to 42	38	37, 38, 39; primary wave absent; second wave, 37 to 43; secondary wave, 43 to 46	The enteric wave corresponding to the main diarrhœa wave is absent; there is, however, a primary wave corresponding partly to the secondary rise of diarrhœa, partly to the overlooked cases in the first part; there is also a secondary wave corresponding to this portion of the enteric curve; the behaviour of the earlier part of the curve suggests shellfish; these, however, do not, materially affect the wave, though they lower the rise in the 13th week

1904	Observed maximum, 31	33 (32 to 34); curve peaked; curve, 29 to 41; incidence of diarrhœa severe	31 ?	31; the primary wave coincides very nearly with the diarrhœa wave. There is, as usual, a secondary rise, corresponding to the descent of the diarrhœa curve; no secondary wave follows this portion; curve extends	There is, perhaps, no real distinction between what are now being called part of the primary wave and what in the earlier years were called secondary waves. They appeared, however, in the earlier years more completely separated off. The later part of the primary wave in 1904 and the secondary wave in 1891 are alike connected with the declining part of the fly curve, and indicate infection from an increased number of centres, as well as from overlooked cases arising at the height of the diarrhœa season; enteric incidence small; the peaks in the 37th and 39th weeks are due to mussels
1905	Observed maximum, 31	32 and 34; the diarrhœa wave extends from 27 to 41, and has a rounded top; *incidence severe*	33	33; the primary wave extends from 33 to 41; a secondary wave, from 41 to 44; both are small; the earlier part of the curve shows a rise 9 to 16, due no doubt to extension by direct infection	The second part of the primary wave corresponding to the declining curve of flies zigzags, and suggests mussels; the secondary curve suggests secondary direct infection; the peak in the 46th week suggests mussels; as a matter of fact the peaks in the 38th, 40th and 42nd weeks are due to mussels, while that in the 46th week is partly due to that cause of infection; the main features of the curve are not otherwise altered; *incidence of enteric slight*
1906	Observed maxima, 34, 35	35, 36; very severe	36	36, 37; primary wave, 35 to 40; secondary wave, 40 to 45; tertiary wave, 45 to 50; primary and secondary waves high; tertiary wave small	The primary wave closely resembles the diarrhœa wave; the secondary wave really corresponds to the second part of the primary wave in years of less acute rise; it marks possibly the action of a diminished number of flies on the increased number of enteric cases; there is, however, usually a gap which suggests difficulties here; the tertiary wave arises, no doubt, from this secondary wave; the zigzags in the secondary wave suggest mussels; incidence tolerably severe

Year	Maximum weeks of flies as determined from the meteorological data and from the course of diarrhœa deaths	Weeks of maxima of diarrhœa deaths, &c.	The so-called critical rise in enteric cases commencing	Weeks of ascent of the enteric wave	Features of the year
1907	Presumed maximum, 39	41; incidence not severe	Not clear; its place probably taken by a mussel increase	Slight primary wave, 39 to 46; no clearly marked secondary wave	Subsequent rise at the end of the year probably due to mussels
1908	Corrected maximum, 32; diarrhœa moderately severe; curve extends from 27 to 45	33 (34)	34	34; primary wave, 33 to 44; great secondary wave, 46 to 52	The primary part of the enteric rise which may be taken to terminate in the 38th week; zigzags; the secondary part, 38 to 44, is clearer; part of the secondary wave is due to mussels; the numbers traced to mussels in the last five weeks are 4, 6, 9, 12, 8; this does alter the character of the peak, but does not much lower its height; explanation (?): possibly it is due to an outburst of direct infection

The total effect of these curves is to produce a strong conviction that, allowing for differences in the disease, the curves of enteric fever represent the action of flies as plainly as do those of diarrhœa, although the result is comparatively slight. Supposing an uprush of enteric fever to occur in the primary wave, and supposing a considerable number of flies still in evidence, there is strong reason why a secondary wave should arise. The cases in the primary uprush will not become infective for two or three weeks.

Some time is lost, as a rule, in transmission, and the secondary wave will therefore not be developed until five or six weeks after the first. Further, a gap will usually arise between the primary and secondary wave, or perhaps one should say between the primary and secondary rises of the fly curve. This will be the more marked the more acute and the shorter in duration the primary curve has been. If we take the average period between infection and death in diarrhœa as twelve days, the enteric curve of cases should, in so far as it is due to the same part of the fly wave, either coincide with the diarrhœa curve of deaths or follow it at an interval of a week. If, now we regard the enteric fly-wave as consisting of a primary and secondary rise, the secondary part separated from the primary for the reasons given, we note that the entire enteric wave of cases commencing nearly coincides in point of time with the entire wave of diarrhœa deaths, with those slight but important modifications from 1891 to 1908 to which I have already alluded. In the earlier years this wave extends on both sides beyond the diarrhœa curve, while in the later it corresponds almost exactly. In 1896 the primary and secondary rises are, for some reason, both slight and indistinct. In 1903 the primary rise is doubtful, but the secondary is well marked. As we have seen, however, when studying the behaviour of enteric fever in districts, as shown on spot charts, the number of districts which take no part in the autumnal rise is very variable. The total phenomenon, though distinct, is small, and we must not therefore be surprised if occasionally it fails.

The difference in behaviour between the diarrhœa and enteric rises may be expressed in other words by repeating what has already been said, that the turnover of the diarrhœa infection as operated by flies is much more rapid than that of diarrhœal infection. Thus, if we take into account the tendency of enteric fever to occur in infective nests, the steady accession of mussel infection from the early weeks of September to the end of April, and the accession derived annually from flies, we might rest content that we have fairly explained the enteric curve.

The alteration in position and magnitude of the enteric autumnal rise between 1891 and 1908 is explained when we assume that a much smaller number of flies could produce the same result in earlier years, there being much more infective matter for them to visit, and no other explanation seems to fit. If this be true, then the connexion between flies and diarrhœa receives support from an unexpected quarter. But the support is even stronger than at first it seems, since with a sufficient number of flies, and easy access to near infectious stools, there would appear to be no doubt that flies do convey enteric fever. It follows that the close correspondence between flies and diarrhœa is almost certainly one of cause and effect.

It will be seen that the evidence from other sources connecting flies and enteric fever is an important part of this argument, and to this I will briefly recur. But I would first consider the curves a little longer, asking the question: Do the facts which I have collected tell the whole story? Is there nothing else, no residual phenomena of importance which are being neglected?

Before considering the evidence in favour of flies being the carriers of enteric fever we may ask whether the facts hitherto elicited suffice fully to explain the curve of enteric fever. There are various features of those curves which throw some doubt on this. Of these perhaps the most striking are the waves occurring late in 1907 and 1908. The most satisfactory way to settle this question would be to take those years in which individual cases have been most fully investigated and see whether there is a residuum of cases occurring in rises for which we have no explanation. When we do this for 1904 and 1905, accepting only in respect of direct infection cases in which the contact appears fairly sufficient, we obtain much reduced curves, which nevertheless show the same general features.

In 1904 the rise in the third and fourth weeks remains. The curve rises in the tenth week to the fourteenth, and again in the sixteenth to the twentieth. The ascent in the thirty-first week and the portion which we have ascribed to flies extending with a break to the forty-first week is clearly marked. Further small rises occur in the forty-third to the forty-seventh weeks, and again in the fiftieth and fifty-first weeks.

In 1905 the rise in the first to the fifth weeks remains as a rise from the second to the fourth. The swell in the curve from the seventh to the twenty-first week is, however, almost obliterated. The fly wave from the thirty-third to the thirty-seventh week remains. There is then, however, a dip of three weeks followed by a second rise from the fortieth to the forty-fourth weeks. The small rise from the forty-ninth to the fifty-second week is well maintained.

The facts for 1904, 1905, and 1907 may be presented thus. The last year I owe to Dr. J. R. Hutchinson, who has given much study to the question of carrier cases of enteric fever.

1904.

Cases commencing in weeks, with, first those connected with shellfish, then those who have contracted the disease after contact with previous cases, shellfish and contact cases being successively subtracted.

	Jan. 9	Jan. 16	Jan. 23	Jan. 30	Feb. 6	Feb. 13	Feb. 20	Feb. 27	Mar. 5	Mar. 12	Mar. 19	Mar. 26	April 2
All cases	3	5	9	11	1	3	5	3	6	8	4	4	7
Shellfish cases ...	0	3	1	3	1	0	1	0	2	1	1	1	2
Less shellfish cases ...	3	2	8	8	0	3	4	3	4	7	3	3	5
Contact cases ...	2	2	0	2	0	0	1	1	2	2	1	0	0
Less shellfish and contact cases	1	0	8	6	0	3	3	2	2	5	2	3	5

	April 9	April 16	April 23	April 30	May 7	May 14	May 21	May 28	June 4	June 11	June 18	June 25	July 2
All cases	7	3	10	10	6	5	6	2	4	3	3	7	2
Shellfish cases ...	1	0	1	1	0	0	0	0	1	1	0	0	0
Less shellfish cases ...	6	3	9	9	6	5	6	2	3	2	3	7	2
Contact cases ...	0	1	1	1	2	0	1	2	0	2	1	2	0
Less shellfish and contact cases	6	2	8	8	4	5	5	0	3	0	2	5	2

	July 9	July 16	July 23	July 30	Aug. 6	Aug. 13	Aug. 20	Aug. 27	Sept. 3	Sept. 10	Sept. 17	Sept. 24	Oct. 1
All cases	3	3	6	6	9	8	7	9	6	10	13	7	18
Shellfish cases ...	0	0	0	0	0	0	1	0	0	0	1	1	6
Less shellfish cases ...	3	3	6	6	9	8	6	9	6	10	12	6	12
Contact cases ...	0	0	3	2	0	3	0	1	3	2	5	1	1
Less shellfish and contact cases	3	3	3	4	9	5	6	8	3	8	7	5	11

	Oct. 8	Oct. 15	Oct. 22	Oct. 29	Nov. 5	Nov. 12	Nov. 19	Nov. 26	Dec. 3	Dec. 10	Dec. 17	Dec. 24	Dec. 31
All cases	7	7	4	7	9	5	7	6	6	5	7	12	1
Shellfish cases ...	2	0	0	2	3	0	1	4	1	1	2	0	0
Less shellfish cases ...	5	7	4	5	6	5	6	2	5	4	5	12	1
Contact cases ...	1	3	4	0	4	0	3	1	5	2	0	1	0
Less shellfish and contact cases	4	4	0	5	2	5	3	1	0	2	5	11	1

1905.

Cases of Enteric Fever notified in 1905, arranged according to the numbers commencing week by week; those traced with definiteness to contact with previous cases, and those associated with the previous consumption of shellfish, nearly always mussels, being similarly arranged, and then deducted. Doubtful cases are here not excluded.

	Jan. 7	Jan. 14	Jan. 21	Jan. 28	Feb. 4	Feb. 11	Feb. 18	Feb. 25	Mar. 4	Mar. 11	Mar. 18	Mar. 25	April 1
All cases commencing	8	6	5	8	12	4	2	4	12	7	13	10	5
Cases traced to contact	4	1	3	1	4	2	0	3	2	4	5	6	3
Associated with mussels	3	3	0	2	2	2	0	1	5	2	3	1	2
The last two deducted	1	2	2	5	6	0	2	0	5	1	5	3	0

	April 8	April 15	April 22	April 29	May 6	May 13	May 20	May 27	June 3	June 10	June 17	June 24	July 1
All cases commencing	10	8	4	6	6	6	3	3	1	1	2	3	2
Cases traced to contact	1	0	0	0	4	4	0	1	1	1	0	0	1
Associated with mussels	6	3	2	6	1	2	0	0	0	0	0	0	0
The last two deducted	3	5	2	0	1	0	3	2	0	0	2	3	1

	July 8	July 15	July 22	July 29	Aug. 5	Aug. 12	Aug. 19	Aug. 26	Sept. 2	Sept. 9	Sept. 16	Sept. 23	Sept. 30
All cases commencing	1	0	1	6	2	5	8	6	5	3	8	12	10
Cases traced to contact	0	0	0	1	0	0	0	1	1	2	0	1	4
Associated with mussels	1	0	1	1	0	1	1	2	1	1	4	4	3
The last two deducted	0	0	0	4	2	4	7	3	3	0	4	7	3

	Oct. 7	Oct. 14	Oct. 21	Oct. 28	Nov. 4	Nov. 11	Nov. 18	Nov. 25	Dec. 2	Dec. 9	Dec. 16	Dec. 23	Dec. 30
All cases commencing	10	6	14	7	11	7	5	3	9	5	8	6	6
Cases traced to contact	1	1	3	0	2	0	0	1	2	1	0	0	0
Associated with mussels	5	2	6	3	4	2	2	2	3	0	2	0	0
The last two deducted	4	3	5	4	5	5	3	0	4	4	6	6	6

1907.

Cases of Enteric Fever commencing in the week ending:—

	Jan. 5	Jan. 12	Jan. 19	Jan. 26	Feb. 3	Feb. 10	Feb. 17	Feb. 24	Mar. 2	Mar. 9	Mar. 16	Mar. 23	Mar. 30
All cases	3	4	5	2	1	3	0	4	3	2	4	4	3
Traced to contact infection	0	0	0	0	0	0	0	2	1	0	0	1	1
Connected with shellfish	1	2	1	0	0	0	0	0	0	0	1	1	0
Less contact and shell-fish cases	2	2	4	2	1	3	0	2	2	2	3	2	2

	Apr. 6	Apr. 13	Apr. 20	Apr. 27	May 4	May 11	May 18	May 25	June 1	June 8	June 15	June 22	June 29
All cases	2	5	2	2	4	5	2	3	3	5	3	1	4
Traced to contact infection	0	0	0	0	1	1	0	0	0	0	0	0	0
Connected with shellfish	0	0	0	2	1	2	1	1	1	2	2	1	2
Less contact and shell-fish cases	2	5	2	0	2	2	1	2	2	3	1	0	2

	July 6	July 13	July 20	July 27	Aug. 3	Aug. 10	Aug. 17	Aug. 24	Aug. 31	Sept. 7	Sept. 14	Sept. 21	Sept. 28
All cases	7	0	0	1	2	4	2	2	2	1	4	3	5
Traced to contact infection	3	0	0	0	1	1	2	1	0	0	0	0	2
Connected with shellfish	1	0	0	0	0	0	0	0	1	1	1	1	2
Less contact and shell-fish cases	3	0	0	1	1	3	0	1	1	0	3	2	1

	Oct. 5	Oct. 12	Oct. 19	Oct. 26	Nov. 2	Nov. 9	Nov. 16	Nov. 23	Nov. 30	Dec. 7	Dec. 14	Dec. 21	Dec. 28
All cases	4	10	6	6	8	12	8	13	15	16	16	19	16
Traced to contact infection	0	0	0	0	0	4	1	3	1	1	1	1	4
Connected with shellfish	1	6	3	2	3	6	4	3	6	7	7	5	5
Less contact and shell-fish cases	3	4	3	4	5	2	3	7	8	8	8	13	7

The effect of these repartitions of cases is somewhat to heighten the impression that there may be something unexplained. Now, this something might be persistent typhoid carriers, although this is unlikely. It is unlikely, I mean, that there should exist the amount of infection from this cause able to cause these disturbances. There is also the possibility that we have not exhausted the modes or means of conveying enteric fever. Not to speak of food infections from overlooked cases, infected closets from the same cause, carriage in infected clothing, there may be other living carriers besides flies. It is, however, difficult to see how these are to operate in winter except along adjoining houses. The living agents known to us are fleas, bugs, cockroaches, mice, and rats. Fleas and bugs we may put aside. Enteric is not found to any extent in their special haunts. Cockroaches deserve some attention. Like flies, they occur in immense numbers, and they are very often present in the poorer sort of houses, especially, I think, near stables. They are in Manchester widely distributed. That they visit beds I have positive knowledge. They often swarm about the kitchen at night, and investigate whatever food is left about. It is not difficult to ascertain whether they have visited food, on account of the smell which they leave. I have no proof, however, that they cause spread of enteric fever. The facts about them which I have been able to collect are these: They exist often in unsuspected numbers amongst cinders. A street was paved with cinders, and the covering of it with stone setts was accompanied by the migration of large numbers of cockroaches into the houses with simultaneous appearance of mice. It is probable that in warm, dry weather they are to be found in considerable numbers in middens. I have been informed by a gentleman who is interested in insects that they certainly do visit middens. They are known to visit closets. Cockroaches have been seen to move towards the house in considerable numbers at dusk, but my informant could not say at what period of the year this occurred. I was informed by a stableman that cockroaches were found in the manure stead in summer in large numbers, and occasionally in winter. This manure stead was at the end of a row of houses, in many of which I ascertained that they occurred in large numbers. I have visited two rows of houses in which cockroaches are numerous. In both, mice occur in considerable numbers. It is possible that mice feed on them. Rats are said to eat them. They are said to be most numerous in the houses in wet weather. They are caught in the "Demon" trap, or killed by poison. They breed slowly, so that they do not return for a considerable time after they have been thinned down; but the young ones come out. One householder informed me that a little whisky put in the cup of the "Demon" trap increased the numbers caught considerably. Flies also appear partial to alcohol. Notwithstanding that they are partial to stables, it is doubtful whether the animals observed in the manure stead were not the beetles which feed on the larvæ of flies, not cockroaches. I also ascertained that many blackbeetles were found in summer in another heap of house manure; but here it was even more doubtful whether they were cockroaches. We have seen that flies also probably live in hollow places in the walls in winter. It is said that the ordinary blatta has no antagonism to flies. The larger blatta, *Blatta gigantea*, is stated in Lardner's Encyclopædia to devour flies. It is possible that the ordinary cockroach is not so innocent in respect of flies as it is believed to be. Now, the rat virus is a species of coli bacillus, and it is just possible that they get this virus naturally from eating cockroaches. Whether there is exchange of any coli virus between these animals or not, it is at all events possible that if present in large numbers they aid in disseminating infection. Rats, of course, usually have access to sewers, and may thus carry infection on their feet. There appears to be a great antagonism between rats and mice. Probably rats kill mice, and eat them. There must be some plagues which destroy both cockroaches and mice, unless they die of sheer inanition. Do beetles sustain the *Empusa muscæ* from season to season? It seems to me that the occurrence and movements of cockroaches, their relations to flies, to each other, and to the occurrence of multiple cases, require to be worked out, even if it be to be put aside. In particular it is desirable to know their movements in summer and winter respectively. The movements of mice and rats appear particularly worthy of study.

This may, at first sight, seem a vain suggestion. Many of our histories of contact infection, however, are unsatisfactory in point of time or in precision as to the mode of conveyance. The occurrence of enteric fever in districts in nests, illustrated by the experience of St. George's Ward in 1905, is in point. The occurrence of these local nests is not confined to the fly season. It may be that the movements of some of our domestic pests will account for the establishment of these nests. They are, however, not likely to be due to cockroaches, which would be more likely to account for the spread of the disease in one family, and from that family to visiting acquaintances. Further, the local nests of enteric are most apt to occur in warm weather, when, of course, they may be due to the movements of flies. At all events, the matter appears worth investigating.

This discussion may be thus summed up: In Manchester, with sufficient energy and skill, the probable origin of most cases of enteric fever can be determined, in some years at all events, in terms of mussels and "contact" infection. "Contact" infection and "mussel" cases are proportionately least numerous prior to the thirty-eighth week of the year, before which period the enteric fever curve begins to ascend. This ascent, which cannot be due to either of the two causes for the continuance of enteric fever which have been named, may be due to transference of infection by flies. When the number of cases of enteric fever, commencing week by week, and the number of deaths from diarrhœa occurring in the same week are plotted out in curves, it will be seen that a rise in the enteric curve occurs corresponding to the summer wave of diarrhœa deaths. This is usually broken up into primary and secondary rises, the primary rise coinciding with the main curve of flies, but coming about a fortnight later. The secondary rise is often larger than the first, and is interpreted as being due to transference by a smaller number of flies of a much increased amount of infection. These waves were larger from 1891 to 1896 than they have been in recent years, notwithstanding that the number of flies was probably much smaller, and began earlier relative to the rise in diarrhœa deaths, although the corresponding temperatures were lower and less favourable to the development of enteric fever than they have since been. The effects of temperature on soil cannot explain this; on the other hand, the death-rate from enteric fever has fallen to less than one-third of its amount in the earlier period. The number of centres of infection of enteric fever was therefore formerly much larger than it now is, and a much smaller number of flies would thus be able to make an impression on the curve. When diarrhœa is high, the enteric primary rise tends to approximate to the curve of diarrhœa deaths, and becomes well marked. These results can only be expressed in terms of a common cause, the difference in the waves depending on the long latent period of enteric fever and its late infectiveness. Enteric fever, however, at all times has a slight incidence in Manchester compared with diarrhœa, and its rises are therefore less marked and definite; so slight, in fact, is the disease that in some weeks no cases are reported. Occasionally, though rarely, the rise corresponding to the diarrhœal ascent is ill-marked, or remains out. Especially has this tendency been manifested in recent years. The rise of the curve of enteric cases has, from starting before the rise of diarrhœa deaths, been gradually moved onwards to coincide with the apex of that rise. These facts are most easily interpreted, it seems to me, in terms of flies operating on the much smaller number of infective centres which now exist.

When all three proved reinforcements of the enteric curve are allowed for, however—contact infection, mussels, and flies—there remain residual increases the causes of which deserve special attention.

If the main autumnal rise of enteric fever be due to flies, it almost follows that the correspondence between flies and diarrhœa is one of cause and effect, a result which we have already reached by a process of exclusion.

External Evidence as to the Connexion of Flies with the Conveyance of Enteric Fever.

The evidence connecting flies with the dissemination of enteric fever in tropical countries appears practically conclusive. Undoubtedly the most important single piece of work on this subject is that of the American Army Commission on the prevalence of enteric fever among soldiers in 1898. In 1898 practically every regiment developed enteric fever, which became epidemic in the encampments both of the Northern and Southern States. "Most, probably all, of the regular regiments developed enteric fever less than eight weeks after going into camp."[1] The spread of the disease was, in the opinion of the Commission, due to camp pollution. The number of cases varied with the method of disposing of the excreta. The tub system and regulation pits were unsatisfactory. Infected water was not an important factor. On one

[1] *Lancet*, 1900, i, p. 1916.

occasion part of the camp used a possibly contaminated water, the other a pure water. Both were alike affected. Only about half the cases were correctly diagnosed. Flies undoubtedly served as carriers of infection. It is probable, also, that the disease was disseminated by dust, though this is regarded as a minor factor.

In his very useful summary of the epidemiological evidence, Mr. Gordon Hewitt[1] quotes Dr. Vaughan, a member of the Commission, as follows: "My reasons for believing that flies were active in the dissemination of typhoid fever may be stated as follows: (*a*) Flies swarmed over fæcal matter in the pits, and then visited and fed upon the food prepared for the soldiers in their mess tents. In some instances, where lime had recently been sprinkled over the contents of the pits, flies with their feet whitened with lime were seen walking over the food. (*b*) Officers whose mess tents were protected by screens suffered proportionately less from typhoid fever than did those whose tents were not so protected. (*c*) Typhoid fever gradually disappeared in the fall of 1898 with the approach of cold weather, and the consequent disabling of the fly."

American opinion seems to run strongly in this direction, and we may refer to the remarkable facts adduced by Jackson as to the connexion between the polluted foreshore at New York and the prevalence of enteric fever in the neighbourhood, carried, as he believes, by flies.

Now the typhoid bacillus is not killed by cold, and, if it is merely a question of infection, our own experience shows that its incidence declines slowly in cold weather. But, of course, the seasonal impression made by flies in this country is comparatively small. On the other hand, we have plenty of dust at other periods of the year, notably in March and June, during which months, however, diarrhœa remains motionless, while June and July coincide with the nadir of enteric fever. It is not until after the commencement of the fly season that either diarrhœa or enteric fever begins to move.

In his Milroy lectures Dr. Waldo developed the thesis that dust, and in particular the dust derived from horse droppings, is responsible for the summer outbreak of diarrhœa. This idea of dust conveyance seems to be an obsession, and suitable rather for the facts of South Africa than for our conditions. It is again mentioned chiefly to draw attention to the very acute letter by Mr. W. Salisbury Sharpe in the *Lancet* of June 2, 1900, in which he develops the idea that flies are the real

[1] Gordon Hewitt: "The Structure, Development and Bionomics of the House Fly," *Quart. Journ. Micro. Sci.*, 1909, liv, p. 347.

carriers. There is so much matter in it that I should like to quote it in full. I must content myself with saying that it contains suggestions which even yet will be found valuable in the investigation of the disease. It may be noted, however, that he points out that diarrhœa prevails most in houses least accessible to the movement of dust, in which the windows of the living-room are kept closed and in which flies gather in large numbers. As regards Dr. Waldo's suggestion that horse manure may contain the specific infective matter of diarrhœa, this is disposed of in the same manner by the seasonal occurrence of diarrhœa which coincides with that of flies. There is, so far as I can ascertain, no seasonal affection of horses preceding the seasonal human diarrhœa to lend colour to the suggestion. There is, however, an interesting connexion between horses and enteric fever quoted by Mr. Gordon Hewitt from a communication by Aldridge to the Army Medical Department Report, 1902, p. 207: "In the British Army in India, 1902-5, the ratios per 1,000 per annum of cases of enteric fever admitted into hospital were: Cavalry 41·1, and infantry 15·5; in the United States Army, cavalry 5·74, and infantry 4·75." The connexion is, no doubt, between horses and the number of flies.

Dr. Snell, of Coventry,[1] in 1906 showed that 70 per cent. of the cases of infantile diarrhœa occurred in the north-east part of his district, close to a large collection of refuse where flies swarmed. There is testimony to the like effect from medical men who were in the Boer campaign. Dr. Arnold H. Watkins, writing from Kimberley,[2] says: "With the advent of cold weather the flies disappear, so does almost entirely the typhoid." Dr. A. B. Dunne[3] writes from Bloemfontein: "Nothing was more noticeable than the fall in the admissions from enteric fever coincident with the killing off of the flies on the advent of the cold nights in May and June." Dr. Howard H. Tooth[4] is of opinion that too much stress has been laid on water supplies. At the Modder River, "devils," or storm dusts, swept through the camp, and would lift the contents of pits. This was not so much the case at Bloemfontein. He does not exclude the action of dust, but observes that at both places there were enormous numbers of flies. He also remarks: "The disappearance of enteric fever and flies with the first appearance of frost at night may be more than a coincidence." All these observers, then, fix

[1] Quoted by Ainsworth, *Journ. Roy. Army Med. Corps*, 1909, xii, p. 485.
[2] *Brit. Med. Journ.*, 1900, ii, p. 787.
[3] *Brit. Med. Journ.*, 1902, i, p. 622.
[4] *Brit. Med. Journ.*, 1900, ii, p. 1368.

on the simultaneous destruction of flies and diminution of enteric fever. The dust then becomes inoperative.

A very interesting paper by Dr. A. H. Ainsworth on "The House Fly as a Carrier of Disease" is given in the *Journal of the Royal Army Medical Corps*, May, 1909.[1] The enteric curve ascends during the heavy rains of the monsoon in Poona, and descends before their decline. This would seem absolutely to dispose of dust as a principal factor. The enteric curve, however, follows on the advent of flies, the curve of which also ascends and descends during the season of heavy rains; moisture is, of course, necessary for their development. There is no correspondence between the amount of rainfall and the amount of diarrhœa, although the prevalence of heavy rainfall coincides in time with the ascent in enteric fever and diarrhœa. The observations on flies relate only to one year, and are defective in amount; they require to be much extended. The author, however, considers flies to be the carriers of infection. This most interesting communication, illustrated as it is by a series of charts showing prevalence of rainfall, enteric fever, and, in one year, of diarrhœa, would seem to dispose entirely of the view that it is by dust that the ascent of these diseases is caused; while, though incomplete, it emphasizes the part played by flies in carrying these diseases. It may be said that the explanations of the observed facts have not all been to this effect. If, however, the statements quoted be correct we can interpret for ourselves, and the observations referred to appear to furnish a practically conclusive case for the potency of the house fly as a carrier of enteric (and diarrhœal) infection at home as well as abroad.

We may now consider those conditions for the conveyance of the infections of enteric fever and diarrhœa which have not been fulfilled in the argument:—

(1) Are house flies present in immense numbers in houses prior to primary attacks in those of infants? In most cases they are. For details I must refer to my annual reports for 1904 and 1905. It is true that this is not always the case; but one does not maintain that every infant is infected through the agency of flies, though many are believed to be so indirectly who are not directly.

(2) Have house flies been shown to convey bacteria? They have. It is unnecessary on this point to multiply references. That they carry away enteric bacilli from enteric stools is shown by Major Firth and Major Horrocks in their communication to the *British Medical Journal*,

[1] *Journ. Roy. Army Med. Corps*, 1909, xii, p. 485.

1902, ii. An important contribution to this subject by Dr. Sellers, working in Professor Delépine's laboratory, is given in the annual report on the health of Manchester for 1906. The collection of the flies was made by a method devised by Professor Delépine, and designed to secure that the traps were free from any micro-organisms except those carried in by flies. The flies were captured at twenty-eight stations, such as to give a good chance that they should be carrying the infective germ of diarrhœa. The largest number taken in one trap was about twenty. As to the methods of examination I must refer to the article itself. Dr. Sellers states: "As regards the actual number of bacteria present, it was found that considerable variations occurred. Generally speaking, a quantity of washings corresponding to a single fly yielded hundreds or even thousands of colonies." Of the suspicious bacteria found there were five which clearly belonged to the *Bacillus coli* group. The total number of flies examined was 380.

It is probable from the above investigation that it requires the visit of a very large number of flies before infection can take place, even supposing that many of these come from infective excreta.

The other conditions laid down for the completion of the argument have, I hope, been fulfilled, and it will be for this Society to consider whether it has been established that no hypothesis other than that of transmission and of infection by flies will adequately explain the diarrhœa curve, or the primary autumnal disturbance of the enteric curve in Manchester.