GENERAL BOARD OF HEALTH.

MEDICAL COUNCIL.

REPORT

OF THE

COMMITTEE FOR SCIENTIFIC INQUIRIES

IN RELATION TO

THE CHOLERA-EPIDEMIC

OF 1854.

Presented to both Houses of Parliament by Command of Her Majesty.

LONDON:
PRINTED BY GEORGE E. EYRE AND WILLIAM SPOTTISWOODE,
PRINTERS TO THE QUEEN'S MOST EXCELLENT MAJESTY.
FOR HER MAJESTY'S STATIONERY OFFICE.

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REPORT.

To the Right Hon. the President of the General Board of Health.

SIR, London, July 14th, 1855.

SOON after your having constituted a Medical Council to advise the Board of Health on matters relative to the then prevailing epidemic of cholera, this Council thought it convenient for the despatch of business that subjects referred to them should be distributed among certain committees of their number; and on this plan we, the undersigned, were requested to become a committee for the scientific purposes of the Council.

Two special duties accordingly devolved on us: *first*, to suggest the institution of particular scientific inquiries which we thought likely, by bettering medical knowledge of the disease, to strengthen the public resources for its prevention and cure; *secondly*, to review, as laid before us, the various fruits of this investigation, and to submit to you our judgment of its results.

While proceeding to report to you on our fulfilment of this task, we beg to acknowledge very gratefully the kindness and confidence with which you have honoured us, in instituting all the inquiries we ventured to suggest, and in procuring from other departments of Government whatever assistance could conduce to our success.

At the same time, we ask indulgence for much that is unavoidably imperfect in our work. To do such things fitly, all needful organization must be prior to the emergency; it would be vain to expect that observations begun in the crisis of an epidemic should have that completeness which science requires, and which only deliberate preparation can ensure. We need hardly remind you that the re-constitution of the General Board, under which you became its President, was effected only on the 12th of last August; that the first meeting of the Medical Council was only on the 6th of September; and that we were appointed a committee two days later—already in the tenth week of a pestilence which some days previously had

attained its utmost extension, and was now in process of decline.

If, however, we cannot speak with unmixed satisfaction of the materials which are before us, we can at least point to their nature and extent as in the highest degree encouraging to future exertions, more extended, more systematic, and more continuous.

Our principal aims, and the methods by which their attainment was sought, have been as follows:—

I. with a view to the DESCRIPTIVE HISTORY OF CHOLERA, we have examined the larger *statistics of this invasion;* as to the places wherein the disease chiefly prevailed; as to the influence of age, sex, and employment in favouring its attack; and as to its own pathological stages and periods;—

II. in the hope to gain more precise knowledge of the CAUSES OF THE DISEASE, we thought it of primary importance that the *air and water of the metropolis* during the epidemic period should be studiously observed, and that special inquiries should, as far as possible, be made into the state of these universal influences in districts actually infected with cholera;—and

III. with the object of increasing for our profession the present insufficient resources of MEDICAL TREATMENT, we have endeavoured to procure comparative *records of various therapeutical experience*, successful or unsuccessful, and have invited from persons versed in such inquiry an elucidation of those questions in the *practical pathology of cholera* which appeared to us most urgent for solution.

FIRST SECTION.

Statistics.

OF statistical material, the following has been before us:

1. We have derived from the General Register Office a *list of all deaths* registered during the epidemic period as caused by cholera or diarrhœa, with particulars as to the

age, sex, and residence of each sufferer, the date of death, and, in many cases, the duration of fatal illness.

2. As the *returns of certain circular forms* (A. and B.) distributed by the Board, we have received from more than 300 medical practitioners, whose names we append to our report, an immense mass of detailed information; and to this we cannot advert without offering our tribute of respect to the public spirit which led so many members of our profession, solely for the general good, to incur an additional task during days of overwhelming occupation. These returns relate to 4,271 cases of cholera and 20,301 of diarrhœa. Besides particulars hereafter to be mentioned in respect to the treatment of the disease, there is contained in them pathological information as to its stages and periods, as to the universality or relative frequency of certain symptoms, as to the prospects of death or recovery at each step of its progress.

From the above sources we gather the following facts of the late epidemic visitation—the third which London has suffered from Asiatic cholera.

(i.) *Progress and Fatality of the Epidemic.*

The summer of 1853 witnessed its commencement.* At that time, as the temperature rose in July, diarrhœa, as well as the common form of cholera, became fatal here; and a few deaths from cholera in the Asiatic form were registered in August in the low districts by the side of the river. Several deaths by the disease occurred in September; and in October, while the temperature fell, and diarrhœa decreased from 723 in August to 283 in October, the cholera spread and became more fatal, so that the deaths by cholera were 335 in October, and 288 in November. On the last day of October 25 persons died of the disease; but subsequently the epidemic subsided, so that towards the close of November the deaths did not exceed four daily; in the month of December the deaths were 43; in January (1854) one death only happened at intervals on each of seven days; one death was recorded on the 10th of February, one on the 25th, and one on the 26th; no death by cholera occurred in March; only four in April, four in May, and three in June.

* Earlier in the year, the disease had prevailed in the ports of the Baltic and on the shores of the German Ocean.

Diarrhœa, however, although it had declined, never ceased; no day in the six first months of the year was without a death, and on some days as many as 8, 10, and 12 deaths by this form of disease were recorded; but it exhibited no disposition to increase. Yet, warned by the course of the former outbreaks, fears were entertained of the more formidable secondary visitation; which were unfortunately justified by the event, for in the eleven days after July 4th, ten persons died of cholera; on the 16th, four died of the disease; and the deaths ran up towards the end of the month until 53, 41, and 44 died on the last three days; on August 1st, 72 persons died of cholera; which continually spreading and multiplying in its course, the deaths on the last day of August were 211. The deaths in July had been 371 by diarrhœa, 308 by cholera; in the month of August the deaths by diarrhœa amounted to 1,022, by cholera to 3,513. No abatement was observed, but the epidemic raged more violently, and the deaths by cholera alone on the 1st of September were 389; on the 2nd, 459; on the 3rd, 329; or 1,177 in the three days, beside 126 deaths by diarrhœa. The eruption had now reached its culminating point; but it subsided slowly, for 4,371 lives were destroyed by cholera and diarrhœa in the first fourteen days of September, while in the month the deaths by cholera were 6,084, by diarrhœa 990. The epidemic declined rapidly in October, and the deaths fell from 70 by cholera on the 1st day, to 3 on the 30th day, making 823 in the aggregate, besides 426 by diarrhœa. In November the deaths by cholera were 52; in December 5, namely, one on the 2nd day, one on the 5th, one on the 6th, and two on the 22nd day. The deaths by diarrhœa in the two months were 175 and 113. The plague was stayed, but it had destroyed in one or other of its forms seventeen thousand people. Such is a rapid sketch of this remarkable epidemic, which can be traced in the annexed Tables, through its fatal course, day by day, from July 1st, 1853, to the end of the year 1854. (*See* Table I., Second Series.)

(ii.) *Estimated Number of Attacks.*

The deaths by the disease were all registered; and the names, ages, occupations, death-dates, and death-places of its victims are all recorded in the books of the General Register Office. The list of the killed is therefore complete; but what was the number of the wounded? What number of the people was attacked by cholera, and what number suffered and recovered from diarrhœa? No complete return of the cases exists; but the forms of return (A. and B.) which were issued by the Board of Health, and filled up in a manner so creditable to the medical practitioners of London, enable us to form an estimate, which cannot differ much from the actual facts.

Thus, the medical returns show that of 3,188 recorded cases of cholera, 1,467, or 46 per cent., terminated fatally; so it may be inferred from this proportion that, as 11,661 persons died, about 25,000 were attacked by cholera. The mortality of the cases in the hospitals was 51 per cent. ($\frac{763}{1502}$); the mortality of cases that were treated at home was 42 per cent. ($\frac{704}{1686}$); and some deviation further still from the average may have actually occurred in the whole population, but the estimate is true within certain limits. Again, 5,271 cases of diarrhœa were recorded in detail, distinguishing the ages, and 87 were fatal, or the mortality among persons actually attacked by diarrhœa was at the rate of ·0165, about 1·65 per cent. And 17,351 cases, 109 deaths by diarrhœa (·00628) may be noted, if we count the additional cases that are returned in numbers without being separately entered. The deaths in London from diarrhœa were 6,258, so it is evident that only a small proportion of these fatal cases—occurring chiefly in children and old people—attracted the attention of the medical observers. But we are already justified in inferring that, as 6,258 died, some *hundreds of thousands* of the population were attacked by the disease.

The medical returns show the power of recovery from an attack of cholera at different ages; so that 35 deaths at the age 15 to 25 imply that 100 persons of the age have been attacked; at the age 45 to 55, 50 deaths imply 100 attacks; at the advanced age, 75 to 85, 71 deaths by cholera imply 100 attacks. So it is with diarrhœa. And if the proportional numbers of deaths to cases in the medical returns are applied to the total deaths that were registered at the corresponding ages, the result gives the number that were attacked at all ages by cholera as 24,917 persons; by diarrhœa of some severity as 329,778; by diarrhœa of so slight a nature as to be only

brought casually under medical observation, about 519,487; making 874,182 persons in the aggregate who were touched by the epidemic, while 1,642,866 persons escaped unscathed. The estimate of the slight cases is based upon imperfect observations; and we set it down here as a mere indication of the wide influence of the epidemic. (*See* First Series, Table XII.; Third Series, Table VIII.)

The aggregate number of deaths by diarrhœa and by cholera in the medical returns was 1,576, of cases 20,648; and if the estimate is taken on these proportions, it will follow that the 17,919 deaths by the two forms of disease imply less than 235,000 cases; but it is evident on the face of the returns that the slighter cases of diarrhœa were in many instances unrecorded, and consequently the severer forms of cholera were returned by the medical observers in a higher proportion than they actually occurred; we have, therefore, made the above estimate on the separate returns. There can be no doubt, however, that, as is subsequently shown, the great majority of the registered deaths from diarrhœa were deaths from cholera in one of its modified forms; and that both these deaths and all those registered as deaths from cholera, should be taken as representing the mortality of one epidemic disease, including cholera and diarrhœa. To arrive at a correct estimate, however, of what the rate of mortality of the entire disease (including cholera and diarrhœa) is, a large addition must be made to the above number of cases for cases of diarrhœa omitted.

London, in the middle of the year 1854, contained about 2,517,048 people; whom we may conceive to be distributed in equal groups of 10,000. The epidemic diffused itself over each of the 251 myriads; and if *one myriad* is taken to represent the average danger and suffering of the whole, it appears that 71 died; namely, 46 by cholera, 25 by diarrhœa; 99 having probably been attacked by cholera, 1,310 by diarrhœa of some severity, and a still larger number (perhaps 2,064) by slighter forms of disorder. It may, indeed be assumed that in one period or other of the epidemic every individual fell more or less under its influence; while to some, however, who tasted the poison, it was only the cause of a temporary derangement, to others who drank of the chalice to the dregs it was the bitterness of death.

In the years 1840–1 the deaths from diarrhœa in London

amounted to 452 and 465; in the four years 1842–5 the deaths fluctuated from 704 to 834; and since the year 1846 this disease has, in the years when cholera was not epidemic, been the cause of death to 2,000 or more of the inhabitants, and must have attacked 100,000 of them annually, or 75,000 more than it did in 1840–1. Summer cholera, within the same period, also became more prevalent; 60 deaths were ascribed to that cause in 1840, and 162 in 1852. A certain number of the deaths from diarrhœa and cholera in the period of the epidemic are therefore referable to these diseases in the old form, and a certain number to the form acquired since the hot summer of 1846. No inconvenience will, however, arise in the subsequent inquiries if the rate of mortality is calculated on the population in 1854, and on the deaths from cholera and diarrhœa in the period from July 1st, 1853, to December 31st, 1854.

During the epidemic many persons labouring under chronic diseases are attacked by diarrhœa and cholera, which prove fatal in a certain number of instances; and in the abstracts that we have had made from the public registers these cases are counted, although the original primary disease was of a fatal nature. Our numbers, therefore, will exceed the numbers referred to diarrhœa in the Registration Tables.

(iii.) *Local Differences of Cholera Mortality.*

The rate of mortality all over London is represented by the deaths of 71 persons in a myriad people in average conditions of exposure. We cannot investigate the mortality in each of the 251 myriads of which London is composed, but we have the means of determining in the usual way the mortality in each of the 36 registration districts, containing populations varying from one (Hampstead, 11,986) to sixteen (Pancras, 166,956) myriads. Here the hospitals interfere to some extent; but a correction has been made by distributing the 800 deaths in the hospitals over the several districts, in the proportion of the deaths that occurred out of the hospitals in each district, so that the disturbance from this cause cannot be considerable. A similar inquiry has been undertaken, for the sake of comparison in each of the 135 sub-districts into which the 36 districts are subdivided; and some cor-

rection has been made for the deaths from cholera in the workhouses, which in certain districts were used as cholera hospitals. (*See* Second Series, Tables III., IV., VI.; Third Series, Table I.)

The districts, arranged in the order of the rate of mortality from cholera, display a regular series of numbers expressive of that rate, ranging from 6, 10, and 11 at one extreme, to 142, 165, and 179 at the other extreme; so each myriad of the people in the different localities of this great city suffered differently, and the observations present us with these enormous disparities in the sufferings of the thirty-six large congregated masses. The range in the rate of mortality by diarrhœa is much less considerable; it extends from 11, 12, 17, in some districts, to 39, 53, and 54, in a myriad inhabitants, in other districts.

(iv.) *Influence of Density of Population.*

The population of London stands on 78,029 acres of ground, so that in 1851 there were on an average 30 persons to an acre. In Lewisham there were 2 persons, in Wandsworth 4, and in Hampstead 5 persons to an acre; the mortality to 10,000 from cholera in these districts was at the rate of 22, 85, and 12 in 1853–4; and 30, 100, and 8 in 1849 in these open districts. In the three densest districts there were 246, 256, and 290 persons to an acre; the mortality from cholera was at the rate of 10 in St. Luke, of 22 in the Strand, and of 23 in the East London City district, in 1853–4; of 34, 35, and 45 in the same districts in 1849. The mean mortality by cholera is, in the two epidemics, at the rate of 43 in the three most open districts, 28 in the three most dense districts.

Again, in the nine districts of Lewisham, Wandsworth, Hampstead, Camberwell, Hackney, Kensington, Poplar, Greenwich, and Rotherhithe, the mean density of the population ranges from 2 to 21 persons on an acre; the mean mortality in the two epidemics was at the rate of 65 by cholera, by diarrhœa 22, in 10,000.

And in the nine densest districts, Whitechapel, St. George in the East, the West London City, St. Giles, St. James, Holborn, St. Luke, the Strand, and the East London City, where there were on an average from 196 to 290 persons on an acre, the mean mortality in the two epidemics was, by cholera 41, by diarrhœa 19, in 10,000.

The mortality by the two forms of disease was 85 in the nine open to 58 in the nine dense districts.

The mean mortality by cholera and diarrhœa, in the 18 most open districts (40 in 10,000), is nearly the same as (42 in 10,000) the mortality in the 18 most dense districts.

If the 135 sub-districts are arranged in the order of their density, the result is similar; the fatality of the epidemic being highest, however, in the districts of an intermediate degree of density. The cholera matter was evidently diffused over every sub-district of London, but it does not appear that the great differences in the density of the habitations of the people exercised any decisive influence on the intensity of its operation, appreciable, at least, by this method of investigation. Its effect was, perhaps, masked by other more potent agencies. This is the more remarkable as the fatality of large classes of disease has been found to increase in a given ratio to the density of the population; and it may be inferred that cholera is not, like some such cases, communicated by the breath from person to person. (*See* Third Series, Tables I., II., III.)

(v.) *Influence of Elevation.*

The population of London is distributed over the low ground on both sides of the Thames, and over a great number of elevations and depressions, which ascend from the south bank of the river up to Blackheath and Norwood, and from the north bank up to Highgate and to Hampstead. The four lowest districts, Newington, Rotherhithe, St. George Southwark, and Bermondsey, are on or below the level of the Thames at high water; the mortality by cholera to 10,000 in these districts was at the rate of 112, 165, 121, and 179 in the last, and 144, 205, 164, and 161 in the previous epidemic.

Hampstead, Islington, Marylebone, and St. Pancras are at average elevations of 350, 94, 87, and 73 feet above the Thames, and the mortality by cholera in these highest districts was at the rate of 12, 11, 17, and 10 in the last, of 8, 22, 17, and 22, to 10,000, in the former epidemic.

The mean mortality by cholera to 10,000 in the two epidemics was at the rate of 156 in the four lowest districts, 15 in the four highest districts. The mortality by *cholera and diarrhœa* to a myriad of population was 189 on the low districts, 34 on the high districts.

If the thirty-six DISTRICTS of London are arranged in the order of their elevation above the high-water mark of the Thames, the mortality by cholera is found not to be invariably in each district inversely as the elevation; but by taking groups of districts together in the two epidemics, a nearly regular series is obtained: thus, the mean mortality by cholera was, to every myriad, 156 in the districts on or below the level of the high-water mark, 91 in the districts of 3 and under 20 feet of elevation, 44 in the districts at 20—40 feet, 36 in the districts at 40—60 feet, 23 at 60—80 feet, 17 at 80—100 feet, and 10 at 350 feet of elevation. (*See* Third Series, Table I.)

The mortality by diarrhœa was at the rate of 33 on the lowest ground, 26 on the second terrace, 19 on the third terrace, 18 on each of the three higher terraces (20—100 feet), and 10 on the highest terrace. Thus, the mortality by diarrhœa varies less than the mortality by cholera at different elevations. Upon the two highest terraces the diarrhœa is as fatal as the cholera; upon the lowest ground the cholera is four times as fatal as the diarrhœa.

The relation between the elevation of the dwelling-ground and the intensity of the epidemic is seen in the annexed Tables of the Third Series.

(Table IV.) Of the 135 SUB-DISTRICTS, arranged in regular order from the highest to the lowest in London;

(Table V.) Of these sub-districts grouped together in fourteen terraces;

(Table VI.) And of the same sub-districts grouped in six terraces of elevation. The form is the same as the corresponding Tables in the Registrar-General's weekly tables; but the mortality is deduced from the deaths by cholera and by diarrhœa in the 18 months, July 1st, 1853, to the end of 1854; and certain corrections are made for the increase of population and for the disturbance that the deaths in hospitals and workhouses occasioned. The results in the Table V. and Table VI. present a near approximation to the true rates of mortality in the 135 sub-districts; and though differing in details, are of the same character as the results that have been deduced above by grouping the 36 districts. (Table I.)

The mortality from cholera ($1853\frac{1}{2}$—4) was at the rate of 13 to a myriad in the highest, 137 to a myriad in the lowest sub-districts (Table V.); the mortality in the same sub-districts from diarrhœa was 21 in the highest,

34 in the lowest; and it will be recollected that *one* death from cholera represents about *two* (2·2) cases of cholera, while *one* death from diarrhœa represents about *sixty* cases of diarrhœa of some severity; consequently the cases of diarrhœa and cholera together must at these rates have been about 1,288 to a myriad in the higher regions, and 1,741 in the lower regions of London. But if it be assumed, as is not impossible, that the cases of diarrhœa and cholera were less fatal on the elevated sub-districts than the cases on the low grounds, then the proportion of persons attacked in the respective regions would differ much less considerably: for, if 1 in 35 cases of diarrhœa was fatal in the lower regions, and 1 in 70 was fatal in the higher regions, the proportional number of persons that were attacked by diarrhœa or cholera in each must have been about 1,490 in a myriad of the population; and taking intermediate proportions, a similar result is obtained for the regions at intermediate elevations.

The distribution of choleraic attacks (though in widely different degrees of frequency, and perhaps also of severity) throughout the whole metropolitan area, seems to establish that the *cholera-leaven*, be it what it may, was scarcely less diffused in the districts that suffered the lowest mortality, than it was in the districts where the disease was tenfold more fatal.

But while the presence of this *leaven* seems to have been universal throughout the districts of the metropolis, the consequences excited by its presence have greatly varied in different localities: and independently of any hypothesis, it may now be stated as the experience of two epidemics in London, that such local varieties of effect, grouped into masses for comparison, have been more nearly inverse to the elevation of soil in the affected districts than proportionate to any other general influence that we could measure.* Thus, approaching London

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* The following formula is from the Report to the Registrar-General on the Cholera of 1848–9, p. lxiii.:—Let e be any elevation within the observed limits, 0 to 350 feet; c be the rate of mortality from cholera at that elevation; also let e' be any higher elevation, and c' the mortality at that higher elevation. Then, if the mortality from cholera is inversely as the elevation, we shall have the proportion,—

$$(1.)\ e : e' :: c' : c = \frac{e'}{e}, c'.$$

By adding a constant element, a, the velocity at which the mortality increases can be retarded to any extent. The equation then assumes the form,—

$$(2.)\ \frac{e' + a}{e + a}. c' = c, \text{ or } (3.)\ c' = \frac{e + a}{e' + a}. c.$$

along the roads from the surrounding country, and descending through the successive regions, succeeding each other in circles, down to the waters of the polluted Thames, we see, in the epidemic, the people fall upon the right hand and upon the left in numbers that increase on every circle, and express arithmetically the growing force of those physical influences, on which the poison of cholera apparently depends for its powers of existence or of development.

The annexed diagram exhibits to the eye the relative intensity of the cholera in the sub-districts of various elevations; it also exhibits a regular curve, with which the series observed closely agrees, except in the part which includes the observations in the Berwick Street and Golden Square sub-districts.

(vi.) *Mortality of Attacks of Diarrhœa and Cholera in its various Forms.*

The medical returns already adverted to, contained more or less complete information of the kind indicated by the Forms A, B, which were drawn up by us, in the midst of the epidemic. Those returns came to hand at distant intervals from the London Hospitals, from private practitioners, and from country districts; and the various sets of returns have been made available, as far as it was practicable, for the various branches of this inquiry; thus 24,572 cases, namely, 4,271 of cholera, and 20,301 of diarrhœa, were brought under our observation. The mortality by cholera in the several sets of English returns, ranged from 41 to 51 deaths on every 100 persons attacked; the mortality from diarrhœa ranged from ·6 to 1·8 on every 100 attacked, (or without decimals, from 6 to 18 on every 1,000), as is shown in the annexed Table:

The value of a in general terms is,—

(3.) $a = \frac{e'\,c' - e\,c}{c - c'}$, and it was taken at 13.

$e + a$ and $e' + a$ represent the abscissas, c and c' the ordinates of the curve. The central perpendicular line in the diagram corresponds to the abscissas, the horizontal lines to the ordinates of the curve, which was calculated from the mean mortality (145) on the lowest ground ($e = -1$), *one* foot below Trinity high-water mark. The equation was, therefore,—

$$\frac{a-1}{a+e'} \cdot c = \frac{13-1}{13+e'} \times 145 = c' = \frac{12 \times 145}{13+e'} = \frac{1740}{13+e'};$$

the e' being made the variable.

TABLE (C.) of MORTALITY of attacks of CHOLERA in several Groups of Returns.

——	Number of Cases.	Deaths.	Mortality. —— Deaths to 100 Cases.
(1.) Cases from all sources -	4,271	1,948	45·6
(2.) Cases in London Hospitals -	1,502	763	50·9
(3.) Cases in private practice, London - - -	1,686	704	41·8
(4.) Total of (3.) & (4.), or of the London Cases - -	3,188	1,467	46·0
(5.) Cases in the provincial towns and districts - -	586	238	40·6
(6.) Some cases in Scotland -	497	243	48·9
(7.) A selection of all cases in which the connexion of collapse Cases, or Cases without collapse, with consecutive fever, could be traced - - - -	3,596	1,767	49·1
(8.) Of the above (7.) were cases of collapse* - - -	2,431	1,627	66·9
(9.) Of the above (7.) were cases without absolute collapse	1,165	140	12·0
(10.) All cases in which age was specified - - -	3,611	1,749	48·4

TABLE (D.) of MORTALITY of Cases of CONSECUTIVE FEVER following CHOLERA (included in No. 7 of Table of Cholera Mortality).

——	Number of Cases.	Deaths.	Mortality. —— Deaths to 100 Cases.
All Cases of consecutive fever -	874	249	28·5
Of the above were :—			
(1.) Cases following collapse -	633	220	35·0
(2.) Cases following cholera without collapse - -	241	29	12·0

* The numbers are taken from the Tables IV. and V., First Series.

TABLE (E.) of MORTALITY of Attacks of DIARRHŒA, Simple or Choleraic.

——	Number of Cases.	Deaths.	Mortality. —— Deaths to 100 Cases.
(1.) Cases from all sources -	20,301	156	·77
(2.) Cases treated in London hospitals - - -	688	8	1·16
(3.) Cases not treated in London hospitals - - -	16,772	101	·60
(4.) Sum of cases in London -	17,460	109	·62
(5.) Cases in the country towns and districts where cholera was epidemic - -	2,176	40	1·84
(6.) Some cases in Scotland -	665	7	1·05
(7.) Cases in which age was distinguished - - -	5,271	87	1·65

From another extended series of returns that were procured through the Poor Law Board of Scotland, it appears that the mortality in that country among persons attacked by cholera, was 47·5 in 100; for out of 14,430 cases, 6,848 terminated fatally. This result agrees closely with the general result of the English cases, of which 45·2 in 100 were fatal. (*See* First Series, Table XIII.)

If we assume for a moment that all the cases commence as diarrhœa, we may infer that of 142,351 persons attacked of all ages, a certain number, say 132,351, suffer from diarrhœa of some severity, and that after the lapse of a certain number of days 2,512 die, 129,839 recover: but 10,000 enter the stage of cholera, and may then be divided into two great classes: 3,240 of the *first class*, who do not fall into collapse; 309 of their number dying, 2,261 recovering, while 670 pass into consecutive fever, 81 to die in, 589 to recover from, that stage; and 6,760 of the *second class* who do fall into collapse, 3,913 dying in that stage, from which, however, 1,087 recover straightway, leaving 1,760 who pass into consecutive fever, in which 612 die, and from which 1,148 recover: thus of the 10,000 that we follow in their perilous journey, 6,760 fall into collapse, 2,430 fall into the fever; of the 6,760 who fall into collapse, 4,525 die either in that stage or ultimately

in the fever stage: of the 3,240 who do not fall into collapse, as many as 390 die: of the 2,430 who pass into the fever 693 die.*

If a single patient is regarded, these numbers will assist in prognosis, as they express the degrees of danger to which he is exposed; thus upon being attacked by unequivocal cholera, the probability is slightly in favour of recovery (·5085); but the chance is two to one that he will fall into collapse, and then it is two to one that he will not recover; if, however, he survive this stage and pass into the fever, it is more than two to one that he will recover. It is seven to one that the patient who does not fall into collapse will recover, and if he pass into the fever he has such an advantage that it is seven to one in favour of his recovery, while, as is before stated, it is only two to one in favour of the recovery from the fever of a patient who has been in collapse.

Such, it must be recollected, is not the natural course of these two forms of disease, but their course under the systems of treatment now in practice in England: and what is the result of the English practice?

The great object of the physician or surgeon in every stage of the malady is to save the patient's life, and with this object in view, to prevent the diarrhœa from passing into cholera, the cholera from falling into collapse. Now it has been seen that of 142,351 persons attacked, about 129,839 recover, while only 10,000 of the cases become cholera, 6,760 fall into collapse.

The question, how far these proportions have been affected, for better or for worse, by various methods of treatment, empirical or rational, has been under the con-

* The following are the data from which the above results have been deduced :—

The number of severe cases of diarrhœa, in London, 329,778 (estimated); deaths by diarrhœa, 6,258; cases of cholera, 24,917 (estimated); cases of cholera in which the several stages were distinguished in the medical returns, 3,596, of which 1,767 terminated fatally.—(*See* First Series, Table IV.; Third Series, Table VIII.)

1,165 of the 3,596 cases did not go into collapse; but 111 died, and 813 recovered from this stage.

2,431 cases went into collapse, from which 391 recovered without proceeding further, and in which 1,407 died.

874 of the 3,596 cases passed into the stage of consecutive fever; and as it was observed that of 808 such cases, 223 had been preceded by cholera without collapse, 585 by cholera with collapse, it was inferred that of the 874 cases, 241 had not been preceded, that 633 had been preceded, by collapse.

249 of the cases of consecutive fever terminated fatally; and as it was observed that of 223 cases, 26 had not been preceded, that 199 had been preceded, by collapse, it was inferred that these two latter numbers should be raised to 29 and 220.

In a few rare instances the consecutive fever, it was stated, passed into other forms of disease, which are not brought into account in this illustration of the course of a complicated disease.—(*See* First Series, Tables IV. and V.)

sideration of some of our colleagues, acting as a special committee in this branch of the subject. We, therefore, only remark on one difficulty of the investigation which has been obvious in the materials we have analysed, and has often misled the public into false appreciation of alleged methods of cure. The choleraic pestilence varies in the severity of its individual attacks, from the degree of a trifling indisposition to that of a most deadly and intractable disease. We have seen that in one form it is fatal to 6, in another to 669 in every 1,000; and, therefore, to avoid great mistakes, any alleged specific requires that its effects should be investigated with the greatest care, through extended and, above all, trustworthy observations. When persons not accustomed to accurate investigations attempt to compare together the results of various treatment, as tested by death or recovery, they are seldom sufficiently on their guard against the immense fallacy of leaving unexpressed the *degrees of disease* against which this or that medicine has prevailed.

(vii.) *Duration of Cases of Cholera and Diarrhœa.*

If the first object of medical treatment is to save the patient's life, the second is to shorten his sufferings, and to accelerate his restoration to health. Now the duration of 3,600 cases of cholera, dating not from the commencement of the precedent diarrhœa, but from the first characteristic symptoms of cholera, was by the medical returns 5·9 (nearly six) days: the duration of the 1,744 fatal cases was 2·68 days (more than sixty-four hours); while the duration of the 1,856 cases of recovery was 9·06 days. We may, therefore, inquire of new methods of treatment, do they shorten the periods of the disease, as well as do they diminish the mortality more or less than the present system of practice?

The duration of 9,590 fatal cases of cholera was returned in the registers of deaths, and was on an average 2·39 days; a result differing little from that above, which is deduced from the medical returns.

If the series of changing phenomena in an attack of cholera are viewed collectively, with reference only to the time, dating from the commencement, we find, by a simple construction (*see* Third Series, Table IX.) that of 3,600 cases 816 terminate in one day (twenty-four hours), namely, 82 in recovery, 734 in death; leaving 2,784 cases,

of which 590 terminate in the second day, 162 in recovery, 428 in death; and so on to the 34th day as may be seen in the Table. The same construction shows that the average chance of recovery increases every hour, every day, from the commencement of the attack, at first rapidly, and then slowly: so that while it is only 1,856 to 1,744 in favour of recovery at the onset, it is 1,744 to 1,010 in favour of recovery if the patient survive twenty-four hours; the patient alive at the end of the second day, though still ill, has the chances of 1,612 to 582 (almost three to one) in favour of his ultimate recovery; at the end of ten days his chance of recovery is 636 to 109 or six to one: so that to gain time, is one great aim in the treatment of this disease, which destroys, in the first twenty-four hours after the manifest characteristic symptoms, *one* in every five that it attacks.

The probability that a patient suffering from an attack of average severity will die or recover in the attack is shown in Table XI. of the Third Series. In the doctrine of probabilities, certainty, it will be recollected, is expressed by *unity* (1), and the various degrees of probability are expressed by fractions, so that if an event can only terminate in one of two ways, the sum of the two fractions that measure the respective probabilities is 1·0; as by hypothesis the case must certainly terminate in one of the two ways. Thus, by the Table, the probability that a cholera patient will die of the attack at first is nearly $\frac{48}{100}$ or ·48; at the end of two days the probability is reduced to ·27, at the end of the seventh day to ·16, at the end of nineteen days to ·07. The probability of ultimate recovery increases as the probability of dying decreases, and is at the corresponding dates ·52, and ·73, and ·84, and ·93.

The Table IX. (Third Series) shows at a glance the probability that a patient suffering from an attack of average severity will recover *on*, *before*, or *after* any day of the disease; and will serve, therefore, either to guide an insurance office in insuring the life of a patient or to direct the physician in determining the effects of various systems of treatment. We shall not pursue this branch of the inquiry further into details, which can only be made clear by the aid of mathematical symbols. But we may here remark, as the same method is applicable to all diseases, that medicine is a science of probabilities, having observation for its basis; and depending, therefore, for its success on the application of the same methods of analysis as are

applied in the other sciences to facts observed on some such extended system as we have endeavoured to illustrate in conducting the inquiry into the laws to which an epidemic disease is subject.

The average duration of 5,271 cases of *diarrhœa*, chiefly of recoveries in the medical returns, was five days, while the duration of 4,150 cases in the registers of deaths was 13 days, these being all severe and more protracted diseases, occurring chiefly in young and in very old people.

The duration of cases of *cholera* is by the medical returns nearly 6 days, of cases of diarrhœa nearly 5 days; so that the two forms differ little in duration, and the 329,778 cases of severe diarrhœa, which, if we take the previous estimate, occurred in London, imply about 1,648,890 days of sickness, while the 24,917 cases of cholera imply 149,502 days of sickness.

(viii.) *Influence of Age and Sex.*

The influence of age on the course of cholera is striking; thus in infancy and advanced age the form of diarrhœa is relatively and absolutely more frequent than it is in the middle period of life, as at that age the spasms and the collapse are more evident than they are when the muscular system is feeble.

The danger of an attack of cholera varies with age; thus at the age of 15–25, out of 100 persons attacked 34·9 die; at the age of 25–35 the deaths to 100 cases are 35·4; at 65–75 the deaths to 100 cases are 58·2. The mortality of cases of diarrhœa also varies at different ages.

In another series of tables the mortality that cholera caused in the population at various ages is shown from the facts of the two epidemics. (*See* First Series, Table XII.; Second Series, Table X.)

The mortality among the male population was at the rate of 47, among the female population at the rate of 45 in 10,000.

(ix.) *Variations of Fatality during the progress of the Epidemic.*

The fatality of the cases of cholera diminished in the progress of the epidemic, so that the disease apparently assumed towards its close some of the characters of diarrhœa; as is shown in the annexed tabular statement of cases returned in London:—

Period of the Epidemic.		Cases.	Deaths.	Deaths to 100 Cases.
Fortnights.	Dates.			
	Total Cases - -	3,188	1,467	46
1	July 2—15 - -	6	2	33
2	July 16—29 - -	41	24	59
3	July 30—Aug. 12 -	272	141	52
4	Aug. 13—26 - -	420	181	43
5	Aug. 27—Sept. 9 -	980	509	52
6	Sept. 10—23 - -	921	393	43
7	Sept. 24—Oct. 7 -	377	151	40
8	Oct. 8—21 - -	118	54	46
9	Oct. 22—Nov. 2 -	39	9	23
10	Nov. 3—16 - -	14	3	21

If periods of four weeks are taken, it will be found that the fatality of the cases in the returns decreases progressively as the epidemic advances, from July 16 to Nov. 16.

(x.) *Comparison of Mortality in the two Epidemics.*

The last, like the previous epidemic, extended over portions of two years; and the deaths, which were not very numerous in 1853, slightly exceeded the deaths in 1848. The deaths by cholera and diarrhœa in 1854 were, however, 5,000 less than the number that would have been told, had the epidemic been as fatal as the epidemic of 1849, allowing ten per cent. for increase in the population.

Years.	Deaths by Cholera.	Deaths by Diarrhœa.	Deaths by Cholera and Diarrhœa.
Year 1849 - - - - - - - -	14,137	3,899	18,036
Deaths that *would have happened* if the population had been the same in the year 1849 as it was probably in 1854 - - - -	15,587	4,299	19,886
Year 1854 - - - - - - - -	10,806	4,000	14,806
Reduction of the mortality in 1854 as compared with that of 1849. - - - -	4,781	299	5,080

The second outbreak began later in the season in the last epidemic than it did in the epidemic of 1849; and the

diminished figure of the mortality arises from the smaller number of deaths in the months of July and August. The disease had lost none of its virulence, and the deaths by cholera in September 1854, being 6,084, exceeded the deaths (5,031) in September 1849 by 1,053. The daily deaths by cholera in the two epidemics at the maximum, were 336 on September 4th, 1849, and 459 on September 2nd, 1854.

If it is considered that the interval between the epidemic of 1831–2 and 1848–9 was *seventeen* years, and that the epidemic which we have recorded followed the second epidemic after an interval of only *five* years, it is evident that the public apprehension for the sanitary state of London should suffer no abatement, but that the most active and complete measures should be adopted to prevent the approach or to mitigate the violence of impending visitations.

Second Section.

Ætiology.

A.—*Atmospheric Causes.*

In reference to the *atmospheric conditions*, general or partial, which prevailed during the epidemic visitation, we have received reports, as follows, viz.:—from Mr. Glaisher, of the Royal Observatory, Greenwich, on the meteorology of London; from Dr. R. D. Thomson and Mr. Rainey, both of St. Thomas's Hospital, on certain chemical and microscopical investigations of air; and from officers of the Board of Health on the sanitary inspection of particular districts in the metropolis.

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1. Mr. Glaisher's Report is of peculiar interest. It presents the result of meteorological diaries kept, not only at ten stations previously existing, but at thirteen others specially established for the occasion; so that from the date of Mr. Glaisher's commission, observations more or less complete were made at no fewer than twenty-three sites in our vast metropolitan area. In reducing these multifarious observations, Mr. Glaisher's "first step was the examination of every reading in comparison with all others taken at or about the same time; the second was the application of index errors, corrections for diurnal range, and all necessary corrections and calculations to

deduce the mean daily value of each element of investigation. The weekly means of the daily values were next taken, and tables formed," exceeding fifty in number, as the basis of Mr. Glaisher's conclusions. From circumstances already adverted to, this admirable system of observation could not be completely organized till the epidemic had attained its climax; so that some gaps unavoidably remain in what, even with this deficiency, is a most valuable contribution to science.

Although Mr. Glaisher's report is in our Appendix, we think it needful here to present a short summary of its results; and we append a table which displays some of them in a compendious form:—

Deaths from Cholera.	*Weeks* ending as follows;	Atmospheric Pressure. Total.	Aqueous.	Total in excess, monthly mean.	Mean Temperature in excess.	Humidity. Max. 100.	Excess, monthly mean.	Density of Atmosphere in excess.	No. of Calm Days.	Proportion of cloud, *covered* or (clear sky, 0).
4	July 8	29·769	·368	- -	−4·5	75	- -	- -	5	} 9
6	15	29·853	·388	- -	−5·0	84	- -	- -	4	
33	22	30·114	·441	- -	+1·3	72	- -	- -	3	} 5
180	29	30·151	·434	- -	+4·1	68	- -	- -	3	
	July	- -	- -	+0·017	- -	- -	−5	+1		
488	Aug. 5	29·864	·429	- -	−3·2	85	- -	- -	1	} 8
671	12	30·010	·402	- -	−1·7	76	- -	- -	5	
772	19	29·968	·394	- -	−1·3	74	- -	- -	6	
869	26	30·067	·398	- -	+0·7	73	- -	- -	2	
	Aug.	- -	- -	+0·104	- -	- -	−39	+2		
1,646	Sept. 2	30·371	·455	- -	+6·3	73	- -	- -	7	} 3
1,869	9	30·335	·371	- -	+2·5	72	- -	- -	7	
1,527	16	30·014	·425	- -	+4·1	80	- -	- -	2	} 6
1,182	23	30·115	·388	- -	+2·7	80	- -	- -	0	
658	30	30·250	·342	- -	+1·2	76	- -	- -	6	1½
	Sept.	- -	- -	+0·199	- -	- -	−57	+2		
398	Oct. 7	29·878	·349	- -	+2·1	80	- -	- -	3	6
227	14	30·198	·330	- -	+1·0	83	- -	- -	2	4½
143	21	29·652	·280	- -	−2·5	82	- -	- -	2	7½
48	28	30·685	·268	- -	−2·2	83	- -	- -	5	5
	Oct. -	- -	- -	+0·058	- -	- -	−16	+1		
25	Nov. 4	30·275	·319	- -	+3·6	84	- -	- -	3	3½
16	11	30·275	·247	- -	−1·7	83	- -	- -	1	6
10	18	29·625	·250	- -	−1·0	88	- -	- -	1	8½
5	25	29·568	·215	- -	−4·5	88	- -	- -	0	8
	Nov.	- -	- -	+0·003	- -	- -	+31	+5		
4	Dec. 2	29·686	·216	- -	−2·5	83	- -	- -	0	6
2	9	29·810	·239	- -	+1·5	83	- -	- -	0	5
0	16	30·029	·248	- -	+3·1	84	- -	- -	0	6
2	23	29·767	·236	- -	+1·8	87	- -	- -	0	7
0	30	30·125	·225	- -	+1·6	86	- -	- -	0	5
	Dec. -	- -	- -	−0·069	- -	- -	−17	−4		

Mr. Glaisher's inquiries have related to the *pressure of the atmosphere*, total and aqueous; to its *temperature*, mean and extreme; to its *moisture*, absolute and relative; to its *density;* to the directions and amount of its *movements;* to the *chemical and electrical* influences that act in it; to *haze, fog*, *mist*, and *rain-fall.*

(i.) The corrected weekly means of the observed *readings of the barometer* had been considerably in excess of their average during February, March, and April; but in the three months next following they presented no important deviation, and only became remarkable towards the end of August. The atmospheric pressure had then risen much above its normal amount, and during the worst period of the epidemic was more continuously great than at any other time. From the 25th of August to the 10th of September the reading was above 30 in.; and on three days in this period as high as $30\frac{1}{2}$. The mean reading for the two months exceeds the corresponding amount in any year of Mr. Glaisher's series; and it is the more noticeable since (as will presently appear) less than the usual effect was due to watery vapour.

(ii.) During the early part of 1854, the *mean daily temperature* of the air had been higher than normal; its excess, for the first 101 days of the year, averaging $3^{\circ}\cdot4$. There had then set in a very cold period, injuring vegetation and killing many hardy plants; and for the ninety-seven days terminating July 19th there had been a daily defect of temperature, averaging $3^{\circ}\cdot3$. The next few days showed a sudden increase of heat; the 25th of July was the hottest day of the year, its temperature rising nearly to 90°, and exceeding the normal by 11°. Three weeks of cooler weather followed; but from the 19th of August to the 11th of October (within which time were the worst ravages of disease) there was an excess of heat, averaging $2^{\circ}\cdot6$ for each of the fifty-four days; and during one week of this period (that ending the 2nd of September) the excess amounted to $6\frac{1}{4}^{\circ}$. After the week ending the 14th of October, and excepting the week ending the 4th of November, the temperature was below its average till December.

(iii.) The *extremes of daily temperature*, and the range between them, have been noted by Mr. Glaisher carefully, and with some curious results. From the Greenwich observations it appears that, except June (which was

slightly in defect) every month of the year showed an excess above the average of diurnal range; March, April, and especially September being most remarkable in this respect; and the total result of this is, that, while for the thirteen years ending 1853 the mean yearly diurnal range was $14^{\circ}\cdot6$, the range for 1854 was $18^{\circ}\cdot1$, being $3\frac{1}{2}^{\circ}$ above the average. But in comparing the extreme readings of the Greenwich and other outlying stations with those of London proper, Mr. Glaisher discovers the startling fact, that his central stations undergo a much less daily range of temperature; that, because of the dense veil which overhangs them, they, during day-time, cannot get equal heat from the sun, nor during night-time can equally cool themselves into space. Such excesses of night-temperature have amounted in the weekly mean to 7°, 8°, 9°, and 10°; and as between particular stations, to 15° and 20°; a period marked by this extreme difference having extended but for twelve nights' interval, from the 26th of August to the 4th of September.

Remembering that, amid the districts which most of all present this high night-temperature, there is spread the vast evaporating surface of the Thames,—a river which (so far at least as London is a drained city) represents the main sewer of our metropolis,—remembering, that from its putrescent banks and waters there arise vapour and miasm in proportion to that level of temperature, we must recognise the full right with which Mr. Glaisher insists upon this feature of our London climate. Almost uninterruptedly, too, the heat of the water is some degrees greater than that of the superincumbent air; for 28 consecutive nights, ending September 12th, this excess averaged $16^{\circ}\cdot3$; and there was another fortnight, beginning a few days later, during which it averaged $16^{\circ}\cdot5$. At such periods we may (as Mr. Glaisher expresses it) infer the water to have been simmering, and the whole area of the Thames to have been giving off incessant and vast volumes of vapour, which, unsustained by the colder air, hovered over the city, thickened its atmosphere, occasioned the frequent prevalence of fog and mist, and explained the less daily range of temperature in stations overshadowed by its influence.

(iv.) The *vaporosity of the atmosphere* is estimated by a twofold standard; first, what quantity of vapour is actually holden in the air—how many grains per cubic foot—and what share does its weight contribute to the total of baro-

metric pressure? next, how near does that vapour approach to its own limit of maximum density—the limit at which (unless its temperature be raised) it admits of no further evaporation into it? An answer to the first question expresses an *absolute* quantity; an answer to the second, a *relative* quantity; and it is requisite to observe this distinction, because, on some damp wintry day for instance, the atmosphere may be within a shade of aqueous saturation, while in actual weight of water it scarcely holds the half of what, at higher temperatures, would leave it still capable of considerable drying-power. According to both standards, however, our atmosphere, during the chief prevalence of cholera, was less full than usual of aqueous vapour. In July, August, September, and October, it was further than usual from saturation; and from June to November it contained, in weight of vapour *per* given measure of air, $\frac{1}{20}$th less than its average.

(v.) In every month, excepting January and December, the *density of the atmosphere* has been in excess; the mean weight of a cubic foot of air having, for the year, been 2 grains above its average.

(vi.) With respect to the *movements of the atmosphere*, Mr. Glaisher's observations relate to the direction of winds, their mean force and velocity. From July 1st to September 11th the wind came alternately from S.W. and N.E. with nearly equal frequency, but with a difference of force greatly in favour of S.W.; in the next 28 days its direction varied more, but on 19 of them was W.S.W.; from October 11th it was W., and remained a compound of west to the end of the year. The daily motions of the air, irrespective of their direction, were much under the average; there was a defect in July of 34·3 per cent., in August of 25·7, in September of 15·3, in October of 29·3, in November of 32·8, from the respective averages of daily horizontal movement. Of the 123 fatal days, from July 1st to October 31st, there were 65 on which calm more or less prevailed; and it appears, as a fact of great interest in relation to the severity of the epidemic, that in the low-lying districts the air was at all times in much less motion than in those of higher level; entirely stagnant in the former, whenever in the latter it was noticed as calm; and when, at these, some hopeful wind blew with a pressure of 23 ounces, those suffering districts got but a sixth of the breeze.

(vii.) From July to the end of the year there were but few *thunder storms;* in fact, no great electrical disturbance took place from the time of the first outbreak of cholera in July, so long as the disease continued. Hail was noted on one day only within the same interval of time, viz. on October 23rd. So far, therefore, as the electrical observations indicate, in connexion with the much less than usual number of electrical disturbances in these months, it is inferred that there was a general deficiency in the tension of the common positive electricity prevalent during the period. No observations upon the electricity of the atmosphere were made till the disease was at its height; at this time the electricity was positive but weak, and continued so till the end of September. Positive electricity, with tension somewhat greater than in September, was present at stations of moderate elevation, always except when rain was falling, in the months of October, November, and December. Common atmospheric positive electricity has therefore been as prevalent as usual. At stations situated nearly on a level with the river Thames, the electricity was generally weaker than at stations of higher elevation, and was more frequently negative.

(viii.) From August 24th till September 4th there was no *ozone* at any station near the metropolis, and very little at any station over the country; a little was shown on September 5th, and from this time onward was general. At all stations of low elevation its amount has been insignificant, and at many near the river not a trace of it has been detected throughout the whole epidemic period; while at places of high elevation it has nearly always been shown, and at intermediate stations occasionally; seeming to graduate itself according to level, and to increase as we ascend from lower to higher ground.

(ix.) *Haze, fog, mist*, were singly or together prevalent on five days of July and eight days of August. The beginning of September was ushered in with a dense blue mist; this in the second week of the month (when cholera was still at its height) was exchanged for a thick atmosphere of fog, which continued with little intermission till the end of the month, and at low places prevailed both day and night. During all this time, the distance was misty, middle-distance indistinct, and sunshine pale and watery; but occasionally the atmosphere became partially translucent, and for awhile, in the higher levels of London, buildings would seem defined with remarkable clearness. The same kind of weather continued in October; and

mist, fog, or haze, in or about London, was recorded on 19 days of November and 21 of December. The fortnight ending September 9th, and the week ending September 30th, were the periods, subsequent to July 1st, in which the sky was least overcast with *clouds*.

(x.) Of *rain*, there was a deficiency in every month of the year excepting May and December; and the whole year's amount was one quarter short of its average. Of 136 days following July 1st, there were 93 on which no drop of moisture fell, and 25 others with but very trifling rain. The crisis of the epidemic was in the droughtiest period.

Mr. Glaisher has given additional value to his report by furnishing some particulars as to the meteorology of the epidemic periods of 1832 and 1849. In those years no observations were made at central points in the metropolis; and, therefore, it is only in respect of outlying stations that the meteorological phenomena admit of strict and detailed comparison. The records of 1832 relate to the atmospheric pressure and temperature, to the direction of the wind, fall of rain, clearness of sky, and frequency of electrical disturbances; those of 1849, except for their non-mention of ozone, admit of almost complete comparison with the present series. Mr. Glaisher's summary of these comparisons is in the following words:—

"The three epidemics were attended with a particular state of atmosphere, characterized by a prevalent mist, thin in high places, dense in low. During the height of the epidemic, in all cases, the reading of the barometer was remarkably high, the atmosphere thick; and in 1849 and 1854, the temperature above its average. A total absence of rain, and a stillness of air amounting almost to calm, accompanied the progress of the disease on each occasion. In places near the river, the night temperatures were high, with small diurnal range, with a dense torpid mist, and air charged with the many impurities arising from the exhalations of the Thames and adjoining marshes, a deficiency of electricity, and, as shown in 1854, a total absence of ozone, most probably destroyed by the decomposition of the organic matter with which the air in these situations is so strongly charged.

"In both 1849 and 1854, the first decline of the disease was marked by a decrease in the readings of the barometer, and in the temperature of air and water; the air, which previously had for a long time continued calm,

was succeeded by a strong S.W. wind, which soon dissipated the former stagnant and poisonous atmosphere. In both periods at the end of September, the temperature of the Thames fell below 60°, but in 1854 the barometer again increased, the air became again stagnant, and the decline of the disease was considerably checked. It continued, however, gradually to subside, although the months of November and December were nearly as misty as that of September. By the close of the year diarrhœa and cholera had subsided, but a high rate of mortality still continued."

It now remains for us to appreciate, one by one, in their relation to life, the several meteorological deviations which Mr. Glaisher records. And this is no easy task; for the pathological meaning of many atmospheric variations, at least in their minor degrees, is hitherto quite unknown. What effect is produced on human life by an inch rise or fall in the barometer, by fluctuations of humidity and dew-point, even by seasons of non-average temperature, is very imperfectly measured. Still less is known of the chemical activities of the atmosphere. We know, indeed, that this boundless ocean of air is, in one-fifth of its volume, *oxygen*; the gas, which more or less rapidly brings all organic compounds into simpler chemical forms, exhausting those qualities that make putridity, and terminating those transitional states in which the powers of morbid infection resides. But there is every reason to believe that the oxidizing power of the air varies at different moments, as assuredly it varies at different spots. *Electrical discharges*—the frequent source of such variations—constantly occur in the atmosphere, developing in it, wherever it extends, that mysterious increase of its oxidizing power which is called *ozone;* a qualification of the air so subtle in its kind, that chemists still doubt whether it be a separate entity; yet in its function so definite, so hostile to organic miasms, so incompatible with them, that its presence enables us to affirm their absence; for wherever it meets them it must be spent in disinfecting their unwholesomeness, neither leaving of them any residue unneutralized, nor itself remaining free, except as predominant force have been on one side of this conflict or on the other. It is likewise probable that the great acts of aerial renovation are modified by the powers of *solar light*, wherein our world floats and revolves; since in every ray of it there are chemical

influences, capable of affecting in turn each breath of the atmosphere they traverse. But these parts of the subject are of recent and unfinished discovery; our means of observation in regard of them are hitherto far from complete; and it would be premature to do more than point to such influences, and to their possible fluctuations, as a field for future most important inquiry.

How requisite is such research, may easily be illustrated. Mr. Glaisher, in whose personal observations we should repose full confidence, informs us, that he can by sight estimate certain differences of mist, which he identifies with corresponding differences of epidemic sanitary condition; that he can connect one tint of mist with the prevalence of cholera, another with the prevalence of influenza; yet that, except for this rude test of colour, he cannot discriminate those mists, and has no hygrometric or other meteorological knowledge of their existence.

Amid such uncertainties, we only venture to glance at the less obscure aspects of this interesting investigation.

The *undue height of the barometer* is an indication to which Mr. Glaisher draws particular attention, as having generally prevailed at the worst moments of each epidemic. During August and September 1854, it stood above its average from $\frac{1}{10}$ to $\frac{1}{5}$ in.; but lest undue importance should be attached to this one element, it must be noted, that in February (when there were but three cholera deaths) it had ranged nearly $\frac{3}{10}$ in., and in March (when there was no such death) more than $\frac{2}{5}$ in. above its average. We know of no direct influence which these atmospheric changes can have produced on human life, nor of any they can have exerted on the rate or kind of chemical change; but to a limited extent (as implying greater density of air) they would operate against vaporous diffusion, and in this degree may be probably estimated as favourable to the stagnation of miasm.

Of the immense *influence of heat* it is scarcely requisite to speak, in respect either of its well-known faculty to accelerate chemical changes, or of the many other differences that follow its range—the rarefaction of air, the lessening of humidity, the excitement of evaporation. Despite some exceptions, probably less real than apparent, it seems that Asiatic cholera, and indeed bowel-poisons generally, are favoured by high temperature; and in comparing together our two last epidemics, with a parallel comparison of their seasons, we are struck with the fact

that in 1854, when the summer temperature began later than in 1849, and quite abruptly rose to its maximum, so too the curve of mortality in that epidemic was peculiar,—seeming to imitate the summer temperature in its deferred commencement and sudden rise.

The *less range of London temperature* is a most important fact. It belongs not so much to our lesser heat by day as to our greater heat by night; it means that London, though in the daytime somewhat less sunned than the outlying districts, sustains by night a considerable excess of temperature, with that more continuous activity of chemical decomposition which such an excess implies; and this influence is the more important, as it is predominantly felt in those low alluvial districts, where the material for decomposition is most rife.

Of *fog*, *mist*, *and haze*, in their mere hygrometrical relations, we know nothing to affect life; but it is requisite to remember that when these hang over districts of London—condensed in their ascent from the "simmering" river and filth-sodden soil—they represent not mere clouds of aqueous vapour, but, too probably, other products of terrestrial exhalation, delayed in their transit to space and withheld from the diffusion they had commenced.

The great *predominance of calm* was doubtless of baneful effect. It is a familiar matter, not only for households but for countries, that free ventilation is an indispensable condition of health. As individuals suffer their temporary inconvenience, when detained in unventilated rooms, and grow poisoned and cachectic if such be their habit of life; so, in recesses of the earth's surface, where, amid great mountain-chains, the ponded air lies unruffled by free whirls of wind, whole communities abort in the stagnant atmosphere, and beget a cretin, goitrous population. In respect of local impurities and the mischief they may engender, no miasm can survive an adequate commixture with air; and in relation to the severity of epidemic disease, it is impossible to doubt that the more or less vehemence of aerial circulation is a variant of the utmost importance.

As leading results of Mr. Glaisher's inquiry, two facts stand in relief:—

1, that the year 1854, and other years when cholera has prevailed, have had their marked meteorological characters, the general tendency of which has been to render

the season defective in those atmospheric changes which renew the purity of air;

2, that these characters, apparently so definite in their meaning, are in their kind such as to prevail with greatly increased development in those low levels of London where all visitations of cholera have most cruelly pressed; for high barometric pressure, excessive night-temperature and hazy air, with absence of wind, of ozone, and of electricity, would all (as the station-tables show) appear in their most marked degrees throughout those alluvial districts:

And, when these two statements are compared, it seems probable that in the atmospheric conditions of the year (or in some unknown influence essentially joined with them) there has been an important factor for the problem of that epidemic mortality.

In concluding our account of Mr. Glaisher's researches, we would submit one more extract from his paper:—

"I have little hesitation in saying, that were the meteorology of our towns carefully ascertained and collated with that of the metropolis, and both together with that of the country generally (of which last I have a foundation of many years' continuous observations) that in a short time we should be in a condition to elaborate a clear insight into the meteorological causes of cholera, influenza, and many phases of disease which now burst upon us with the suddenness and devastating power of a divine and wrathful visitation."

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(2) SPECIAL EXAMINATIONS OF THE ATMOSPHERE were to some extent conducted, both chemically and microscopically, with the object of determining, in reference to the causation of the disease, whether any peculiar organic forms or unusual chemical products could be detected, either in the general air of an infected locality, or in its sewer-gases, or in the immediate atmosphere and exhalations of the sick.

The spot chosen for these observations was in the low-lying district of St. Olave, Southwark, where the epidemic had great prevalence, and where the wards of St. Thomas's Hospital gave the observers every facility for the requisite access to infected persons. The experiments were conducted by Dr. Thomson and Mr. Rainey, whose respective reports are contained in our Appendix, and whose method consisted in examining (the former chemically, the latter

miscroscopically) certain quantities of distilled water and of sulphuric acid, through which had been drawn, by a well-devised suction-apparatus, large volumes of the air which it was wished to investigate.—

From these examinations appeared as follows, viz.:—

(i.) In the atmosphere of a ward filled with cholera patients, while the disease was at its height, there were diffused various substances; some not possessed of life—the familiar dust of an inhabited room,—minute hairs, particles of smoke, epidermic scales, vegetable fibres of different kinds and colours, starch granules, &c.; others distinctly having life, and showing growth or movement. Of the latter, Mr. Rainey reports, that they had the appearance of small flocculent masses, visible to the naked eye, in the fluid in the bottom of the vessel; that, examined at the same time by Dr. Thomson and himself, they were found to consist of the mycelia of fungi, apparently in an active state of vegetation, mixed with the dusty impurities before mentioned; that he could discover no appreciable difference between these growths and the mycelia of fungi which had formed in solutions of vegetable substance after exposure to the air where no cholera was present; that besides the fungi, there were extremely minute, colourless, indistinctly beaded fibres (resembling in their general character that form of *Vibrionia* called *bacterium*) so abundant as to cover some of the larger branching fibres of the mycelium; and that these he does not recollect to have seen on mycelia growing in astringent vegetable solutions prepared for the purpose of producing fungi. Dr. Thomson adds, that this air gave very evident chemical signs of containing organic matter.

(ii.) In the atmosphere of a ward, only partially filled with cholera patients, when the disease was very much on the decline, the dead and living form, separated by filtration, were of the same kind as those just described; but the vibrionic fibres were much less numerous than in the former observation.

(iii.) In a third examination, made when cholera had left the district, and when, consequently, the ward was empty, dust particles were found, with the mycelia of fungi in considerable abundance, and apparently in active vegetation; but Mr. Rainey, with very careful search, could find no trace of the vibrionic forms.

(iv.) In the external atmosphere adjacent to the hospital, various dusty impurities were observed, as also sporules

and fungi to a considerable extent, but no vibriones. The collection of this air for examination began on the 21st October.

(v.) In air collected from within a sewer during twenty-seven days, beginning 22d November, there was found less dusty admixture than in the upper air. Vibriones were seen in much larger quantity than in any of the previous specimens, traversing the field of the microscope with great rapidity, and fungi were also present.

(vi.) The water through which sewer-gas had passed was strongly alkaline with ammonia, the result of organic decomposition; while the other specimens gave a powerful acid reaction, apparently from products of combustion contained in them.

The above results of a local examination have, at present, little more than a negative interest. The presence of fungi and their sporules in the atmosphere, appears to have had no relation whatever to the proximity of cholera patients, to have continued in the ward when vacated of all inmates, and to have been found in the exterior air (including that of the sewer) when cholera had long ceased to be epidemic in the district and in the metropolis. More importance might at first sight seem to belong to the presence of vibriones in the air, especially since they diminished when cholera patients were fewer in the ward, and vanished when no such patients remained. Yet, considering what is generally known of the habits of these peculiar forms, we cannot conclude that this was an essential coincidence. The development of vibriones is intimately related to animal decomposition; and the discovery of their profuse existence in sewer-gas, when cholera was no longer present, illustrates how they may multiply in an air that is loaded with organic miasm. These creatures are far too habitually about us for it to seem probable that they represent any new element in the causation of disease. We suspect that their diffusion in the cholera ward at the time of Mr. Rainey's first examination (when that ward contained its full complement of patients) simply indicated that the air was profusely charged with animal exhalation; that subsequently their existence varied with the number of persons occupying the ward; and that under similar conditions of temperature, ventilation, and cleanliness, they would have been equally abundant, though the inmates had been suffering from other disease.

We cannot pass this very interesting part of our subject without expressing our regret that researches of so much importance could not have been commenced at an earlier date, and been made more comprehensive in their scope. Many reasons will appear in the course of our report, to justify us in saying that a very complete and exact inquiry into the chemistry of organic decomposition during the epidemic prevalence of cholera—especially into the successive transformations of animal refuse at such times—might furnish all-important information as to the characteristic poison of the disease. It was with this belief that we suggested the necesssity of submitting sewer miasm to examination; but from circumstances, quite beyond control, this part of the inquiry could not be undertaken till the time had passed for finding in it the solution we desired.

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3. Valuable information relative to local atmospheres has been gathered from other sources than direct chemical analysis. Nine sanitary inspectors were employed to visit all the localities most severely affected with cholera; and Dr. John Sutherland, late Superintending Inspector of the Board, has compiled from their statements an account, which is already before Parliament.*

From Dr. Sutherland we learn that "all the inspectors agree in stating, as the result of their experience, that in those districts where cholera had become localised they found it connected with obvious removable causes"—these causes, so far as relates to the present subject, being such as affect the purity of the air, by loading it with the miasms of decomposing organic matter; that in considerable parts of the metropolis "there are large masses of population dependent for their drainage on open ditches, tidal ditches, old badly-constructed sewers, and still worse house-drains,—the result of the whole being that the *excreta* of a large part of the metropolis are not conveyed away, but are left to putrefy and rot in the open air, in cesspools under houses, or in large underground sewers, always generating foul gases, which are poured out into the streets or into the houses while in the more open districts the exhalations from the ditches keep the atmosphere in a con-

* Letter of the President of the General Board of Health to the Right Honourable Viscount Palmerston, accompanying a Report from Dr. Sutherland on epidemic cholera in the metropolis in 1854; presented to both Houses of Parliament by command of Her Majesty.

stantly malarious condition;" that "the sewerage of all the localities is in a most imperfect condition;" that "a great sanitary evil incident to districts south of the river is the reflux of the tide through the sewers at high water, not unfrequently flooding the basement of houses, and in some parts rising in the gully-grate up to the level of the street;" that from the discharge of our metropolitan sewage into the river, "the banks at low water are in a filthy state" and "the exhalations from these pestilential banks of mud under a hot sun are most injurious to the purity of the air;" that "the pavement of courts and alleys is in general very defective, their gutters and those of the smaller and poorer streets badly constructed, retaining foul water before the doors of the houses;" that "the narrow back streets are very imperfectly cleansed, and the courts and alleys at ordinary times entirely neglected;" that bitter complaints are made of the non-removal of house refuse, which, with its various organic contents, is liable, for want of a properly organized system of dustmen, to remain as a nuisance for weeks; that in large districts of the metropolis there is "total absence of any comprehensive plan of laying out ground for building purposes, with a neglect of all arrangements for ensuring a free circulation of air round dwelling houses, and the existence of numerous narrow overcrowded courts and alleys, many of them mere *culs-de-sacs*;" "that in all the poorer classes of dwellings the means of ventilation are defective in the highest degree, the accommodation wretched and confined, the houses overcrowded and badly lighted, intermingled with private slaughter-houses, cowhouses, stables, pigsties, and public privies;" that "cellar dwellings are still in use in many parts of the metropolis;" that "the influence of noxious trades and nuisances in predisposing to attacks of cholera has been observed during the late epidemic;" dust-contractors, bone boilers, and gluemakers being specially mentioned, with "the effluvia proceeding from filthy stables, cowyards, and pigsties," and instances being referred to where whole families, exposed to such agencies, have been attacked, and in part fatally, by choleraic disease.

In contrast to the above characteristics of places in which cholera has prevailed, Dr. Sutherland refers to the condition of certain spots that have enjoyed a disproportionate immunity from the disease, apparently in result of sanitary amelioration. He cites instances where, at low levels in the metropolis and amidst a general infection of

the district, such an exemption has been enjoyed by particular groups of houses, which in former visitations had shared an equal lot with the rest, but which in the interval since those visitations, at the suggestion of a fatal experience, had had removed from within their atmosphere certain definite sources of contamination.

In the same sense it is reported that in common lodging-houses, formerly (by reason of overcrowding and filth) the fields of rich harvest for choleraic death, there has now, under that system of police-regulation which is due to Lord Shaftesbury's Act, appeared so little evidence of such infection, that Dr. Sutherland thinks it "necessary to make some allowance for imperfect information on the subject."

Similarly, among 2,791 persons who, at various points of the metropolis, were living under the auspices of societies for improvement of the dwellings of the poor, only six deaths by cholera occurred; of which one seemed due to an exterior infection, one to a poisonous supper of stale crab, and the remainder (which were all in a single family) to some cause evidently of most limited operation.

The cases of Bethlem Hospital and the City House of Occupations, on which Mr. Lawrence has favoured us with a memorandum (Appendix, No. V.) give their share of testimony to the same effect: these well-regulated establishments, with their population of 700 persons, having suffered no death from cholera while it prevailed most intensely around them.

The instance of the city of London, with 130,000 inhabitants, suffering in the late invasion 71 per cent. less cholera mortality than in 1849, is another illustration which Dr. Sutherland might have adduced of the epidemic lessening its pressure on a population in proportion as the sources of atmospheric impurity have yielded to sanitary improvement.

B.—*The Water-supply of London.*

Inquiries into the water-supply of houses and districts suffering from cholera have been conducted under two heads, respectively *Chemical* and *Microscopical*.

1. The first of these, entrusted to Dr. Thomson, has given results, the sum of which, as regards our supply by the great trading companies, is embodied in the following table:—

GRAINS of ADMIXTURE per Gallon in WATERS of the under-mentioned COMPANIES.

——	I. Lambeth Company.	II. Grand Junction Company.	III. West Middlesex Company.	IV. Chelsea Company.	V. Southwark and Vauxhall Company.	VI. New River Company at New River Head.	VII. New River Company supplied to Soho, from a well near the Hampstead Road.	VIII. East London.	IX. Kent Company.
1. Organic Matter -	1·390	1·920	2·080	5·410	3·640	2·330	1·980	1·940	1·480
2. Silica - -	·350	·090	·520	1·511	·240	·180	·780	·320	·420
3. Sesquioxide of Iron, Alumina, and Phosphates - -	·215	·730	·460	·639	·460	·400	·210	·520	·130
4. Carbonate of Lime -	10·144	8·870	9·919	9·550	10·700	11·985	7·085	11·997	9·540
5. Sulphate of Lime -	2·149	2·368	2·109	6·432	3·179	1·326	1·523	·897	3·085
6. Chloride of Calcium -	—	—	—	—	1·363	—	—	—	—
7. Nitrate of Lime - -	trace	trace	trace	trace	·076	trace	trace	trace	trace.
8. Carbonate of Magnesia	0·592	0·720	·720	·438	0·500	·855	1·185	·743	·210
9. Carbonate of Soda -	—	—	—	—	—	trace	4·909	—	—
10. Sulphate of Magnesia	—	—	—	1·390	—	trace	trace	trace	trace.
11. Chloride of Magnesium	·617	·542	·360	1·947	2·101	trace	—	·237	·949
12. Sulphate of Potash -	·730	·553	·577	2·903	2·413	·884	1·266	·682	1·153
13. Sulphate of Soda -	—	—	—	—	—	—	8·051	—	—
14. Chloride of Sodium -	·966	·947	1·637	29·797	16·001	2·355	7·807	1·125	·874
Total - -	17·153	16·740	18·443	60·017	40·673	20·315	34·796	18·461	17·841
Residue by Evaporation	17·440	16·920	18·970	60·170	41·780	20·780	35·050	18·300	17·760
15. Carbonate of Ammonia	·064	—	—	—	·840	—	—	—	—

In regard of the mineral impregnations shown in the above analyses, two features deserve particular notice; first, in all the specimens, that large quantity of lime-salts to which London water owes its costly and inconvenient property of *hardness*; secondly, in the two waters (IV. and V.) which are derived from the Thames at Battersea, the remarkable evidence given by chemical analysis, that an admixture from the sea reaches to this distance inland, and renders the river *brackish* with chloride of sodium.

Much significance belongs to what is set forth in the first line of the table—the proportions of *organic matter* dissolved in the several waters. Here great differences present themselves; for, as we compare together, in the first five columns, those waters which are drawn from the Thames, we observe that the admixture of such matter increases from 1·39, where the source of supply is at Thames Ditton, to 5·41, where it is at Chelsea. With the fact of this increase we remark that other materials, nitric acid and ammonia, have simultaneously risen into sensible quantity,—materials which are derived from the conversion of animal products. Without anticipating the demonstration presently to be founded on the microscopy of these waters, and even setting aside our knowledge of where the sewers disgorge into the river, it is already easy, on chemical evidence, to say that the Southwark and Vauxhall and the Chelsea Companies, pump their supply from a source profusely contaminated with the refuse of animal life.

With respect to water-supply generally, nothing can better prove a liability to foreign admixture than any *uncertainty of composition*; and Dr. Thomson, having for the most part made repeated analyses of waters severally purporting to be one in kind, observes, that the supply of each company, examined at different times, shows extensive fluctuations of impurity. The following table exhibits the range of such differences, as noted by him; and he adduces these facts to explain how widely his own report differs from that* on which was founded the legislation of 1852,—the latter (he says) deducing its conclusions from samples of water drawn under too favourable circumstances, and strangely representing, as least contaminated with foreign

* Report on the Chemical Quality of the Supply of Water to the Metropolis, by Professors Graham, Miller, and Hofmann; presented to Parliament by Her Majesty's Command. 1851.

matter, those which were taken nearest to London and most under influence of the tide.

GRAINS *per* GALLON of FOREIGN ADMIXTURE in the under-mentioned COMPANIES' WATERS.

Lambeth.	Southwark and Vauxhall.	Chelsea.	New River.	East London.	Kent.
12·12 to 17·98	22·50 to 72·66	36·96 to 65·66	15·75 to 35·05	17·02 to 19·60	15·02 to 21·10

2. The microscopical examinations, conducted by Dr. Hassall, form a necessary supplement to the chemical inquiry; and in referring to his results we will only premise that water, scrutinised with the highest magnifying power, reveals, if pure, no visible shapes whatever, and that consequently all such shapes discoverable by the microscope do, in their several kinds and grades, constitute a foreign impurity.

Having examined many specimens of water obtained from houses wherein one or more of the occupants had suffered from cholera; such water being supplied by the Southwark and Vauxhall, the Lambeth, the Kent, the East London, the New River, the Grand Junction, and the Chelsea Companies; Dr. Hassall reports among his results—

That the whole of the numerous specimens of water subjected to examination contained organic matter, dead and living, animal and vegetable; that the quantity and kinds of organic matter varied considerably in different cases, but were usually more or less constant for the same water; that the waters which contained the greatest number and variety of productions, dead and living, were from houses supplied by the Southwark and Vauxhall Company, that these abounded in living animal and vegetable forms of different genera and species; containing also a large quantity of dead organic matter, amongst which were frequently to be detected fragments of the husk of wheat, hairs of the same, starchy matters of different kinds, cells of potato and other vegetable tissues, with, in some cases, fragments of altered muscular fibre—these latter structures and elements being undoubtedly derived from the fæcal

matter contained in the sewage; that the same species of organic productions were present in the waters obtained from houses supplied by the Chelsea Company, although in greatly diminished numbers—a result which might have been expected, since this company derives its supply from the same part of the Thames as the Southwark and Vauxhall Company, but filters it before delivery; that the water procured from houses supplied by the New River, Grand Junction, and East London Water Companies, but especially the two former, all contained a great many organic productions; that the water obtained from houses supplied by the Lambeth Company contained fewer organic productions than any of the rest; that amongst the productions present in the water of the Southwark and Vauxhall, the Chelsea, and the West Middlesex Companies are several which are found only in brackish waters; that the specimens taken from cisterns supplied by the Southwark and Vauxhall Company were very impure indeed; far more so than any of the other waters examined, some of them being demonstrably contaminated with fæcal matter.

Most of the above expressions describe the respective waters as drawn for domestic consumption, from the various butts and cisterns to which the companies supply them; and as these receptacles for the most part favour the development of infusorial life, at the expense of dead organic matter, it may be assumed that the same waters drawn from mains or service-pipes might have presented larger quantities of dead tissue or excrement, and less maturity of living organisms. It hardly needs to be remarked, however, that butts and cisterns (however objectionable may be their use) cannot originate animal or vegetable growth; and the derivation of such products is made obvious by Dr. Hassall's statement of what he observed in proceeding to the very sources of supply, and examining the waters of the Thames, the New River, and the Lea; viz., that organic matter, both dead and living, animal and vegetable, was present in very considerable amount in the whole of these waters; and that the living forms were discovered in considerable numbers, not merely in the deposit, but in nearly every drop of each of the waters, after they had stood the usual time, and after all but the lightest solid matter and the most active living infusoria had consequently subsided.

3. Both Dr. Thomson's and Dr. Hassall's inquiries have

extended to the examination of many well-waters in and about London.

In respect of superficial wells—those common sources of pump-water in the metropolis and elsewhere, the testimony now given strongly corroborates all that has frequently been urged as to the dangerous nature of such a supply. Both observers, though from different points of view, discover in these waters just such qualities as might be expected, from their having filtered through a porous soil, full of organic impurities,—that they contain sometimes evident sewage matter, sometimes an abundance of nitrates or of ammonia derived from the decomposition of animal substances, sometimes a variety of those animal and vegetable organisms which attest the progress of decay.

The deep well-waters, in respect of organic contamination, are usually in strong contrast to these; and in many of the specimens examined by Dr. Hassall there were no traces, or barely any, of infusorial life. Where such were found, their presence was to be accounted for by reference to special circumstances; by a communication of the well with some adjoining pond, or by its having had refuse wilfully thrown into it. But for these influences and the like, Dr. Hassall believes, "that scarcely a single organic production of any kind would have been found in any one of the (deep) well and spring waters subjected to microscopical examination." The mere absence of such productions does not in itself establish the fitness of water for drinking, since there may remain various mineral admixtures to render it inconvenient or unwholesome; but both Dr. Thomson and Dr. Hassall refer with praise to the case of Woolwich, as one where a large population is supplied with water which presents an entire freedom from infusorial life, and which, though directly derived from the chalk formation, is artificially softened to about half the hardness of our Thames supply.

From such results, chemical and microscopical, as we have quoted from Dr. Thomson's and Dr. Hassall's reports, still more from their details, for which we must refer to documents printed in our Appendix (Nos. VII. and VIII.), it is evident that the commercial water-supply of London is derived from impure sources.

That furnished by the Lambeth Company is the best. Being taken from the river at Thames Ditton, it fulfils the requirements of the Metropolitan Water Act, and illustrates what after the present year will be supplied, with the sanction of the Legislature, to the greater part of the Metropolis. It is the best, but it is not good. Even in it Dr. Hassall finds "in not inconsiderable numbers, organic productions dead and living, animal and vegetable;" even in it Dr. Thompson finds traces of nitric acid, enough ammonia "to indicate an intermixture of sewage," and such proportions of organic matter as ought not to be insignificant to an educated community.

After the Lambeth Company follow, in the order of deterioration fixed by Dr. Thomson's analyses, the Kent, Grand Junction, East London, West Middlesex, New River, Southwark and Vauxhall, and Chelsea Companies. The last two greatly surpass the others in badness, and between themselves there is this difference:—while both draw from that part of the river, where the water is brackish from marine tides, and where an immense infusion of sewage proceeds uninterruptedly, the Chelsea Company seems to have the worse source for collecting, the Southwark and Vauxhall the lesser care for distributing its supply. Whether because of some greater influence of the tide along the northern side of the river at Battersea, or because of more sewage being discharged on that than on the southern side, the Chelsea water shows a much greater amount of dissolved impurities, but (apparently as the result of filtration) far fewer visible forms; while in the Southwark and Vauxhall water this evidence of unfiltered contamination reaches its highest degree, revealing to the microscope, not only swarms of infusorial life, but particles of undigested food referable to the discharges from human bowels.

It likewise appears from the evidence before us that the superficial wells of London afford a supply which, though often preferred for beverage in houses subject to the payment of water-rate, is generally not superior in quality to that distributed by the companies, and is liable to an aggravated form of the same contaminations.

Such having been the qualities of water consumed in the various parts of London during the late epidemic, it remains for us to examine how far the consumption of

these waters may have influenced the severity of cholera in London.

With respect to the *mineral ingredients* set forth in Dr. Thomson's analyses—although every grain of such admixture represents a deviation from the absolute purity of water—we have no reason for ascribing to them any appreciable influence in the matter adverted to. Considerable quantities of these mixed salts might be taken at a dose without producing serious injury to health; and we have no knowledge that, from their habitual consumption in small daily doses, there results any such chronic ill-effect as we might consider a probable predisposition to cholera.

With respect to the *living animal and vegetable forms* traced by Dr. Hassall through the whole series of waters, there seems no evidence that they, by their own action on the human body, could be productive of choleraic symptoms. There are indeed many instances, human and brute, of disease engendered in the living body through the tenantry of parasitic organisms, animal and vegetable; and, for aught we know to the contrary, many of the creatures described by Dr. Hassall may be capable of sustenance and multiplication within the bowels of those who swallow them. But in every known case where it can fairly be presumed that parasites are the causes of disease, they exist as a palpable morbid product occupying some considerable share of the affected body. The silkworm destroyed by *muscardine* dies because its whole body is riddled with parasitic vegetation, so dense that at last a mere heap of mould remains in place of the absorbed and disorganized animal; and every molecule of that mould makes evident the nature of the destructive process. What we know of parasitic diseases in the human subject—of hydatids and porrigo, for instance—tends all to the same point: in whatever way the foreign occupant have proved hurtful, whether it have starved the proper substance of the body on which it was graffed, or have provoked particular textures to acts of inflammation, or have choked their functions by its pressure,—itself, the causative thing, remains as a material shaped body, susceptible of ocular demonstration, side by side with its effects, and having bulk proportionate to them. Analogy would, therefore, lead us to infer that parasites could produce no attack of exhaustive purging and vomiting, except by having first

along the digestive canal multiplied to such swarms that they would be obvious to the most casual observer, both in the discharges of living patients and in the subsequent examination of the dead.

So far as this argument renders it improbable that the metropolitan water-supply produced cholera by means of the infusorial life engendered in it, the conclusion is strengthened by further remarks of Dr. Hassall's:—"Of the (living) organic products discovered in the waters examined, the great majority belonged to species which are known, and which have been long described in systematic works; and since the greater number of these are present in these waters at nearly all seasons, and since they are, therefore, constantly consumed, it is clear that they are in no way concerned in the production of cholera;"—"the number of those, the names of which were not known or their nature undetermined, was not considerable; and of these there was no one common to all the waters obtained from cholera houses which could be supposed to be influential in any degree in the production of the epidemic."

We do, however, attach very great importance to the fact, that nearly all the waters consumed in London, show a remarkable aptitude to develope low forms of animal and vegetable life; but this importance belongs, in our judgment, not to any direct influence exerted by such organisms on our own, but to the indications which their development affords that the waters wherein they grow are fraught with dead organic impurities.

The admixture of *decomposing organic matter* in the water-supply of the metropolis being attested equally by chemical analysis and by the microscopical evidence just adduced, we do not hesitate to speak of this contamination as one that may have exercised great influence on the spread of cholera among the population. The general history of this disease establishes its infinite preference for localities that are fœtid with organic impurity; and it is impossible to conceive either any specific chemical changes arising in the air of a district, or any morbid action excitable by it in the living body—such changes or such action being due to its contamination by dead organic admixture—without recognizing that the water of the district likewise—great solvent of air as it is—must, if similarly polluted, be liable to undergo the same alteration, and to originate the same effects, as those of the atmosphere around it.

The present state of scientific knowledge does not justify dogmatic assertions on this subject; but there are reasons for believing, in respect not only of cholera, but of many kindred diseases, that the means and agencies of morbid infection stand in intimate relation to decaying animal products within and without the body; and the slightest taint of organic decomposition within the drinking water of a large population, therefore, constitutes a danger, which we cannot but regard with as much alarm as disgust.

If, in concluding our present section, we review the discussed materials in their bearing on the general ætiology of cholera, we think that the facts recorded enlarge the basis of previous knowledge.

The doctrine of epidemic cholera which has gained almost universal acceptance, does not affect to explain what may be that power—the exciting cause of the epidemic manifestation—which at intervals of time has forayed from place to place about our globe, sometimes vaguely spreading over a widened area, sometimes seeming to move in more defined procession, and which now for the third time has shed its fatal influence on our land. But with this mystery still unsolved, there has grown more and more into shape a doctrine which is both intelligible and practical;—that the undiscovered power in its wanderings acts after the manner of a *ferment*, that it therefore takes effect only amid congenial circumstances, and that the stuff out of which it brews poison must be air or water abounding with organic impurity. Taking this as hypothesis, and testing it by the facts before us, we find that it would include and explain them.

Either in air or in water, it seems probable that the infection can grow. Often it is not easy to say which of these media may have been the chief scene of poisonous fermentation; for the impurity of one commonly implies the impurity of both, and in considerable parts of the metropolis (where cholera has severely raged) there is rivalry of foulness between the two. But, on the whole evidence, it seems impossible to doubt that the influences, which determine in mass the geographical distribution of cholera in London, belong less to the water than to the air.

In our statistical section it appears that gradual exemption from the epidemic mortality "has more nearly followed the degrees of elevation of soil than been proportionate to

any other general influence we could measure." But in this lessening scale—varying from a death-rate of 156 beside the river to one of 10 at the highest levels of London—there are so many and so considerable exceptions, as to show that the low level invites the epidemic invasion by reason of some attribute of its position which may elsewhere equally abound. Such an attribute is the excess of organic impurity, which (from the relation of the river to our London drainage) habitually saturates those alluvial parts; but which, through sanitary mismanagement or personal neglect, may prevail against the intentions of nature at the highest levels in the land.

Even among the best-placed parts of London there are districts so habitually foul and unwholesome, that a spectator might wonder whether by any result of tidal drainage the southern flats of the Metropolis can have been rendered more fœtid and poisonous than they. But the meteorological history of London here comes to our aid; explaining how, even if there be equal filthiness in all districts, the poisonous consequences of filth must be inverse to the elevation of soil. For on the supposition (which this result greatly confirms) that the choleraic infection multiplies rather in air than in water, meteorology explains how the balance of healthfulness is weighted in favour of the higher levels, by their less participation in the high night-temperature of the metropolis, by their comparative clearness from mist, and above all by the curative resources of more free ventilation.

Supplementary to the Second Section.

REMARKS ON THE OUTBREAK OF CHOLERA IN SOHO.

One local outbreak of the epidemic—that which befel the neighbourhood of Berwick Street—was of such severity as to suggest that some especial causes must be concerned in its production. The circumstances of this remarkable attack have been investigated in a house-to-house visitation of the affected locality; and the results of that inquiry are stated in a Report (App. No. IV.) by the three gentlemen who conducted it—Dr. Fraser, Mr. Hughes, and Mr. Ludlow.

In the three registration sub-districts of St. Anne's, Golden Square, and Berwick Street, together comprising a population of 42,000 persons, it appears that there

occurred 537 deaths from cholera; being at the rate of 128 to every 10,000 inhabitants, while the general cholera rate of the Metropolis was only sixty to the same number.

This high mortality was the more remarkable, as the affected districts are not situated at a low level, nor disproportionately inhabited by a poor population.

A striking feature of the outbreak was its extreme suddenness, as measured by the large number of persons almost simultaneously attacked. Its greatest local diffusion appears to have been reached on the second, if not on the first day, from its commencement; it remained of equal prevalence for two days, and on each of the two following underwent a decline of 50 per cent.

In respect of this explosive appearance of the epidemic, it deserves particular mention, that for some time the district had been enjoying an exemption from disease quite out of keeping with its sanitary condition. Influences, universally recognised to be causes of disease, had been present, but in a manner for which we are unable to account had remained almost inoperative; so that, till the very eve of this dreadful outbreak, the district might have boasted itself as one of average healthiness.

This fact is one of so much interest and importance that we have thought it advisable to have it thoroughly investigated by an inquiry into the mortuary statistics of the last seven years. The death-rate of the district during this period, if we could exclude from it those few days of epidemic visitation, would have been only $20\frac{1}{2}$ per 1,000 per annum; which, though far above what sanitary science can ensure to well-regulated districts, is considerably below our average metropolitan death-rate. Zymotic diseases, too, had made less than $\frac{1}{22}$ of this total.

It likewise deserves mention, that of the 537 cholera deaths of the late epidemic, 323 occurred in houses which, during the past seven years, had suffered no deaths from other zymotic disease; and in comparison especially with the slight visitation of cholera which this district suffered in 1849, we discover that out of 86 houses in which cholera deaths then occurred, only 10 re-appear in the list of 310 houses in which cholera deaths occurred in the late epidemic. Since these results of our inquiry differ from what has been generally believed of the habits of the disease, we append (*see* p. 119) in detail the tables on which they are founded.

With respect to the causes of this particular outbreak, we find no apparent exception to the conclusions arrived at in the preceding section of our Report. Anticipating that for such an epidemic prevalence of cholera (whatever may have been the foreign influence that excited it) there must have pre-existed a certain local state of uncleanliness with putrefiable matters, we trace no anomaly in the visitation.

That such local uncleanliness prevailed most intensely throughout the suffering districts, is evident from the reported results of house-to-house visitation. The exterior atmosphere was offensive with effluvia from ill-conditioned sewers; the houses were almost universally affected in the same manner, partly from the same source, partly from their own extreme defects of drainage and cleanliness, partly from unregulated slaughtering and other offensive trades; the inhabitants were overcrowded, perhaps to the greatest degree known even in London; and the general architecture of the locality was such as to render it almost insusceptible of ventilation.

On the principles to which we have referred, and which we believe to be commonly recognised as presenting the most probable theory of choleraic irruptions, it will be obvious that the locality, notwithstanding its high level, contained every predisposing condition which (given the exciting cause) should render it prone to a violent epidemic explosion; and we believe that any person conversant with the laws of disease might have predicted its extreme liability to suffer what afterwards befel it.

Why, however, this district should have suffered in marked disproportion to many other districts, hardly, if at all, superior in their sanitary arrangements; or why, generally, it should be the tendency of cholera in its visitation to select particular foci for extreme outbreaks, instead of diffusing itself more equally over all ill-conditioned districts, is a difficulty which hitherto we have no scientific material to solve.

The meteorological conditions of the district were not minutely examined at the time of the attack: but the Reporters' mention of an atmospheric haze, and of a singularly stagnant, sultry, and oppressive air, leads us to believe that, if scientifically observed, they would have been found accordant with the generalizations in Mr. Glaisher's Metropolitan Report.

In explanation of the remarkable intensity of this outbreak within very definite limits, it has been suggested by Dr. Snow, that the real cause of whatever was peculiar in the case lay in the general use of one particular well, situate at Broad Street in the middle of the district, and having (it was imagined) its waters contaminated with the rice-water evacuations of cholera patients.

After careful inquiry, we see no reason to adopt this belief. We do not find it established that the water* was contaminated in the manner alleged; nor is there before us any sufficient evidence to show, whether inhabitants of the district, drinking from that well, suffered in proportion more than other inhabitants of the district who drank from other sources.

There is mentioned, however, a remarkable instance in which it seems probable that the water of this well did really act as a vehicle of choleraic infection; but (assuming the absence of fallacy in the case) this probability might easily be admitted, without its therefrom resulting that infection depended on the specific material alleged. The water was undeniably impure with organic contamination; and we have already argued that, if, at the times of epidemic invasion there be operating in the air some influence which converts putrefiable impurities into a specific poison, the water of the locality, in proportion as it contains such impurities, would probably be liable to similar poisonous conversion. Thus, if the Broad Street pump did actually become a source of disease to persons dwelling at a distance, we believe that this may have depended on other organic impurities than those exclusively referred to, and may have arisen, not in its containing choleraic excrements, but simply in the fact of its impure waters having participated in the atmospheric infection of the district.

Third Section.

Practical Pathology.

One of the earliest duties which devolved on us, as a committee for the scientific purposes of the Medical Council, was to suggest means for collecting and utilising

* Dr. Thomson and Dr. Hassall examined this water, though not at the moment of the outbreak, and their account of it will be found in their respective reports.

the results of detailed medical experience in its individual circles of practice. For it was hoped that, in comparing together the contributions of many independent observers we might educe trustworthy information on various disputed particulars.

We accordingly prepared for circulation among members of our profession the Forms (A. and B.) already adverted to. These were so constructed that the returns, when complete, would inform us not only as to the stages of the disease, their duration, fatality, and relative frequency, but likewise as to each respondent's personal experience in the application of different remedies.

We now account in full for the mass of valuable information received in reply to this appeal. All that related to the history of the epidemic, irrespectively of medical treatment, has been extensively used in the statistical section of our Report, and has furnished material for our first series of Tables. The remainder, recording the therapeutical experience of more than 300 contributors, and representing in this respect an unprecedented multitude of authentic evidence, was deemed of sufficient value to justify its being referred to a special section of the Medical Council, whose report on the results deduced from it is already before Parliament and the public.

There was another class of contribution which we were most anxious to encourage. However painful the truth, we could not disguise from ourselves, as to the advanced stages of cholera, that nearly all resources of medicine seemed equally powerless for good; that practitioners, summoned to the relief of this dreadful disease, could scarcely decide between any two lines of treatment except for an occasional certainty that one of them would positively harm; and we felt deeply assured that—failing some happy chance of empiricism, there could be but one escape from this helpless condition of our art. We believed that the varieties and irresolution of practice corresponded to the actual unripeness of pathological knowledge, to the absence of those scientific principles which alone could give secure and uniform guidance: for, how futile to discuss the hopefulness of this or that experiment in treatment, while our profession was still unfixed as to what action in the suffering body it should be the definite aim of medicine to effect! If better success was to be won, we thought it must be through stricter studies in the nature of the disease, through learning as it were the mechanism of

its production, and appreciating, fact by fact, the full significance of all its symptoms.

During the preceding epidemic, both here and especially on the Continent, researches of great interest and importance had been carried on. Yet a vast deal remained for inquiry; and we hoped it might be conducive to good, that this state of the case should be represented to persons who were likely to take part in scientific investigations.

We accordingly suggested "that there still remain in the pathology of the disease many undetermined particulars; and that in respect of these, great assistance would be given if the physicians of London hospitals, and other officers of public institutions within whose province it falls to be cognizant of such matters, would direct their attention to specific pathological questions, and would communicate to us the results of their inquiry." Being then requested to specify those problems in the disease which we considered most deserving of study, we prepared a *Memorandum* on the subject which was transmitted to the chief medical officer of the various hospitals and infirmaries in the metropolis.

The epidemic, however, had already passed its climax, and soon rapidly declined and ceased. There was consequently little time or opportunity for new scientific observations; and although some of the problems proposed might have been elucidated by the results of experience already gained, no replies were elicited from public institutions, except *St. Bartholomew's*, *St. Thomas's*, and the *Middlesex* Hospitals, and the *City of London and East London* Dispensary. Nor, independently of our appeal, did the practical pathology of cholera during its recent visitation appear to attract any active scientific research. In the epidemic of 1848-9, new methods of investigation had been most zealously employed; and in 1853 the subject had lost its novelty, even for the younger cultivators of pathological science.

Reports bearing on different points of the inquiry were, however, received from Dr. R. D. Thomson, Dr. Hassall, and Mr. Rainey. These are printed in our Appendix, either entire or in part; and their contents, as well as other facts communicated to us, will be noticed here only so far as they tend to elucidate those questions to which our Memorandum had called attention.

1. The solution of the first question—*Through what channel does the exterior cause or poison of cholera first enter or affect the human body? is it through the lungs? or through the stomach and intestines? or otherwise?* obviously might be aided by evidence of various kinds; for example—1, by facts showing whether the atmosphere of localities infected by cholera has, or has not, properties favourable to the existence and increase in it of an organic or inorganic poisonous matter; or whether it contains or does not contain some matter of a peculiar character which might be regarded as the poison itself; 2, by evidence of the presence of such peculiar matter in the air-tubes of the lungs; 3, by facts demonstrating, on the other hand, its presence in the water drunk or in other articles of diet; and, 4, by the demonstration of its existence in the stomach and intestines, or in the discharges from them.

Some of the reports laid before us contain information relative to these several points.

(i.) The important observations of Mr. Glaisher, already noticed at length, demonstrate that not only in 1854, but likewise in 1849 and, as far as can now be ascertained, 1832, many conditions of the London atmosphere were such as would favor the retention and increase of any poisonous matter in the air, and that these conditions increased concurrently with the rise of the epidemic, and diminished with its decline. Such facts accord well with the view that the poison of cholera enters the human body through the lungs.

Researches already mentioned, of Dr. Thomson and Mr. Rainey, on the chemical and microscopical qualities of the air, in infected districts and in the immediate vicinity of the sick, have failed to advance knowledge in this particular. As was stated in our last section, they discovered in such atmospheres as they examined no ingredient which, with any plausibility, could be considered special to the prevailing disease.

(ii.) In further search for such possible foreign elements, should they have entered the body by respiration, Mr. Rainey and Dr. Hassall examined microscopically the lining membrane of the air-tubes in persons dead of cholera. Mr. Rainey, having explored the windpipes of eleven persons, as soon as possible after death, "could detect nothing like spores or the mycelia of fungi, or the vibrio-like fibres in any one of them." Dr. Hassall found vibriones abounding in the mucus of the bronchial tubes, but remarks that their presence was probably due to incipient putrefaction.

The general result of these observations, then, is negative in regard to the presence of any peculiar bodies in the air or in the lungs. For, although vibriones were found very abundant in the water through which the air of an hospital ward was passed when it was full of cholera patients, and very few or none when the ward was empty, we have already shown that in these coincidences there is no proof of vibriones having any direct relation with the cause of cholera.

(iii.) That the exterior cause or poison of the disease may enter the human body through the stomach and intestines, has been conceded by many persons who believe they can refer the causation of certain cases to the consumption of foul water, or other putrescent articles of diet; and in our last section we stated the reasons which would lead us to regard such consumption as a probable risk of empoisonment. But, as regards the most possible means of such infection—the water-supply of the metropolis—neither Dr. Thomson by chemical inquiry, nor Dr. Hassall with the microscope, has been able to identify in it any matter, special to the epidemic time or to the infected localities. Indeed (as we have already quoted) the latter observer expressly states that the very various animal and vegetable living products which abounded in the waters of cholera districts belong to species which are well known, and are to be found in the waters of districts not visited by the epidemic; that they have no especial relation with the presence of cholera; and are important only as affording evidence of the impurity of the water.

(iv.) Again, in the intestinal discharges Dr. Hassall has discovered no sporules or threads of any species of fungus, and no peculiar body of any kind, other than vibriones. Vibriones, he states, are constantly present in extraordinary numbers, not only in the matters discharged from the body, but also in the fluids taken after death from the intestines themselves, as high as the duodenum. But these bodies exist in other morbid intestinal discharges, and according to Mr. Rainey, they may be found also in the contents of the intestinal canal after death from other diseases, even when the examination is made at an early period. Their extraordinary number in the rice-water discharges, therefore, probably shows merely a great proneness to decomposition in the fluids poured into the intestines in cholera. It is questionable whether there is any connexion between the presence of vibriones in such vast number

in the intestinal discharges and the fact observed by Dr. Thomson and Mr. Rainey that the same bodies are found in the water through which the air of a cholera ward has been passed. Dr. Hassall states that neither vibriones nor the sporules of fungi rise into the atmosphere through the mere evaporation of the fluid containing them. Yet, as in a cholera ward, portions of the intestinal discharges would certainly be spilled on the floor or on the bed linen and become dry there, vibriones contained in them might in this dried state be disseminated through the air of the ward. We have already stated our suspicion that, when the scope of such examinations is extended, the presence or absence of vibriones in the air, or in the water through which it is filtered, will be found to depend on other circumstances—on the degree of crowding in the ward, and on conditions of ventilation and temperature, which would affect the quantity of organic matter diffused, and the rate of its putrefaction. But all doubt on the matter might be soon solved by parallel examinations of air obtained from rooms equally full of non-infected persons.

In reference to the possibility of infection through the gastro-intestinal membrane, we may here mention an exclusive form in which the doctrine has been urged,—to the effect that this is the only channel through which infection can occur, and that its invariable means consist in the swallowing of matters (chiefly water) specifically contaminated by the fæces of previous choleraic patients. We cannot doubt that drinking-water fouled with excrements, whether diarrhœal or healthy, would represent in a high degree those qualities of organic taint, against which we have already in general terms expressed our opinion. But, while quite prepared to admit the danger of that *class of ingesta* to which such water would belong, we can find nothing in support of the exclusive theory adverted to; and we believe, as already stated, that the geographical distribution of cholera in the metropolis, at each of its three visitations, has in its main features expressed, beyond the possibility of reasonable doubt, that its diffusion chiefly depends on other than dietetic influences.

2. In reply to the second question, "*Has the disease a "period of incubation? if so, how long? and on what is "it contingent?*" Dr. M'Loughlin remarks, that, during the prevalence of the epidemic, all persons who do not actually labour under cholera or diarrhœa suffer from a

slighter disturbance of the stomach and bowels, or from an unusual susceptibility to the action of purgative medicine. Further information is desirable respecting the latter condition, which seems sometimes to be well marked. But the knowledge which the question was chiefly intended to elicit was, whether an interval of definite or indefinite length elapses between exposure to the exterior cause of the disease and the first manifestation of its symptoms.

3. With reference to "*the communication of the disease "from person to person,*" no large body of evidence, either of a positive or of a negative tendency, has been received. In the paper communicated from the Middlesex Hospital, it is stated that in that institution "no disproportionate "liability to the disease has been manifested in those who "have been engaged in attendance on the sick, or engaged "about their dead bodies, or occupied in cleaning their "linen." "One patient and two nurses were attacked "while in the hospital." In St. Thomas's Hospital the staff of "sisters" and "nurses" consists of 19 sisters and 47 nurses. One sister, who had at first the charge of the special wards* for cholera, and four nurses not in attendance in the cholera wards, passed through the different stages into complete collapse. The sister recovered; three of the nurses died.

Mr. Whitfield, the resident medical officer of St. Thomas's Hospital, expresses his disbelief in the communication of the disease from person to person, but says he has seen many remarkable cases of the occurrence of the disease after communication with persons affected at the time or recently. He instances two cases. In one he attributes the attack to the alarm excited in the patient by her being told, when suffering from bilious diarrhœa, that the person in the next bed had had cholera—consecutive fever at the time of admission: the bilious diarrhœa almost immediately became choleraic. The other was the case of a nurse who became very sick and faint while engaged in rinsing out the sheets and linen of a cholera patient, and was attacked with cholera which rapidly proved fatal. The sheets and linen had been left unwashed from the previous day, and Mr. Whitfield believes that this case is an illustration of Professor Liebig's remarks, and that the nurse "imbibed

* It may be remarked that these wards are the least favourably placed parts of St. Thomas' hospital, and that the hospital itself stands in the middle of a district where cholera has always prevailed with peculiar severity.

"the poison, not by communication with the person, but "from the decomposed secretion."

We have already referred to a speculation, so far as it relates to the human subject, that all diffusion of cholera depends on a deglutition of some amount of choleraic fæces, conveyed from the sick to the healthy in various articles of food, and especially in water. Professor Liebig had stated as the result of experiments, that the rice-water discharges acquired at a certain stage of putrefaction the property of inducing a disease similar to cholera, in animals to which they were administered. Dr. Thomson communicates the particulars of an experiment of this kind, instituted by himself. The result was negative. But Dr. Thomson does not regard this single observation as conclusive. Moreover, it cannot be expected that the question will be settled in this way. There is no sufficient ground for thinking that the lower animals are susceptible of epidemic cholera; and the fact of diarrhœa being produced in them by such experiments as those of Liebig, would merely prove that the discharges, when putrescent, are noxious, not that they contain the particular poison by which cholera is generated in the human subject.

Some microscopic observations of Dr. Hassall on the clothes of cholera patients have an indirect bearing on the question of the mode of diffusion of cholera. For, even though the cause of cholera be not an emanation from the bodies of the sick, if it be a matter which increases in foul air, it may possibly be conveyed in the clothes of men from one locality to another. Dr. Hassall found, as was to be expected, that animal matters in which living organic products were visible could sometimes be extracted from the clothes of cholera patients; but he found no peculiar organic body. Sporules of fungi were very rare, and vibriones were abundant only in clothes which were stained with rice-water discharges.

4. The seventh question suggested was, "*Does cholera "begin as a morbid process of the gastro-intestinal mucous "membrane, or is this preceded by some state of general "poisoning which requires the gastro-intestinal membrane "to act as an emunctory? Is the state of collapse deter-"mined by this gastro-intestinal flux, and in proportion to "it, or can it arise independently of any such flux? How "are the lividity and the cramps determined and propor-"tioned?*

To the first two clauses of this question no direct answer has been received by us; yet they are of very high importance. The flux from the gastro-intestinal mucous membrane is, doubtless, the first of the more obvious phenomena of the disease, and may perhaps be in every sense its beginning; but, on the other hand, it may be that the cause of cholera affects primarily some other part of the economy, as the blood or the nervous system, and acts only through them upon the mucous membrane of the stomach and intestines; and it may be that the affection of this membrane is essentially the process by which some poison is eliminated from the body. Both views have been asserted by writers on the disease; and as they almost necessarily lead to opposite methods of treatment, it was thought desirable by this question to invite closer examination of the point.

With respect to the next clause of the seventh question, —"*Is the state of collapse determined by the "gastro-intestinal flux, and in proportion to it, or can it "arise independently of such flux?*" Some relevant information is found in the returns of cases of diarrhœa and cholera treated during the late epidemic. The number of cases of collapse reported is 1,798, and in all but six cases the collapse was preceded by one or more of the earlier stages of the disease, which are attended by intestinal discharges. More accurate inquiry would most probably have elicited the fact, that even in the six exceptional cases some intestinal flux occurred before the phenomena of collapse were developed. In some or all of the six cases the amount of liquid matter discharged may have been inadequate to account for the collapse, but, even then, there may have been poured out from the blood a large quantity of fluid which still remained in the intestines. The following is an extract from the report received from the Middlesex Hospital:—" A child about "$3\frac{1}{2}$ years of age was admitted in a state of complete "collapse, having had only one motion and having vomited "but slightly since the commencement of the disease. In "this, as in many similar instances, it may be asserted, "that the flux had already taken place into the bowel, "but had not been discharged *per anum*, for soon after "admission the child was profusely purged."

In the Middlesex Hospital report, it is stated that although in general the severity of the collapse was in proportion to the amount of discharges, it was not so in

all cases. This is in accordance with previous experience, but it needs to be determined by further observation, whether the differences in amount of the discharges producing a given effect, and the differences in effect resulting from discharges of given amount, can be referred to diversities in age, in nervous power, in tendency to syncope, in the quantity of the fluid in the body, or in other like conditions of the individual patients.

5. The eighth group of questions related to the pathology of the *consecutive fever;* and the two questions "*To what "extent does it depend on the previous occurrence of "profuse discharges? or on the completeness of collapse? "Does stupor in this stage always depend on uræmia? or "on what?*" are in some measure elucidated, or at least touched upon in the papers before us. The tabular returns of cases of diarrhœa and cholera having been drawn up for the most part after the cessation of the epidemic, of course could not be referred to with confidence for the settlement of questions relative to minute points of pathology. But there is no reason to doubt the trustworthiness of the information of a general character which they afford in regard to the connexion between consecutive fever and collapse. 874 cases of consecutive fever are reported, 249 fatal, and 625 recovered from. In all the cases the fever stage was preceded by cholera; but the presence or absence of collapse is mentioned in only 808 cases; and in 223 cases of this number, or 27·6 per cent., the cholera had not passed into collapse. Here, then, it appears that the occurrence of consecutive fever is not closely dependent on preceding collapse. But the result is very different when the 225 fatal cases, included among the 808 cases above mentioned, are examined separately: for, of these, 199 were preceded by collapse, and only 26, or not quite 11·15 per cent., by cholera without collapse; so that the severer forms of the morbid states included under the term consecutive fever, seem to be in a very large majority of cases connected with a preceding condition of collapse.

These deductions from the tabular returns are entirely consistent with the results obtained at the Middlesex Hospital, as they are stated in the subjoined conclusions extracted from the report from that institution:

(*a.*) "Nearly all those cases in which collapse was "complete and purging profuse, fell into a greater or "less degree of consecutive fever."

(*b.*) "Many cases passed into the most extreme stage "of collapse, and yet, after remaining in that condition "for many hours, recovered with the slightest degree "of consecutive fever."

(*c.*) "Of those cases which did not pass into a state "of complete collapse, several were followed by a slight "and two by a severe form of consecutive fever."

(*d.*) "All the cases of severe fever, with the two "exceptions above mentioned, were preceded by severe "collapse and considerable discharges."

(*e.*) "The degree of fever appears to have borne some "relation to the duration of the stage of collapse; there "are, however, numerous exceptions to this rule."

With reference to the question—"*Does stupor in this stage* (*consecutive fever*) *always depend on uræmia? or on what?*" the following remarks occur in the Middlesex Hospital report:

"Of the cases which died in a state of coma, with one "exception, all had suppression of urine; and in the ex-"ceptional case the quantity of urine was diminished."

"Case 39, fell into a state of complete coma from which "he appeared sinking. After remaining in this condition "for more than 24 hours he recovered. Throughout all "this stage urine was passed copiously, and of good "specific gravity. And although the urine contained "a trace of albumen (as was the case with most cholera "patients), there did not appear to be any reason for con-"sidering that uræmia was present. The coma much "more resembled that seen in typhus."

6. The import, tendency, and characters of the diarrhœa, which prevails epidemically together with cholera, formed the subject of the ninth group of questions.

(i.) "*When diarrhœa and cholera prevail together epidemically are they* (*with differences of degree*) *the same disease?*"—This question must now, doubtless, be answered affirmatively. The larger part at least of the diarrhœa which is generally so rife in localities where cholera exists, must be ascribed to the same cause and must be regarded as only a slight degree of the diseased action which in a higher degree becomes cholera. But if diarrhœa when aggravated is cholera, how comes it that so many deaths are during a cholera epidemic registered as caused by diarrhœa? During the 18 months from July 1853 to December 1854, 11,661 deaths from cholera were registered in London, and as many as 6,258 deaths from

diarrhœa. Making an ample deduction from this number on the score of the deaths from bowel complaints of various forms which occur annually when cholera does not prevail, there remain 4,000 deaths referable to the epidemic and registered as due to diarrhœa. How did these 4,000 cases differ from the 11,661 cholera deaths? That many of them presented distinctive features cannot be doubted. In the first place the average duration of the 4,150 cases of fatal diarrhœa registered was 13 days; so that a large portion of them must have been cases in which the disease ran a slow course; and, in the second place, children and old persons above the age of 65 furnished a large majority of them. Now there are fatal cases of the disease in which the lividity of the surface and the cramps are either very little marked or are absent, and these cases are observed chiefly in children and old persons, and are often of a lingering character. They, therefore, in all probability, form a part of the cases which appear in the register of deaths as fatal cases of diarrhœa. In the hospitals of London few deaths were attributed to diarrhœa, only 42, while the deaths attributed to cholera were 800. In workhouses, on the contrary, 400 deaths from diarrhœa were registered, with 924 deaths from cholera. This difference is in part explicable by the small proportion of children and old perrons among the patients admitted into the general hospitals, and the large proportions of persons of those periods of life among the inmates of workhouses. Again in the districts on the higher levels the registered mortality from diarrhœa was in proportion to the registered mortality from cholera far greater than it was in the districts on the lower levels. And this may in part be explained on the assumption that in the higher districts where the epidemic cause was not generally in strong action, the fatal cases would often be less intense in character and less rapid in their course. These cannot, however, be accepted as the sole reasons of the predominance of diarrhœa amongst the deaths from the epidemic in the higher districts of London and of the small proportion it forms amongst the deaths in the hospitals. For in some of the hospitals where the deaths from cholera were numerous, no deaths from diarrhœa were registered, and in some of the sub-districts of London which have not a high level and which suffered severely from the epidemic, the deaths from diarrhœa exceeded in number the deaths from cholera. The truth probably is that the choice of

the term used in registering the deaths was often made somewhat arbitrarily; that in private practice, especially in districts where the disease was not very rife, there was a disposition to give the less formidable name of "diarrhœa" even to cases which had the features of cholera distinctly marked; whilst in hospitals, a more strictly pathological view of the matter being taken, all, or nearly all, the fatal cases were denominated deaths from cholera.

The deaths registered, as caused by diarrhœa, so far as they were due to the epidemic cause, were, then, cases of cholera more or less modified in their features, and had no closer relation with the ordinary cases of diarrhœa which were not fatal than the deaths registered as "cholera deaths" had; for both classes of deaths were alike the results of the same morbid process which in its slightest degree was the "simple diarrhœa."

(ii.) On the mutual relation of these two grades of the epidemic influence, we have received a communication from Dr. M'Loughlin, who is known to have devoted much time and labour to the inquiry. He strives chiefly to prove that cholera is *always* preceded for a certain time, some hours, some days, or some weeks, by a "premonitory diarrhœa;" and he does not admit the occurrence of exceptional cases. It may be doubted whether the single profuse discharge which in a few cases precedes for an hour or little more the state of developed cholera, can be regarded as constituting *premonitory* diarrhœa. But it cannot be disputed that these cases are very rare, and that cholera is in the vast majority of cases preceded by a stage of diarrhœa which affords time for treatment.

(iii.) No evidence has been laid before us which might aid to solve the question, "*Does the diarrhœa, if left to "itself, generally and safely tend to spontaneous recovery; "or do such cases, without medical treatment, frequently in "proportion to their numbers, pass into true cholera?*" And it will always be difficult to obtain satisfactory evidence with regard to this question.

For although the number of cases of cholera which were neglected in the stage of diarrhœa may be learned with accuracy, it will be scarcely possible to ascertain the number of cases of diarrhœa in the same town or district which terminate by recovery in that stage although not brought under medical treatment. The small proportion of the cases of diarrhœa which pass into cholera when remedial means are employed is shown in the "Report on

the Results of the different Methods of Treatment pursued in Epidemic Cholera."

7. No new *chemical analysis of the blood* in cholera has been communicated to the Medical Council. But as the changes which the composition of the blood undergoes necessarily bear a close relation to the composition of the fluids poured out from the blood-vessels into the intestinal canal, reference may here be made to the Report of Dr. R. D. Thomson (App. No. X.) on "the Chemistry of the Rice-water Excretions."

Some observations on the physical and microscopical characters of the blood after death from cholera will be found in Mr. Callender's minute account (App. No. XIII.) of twelve autopsies made at St. Bartholomew's Hospital. Remarks by Dr. Hassall on the same subject, and on the state of the urine in cholera, are likewise contained in our Appendix (No. XI.)

In any future visitation of cholera, more extensive and systematic inquiries, it is to be hoped, will be instituted relative to the pathology of the disease; and in order that these inquiries should produce large results, it is most desirable that they should be commenced at an early period of the epidemic. On this account (*see* p. 127) we append to our Report the Pathological Memorandum, which elicited the various answers discussed in our present section; and which may at least serve to remind pathological inquirers at a future period of the position at which our knowledge had arrived at the time of the last epidemic, and of the directions in which its advance was then felt to be most needed.

In closing our present Report, and therewith terminating our labours as a committee for scientific inquiries in relation to the late epidemic, we would venture respectfully to suggest, with a view to future emergencies of the kind, that if such inquiries are to be fruitful of result, they should to some extent be continued in the absence of the disease which they aim at elucidating.

Just as there can exist no science of morbid anatomy, till the structure of the healthy body be first well learnt; just as there can be no medical knowledge of what makes a symptom of disease, till physiology have first established a standard of natural function; so it is, we believe, with

the studies which, in their future development, may perhaps explain the birth of epidemic disease.

The preparatory steps of such studies are even yet scarcely made. In attempts to elucidate the exact nature of those processes by which epidemic poisons are generated, almost insuperable difficulties have been found; not from want of apprehension as to what the factors may be in such exceptional generation of poison, but from lack of definite knowledge as to the normal working of these factors.

It seems certain that in the chemistry of organic decomposition there is concealed a large share of the mystery we would solve; and it is impossible yet to say, how much of the remainder may belong to undeveloped branches of meteorological science.

Under any future epidemic invasion which our country may unhappily suffer, it is in these departments of natural science that temporary and exceptional phenomena will claim renewed investigation; and we hope we do not outstep a proper fulfilment of the trust reposed in us, when we suggest, that in this intervening time are comprised golden opportunities for rendering probable the success of investigations then to be undertaken,—opportunities for making effective progress in the preliminary studies to which we advert, and for establishing a better normal standard than is yet discovered, to measure the chemical and meteorological anomalies of an epidemic period.

We have the honor to be, Sir,

Your obedient humble Servants,

N. Arnott.
William Baly.
William Farr.
Rich^{D} Owen.
John Simon.

SUPPLEMENT TO THE REPORT

OF THE

COMMITTEE FOR SCIENTIFIC INQUIRIES.

No. I.

The following is the letter circulated among Members of the Medical Profession by the General Board of Health in September 1854:—

Letter addressed by the President of the General Board of Health to Medical Practitioners.

General Board of Health, Whitehall,
September 1854.

Sir,

My experience of this Department, brief as it is, has strongly impressed me with a sense of the great want that is now felt of some systematic record of cases of choleraic disease, their treatment, and results, with a view to determine, in so far as may be possible, the best mode of meeting this formidable epidemic.

Hitherto no successful attempt has been made to collect such a record; and as I find that my feeling of the want of it is very generally shared by the medical profession, I have obtained the sanction of Her Majesty's Government to the nomination of a Medical Council, representing all branches of the profession, and consisting of Dr. Paris, Sir James Clark, Dr. Alderson, Dr. Babington, Dr. Tweedie, Dr. Baly, Mr. Lawrence, Mr. Simon, Professor Owen, Mr. W. B. Ward, Mr. John Bacot, and Dr. Farr.

Under the sanction of this Council the following form of return has been prepared for transmission to all qualified practitioners in the metropolis, to be filled up by them, with a view to obtain their experience of the present epidemic.

The deaths from cholera, as well as from all other causes, are registered in England; but it is evidently desirable that in this as in other countries the cases of recovery, as well as of death, should be systematically observed and recorded. But this can only be accomplished by the cordial co-operation of all the medical men in practice, which the Board hopes, in this important matter, to obtain, by acting on the advice of a Council in which all branches of the medical profession are represented.