SECTION III.

CHEMISTRY, METEOROLOGY AND GEOLOGY.

ADDRESS

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PRESIDENT OF THE SECTION.

The special aim of this address is to show that the relations of weather to the distribution of deaths from certain diseases, that is, to the fatality of those diseases, has now been so definitely ascertained, in so far as regards this country, that no account of these diseases can be regarded as complete which fails to treat of those relations. In other words, the ascertained relations of weather to deaths from the diseases in question, must now be accepted as part of their known natural history. As regards certain diseases the facts have been analysed, sifted, and cross-questioned, with the result of showing that the relations between weather and deaths from those diseases are so steady as to be manifestly the outcome of an obedience to law. It also appears that what is now known to be true of the diseases which have been investigated with some fulness, will probably be found to be as true of many other diseases when they have been submitted to an equally pains-taking investigation.

This is the special aim of the address. All I desire is to show the *constancy* of the relations of weather to disease. It is with the *constancy*, not with the *characters*, of these relations that I wish to deal. Had I dealt with their characters and with the speculations which these fairly raise, I should probably have been followed with more ease and greater interest; but dry and difficult as my subject is, I am not without the hope that I may succeed in establishing my position.

Such a question as what are the relations of weather to deaths from particular causes can only be answered by statistics. But

it is difficult, on looking at long columns of figures, to see what they teach; and when vast masses of figures are dealt with, as is the case in the research of which I am about to disclose one important result, it is practically impossible in an address like this to present the figures themselves in a way which would not be confusing.

It happens, however, that the figures can be presented graphically, so as instantly to exhibit what they teach; and such a presentation of them is seen on the numerous diagrams which hang on the wall.

Unless, however, the method of thus presenting numbers graphically is clearly understood, I shall not succeed in demonstrating what I wish to demonstrate.

The horizontal black line in each figure represents the mean weekly death-rate on an average of the fifty-two weeks in the year, or group of years under investigation, and it may of course have reference either to all the causes of death, or to deaths from some one cause. With this—the average weekly death-rate—the death-rate for each week separately is compared; and the difference above or below is calculated in percentages of the mean weekly death-rate for the whole period. When the percentage for any week is *plus*, the amount is marked, according to some adopted scale, on that one of the vertical lines which corresponds to the week in question, and above the mean line, of course. When, on the other hand, the percentage for any week is *minus*, a mark showing the amount, according to the same scale, is placed below the mean line on the vertical line representing that week. When the result for all the weeks is marked on the fifty-two vertical lines, the marks are joined by a line drawn through them, and we have then a curve sometimes above and sometimes below the mean line—showing at a glance the weeks in which the mortality was above or below the mean, and the measure of that excess or defect.

The relations of weather to disease have long occupied the attention of Mr. Alexander Buchan and myself. In our first efforts to ascertain those relations we dealt chiefly with the eight principal towns of Scotland, but it soon became apparent that the smallness of the population of these towns, the division of time into months instead of weeks which the Scottish Registrar General adopted in his returns, and the shortness of the period over which these returns extended, made Scotland unsuitable as an area from which the facts we needed could be gathered. We then turned to London, 1st, because it had an enormous population contained within an area so limited that it might be regarded as having one uniform climate during each of the seasons of the year, and 2nd, because it possessed full weekly returns of weather and of deaths from different diseases extending over a long series of years. Accordingly some six or eight years ago, we made a complete discussion of all the relevant facts for London for the thirty years from 1845 to 1874 inclusive, and we obtained, even for diseases which had a small total mortality, results which are expressed in these curves, shewing relations between weather and deaths from special diseases which we thought would turn out to have the value of constants.

I have already explained that if we know the deaths from any particular cause for each week of a long series of years, we can ascertain the average distribution of deaths from that cause over the different weeks of the year. When represented graphically, in the way I have described, the number thus obtained must yield a curve or figure of some kind. It may possibly be a straight line, or something approaching to one, which would mean that the average mortality from the cause in question for any one week was the same, or nearly the same, as that for every other week. On the figure or curve may be an irregularly serrated line—that is, the number of deaths in the different weeks may fluctuate irregularly, rising and falling frequently and without method. These results may occur either in the case of a disease which is highly mortiferous, or in the case of a disease which kills but few. In neither of the results should we have the expression of any seasonal influence on the progress of deaths from the cause in question.

But, on the other hand, the numbers may yield a curve or figure, which seems to be the outcome of some influence, which recurs periodically, and which causes the curve after it begins to fall to go on falling to a certain point, and in like manner after it begins to rise, to go on rising to a certain point.

When we get a curve of this kind, we are justified in regarding it as probably possessing significance and value. If it embodies the results of a large number of years, and deals with a population sufficiently great and sufficiently concentrated, we may almost safely treat it as a constant. In other words we are fairly safe in concluding (1) that deaths from that cause in the same community for shorter groups of years, or even for single years, would be distributed over the different seasons substantially in the same manner; and (2) that such divergencies as occurred would be affairs of degree and not of kind, and would answer to corresponding divergencies between the seasonal weather of the shorter groups of years and that of the long series of years.

That we are safe in doing this with regard to a large number of the causes of death has been placed beyond question. In London, for instance, the curve of deaths from diseases of the respiratory organs, representing a mean of thirty years, is almost exactly the same as the curve representing the mean of any five or ten of the thirty years. It is, indeed, essentially the same as the curve for any one of the thirty years.

A like thing can be said regarding deaths from diseases of the abdominal organs, and perhaps also regarding deaths from diseases of the nervous centres.

But the steadiness which I desire to disclose, goes beyond the numbers which show the total deaths from a great class of diseases. It appears as decidedly and strikingly, when we deal separately with the numbers representing deaths from the special diseases which make up the class. Thus:—The curve of deaths from pneumonia has certain characters which distinguish it from the curve of deaths from bronchitis; and the dysenteric curve in like manner, has features which distinguish it from the diarrhœal curve. These distinguishing characters do not simply appear in the mean of the thirty years; they appear if we break up the thirty years into quinquennial or decennial periods. There is a kinship between the diseases which form the groups or classes of the Registrar General, but they are nevertheless in many respects distinctly differentiated from each other. That kinship shows itself also in the relations of weather to mortiferousness. For instance, speaking broadly, the homicidal tendencies of all diseases of the respiratory organs are strongest at one and the same season, and the same is true of diseases of the abdominal organs. The one class of diseases kills most in cold, and the other kills most in hot weather. But the special diseases which form the classes, though they show a substantial agreement, are nevertheless differentiated from each other by steady peculiarities in their relations to weather.

It may be confidently asserted that it never happens that the curve of deaths from diarrhœa assumes the characters of the curve of deaths from bronchitis, or *vice versâ*. It would never be possible to mistake the one curve for the other. This is equally true of the curves of deaths from a great many other causes, which are as decidedly and as unfailingly characteristic. These statements may perhaps appear strong and novel, but they are beyond question correct. Their accuracy has been fully proved —has been demonstrated, I may safely say. I make them now merely as a preparation for the further illustration which I am about to adduce, of the steadiness of the relation of weather to deaths from particular causes. All that I have asserted may be quite true of such causes of death as bronchitis, pneumonia, diarrhœa, and dysentery, and yet may be quite untrue of such causes of death as small-pox and scarlet fever. These last occur as epidemics. They kill large numbers in some years and comparatively few in others. It seems, therefore, almost improbable that deaths from them can exhibit a steady obedience to seasonal influences. With regard to them we are almost prepared to expect, that what may be true of a thirty years' period will be found to differ *in toto* from what shorter periods show; and that consequently we cannot construct curves for them, which will have the value and dignity of constants. This I think is what would be naturally expected, and I proceed now to show how far the facts answer the expectation.

First then as regards small-pox:—The mean curve of death from this disease for thirty years is a simple curve, and shows a mortality above the average from Christmas to the end of June, and a mortality below the average from the beginning of July to Christmas.

Now during the thirty years there were nine distinct small-pox epidemics, but the epidemic from the autumn of 1870 to the end of July, 1872, was greatly more severe than any other. Before and after that time the deaths from small-pox on no occasion exceeded seventy-one for any week; but in 1871, during the three last weeks of April and the first week of May, the deaths averaged 273 a week, rising to 288 in the first week of May. If we take away the deaths which occurred during this abnormally high epidemic, and average what remains, we obtain a result which shows that the maximum death-rate is during the first seven weeks of the year, and the minimum in September and October. The withdrawal, therefore, of the figures yielded by the remarkable epidemic of 1870–72 does not alter the character of the curve for the whole period, except in reducing its sensitiveness.

In order to see how far the leading characteristics of the small-pox curve maintain themselves during the whole thirty years' period, a table was prepared showing for each of the five-year periods, from 1840 to 1874, the number of deaths which occurred during the four weeks of January and the four weeks of September, being the four consecutive weeks which give the highest, and the four which give the lowest averages respectively in the curve for the whole period. The result is, that the table shows that during each of the seven quinquenniads, the excess of deaths in January as compared with September was constant —the excess varying from 21 to 164 per cent., the average excess being 72 per cent.

The constancy of these results in the case of a disease like small-pox, which is commonly thought to have no steady relation to season, is remarkable.

So far, then, the cross-questioning or analysis of the mass of facts embodied in the curve, which was treated as revealing a constant, confirms the propriety of so treating it.

But the recent occurrence of an epidemic of small-pox in London—in 1881—the deaths from which are not included in the series of years referred to, namely from 1840 to 1874, affords an opportunity of applying another test, and a severe one. We shall deal with a single year, and that the year of an epidemic, and ask whether, during that short and exceptional period, the progress of the deaths from small-pox was so controlled by the seasons as to be above and below the mean during the weeks when they are above and below the mean in the thirty years' curve.

The answer to this question is that the curve of constancy is above the mean from the beginning of January to the end of June, the maximum being in the last week of May, and the minimum in the last week of September. There is a secondary maximum in January. The curve is below the mean from July to January.

Turning now to the curve of the 1881 epidemic, it appears that almost exactly the same words describe it. During this epidemic the mortality was above the mean from the middle of January to the middle of July, and below it for the rest of the year. The only difference thus is that the curve of the epidemic was a fortnight later of rising above its mean, and a fortnight later of falling below it again. The absolute maxima of both curves occur at the same time, that is, in the last week of May.

The fact that the departures above and below the mean are greater in the epidemic curve than in the curve of constancy, constitutes a mere difference in degree, not in kind. In both it is the same season which intensifies the mortiferousness of small-pox. The application of this further test, therefore, gives additional support to the value of the small-pox constancy curve, and shows it to be a feature in the natural history of that disease, which cannot properly be ignored, and the knowledge of which may prove of practical value.

I have now to show how the facts stand in regard to scarlet fever; and here I am able to present a still more minute analysis of the deaths. During the thirty-five years, from 1840 to 1875, there occurred six epidemics of this disease in London, reaching their absolute maxima of fatality in 1844, 1848, 1854, 1859, 1863, and 1870. The thirty-five years therefore, naturally break themselves up into six periods, each including an epidemic—the first from 1841 to 1846, the second from 1846 to 1851, the third from 1851 to 1857, the fourth from 1857 to

1861, the fifth from 1861 to 1867, and the sixth from 1867 to 1873. The year at the beginning, and the two years at the end of the period are not dealt with—the first representing a period falling to its minimum, and the second a period rising to its maximum—the last being in fact an epidemic then prevailing in London, and not at an end.

The deaths occurring in each of these six periods of five, five, six, four, six, and six years have been dealt with separately, and six curves have been constructed, showing the mean yearly deaths from week to week for each period. It will be at once seen that the curves for all the six short periods are essentially the same as the curve for the long period. They are below the line of mean mortality during the same period of the year, and above it during the same period. In rising from the low period to the high period they all cross the mean, either in the same week, or in the week before or after—towards the end of August or beginning of September; and nearly as regularly do they pass out of the high period back again into the low period in the month of January.

The figures for the individual years have not yet been separately and fully examined, but it has been ascertained that the same seasonal influence over the distribution of deaths from scarlet fever runs through them. If, for instance, we prepare a table showing the deaths from scarlatina in each week of each of the thirty-five years, and write those numbers in red which are decidedly above the mean weekly death-rate for the year in which they appear, those in blue which are decidedly below, and those in black which are near the mean, we get a table with three great columns of figures in red, blue, and black. The columns are not absolutely straight, but they are sufficiently so to demonstrate the unfailing operation of some seasonal influence controlling the distribution of the deaths from scarlet fever over the weeks of the year.

I have hitherto been speaking only of deaths in London, which was selected, for the reasons I have given, as the place affording the best materials for this research. But when it was found that the London data disclosed this striking constancy in the scarlet fever curve, it was naturally asked whether this might not be something peculiar to London, and whether the same distribution of deaths from scarlet fever over the weeks of the year would be found to occur in other localities. Accordingly, the facts for twenty-seven towns in England, Scotland, and Ireland have been examined. The smallness of many of these towns, the shortness of the period during which the registration of deaths with their causes has been in operation, and in some instances doubts as to the trustworthiness of the registration—all tend to weaken the value of curves based on the data, which these localities supply. But in spite of these drawbacks, the general results strongly corroborate all that has been said about the constancy of the scarlet fever curve in London, and show it to be a constant not for London only but for Great Britain and Ireland. The curves for Norwich, Bristol, Wolverhampton, Birmingham, Leicester, Liverpool, Manchester, Salford, Oldham, Newcastle, Belfast, Dublin, Glasgow, Edinburgh, Dundee, Aberdeen, Greenock, Paisley, Leith, and Perth are essentially identical with the curve for London. The varying sensitiveness, that is, the extent of the departures from the mean, above and below it, has little or nothing to do with the exact point now under notice, which is the influence of weather on the mortiferousness of scarlet fever, and not the greater or less prevalence of the disease in different localities.

A few of the curves,—very few,—those, for instance, for Hull and Sheffield, show a considerable departure from the characters of the London constant. I cannot explain these rare exceptions, but I have little doubt the explanation will disclose itself as the research goes on. In the meantime, persons accustomed to statistical inquiries will see in them nothing more than the exceptions which prove the rule, and which show that the broad results are not the outcome of any special way of handling the figures.

Turning now to the London curve of constancy for hooping cough, we find that it is almost the opposite of that for scarlet fever. This disease kills by preference from the middle of December to the middle of June, and has its maximum of destructiveness in February, March, and April. During the other months of the year it is less lethal, being least fatal to life in September and October.

This is what the curve for thirty years shows. But if we split up the period into six quinquenniads, will the figures for each of the five-year periods show the same or a similar distribution of deaths from hooping cough over the weeks of the year? This has been done, and the results disclose not merely a similar but an almost identical correspondence with the curve of constancy. It would not, indeed, be easy to fancy the outcome of a research better entitled to be called a demonstration than that which is furnished by these analyses of the London curves for small-pox, scarlet fever, and hooping cough.

But, again, as regards hooping cough, it may be asked whether the distribution of deaths over the year, which the London curve discloses, is not something peculiar to London. In order to answer this a set of curves has been constructed, which show the distribution of deaths from hooping cough over the weeks of the year for twenty-six towns in England, Scotland, and Ireland. It appears that what happens in eight-tenths of these is substantially the same as that which happens in London. The exceptions are few, and, in view of the circumstances affecting the data supplied by small towns, not important.

I hope no one will require that I shall show what practical advantages have been, or will be, conferred on mankind by these researches. In making them we did not concern ourselves in the least with that consideration, believing that additions to knowledge must always prove beneficial—though the time and the way, the when and the how, it may not be easy or possible to see and predict.

It so happens, however, that in these researches into the relations of weather to health and disease, there are already many indications of a practical utility.

To some of these indications I shall briefly allude, because they happen at the same time to illustrate further that constancy to which I have been continually making reference.

For instance, the London thirty years' curve for diarrhœa is very striking. It begins to rise rapidly above its mean in the end of June, and by the end of June and beginning of July is 300 per cent. above its mean. From this point it falls nearly as rapidly as it rose, and gets again below its mean in the middle of September.

The diarrhœal curve for each one of the thirty years is essentially the same in its character. So also is the diarrhœal curve for all the other towns in England and Scotland, and also for the whole of these countries, including both urban and rural districts.

Notwithstanding this general agreement there are differences. There are differences, too, in London, and in every other locality, between one year and another. No two years and no two places are absolutely the same. Though there may be a substantial and essential agreement, there is not an absolute agreement, and the question naturally arises:—Are these differences between the curve of constancy for London and the curve for particular single years in London, or between the constancy curve for London and the curve for other localities, merely differences of degree; or do they answer to corresponding differences of weather; and if so, to which of the elements of weather do they exhibit an obedience?

Little has yet been done in the way of answering this large question, but something has been done. As regards diarrhœa, for instance, temperature is commonly believed to be the element of weather of greatest potency. In this country, when the mean temperature of a week reaches 55°, the deaths from diarrhœa

begin to rise, but the diarrhœal curve is not raised in an equal degree by every additional degree of temperature above 55°. It is seen, indeed, that a rise from 55° to 60° has a less effect than a rise from 60° to 65°, and still less than a rise from 65° to 70°. It may eventually be possible to determine for different localities the progressive rate at which each degree of mean weekly temperature above 55° raises the death-rate from diarrhœa, but as yet the analysis of the data has not been carried sufficiently far to give any indication of this.

Some light, however, may be thrown on the influence of temperature in shaping the diarrhœal curve by examining the facts relating to any three years included in the thirty years, one of which shewed a death-rate from diarrhœa running an average course, another a diarrhœal death-rate considerably above the average at its maximum, and the third showing a diarrhœal death-rate below the average at its maximum. Three such years occur consecutively from 1859 to 1861 inclusive, so that the data or numbers relate to a period which could not have been materially influenced by changes in social conditions or sanitary arrangements.

For each year the deaths from diarrhœa per 1,000 of the corrected population have been calculated for each week, and the results tabulated. In 1859 the rise in the hot months is highest and earliest; in 1861 it is lower and later; and in 1860 still lower and still later. All three curves, however, are substantially the same in character. They merely differ in degree or intensity. They all show a marked, though not an equally marked, period of maximum fatality from diarrhœa in July, August, and September.

I now proceed to compare these curves with the temperature curves of London for the same years. It appears from them that in 1859 the summer heat was greatest and earliest; in 1861 lower and later; and in 1860 lower and later still. In other words the divergencies between the two sets of curves are close, complete and direct in their character. That is:—With a greater and earlier summer heat, we have a greater and earlier increase of mortality from diarrhœa; and with a lower and later summer heat, we have a less and later increase of the deaths from diarrhœa; and with a still lower and later summer heat, we have a still less and later increase of mortality from bowel complaints.

Such divergencies therefore, from the thirty years diarrhœal curve, as we see here, are clearly of the nature of the exceptions which prove a rule—being, when thoroughly sifted and examined, in real accord with the rule.

I have alluded in this address to classes of disease as well as to special diseases; and I have indicated that weather generally affects the different members of the classes in much the same way. The curves for thrush, tubes mesenterica, enteritis, jaundice, etc., are all of the same character as the curves for diarrhœa and dysentery, though less pronounced. All these diseases kill preferentially in hot weather, and they are all diseases of the abdominal viscera. There is not one disease of these viscera which kills by preference in cold weather. They all reap their harvest in hot weather. So that if we find a disease causing death by preference in the hot weather of this country, enough is now known to justify us in concluding that the disease in question is probably one which involves an abdominal organ. In this way the researches I have been dealing with become an aid, by suggestion or corroboration in pathological enquiries. The deaths from plague, for instance, exhibit in an emphatic manner the diarrhœal curve—in so clear and emphatic a way that it is difficult to resist the belief that the great plague, and the historical plagues of London generally, were of the nature of cholera, and not of the nature of typhus.

Again, the curve of deaths from bronchitis is more or less closely repeated in the curves for asthma, pneumonia, laryngitis, pleurisy, and croup. In other words, the diseases of the respiratory organs show one result of weather, though with varying force and modifications of character—cold always increases their deadliness. They all do their dismal harvesting in the dead of winter.

In like manner curves for diseases of the nervous system have much in common. They could not be mistaken for curves showing deaths from diseases of the respiratory or abdominal organs. They are most fatal in cold and dry weather. Like other diseases, they kill all the year round, but their favourite time for killing is late spring.

These three groups include a large number of the diseases given in the Tables of the Registrar General.

There are diseases, however, outside of these groups, between which a relationship has been accepted by many—as, for instance, between erysipelas and puerperal fever—and it so happens that the curves for these diseases tend to give support to the idea of such a relationship.

The prolongation of the high mortality from hooping-cough to the end of May differentiates it from the other curves for diseases of the respiratory organs, and gives it a combination of the characters of the nervous and the respiratory curves, to which it seems entitled.

In the case of gout, too, this nervous feature appears with suggestiveness, and perhaps explains why the active gouty inflammation does not pass into suppuration, as other similar active inflammations so frequently do.

When we find a curve taking the strong shape of the diarrhœal curve, may we not correctly speak of it as revealing an annual epidemic? I go further and ask whether there is not for every disease something of the nature of an annual epidemic? It appears to me that there are reasons for answering this question in the affirmative. The thing which we call an epidemic of scarlet fever, when minutely examined turns out to be simply a great intensification of what is an annually recurring feature in the natural history of that disease. It has been shown that the epidemic does not transfer the maximum mortality from the end of the year to the beginning. It merely intensifies the normal rise. So with diarrhœa. The deaths from that disease in 1859 were vastly in excess of the deaths from it in 1860. In other words, there was then an epidemic of fatal bowel complaint, but the distribution of the deaths over the year was essentially the same as that for 1860, and for that of any of the thirty years included in the discussion.

Even with small-pox, which we commonly regard as uninfluenced by weather, I have shown that the epidemic of 1881 left the curve for the year unaffected except in intensity. That epidemic may be fairly enough regarded as nothing more than a great increase of the normal annually-recurring destructiveness of the disease. It was nothing more than an exceptionally rich crop reaped at the usual time.

It was only natural and reasonable, therefore, for a medical friend of mine when, in conversation about a prevailing epidemic of scarlet fever, he was asked whether he thought it likely that it had reached its climax, to get up and turn to the scarlet fever curve for the answer. On examining that curve and the published weekly returns of deaths, he said that the decline of the disease had, in his belief, commenced, and he turned out to be right.

If these views are correct something further seems to follow. When scarlet fever breaks out in a small town or village, and steadily acquires more and more force, attention becomes directed to the insanitary state of the locality. Inspections by health officers follow, and effect is promptly given to the recommendations they make. The epidemic abates and the results are announced as an evidence of what sanitary science can accomplish; but am I not entitled to ask if in reality anything more has taken place than the decline of the epidemic at nature's appointed time—the ending of an exceptionally rich annual harvest. The sanitary measures adopted may have accelerated the decline;

but we are bound to bear in mind that without them it would probably have come at the time it did come.

Professor F. de Chaumont, F.R.S., remarked that they must have all listened with the greatest pleasure to the extremely eloquent address of Dr. Mitchell. His name was so well known that the excellence of his address could have surprised none of them. Nevertheless the address and the facts which the President had laid before them, showed the wonderful constancy of the laws which governed the career of this world, and suggested many reflections, teaching, particularly, the lesson of humility. The address warned them, too, not to plume themselves too much upon the success of their efforts, and pointed out that they might be only aiding, in a very small degree, events which would occur in the ordinary course of time. Thoughts such as these must necessarily arise from a study of this kind, and an address such as this brought every part of a question before them in a much more graphic and distinct manner than they could obtain by merely reading, for instance, the descriptions of epidemics. The address they had heard corroborated the manner in which true science was progressing, by pointing out that what people were in the habit of calling unusual epidemics were merely the ordinary course of affairs. When he was a student it was urged that physiology and pathology were totally different things,—the one a science of living bodies in health, and the other of living objects in disease. Now, of course, it was well known that there was only one science of living bodies, and those that were called pathological conditions were only physiological ones exaggerated. He was sure he should meet their approval when he said that if the Congress had had no other outcome than this address, those who had travelled many miles to attend at Newcastle would be amply repaid for assembling there. He concluded by proposing that a hearty vote of thanks should be accorded to Dr. Mitchell for his address.

Mr. H. E. Armstrong, M.R.C.S, said he had great pleasure in seconding the motion. He had for many years taken a deep interest in the question with which the President had dealt in his address. Dr. Mitchell had examined the subject more broadly than he (the speaker), as an Officer of Health, could do, but the general results expressed in the paper corresponded with his own particular experience in Newcastle. With regard to small-pox, the audience must have been very much struck with the startling results which had been put before them. Sydenham's observations upon this subject were very striking. Epidemics of small-pox in his day frequently lasted two years; they gained greatest intensity in summer, and finally disappeared on the approach of the second winter. The worst were those which commenced in the early part of the year. The paper they had heard was undoubtedly a most valuable one, and it gave him great pleasure to second the vote of thanks.

Captain Douglas Galton, C.B., said he was sure they would carry by acclamation a vote of thanks to Dr. Mitchell for his most interesting and valuable opening address.

The vote was then accorded by acclamation.

Dr. A. Mitchell thanked the Section for the vote they had accorded him, and for having listened so attentively to what he had said. He was at first afraid that his address would prove uninteresting, being so statistical in its character.

On "*The Influence of Minute Suspended Matter on Health; its Detection, Collection, and Examination,*" *by* H. C. Bartlett, Ph.D., F.C.S.

The almost universal belief that health is influenced in a high degree by the air we breathe, and by the fluids we drink, or take in combination with solid food, is the reasonable result of the accumulated experience of all classes of people, extending from the earliest times to the present moment. It is no argument to the contrary to repeat the late Sir William Fergusson's silly sneer against science, to the effect that he did not greatly care about the quality of drinking water so long as it was to be obtained in abundance. Nor does it prove that nauseous and offensive trades are devoid of danger to the neighbours because it can be shown that some or perhaps many of the workpeople engaged do not apparently suffer directly from the unsavory emanations among which they pass the greater portion of their time.

We know that many of the noxious matters, against which we have to contend, are in a soluble form, as is evidenced in the nitrogenous, albuminoid, or ammoniacal constituents of some of the clearest filtered water; and the presence of copper, lead, or other poisonous metals, or metallic salts in drinking water, is almost invariably detected in solution. In the same manner, pollutions of the air are frequently diffused in a gaseous form, and require great delicacy of chemical investigation to estimate the relative quantities in which the gases may be taken up by the atmosphere, and still more so to ascertain in what proportions such gases must be deemed distinctly injurious to health and life when breathed through the lungs of the strongest or the more delicate of humanity.

We have more than thirty years' experience to confirm Dr. Angus Smith's experiments as to the usual amount of carbonic acid in the air of healthy towns, namely, 38 parts in 100,000. When this quantity is doubled, or quadrupled, as it may be, according to my experiments, in dense fogs, or in not very crowded school-rooms, and this increase is the result of the respiration of men and animals, together with the imperfect combustion of much illuminating gas and coal, the direct effects are unmistakable. Persons constantly breathing such contaminated air suffer from headache, depression, and want of appetite; become pale and dejected, and prove their lowered condition of vitality by falling an easy prey to all kinds of epidemic disease. But if the carbonic acid in the air breathed is not accompanied with other products of respiration, or of imperfect combustion, and is taken into the lungs diluted only by pure air, as is frequently found in the bottling of aërated waters, then it is doubtful how much increase of the proportion of carbonic acid to the air breathed may be habitually inspired with impunity. I can only give my own testimony to the fact that I have examined the air of breweries, soda water factories, and other places where pure carbonic acid mixes with the air in more than twenty times the proportions I have mentioned, and no ill effects of living in such an atmosphere have hitherto been proved to ensue.

The question then arises whether the poisonous effect of carbonic acid in conjunction with other matters given off by the exhalation from the breath of many persons in confined spaces, such as theatres, together with the by-products of imperfect combustion of coal gas, are due to the combination of other gaseous products. If so, the chemical analysis of the atmosphere of crowded theatres points chiefly to carburetted hydrogen, and the vapours of sulphur, either as sulphuretted hydrogen, bi-sulphide of carbon or as sulphurous acid gas. But these, in the quantities shown by analysis, do not certainly account for the toxic influence, for men labouring in gas works breathe without loss of health the sulphur gases, carburetted hydrogen, and carbonic acid in the air, to an extent far beyond the proportion in which these gases are found in the atmosphere of the most heated and crowded theatre. I have therefore been forced to the conclusion that the serious and even dangerous effects of bad ventilation must be, and are, mainly due to matters carried in the air which are not in a gaseous form, and may be found suspended as minute particles.

For many years it has been ascertained that solid particles of matter, either inorganic or organic, living or dead, are suspended in the air. Practically, a collection of these same particles may be found, usually, also, in suspension, in almost all drinking

water open to the air. Absurd discoveries have been not unfrequently announced, by which the so-called floating disease germs have been written about, and the public mind, eager to take up sensational impressions, has been scared and unnerved with notions of blight and minute insect flights carrying with them the seeds of dreadful pestilence, as if that were the peculiar function of certain kinds of migratory insects. Before taking the responsibility of again rousing an undue interest in the subject of aëroscopy, I am anxious to assert, as forcibly as possible, the difference in the evidence I have obtained of the microscopic particles found in contaminated air and water, from the inspiration and ingestion of which lowered vitality and specific diseases have resulted, as contrasted with the absolute want of proof that any of these particles are disease germs in themselves.

I am equally desirous to point out that the balance of evidence actually is in favour of some epidemic diseases being carried and widely spread by clouds of insect life, travelling from country to country, usually from east to west, over a tolerably well defined area in which alone the insect flights have been observed. Scientific opinion is also fairly agreed that many contagious diseases are communicated directly from person to person by flies, and possibly by other insects, resting on the sufferers, taking with them the *contagium morbi* and leaving with it the reproduced disease on the skin or epithelium of the next one, two, or more persons on whom the insects may alight.

We know the desquamation of minute particles of skin in scarlatina does carry the contagium, and convey the disease to healthy persons, even after lying dormant for a period of more than twelve months, and that such minute particles may be retained and carried by the improper keeping of books or articles of clothing used in the sick room. The careful examination of flies caught in hospitals confirms the statement that portions of epidermis, epithelial cells, and pus corpucles, have been found adhering to the *antennæ*, the *proboscis*, and the *tarsi*. If, then, the particles of dead skin retain the *materies morbi*, and the flies convey the particles of skin, it is not necessary to believe with my venerable medical acquaintance, who declares that if scarlatina should ever make its way to the sea-side resort of which he is the ornament, the bare possibility of which he is sceptical, it can only be by the flies taking the disease themselves at the adjacent but opposition watering place, and coming over to communicate it in the delirium preceding suppressed eruption.

I have drawn attention to the possibility of propagating, and perhaps perpetuating, contagious diseases by the carriage of minute particles of skin, or other *dejecta*, long ago thrown off from patients since recovered from the disease. It would be an advance if we could recognise from the appearance of the particles so thrown off the difference between those likely to carry contagion and those which are effete and harmless. We could then devote our study to the former, and, perhaps, eventually detect the actual morbific matter. But this is not yet demonstrable, and we may waste a life-time in the microscopical and chemical examination of desquamations which, although coming direct from persons suffering from contagious disease, may not be imbued with the slightest trace of the specific disease germinal matter, or the germinal matter may be in such a condition as to be dormant at the time of examination, and be then unrecognisable. I have, therefore, given up for the meantime the investigation of specific disease contagium, and devoted more time to the general effects of floating particles on health. In 1870, my friend, Dr. Maddox, collected upon glass plates moistened with glycerine, the floating particles ascending from the sewer gratings, and carried out a most instructive series of micro-photographs, from which much might have been observed if they had been preserved for examination under the present high powers of the microscope with the illumination now afforded. This is, unfortunately, a great loss to all investigators, as more than six months were laboriously spent, under conditions of no small danger, in obtaining the original specimens at a time of a severe cholera outbreak; but the specimens and photographs were wiped off the plates, and nothing remained but to proceed to collect a fresh series to work upon.

At first I was impressed with the value of Dr. Maddox's aëroscopic method of collection, and wherever I found a frequent bad smell in my neighbourhood, either from the sewers or drains, or from dustholes, or from the holes of rats and mice in the floors or paved yards, there I exposed my glycerine-covered slides and submitted them to the most careful examination under a twenty-fifth power. I will not recount my own disappointment in finding nothing of interest to me beyond various inorganic and mineral matter, crystals, and organic colloid matter, grains of pollen, and minute germs of fungi, microphytes, and monads. Nothing, in fact, to be specially connected in any way with the bad smells, which are so invariably associated in our minds with unwholesome air, bad drainage, sewer gases, want of ventilation, rapid tainting, and putrifaction of all articles of food in the immediate vicinity, and the indescribable, but well recognised effluvia which attend the combination of many of the foregoing, and are classed by practical and well informed Inspectors of Nuisances as "insanitary stinks, requiring disinfection."

Exposing one of my plates over a gully hole in the shade, on one occasion, I observed a pencil of sunshine, accidentally reflected from a polished brass plate, throw a brilliant light over the gentle current rising and impinging, as I thought, upon the moistened surface. Thousands of glistening flecks and motes played in that unsavory stream of air, but on approaching the surface of the moistened plate, the current of air, or, certainly that portion of it with the dazzling particles dancing in the sunbeam, divided itself and passed away, as if repelled by negative electricity. Was it possible, I asked myself, that the more minute particles carried by the sewer gases might evade being caught in the glycerine by their very lightness of suspension in air? This only partial glimpse of the truth, as I have since ascertained, induced me to seek some other method of collection. I next attempted to obtain my specimens by filtering the air through long filters of cotton wool, as was suggested by Deusch and Schrœder, who found that flesh did not decompose when no air surrounded it, except that passed through a sufficient length of this material. Again difficulties beset me, for while succeeding probably in intercepting most, if not all, the floating particles, the crop of minute particles given off from the cotton wool itself by shaking, completely covered up and masked those I was in search of by an *embarras de richesse.*

I forget by whom the idea was originated that gun cotton teazed out into wool might be used to filter out the suspended particles of air, but it was after Mr. Crooks had used ordinary cotton wool in his experiments on the cattle plague, whereby he was satisfied that the poison or virus suspended in the breath of the cattle was obstructed and retained by the wool. Dr. Lionel Beale submitted the impregnated cotton wool to the highest powers of the microscope with similarly negative results to my own. The ingenious notion of substituting nitrated cotton (gun cotton) for ordinary cotton wool was valid so far as this prepared cotton may be dissolved in ether, leaving the extraneous matter not soluble in ether free from the cloud of cotton particles; but here again I found certain of the most interesting of the minute forms, sometimes to be obtained, were deprived of their individuality by the fatty contents being abstracted by the ether, and the dry sarcode left no longer worthy of investigation.

Professor Tyndall having conclusively proved that a long filter of cotton wool so purified the air from floating particles that not only meat and other easily putrescible matters were kept indefinitely from putrefaction, but that a pencil of the electric light, admitted to illuminate a portion of a jar of purified air, showed no floating particles. Here was a truth again and again proved by experiment. No floating particles, no decomposition or putrefaction of articles prone to putrescence, so long as the air in which they are immersed is effectually filtered. This affords so important and conclusive an argument that decomposition and putrefaction are mainly caused by the floating particles which may be removed from air, and which are so largely present in all air which has not been filtered through cotton wool, that I was extremely unwilling to give up the hope of examining the particles thus filtered out. All experiments to isolate the particles from those of the cotton failed, and it was not until then that I thought of using the peculiar and beautiful waste product of the smelting furnace, slag-wool.

It is necessary to interpolate a reason for not being content with taking a specimen or sample of the air to be examined, and allowing the floating particles to subside by long standing in a perfectly quiescent state. Professor Tyndall thought it advisable to prove that this subsidence does take place if the air in the vessel is kept quite quiet long enough. This is the case, and the particles so deposited may be classed as to lightness by remarking that the slightest electrical attraction to the sides of the glass cone, in which the experiment was made, was enough to hold to the upper portion of the side the lighter particles, others in gradation of gravity being deposited lower, with the most weighty at the bottom.

But I have found, by the electric pencil of light, floating particles in suspension many days after perfect quiescence was obtained, and these I have since found to be the most interesting of all, being the minute living germs of animal or vegetable life, which sometimes perish or become themselves decomposed before they can be deposited by subsidence.

Hence I was induced to endeavour to filter out the minute floating germs and other matters from the air by the use of slag-wool, so as to obtain my specimens as quickly as possible, to prevent the premature death and decomposition of the more delicate of the organisms.

The great importance of rapidly collecting and examining the floating particles, is shown by the fact that in most drawings and micro-photographs of pus corpuscles, there are depicted as sharply defined spheroids enveloped in what may be termed stout cell membrane. But these pus corpuscles are of comparatively little interest when found in that condition, as they are the dead corpuscles, from which fresh pus cannot be reproduced either by contact or innoculation.

In no instance have I found a living pus corpuscle of clearly marked round form; on the contrary, the vitality of the cell is shown by numbers of excrescences or protrusions from the cell-wall, which is of extreme pliability and delicacy, and in these

protrusions there is a constant movement, elongating and extruding from time to time from the parent cell, or apparently shrinking back almost entirely within its circumference. Very careful and continued investigation proves that these excrescences become in time detached from the parent cell, and if they find suitable nourishment from the surroundings to which they may be removed, they become true pus corpuscles and throw off other excrescences so as to multiply and spread the pus in or upon moist tissue, so as to indicate what is known as inflammation.

Inflammation of tissue cannot be said to be a specific disease in the sense that scarlatina or small-pox are specific diseases, yet it is probable that inflammation is never produced without the specific formation of pus cells, and it is certain that the introduction of living pus cells either by contact or inoculation will reproduce pus and inflammation in or upon tissues which would not of themselves at that time produce pus corpuscles or become similarly inflamed without local injury or smart irritation.

The condition, therefore, which is essential in the examination of all suspicious floating particles, either in air or in water, is they should be collected as rapidly as possible and investigated immediately, if we are to distinguish those in active vitality from the dried up or dormant, or from the dead and effete.

In using the slag-wool for the purpose of collecting floating particles from air or water, I first heat it to about 600° Fahr., and retain it at that temperature for an hour or more. I find no organic matter remains after this exposure to heat, and the inorganic ash can easily be shaken off the surface of the wool so as not to interfere with the fresh particles it is desired to examine.

A perfectly clean dry tube is packed with a long filter of the previously heated slag-wool, and the air or water drawn through the tube in any required quantity, either by an aspirator of known dimensions, or a suitable pump. When observing the minute particles filtered from the air I merely tap the end of the tube smartly against the glass microscope slide, cover the central portion with the thinnest obtainable glass, which must not be pressed directly upon the particles, but be kept separate from the slide by an annular ring of fine gold-beater's skin; this is very convenient. If the particles are moist from having been filtered out of water, I wash them off the slag-wool with perfectly pure distilled water, using only a few drops at a time, and instead of placing gold-beaters skin under the covering glass I use gold leaf which is rather troublesome to adjust, but admirable in its thinness, as it allows a 50th power to work well through the covering glass on to the slide, in fact, quite as well as with an uncovered slide.

All the movements of living corpuscles can then be observed, and the germination of bacteria can be seen from germs which were previously beyond our highest power to identify or even to detect the presence of except by the employment of a ray of electric light. A warm stage and moist slide are required to witness these highly interesting movements and processes of germination, but if the ray of electric light does not show floating particles no growths from minute germs ever make their appearance if the manipulations have been conducted with sufficient care.

Holding a strong opinion against the probability of finding specific disease germs in any form by which our present powers of observation can recognise them, I have endeavoured to collect evidence of the floating particles which are peculiar to the air and water admitted to create that lowered condition of health which is so much better recognised than its immediate causes are understood.

Four years ago I was instructed by the Committee of Lloyds to examine and report upon the condition of the atmosphere of the underwriter's rooms, about which there had been almost universal complaint. An easterly wind blowing up the Thames for some time at certain states of the tide caused a most unpleasant effluvium at the sewer gratings, and as the intake of air for the ventilation of these rooms was not very remote from the sewer openings, it was not remarkable that when the stench was very strong the underwriter's rooms were hardly bearable. I made a chemical analysis of the air taken from various parts of the rooms, and took a large number of specimens of the floating particles as well as I could with the glycerine covered slides in Dr. Cunningham's apparatus, an improvement upon Dr. Maddox's aëroscope.

The analysis showed a slight deficiency of oxygen and an excess of carbonic acid and ammonia, and the floating particles were so abundant as to accumulate in the vane in the tangible form of dust, besides depositing on the moistened collecting glass.

The dust formed a putrescible ferment and a deoxidiser, reducing nitrates to nitrites, and the particles showed under the microscope the usual bacteria, fungi sporules, oil globules, epithelium cells, and minute portions of animal and vegetable tissue; being, in fact, sewer air diluted with street air, and rendered worse by the exhalation of the large number of persons transacting business all day long.

The obvious remedy was to remove the intake of air from the vicinity of the sewer openings to the upper portion of the Royal Exchange, and the employment of other means of purifying the air which have been so far successful, that the well known underwriter's headache is now only epidemic at periods when the great storms are at sea, and not up the river, and it is surmised to be caused more by the news of wrecks than by the present by no means perfect atmosphere of the rooms. But the points of interest in the outcome of these experiments have since been carried further by a more minute and patient examination of the floating particles which are carried into the air of rooms in which no very unpleasant odour is usually perceptible beyond that perhaps which gives rise to the feeling called "stuffiness." The expression "badly ventilated," "muggy," "close," and "oppressive" as applied to the air of a room, particularly when these feelings are experienced after a meal to a greater extent than would be the case in other rooms, may generally be taken to indicate an inlet of impure air. This intake may be hidden in the most recondite manner; it may enter beneath an asphalted basement and ascend by a bell wire tube into the upper rooms, as I have more than once found it in the private rooms of some of the grandest hotels in London and Brighton, where no expenses had been spared in the construction. The inlet of impure matters may be conveyed behind skirting boards, through window and shutter casings, in the hollows of cornices and pilasters, and even by down draughts in what ought to be up-cast air shafts. But when I find the floating particles to be composed of living organic matter, and that matter contains cells or corpuscles of animal origin, possibly competent to reproduce pus or other degenerate cell growths, including, perhaps, contagium or tubercule, I consider the air of that room viatiated from some impure source, whether the inlet can be discovered or not.

The floating particles of living organised animal matter rapidly collected, and examined by the higest powers, so as to show that it is in energetic vitality, reproductive or fermentative, form an index to contaminations which so frequently accompany such morbid particles, as to lead me to conclude that these highly objectionable and dangerous *minutiæ* are sustained and perhaps stimulated by certain currents of air in which they float. The chemical composition of the air in which these matters are found in the greater abundance, distinctly proves its unwholesomeness, and while these currents of air may not be, and seldom are, fair samples of the average atmosphere of the rooms into which they enter, it is because of their distinctive characteristics that they can be sometimes detected by persons gifted with a very acute sense of smell.

I have frequently taken samples of the incoming stream of vitiated air which had scarcely any peculiar odour. In these I have found the oxygen reduced, and the carbonic acid and ammonia very considerably increased. Bearing floating matters which may be quite free from specific disease germs, and diluted with the purer air from the windows and doors, such contaminations are precisely those which I generally find in some part or other of the rooms and places of public resort, which cause headache, nausea, and impaired vigour of mind and body. Without infecting the inhabitants with specific diseases, air of this kind produces precisely that lowered condition of vitality, which we are told on the best authority, conduces most largely to the absorption of every description of infection.

Similar floating particles are frequently found in water cisterns even where scarcely any can be found in the air of the living rooms, but I cannot at present connect their presence as indicating injury to health so clearly as I do the floating particles generally carried in impure air. If, however, the analysis of the water in the cistern proves it to be inferior to that in the mains, and the quantity of living particles is very obvious, I should condemn the water, and search out the inlet of impure air which carries the floating matter and so contaminates the water. In several instances I have lately found entire families suffer from impaired health, only attributable, as far as was known, to water impregnated with objectionable matter from the air. But as the pollutions proceed from the floating particles in the air, the injury to health may have been due to the impure air finding its way at certain times into the sleeping appartments. This I have known to occur at night, although it could not be detected at any time in the day.

Without laying too heavy a stress on the influence of the minute particle *per se* on health, I am impressed with the indications some of them afford of the introduction of the impure air which supports them, but this is only reliably the case when the matters found are animal organisms, floating about in vigourous vitality.

The PRESIDENT rose to move a vote of thanks to Mr. Bartlett for the paper he had contributed, and which he said was of general interest. In order that the business of the Section might be speedily concluded, however, it was not desirable that they should discuss this subject.

The vote of thanks was seconded and carried.

On "*The Improvement of Climate with Slight Elevation,*" *by* THE HON. F. A. ROLLO RUSSELL.

RECENT observations have completely disproved the rule which only thirty or forty years ago was believed to represent the facts, that temperature decreases regularly with increasing altitude.

From Mr. Glaisher's observations at Greenwich from June 25th to August 6th, 1868, it appeared that from 9 a.m. to 3 p.m. the temperature at 50ft. was from 1°·9 to 3°·9 lower than at 4ft., but from 6 p.m. to midnight from 0°·4 to 3°·4 higher. At 10 p.m. and midnight temperature was 3°·4 and 3°·3 higher at the upper point. Humidity was found to be from 4° to 6° greater at 50ft. than at 4ft. between 9 a.m. and 3 p.m., but after 6 p.m. it was less, and at midnight 6° less.

From May to December, 1873; from January to July, and during December, 1874; and during January, February, and March, 1875, observations were taken at Kew Observatory, 10ft. above the ground, and at the Pagoda, 4400ft. distant E. by N., at the heights of 22ft. 6in., 69ft., and 128ft. 10in., all at the hours of 9 a.m., 3 p.m., and 9 p.m. In examining some of the results obtained, which were published in a Blue Book, we shall call the Observatory Station O, the lowest Pagoda Station P, the middle P′, and the highest P″. For the period of 19 months

The mean maximum reading	at O	was	56·2
" " "	at P	"	55·7
" " "	at P′	"	55·28
" " "	at P″	"	54·57
The mean minimum reading	at O	was	42·2
" " "	at P	"	42·3
" " "	at P′	"	42·67
" " "	at P″	"	42·83

In January, 1874 and 1875, the mean diurnal range

At O was	....	10·7 and 7·7
At P "	....	11·2 and 7·6
At P′ "	....	10·6 and 7·3
At P″ "	....	9·6 and 7·2

In July, 1873 and 1874, the mean diurnal range

At O was	....	18·4 and 21·4
At P "	....	16·7 and 19·7
At P′ "	....	14·5 and 18·3
At P″ "	....	14·2 and 16·8

In May, 1873 and 1874, the mean diurnal range

At O was	........:	16·0 and 17·1
At P ,,	....	14·5 and 16·3
At P′ ,,	....	13·9 and 15·0
At P″ ,,	....	12·9 and 13·6

The total mean monthly diurnal range for the nineteen months was as follows:—

At O	14·03
At P	13·34
At P′	12·54
At P″	11·68

The monthly means of maxima and minima were as follows:—

At O	56·2 and 42·2
At P′	55·78 and 42·57
At P″...........	55·07 and 42·75

The monthly mean maxima in July, 1873, and minima in January, 1874, were:—

At O	73·2 and 36·5
At P′	71·7 and 37·2
At P″	71·1 and 37·6

In winter, with a clear sky, the temperature at P″ was on an average 0°·54 higher at 9 am. and 0°·86 higher at 9 p.m than at 22ft. 6in., the station P. And in foggy weather 1°·13 and 2°·80 higher respectively. The following extreme differences were observed in foggy weather in September and October, 1873, at 9 p.m.:—

In September	at P′,	4·4 above P.
,, ,,	at P″,	6·1 above P.
In October	at P′,	6 above P.
,, ,,	at P″,	10·8 above P.

There is no instance of temperature at this hour being more than 1° lower at P″ than at O.

At 9 p.m., during anti-cyclones, temperature is between 1°·5 and 2° higher at P″ than at P, and humidity between 3 and 6 per cent. lower.

During fogs in summer, the means at P for temperature were 51°·1 and at P″ 55°·25, and for humidities 90·3 and 78·5 respectively. In winter fogs, temperature at P was 32°·61 and at P″ 35°·41, and humidity 94 and 89·1 respectively.

The mean humidity at P, surrounded by trees, for the whole time was 78·1, at P′ 76·8, and at P″ 76·8.

At 9 a.m. and 3 p.m., the mean humidities differed little. At 9 p.m. they were, at O, 81·4; at P, 82·7; at P′ 80·4; and at P″ 80·1.

The following table shows the average humidities at 9 a.m., 3 p.m., and 9 p.m.:—

		O	P′	P″
9 a.m.	Summer ...	72·8 ...	72·5 ...	72·7
	Winter ...	89·0 ...	87·2 ...	86·9
3 p.m.	Summer ...	60·7 ...	61·2 ...	61·5
	Winter ...	79·8 ...	78·6 ...	78·8
9 p.m.	Summer ...	77·9 ...	75·5 ...	75·1
	Winter ...	87·0 ...	84·8 ...	84·6

Observations have now for some months been carried on by the Meteorological Society at the top of Boston Tower, 270ft. high; on the belfry, 170ft. high; and at 4ft. above the ground below; and, so far, the results are similar to those for Kew, though, of course, at these great heights, temperature is, on the whole, lower than near the ground.

At the Radcliffe Observatory, Oxford, observations have been taken for many years of the maximum and minimum; and 10 a.m. temperatures at 105ft. and 5ft. above the ground.

The excess or defect of mean monthly temperature, calculated from the means of maxima and minima, at 105ft., compared with 5ft., for the 10 years, 1863-72, is as follows:—

Jan.,	+0·2	Apr.,	+0·1	July,	0·0	Oct.,	+0·5
Feb.,	+0·1	May,	−0·1	Aug.,	+0·5	Nov.,	+0·3
Mar.,	0·0	June,	−0·3	Sept.,	+0·7	Dec.,	+0·3

This gives an excess for the year in favour of the upper station of about 0°·2. At this observatory the difference between the minima in the hard winters of 1879 and 1880 was very slight. It may be well to remark, however, that the upper thermometer is here much exposed to the sky, rain, and wind, and that evaporation of moisture from its surface would be more rapid than on the lower one.

At Camden Town, Mr. Symons found in the years 1867-71, from observations on two thermometers, one 4ft. and the other 20ft. above the ground, that at 9 a.m. there was, on the whole, a slightly higher temperature, about 0°·3 at the upper point, and that the mean monthly range was from 0°·50 to 0°·75 less.

Prof. Ragona Scina found the temperature at 98ft. above the ground always colder than at 121ft. at midnight, the means for the months October to April ranging from 0°·91 less in October to 0°·38 less in March.

With regard to differences of temperature between hill and valley, we find results similar to those for artificial elevations, at least when the upper station is situated on the edge of a not very extensive eminence.

Mr. Dines, in 1871, made observations at two stations, Cobham and Denbies, Cobham being situated close to the river Mole, 65ft. above the sea, and Denbies at an elevation of 610ft., the two places being separated by a gradual slope of $6\frac{1}{4}$ miles.

The following table gives the

	Means of maxima.			Means of minima.	
	Denbies.	Cobham.		Denbies.	Cobham.
January ...	35·2	37·7	...	28·5	27·3
February ...	45·8	48·8	...	36·7	36·3
March ...	53·2	55·9	...	36·4	35·3
April ...	55·0	58·9	...	40·9	40·7
May ...	61·9	66·4	...	41·7	40·0
June ...	63·9	68·4	...	45·9	46·4
July ...	69·4	73·2	...	51·9	53·1
August ...	75·5	77·7	...	53·7	51·5
September ...	65·7	69·6	...	48·8	47·1
October ...	56·9	59·8	...	42·2	40·4
November ...	42·7	45·6	...	31·9	30·0
December ...	40·6	43·2	...	33·0	31·7

The mean of all the maxima for 18 months was about 3°·2 less on the hill than in the valley, of all the minima, about 1° higher.

The hottest day produced 91°·4 at Cobham, and 84°.2 at Denbies.

The coldest night produced a minimum of 1°·2 at Cobham, and of 14° at Denbies.

The minima were six times below 10° at Cobham, with an average of 6°, while at Denbies the average minima on the same nights was 15°.

The mean daily range at Cobham was 19°·0, at Denbies, 14°·8.

Mr. Dines has also made observations in 1876, 1877, and 1878, on the temperature at 4ft., and at 50ft. 9in. above the ground. From these he found the average of mean maxima to be 1°·23 lower at the upper station, the mean minima 0°·89 higher, and the mean daily range 2°·11 smaller. The mean daily range was 15°·47 at 4ft., and 13°·36 at 50ft. On many evenings about sunset temperature was much higher at the upper level. For instance, on the 31st of October, 1880, at 7 p.m. on the tower it was 68°, at 4ft. above the ground 58°·5; and on March 16, 1882, 54°·1 on the tower, 44° at 4ft., and 30°, by an exposed thermometer, at 6in. above the grass. These great differences seem to occur only when the sky is clear or covered with cirrus. Mr. Dines confirms the view of Mr. Glaisher that in the middle of summer equality of temperature occurs between 5 and 6 a.m., and p.m., and we may, perhaps, place it with tolerable accuracy at about one hour and three quarters after sunrise and before sunset, throughout the year.

On June 16th, 1882, I obtained the temperature at 2ft. above the ground betwen 4.30 and 4.50 a.m.; on the top, and at the bottom of Richmond Hill over the grass of the Park. The night had been fine, and the sky was still clear at 4.30. There was no mist on the ground, but the sun was shining dimly. The temperature at the top was 40°·5, and on the level below, about 300 yards distant, 38°·5.

Six found a difference on some clear nights of 10 deg. in the temperature at 220ft. from that at 7ft., the upper station being always warmer at night.

Observations taken in Switzerland a few years ago shewed that a much milder and more equable climate prevailed at a few hundred feet above the valleys than at the bottom, where the vegetation of the hillsides would not thrive.

In an ascent of Snowdon on April 15, 1875, with various meteorological instruments, I took the temperature about every 300 feet in ascending, and every 500 feet in descending, and from the results concluded that on that day, at 9.15 a.m., temperature at 860ft. was slightly higher than in the valley, and at 7.10 p.m., at 730ft., about the same as at Llanberis. The weather was fine and hazy.

The frost of December, 1879, was remarkable for its great severity and long duration, and Mr. Marriott has collected valuable statistics regarding its distribution in the British Isles. From these it appears that at Farley, 638ft. above sea-level, the absolute minimum recorded during the frost was 17°·0, while at Oakamoor, 350ft. above sea-level, and only a mile distant, the great cold of 1°·1 was reached. Similarly at Cheadle 646ft. high, the minimum was 17°.6, while at Tean, 470ft. high, the minimum was 2°.0. These stations are situated respectively on the hill and in the valley.

Let us now compare the results, chiefly those obtained at Kew Observatory; with the results of observations at six climatological stations in the year 1880, three of which are situated inland and three on the sea-coast.

The following table exhibits the altitude of each station, the daily means of minima and maxima, and the mean daily range.

The stations are chosen at random, and from comparisons with others, I believe they are fairly representative:—

	Height above sea.	Mean Min.	Mean Max.	Mean Range.
Isleworth	68	42·0	57·4	15·4
Aspley Guise	433	41·4	55·7	14·3
Chester	65	41·9	56·4	14·5
Teignmouth	50	45·3	57·8	12·5
Worthing	28	44·4	56·0	11·6
Colwyn Bay	180	43·2	55·5	12·3

The mean of the mean daily range of the three inland stations was 14°.73, and of the three coast stations 12°·13.

The mean daily range of the environs of London is given by Dr. Clarke at 15°.

We have seen that the mean daily range at Kew Observatory for the 19 months Pagoda observations was 14°·03. In these 19 months, however, autumn is not fully represented. By adding to the Kew Observatory range the quantity 1°·37, which makes it equal to Isleworth, and to the ranges for the Pagoda their due proportions, we shall get values comparable with those of the other stations, and the following table gives the results:—

	Mean Daily Range.	
Isleworth	15·40	
Greenwich	15·2	for 1880.
Kew Observatory	15·40	
Chester	14·50	
Aspley Guise	14·30	
Pagoda, Kew (P)	14·63	
" " (P′)	13·76	
" " (P″)	12·82	
Teignmouth	12·50	
Colwyn Bay	12·30	
Worthing	11·60	
Cobham 4ft.	15·47	
" 50ft.	13·36	

Here it appears that the daily range of all the inland stations exceeds that of the two higher Pagoda stations, and that the daily range of P″ exceeds by only 0°·32 the daily range of Teignmouth, and by 0°·52 that of Colwyn Bay. Aspley Guise, which stands high, and has the smallest daily range of the inland stations, yet exceeds P″ in daily range by 1°·48.

At two seaside climatological stations there are two points of observation, at unequal altitudes, and from these we gather that, at the seaside, daily range increases fast with altitude

and distance from the sea—exactly the converse of the rule at inland places with regard to hills and valleys. Indeed, at a short distance from the sea, on high ground, the moderating influence of the ocean reservoir seems to be lost. But a depression or valley terminating at the sea-shore would shew a much lower evening temperature, even close to the sea-shore, than the top of an adjoining cliff. It seems that the small daily range of the sea-shore must be due, first, to the balancing radiation from or to the equable ocean mass; secondly, to more copious moisture preventing radiation to space; and, thirdly, to sea-breezes. Thus, the warmest situation would be one close to the sea, a little raised above it, say 25ft., with a very full view of the sea, and not too wide an exposure to the sky.

We have seen that the greatest differences between the temperature near the ground and at some height above it have been observed during ground fogs. Radiation then seems to proceed from the fog stratum through the drier air above it almost as from a grassy surface. After the hard frosts of the end of January, 1880, a very light warm current from the south supervened on the 30th and 31st, greatly raising the temperature. The ground having been much chilled, did not thaw in the shade, even when the thermometer rose on these days to 45° and 50°, and an exceedingly dense ground fog was found. At Greenwich the highest temperature on the 30th was 50°·7, but at midday a thermometer sunk 1 inch below the surface of the earth registered only 34°. On the 31st temperatures of air and ground were respectively 48°·6 and 33°·2, and on the 1st of February 45°·7 and 32°·8. These enormous differences, though probably not so great as would have been registered between the soil and the air 30ft. or 40ft. above it, are sufficient to account for the dense ground fog produced. The air must have been cooled far below its dew point. The following table exhibits the maximum temperature in air of several days in January and February, the mean temperature of those days, and the temperature at a depth of 1in. below the surface of the earth at midday.

	Max.	Mean.	1in. below surface.
Jan. 30	50·7	38·8	34·0
" 31	48·6	38·5	33·2
Feb. 1	45·7	34·6	32·8
" 2	47·2	35·2	37·5
" 3	45·5	40·7	35·2
" 4	42·1	36·5	33·2
" 5	45·6	35·2	33·2

The various observations above noticed clearly establish the following conclusions:

1. That the mean temperature, at a height of 100ft. above the ground, does not appreciably differ from the mean temperature at 5ft., but seems slightly to exceed it.

2. That the means of daily maxima, at heights of 69ft. and 128ft. 10in., fall short of the mean maxima at 10ft., and still more of the mean maxima at 4ft.

3. That the means of daily minima, at heights of 69ft. and 128ft. 10in., exceed the mean minima at 10ft., and still more the mean minima at 4ft.

4. That there is a certain altitude, apparently about 150ft. above the ground, at which, while the mean temperature is equal to that at 4ft., the maxima are lower and the minima higher than at any lower point.

5. That, on an average of 19 months, the mean of maxima was about 1°·5 lower at 128ft. 10in. than at 10ft., and the mean of minima about 0°·55 higher.

6. That, in cyclones, the higher, and, in anti-cyclones, the lower points, generally have the lowest mean daily temperature.

7. That the mean night temperatures are always highest at the higher points, and the mean day temperatures always lowest.

8. That at sunset in clear or foggy weather temperature falls much faster near the ground than at some height above it.

9. Equality of upper or lower temperature seems to occur about two hours before sunset and after sunrise, but varies with the season.

10. That in clear weather and low fogs, between sunset and sunrise, temperature is always, or nearly always, higher at heights varying from 50ft. to 300ft. above the ground, than at heights varying from 2ft. to 10ft., and 22ft.

11. That in bad weather the higher points are coldest, both by night and day.

12. That in foggy weather, especially in ground fogs, temperature is very much lower near the ground than at heights of 50 to 300ft.

13. When a thaw takes place after a hard frost, temperature is much higher at some elevation above the ground than near it.

14. A thermometer at 50ft. has shewn a temperature 10° above that at 4ft., and 24° above that of the grass at 7 p.m.

15. The mean daily range decreases rapidly with height, and at 128ft. is about 2°·5 less than at 4ft.

16. The mean daily range at a height of 128ft. nearly approaches that of the English sea coast, and the mean daily range at 69ft. is about midway between the range of inland and of sea-side stations.

17. The mean humidity at 69ft. and 128ft. is more than one degree less than at 22ft. surrounded by trees.

18. In the middle of the day humidity is slightly greater at 69ft. and 128ft. than at 10ft., but at night between two and three degrees less.

19. The humidity at heights from 50ft. to 300ft. is at night much less than near the ground, and the mean humidity is less than at most seaside stations.

20. Places situated on hills or slopes from 150ft. to 700ft. above a plain or valley have a much smaller annual range, and also a smaller daily range than places on the lower level.

21. A higher station (545ft.) may, on the coldest nights, register minima 12 or 13 degrees higher than the lower, and on the hottest days maxima 7 degrees lower.

22. It appears that at seaside stations minimum temperatures decrease and daily range increases with elevation from the shore.

Summing up these results we find that a height about equal to that of the upper rooms in a high house a more equable and drier climate prevails, than at lower levels, drier than at the sea side, and with a daily range not much greater, much less cold on the coldest and on foggy nights than down below. Conditions on natural elevations are on the whole similar.

With regard to their bearing upon health, we may conclude that, in ordinary circumstances, delicate persons should not sleep on a ground floor, or live in low, confined situations, and that living near the top of a high house or on the ridge of a hill, might be of great benefit in many cases of lung and throat diseases, and in cases where night air has a bad effect. That the ground floor of all houses should be built above the ground level and well ventilated underneath, and that no houses penetrable by damp, especially cottages in the country, should be considered habitable in which these precautions have not been taken. Of course, basements warmed constantly by fires and surrounded by drained and paved soil are less objectionable.

That in a thaw after hard frost, when colds and chills are frequent, fires are more necessary in lower rooms than during frost. That in frosty weather there must be some economy in admitting fresh air to a house from the upper part, where the air enters less cold than near the ground. That chills are more probable in low situations and from draughts in lower rooms about the time of sunset than at higher levels. That close proximity to rivers, where extremes of temperature and damp mists occur, is most unwholesome for rheumatic and delicate persons. That for soldiers in time of war, and all who camp in the open air, it must be of great advantage to be raised a little above the ground at night. Some of these conclusions only confirm principles which have long been known and acted upon.

A unanimous vote of thanks was passed to the author.

"*On the Influence of the Purity or Impurity of the External Air on the Health and Moral Tendencies of a Dense Population,*" *by* RALPH CARR ELLISON.

IT appears to me, as an old inhabitant of the district contiguous to Newcastle, and one who has known the banks of the Tyne intimately from 1820 downwards, that in studying the sanitary position of this city and its neighbourhood, one of the first considerations must be whether the atmospheric conditions are upon an equality with those of some other great manufacturing centres, such as Manchester. Again, whether the host of tall chimneys and steam-vessels and tugs which line the shores of the Tyne or traverse its waters, emit respectively more or less of opaque smoke than similar tall chimneys and steamers lining or navigating the Thames between Westminster Bridge and the sea.

If we find that the air, as at Manchester or along the Thames, is continually occupied by a certain haze of smoke, to which each tall chimney is seen to be contributing a certain permissible stream, which, however, it never long exceeds with impunity—then we may say that the path of sanitary science at Newcastle and along the Tyne immediately above and below the bridges, for some five miles either way, does not present any very abnormal atmospheric problem, but only such as sanitary students are accustomed to meet in other great seats of manufacture or other ever-busy sea-ports.

But is this so? I fear not. In the vicinity of the Tyne uch is the cheapness of the coal, that it has been always used with a most lavish and wasteful profusion. Mechanical stoking in connection with furnaces adequate to the work to be done has rarely been adopted. Manual stoking seems to be the rule, and that by men working in the most old-fashioned and perverse method, according to which all fresh fuel, instead of being placed in front of the furnace, is thrown back under the very shaft of the chimney, so that its cloud of intensely thick and black smoke shall pass directly into the atmosphere without ever having passed over the furnace, which might under better management have converted most of it into flame, thereby greatly economising fuel. So obstinate are the unskilled men employed as stokers, that they rarely thrust back the red hot cinders in front of the fire, in order to make room for a fresh supply to be laid in that position, so that its smoke might pass over the whole body of fire behind and be commuted into flame. They say, either that they are not supplied with the proper implement for thrusting back the fuel, or they decline to use it.

Again, the cheapness of coal fuel is a continual temptation to defer the construction of new and better furnaces. The old ones may be made to do the work by forcing them with a lavish supply of coal, which unskilled stokers are allowed to apply in any way they may be pleased to prefer.

Such are the unhappy tendencies which prevail on the estuary of the Tyne. To neutralize them and render our tall chimneys as little corruptive as may be to atmospheric purity as those upon the banks of the Thames or the Irwell would require the anxious deliberations and the unceasing vigilance of our corporate bodies in Newcastle, Gateshead, and elsewhere, and hearty co-operation on the part also of county magistrates.

I am unable to say that really adequate efforts have yet been directed to this beneficent work.

The discharge of densely opaque black smoke from the chimneys of furnaces, factories, breweries, and the like, in vast volumes, after every replenishment of fuel, and this for long periods, is the rule and not the exception. There is, however, one honourable and distinguished exception, that of Sir William Armstrong & Co., at Elswick, where a vast business is conducted with as little production of smoke as possible, and with corresponding economy of fuel. I know not whether any other such exception can be quoted. The smoke pall which hangs over Newcastle week after week, from Monday morning till noon on Saturday, is fearful to behold. Gateshead responds with slightly abated proportional contribution to the darksome cloud. Whether the general display will reach its normal intensity during the week of the Sanitary Congress will be an interesting matter to observe.

We are clamorously assured, however, that mere coal-smoke, unlike that from lead works, is not injurious to human health. So say the manufacturers, and so repeat all who are anxious for their good-will. But what say the medical profession when these gentlemen and their families call in their advice for chronic dyspepsia, for maladies of the respiratory organs, and for general debility? What say they to these applicants and to a host of patients from the families of tradesmen who are well-to-do and able to expend money in search of restored health. They are told that the first condition of improvement must be sought in removal from the air of Newcastle. They are apprized that the air of this town and neighbourhood is sadly deficient of ozone, that vivifying and invigorating principle which renders country air so healthful and so delightful. Sometimes they seek the moorland resorts of Northumberland at Rothbury or Wooler; on the sea coast at Seaton Carew and Redcar are favourite resorts to the southward, together with Saltburn. Between the Tyne and the Tweed are Cullercoats, Whitley, Newbiggin, Alnmouth, Bamborough, and Spital, all depending for their prosperity greatly to visitants from Newcastle coming under medical advice. In like manner the popular spas of Croft and Gilsland.

But the working men and their families cannot come; they can enjoy no such means of recruiting impaired health.

Compare the pallid faces of the men, women, and—alas!—above all, of the children born and brought up in this great town. Compare their pallid countenances with the ruddy aspects of the good folk of the North Riding of Yorkshire, or of the City of York even. Compare them with the still ruddier North Northumbrians. And then go and believe the cruel myth that coal smoke does no harm to human health.

To compensate for the want of exhilarating ozone in the air, an inordinate appetite for the stimulating food of butcher meat besets our town and colliery populations, and, concomitant with this, a raging thirst for beer, ale, and ardent spirits. The air has lost its virtue; it is charged with sulphureous fumes and with bitter black carbonic dirt, which must be absorbed into the lungs; and these are two ingredients which we are told are not unwholesome to man.

Alas for the young children! Are we to believe that these little pale-faced creatures could ever keep up the present number of the population? Assuredly not. The town population requires to be incessantly recruited from the country, or it would gradually die out. And yet, forsooth, coal smoke is not unwholesome, in whatever excess!

Now let us see how the smoke affects the ventilation of dwelling houses—even of the best class of dwelling houses in Newcastle and Gateshead.

Go into the streets of these towns on the finest day in summer, and there will hardly be seen a window open. The

good lady of each house and her domestics are afraid of "the blacks" which would enter. Let any one think what this perennial non-ventilation of eating-rooms and of bed-rooms must imply—the impurity which it of necessity involves. And yet coal smoke is not unwholesome; it needs no regulation; let manufacturers make as much of it as they and their stokers are pleased to think convenient!

As regards intemperance, it is too true that the people of rural villages in the purest localities as to air are inclined to excess in drink on market days, or on Saturday evenings, when they get together. But it is not true that they are equally intemperate with our urban population—even when equally well furnished with money. And, above all, they take their cue from the unhappy example of the urban people, as it reaches their knowledge. Were our great towns more temperate, our villages would soon respond with marked improvement, now that education is everywhere diffused.

One of the worst effects of unregulated excess in the discharge of smoke from a host of tall chimneys is the perpetual state of unsightly dirtiness which it compels the people of the working class to live in. They can scarcely remain personally clean, or enjoy the honest pride of a clean shirt, except for a very few hours. The consequence is an inevitable loss of self-respect, which is much to be deplored. A considerable number of the upper class in the town and neighbourhood are painfully aware of the evils I have attempted to describe, most briefly and imperfectly; above all, the clergy and the ministers of religion generally are painfully alive to them, as standing seriously in the way of the impovement and elevation of their people.

Medical men are universally ardent advocates of smoke regulation and abatement.

The architects are all heartily on our side, for their noble art is degraded in all its productions by the visible pollution with which it is soon coated.

The owners of mansions and landed property, and the inhabitants of villas in the vales of Tyneside, Derwent, and Ravensworth are all heavily visited by the smoke-bane of Newcastle and Gateshead, which closes over them like a pall, whenever an easterly or north-easterly wind may bring night over them at mid-day.

In the case of London, it is the smoke that proceeds from a countless multitude of domestic chimneys, which is the most difficult to regulate and diminish. Not so in Newcastle, where there is no such stupendous extent of territory covered by dwelling-houses.

Accordingly we find that on Sundays, when none but domestic chimneys are in operation, the smoke over the town ceases to be serious or worthy of much attention, even in winter time, though, doubtless, better arrangements would much reduce it.

If I be asked, what is the path of improvement which may be hoped for in Newcastle for the diminution of its present utterly disgraceful cloud of smoke? I reply, that the time has evidenly come when all those who feel and deplore the presence of so great an evil may associate and organise themselves into an active and efficient body of reformers, when representations, petitions, and deputations cannot be long without effect on the Corporations of Newcastle and Gateshead. Nor is it necessary to stop there. They can have recourse to the Local Government Board in London for information and advice as to the best method of urging this important subject, not only on the Municipal Bodies, but upon the Urban and Rural Sanitary Districts in the two adjoining counties which border the navigable portion of the Tyne. There is endless work to be done, and each step will produce some perceptible amelioration. Let us then begin.

The PRESIDENT moved a vote of thanks to the reader of the paper, which, he said, had quite proved that people could get rid of smoke if proper steps were taken. It was clear that the law was sufficient for the purpose if it were only put in force, which it had not been up to the present. The attention of the authorities could not be be too often directed to this matter, which was one of public interest.

Mr. C. S. SMITH said he thought the paper was a very useful one, and the reader of it had certainly called attention in it to a subject which was worthy of a very great deal of attention. The smoke was, no doubt, a very great nuisance, but they had to take into consideration the fact that the prosperity of this district very greatly relied upon manufactures, in which the use of coal in large quantities was absolutely necessary. The authorities had always found some difficulty in putting the restrictions in regard to smoke very firmly into force, as they feared that they might interfere with the prosperity and trade of the district. Alkali manufactories were an abominable nuisance, as they emitted a deleterious smoke which did harm to vegetation, and, he presumed, to human life. The most stringent measures should be used in regard to alkali works; but he pointed out that the power in regard to them was exercised by Government officials. He said that such meetings as the Sanitary Institute were holding in Newcastle were calculated to cause Parliament to give greater consideration to further powers sought by municipal bodies.

Captain DOUGLAS GALTON, C.B., said he should like to impress upon the inhabitants of Newcastle that much had been shown by two exhibitions in the course of last winter, one at Manchester and the other in London, that it was perfectly within the power of the owners of manufactories to prevent the emission of black smoke. It was quite certain that all black smoke from bituminous coal could be prevented, and he hoped that within a short time they would have that fact recognised all over the country. At several works in South Staffordshire the formation of black smoke was avoided by the adoption of some of the various means shown at the Manchester and the London Exhibitions, and if the manufacturers in the Newcastle district would turn their attention to this subject, he was sure they would get rid of the smoke nuisance.

After a few remarks from Mr. E. C. ROBINS, the vote of thanks was agreed to.

"*On the Establishment of a Library for a Special Branch of Sanitary Information at Newcastle*," by R. B. GRANTHAM, M.Inst.C.E., F.G.S.

HAVING for some years taken an interest in Sanitary matters professionally, and in occasionally writing upon subjects connected with the Science, and promoting its objects, at one period as Chairman of the Committee of the British Association, for seven meetings of that Society, I am on this occasion desirous of taking part in the proceedings of this Congress, and beg to suggest that a distinct and special collection of authentic publications on Sanitary Science should be formed here, if those who have the means and opportunity of aiding such a suggestion will agree to it, and will interest themselves in forming and maintaining such a collection. It would prove of great service to those who practice in that Science, and I should hope by quoting the words of Dr. Alfred Carpenter, in his excellent inagural address to the Sanitary Institute in the Session of 1880-81, it would promote my object, viz., that "the arrangements which the Council have made for the formation of a Local Branch of the Institute in the town which invited them, will be the means whereby a lasting impression may be made upon the district, and something be left behind which shall mark the visit, and make it remembered by the student in Sanitary Science whose enlightenment has commenced with the visit of the Institute to his place of residence."

I am also desirous of promoting and extending by all the means at command the teaching for which the Institute was established, and for which it is dependent upon the support of all who value the principles that it is its endeavour to establish.

The following is a list of some of the works and documents, which will be required to form a distinct Sanitary Science Section in a Library.

The principal Acts of Parliament affecting Sanitary legislation, viz.:—

Gas Works Clauses Act, 1847.
Water " " "
Public Health Act, 1875.
" " " Water, 1878.
Rivers Pollution Act, 1876.

Local Government Board. Model Bye Laws for Sanitary Authorities.
Chambers, George F. Digest of the Law relating to Public Health and Local Government.
Glen, W.C., Q.C. Public Health Act.
Michael and Will. On Gas and Water.
Woolrych. On the Metropolis Local Management Acts, 2nd Edition.
Denton, J. Bailey, M.Inst.C.E. Sanitary Engineering.
Field, Rogers, M.Inst.C.E. Bye Laws and Regulations with reference to House Drainage.
Galton, Douglas, C.B., F.R.S. Observations on the Construction of Healthy Dwellings.
Latham, Baldwin, M.Inst.C.E. Sanitary Engineering.
Rawlinson, Robert, C.B. Suggestions as to Drainage, Sewerage, and Water Supply.
Report of the Committee of the Local Government Board on Treatment of Town Sewage.
Sixth Report of the Royal Commissioners on Pollution of Rivers.
Parkes, E. A., M.D., F.R.S. A Manual of Practical Hygiene.
The Principal Acts relating to the Duties of Inspectors and Surveyors under the Public Health Act, 1875:
Public Health Act, 1875.
" " Water, 1878.
Canal Boats Act, 1877.
Sale of Food and Drugs Act, 1875.
Rivers Pollution Act, 1876.
Artizans and Labourers' Dwellings Act, 1868.
Alkali, &c., Works Regulation Act, 1881.
Corfield, W. H., M.A., M.D. The Laws of Health.
" " Dwelling Houses: their Sanitary Construction and Arrangements.

de Chaumont, F. S. B. F., M.D., F.R.S. Manuals of Health. The Habitation in relation to Health.
Grantham, Richard B., M.Inst.C.E., F.G.S. The Reports of the Committee of the British Association in the years 1870 to 1877, on the Treatment and Utilization of Sewage.
Corfield, W. H., M.A., M.D. The Digest of the same.
Grantham, Richard B., M.Inst.C.E., F.G.S. On Public Slaughterhouses.
Grantham, Richard B., M.Inst.C.E., F.G.S. On the Drainage of Slough, Buckinghamshire.
The Transactions of the Sanitary Institute of Great Britain. Vols. I. II. III.

The works mentioned in the foregoing list are in the library of the Sanitary Institute, and they denote the kind of works which I am endeavouring to recommend as suitable to the purpose of commencing a collection of Sanitary works.

I have collected and have had bound together the Reports of the Committee of the British Association for the Treatment and Utilization of Sewage, and beg to present the volume as a commencement of the formation of the collection.

In the Appendix of the Third Volume of the Transactions of the Sanitary Institute is a list of the ordinary meetings of the Institute for the Session of 1881 and 1882, when the following papers were read:—

The Law in relation to Sanitary Progress, by W. H. Michael, Q.C., F.C.S.
Suggestions for the Management of cases of Small-pox and other infectious diseases in the Metropolis and large towns, by B. W. Richardson, M.D., LL.D., F.R.S.
The Present State of the Sewage question, by Professor W. H. Corfield, M.A., M.D.
Inaugural Address, by Alfred Carpenter, M.D.
The Administration and Hygiene of British Hospitals, by H. C. Burdett, F.L.S., F.S.S.
The Range of Hereditary Tendencies in Health and Disease, by G. Gaskoine, M.R.C.S.
An Obstruction by the Law to Sewage Disposal, by H. C. Stephens, F.L.S., F.S.S.

I have been in communication with Mr. H. E. Armstrong, one of your Honorary Secretaries, and he informed me that the Literary and Philosophical Society of this town possesses in their rooms one of the best libraries in the Provinces, and that a part of their premises have become available for this collection.

I know that great exertions are being made to establish a Library at Nottingham.

It has been intimated to me that the Local Government Board in London would supply to a Library at Newcastle some works and official papers, but with the stipulation that they should be properly provided with a suitable place, and be preserved and maintained for the use of those who take an interest in Sanitary Science.

On behalf of the Sanitary Institute I beg to present to the Free Library of Newcastle the books accompanying this paper.

Mr. C. S. Smith proposed a vote of thanks to Mr. Grantham for his paper, and also for the presentation he had made as the nucleus for a library.

Mr. T. P. Barkas seconded the motion, and said that they had in the town two libraries, one of which was one of the finest in the United Kingdom. They were about to spend £5,000 on books for the reference library now. He thought it would be best that these volumes should be given to the public library.

Mr. E. C. Robins, F.S.A., pointed out that the Government would probably be much more liberal to public free libraries than they would to subscription libraries. The chance of getting blue books and reports would be very much stronger in the one case than in the other.

The vote of thanks was then carried, and Mr. Grantham replied.

THE FOOD AND ENERGY OF MAN.

A LECTURE

By Prof. F. S. B. F. de Chaumont, M.D., F.R.S.

CAPT. DOUGLAS GALTON, C.B., F.R.S., IN THE CHAIR.

Although eating cannot be said to be in any way a new fashion, it has nevertheless been reserved for modern times, and indeed we may say the present generation, to get a fairly clear idea of the way in which food is really utilised for the work of our bodily frame. We must not, however, plume ourselves too much upon our superior knowledge, for inklings of the truth, more or less dim, have been had through all ages, and we are now stepping into the inheritance of times gone by, using the long and painful experience of our predecessors as the stepping stone to our more accurate knowledge of the present time. In this, as in many other things, we are to some extent in the position of a dwarf on the shoulders of a giant: the dwarf may, indeed, see further than the giant, but he remains a dwarf, and the giant a giant.

There is an old saying that "an army marches on its belly," by which no invidious comparison to snakes, or otherwise, was meant, but merely that soldiers must be fed if they are expected to do useful work. During the great wars of the early part of this century it used to be a common joke among the French that you should never ask an Englishman if he has fought well, but if he has dined well, and then you could be secure of his fighting. Many a true word is spoken in jest. On the other hand, many have affected to despise food, like the hero of Tennyson's "Maud," who complains that he has even to care for "wretched meat and drink;" or, like the modern æsthetic school, who profess to dine off the contemplation of a poppy or a lily, or it may be, a sunflower. Food is said to be a gross thing; but, then, so are coals, working in which is not conducive to refinement or delicacy of appearance—but without coals, where should we be? either in this *omphale* of terrestrial carbon, or anywhere else! The same with food; it may not