Air and rain. The beginnings of a chemical climatology / by Robert Angus Smith.

Contributors

Smith, Robert Angus, 1817-1884.
London School of Hygiene and Tropical Medicine

Publication/Creation

London : Longmans, Green, and Co., 1872.

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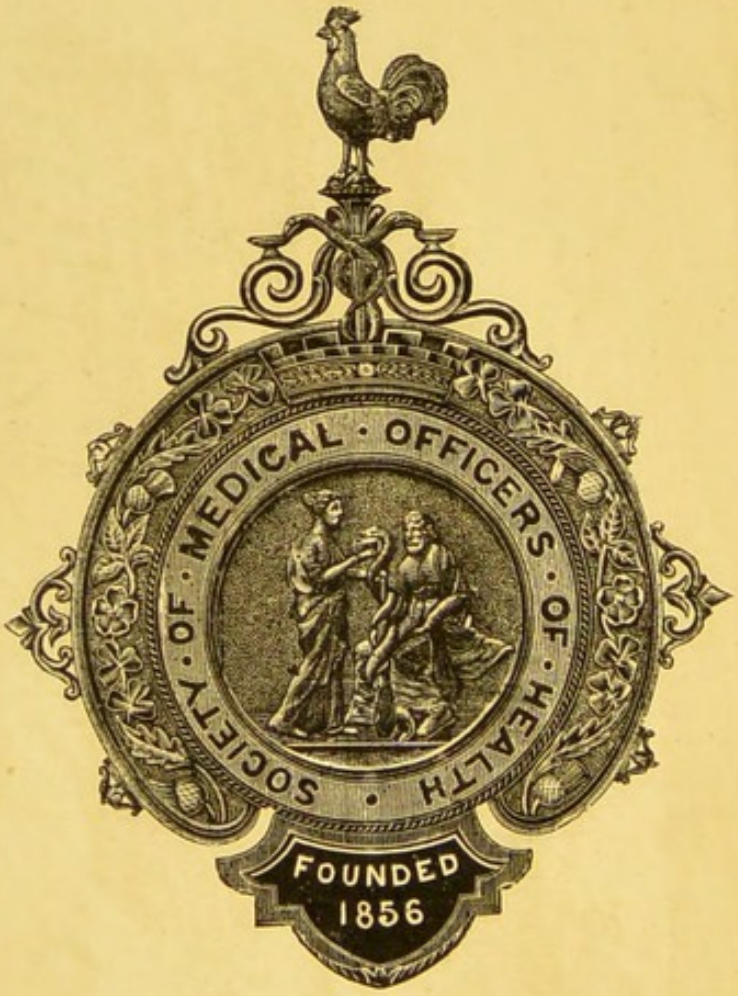
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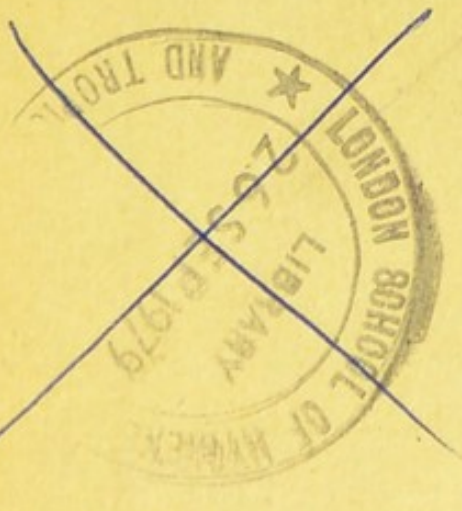
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THE BEGINNINGS

OF

A CHEMICAL CLIMATOLOGY.

LONDON: PRINTED BY
SPOTTISWOODE AND CO., NEW-STREET SQUARE
AND PARLIAMENT STREET

1369
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AIR AND RAIN.

THE BEGINNINGS

OF

A CHEMICAL CLIMATOLOGY.

BY

ROBERT ANGUS SMITH,

PH.D. F.R.S. F.C.S.

(GENERAL) INSPECTOR OF ALKALI WORKS FOR THE GOVERNMENT.

LONDON:

LONGMANS, GREEN, AND CO.

1872.

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TO

JUSTUS VON LIEBIG

THIS VOLUME IS INSCRIBED,

NOT MERELY AS A PROOF THAT I STILL ADMIRE HIM,

AS I DID WHEN YOUNG AND LISTENING TO HIS TEACHING,

BUT ALSO TO REMIND MY COUNTRYMEN HOW MUCH

WE OWE TO HIS GENIUS AND LABOURS.

R. A. S.

PREFACE.

THIS VOLUME could not have been made interesting to general readers without sacrificing that which I suppose to be its best point—namely, the large number of facts. Another more popular in character might perhaps be made to follow with propriety. We have been wearied with speculations about the Air, and it seemed to me advisable to begin building a systematic foundation with experiments which may be repeated frequently, as they have been already repeated. The general character of the result has been arranged, one may say, by the observations and thoughts of many generations. Still I cannot forget that when I came to Manchester in the last year of Dalton's life, I was reminded of him saying that chemical experiment could not distinguish the air of that city from the air on Helvellyn. Dalton may be called the father of meteorology as well as of strict scientific chemistry, and the saying shows how little that which had been done had affected his mind. This volume will make clear how different the matter stands at present. Chemistry has not hitherto done enough in sanitary enquiries, and it ought to be able to relieve medical men of much of their heavy responsibility, although no subject relating to health can be entirely taken out of their hands.

The methods adopted have been the simplest, having been forced on me by degrees; and, whilst the work is abundantly novel, it is certainly not so in the analytical methods employed, but chiefly in the number of expements, the trust reposed in the mode of washing the air, and seeking in the solutions for the substances suspected to injure it.

As to the modes of analysing the solutions, I have used such as were well-known for other purposes in nearly every case. Analytical methods of Liebig, Bunsen, Nessler, and Messrs. Wanklyn, Chapman, and Smith have been freely employed to bring about the results; and one of the most useful portions arises from the application to the air of the modes, devised by the three last-named gentlemen, of decomposing certain organic substances: it is a great addition to the plan of Forchammer, of which it may be said to be a higher development.

Most of the work, judging by the space occupied in the volume, has been done whilst acting for the Royal Mines Commission, or as Inspector under the Alkali Act. In the latter case it was undertaken voluntarily, but I preferred to publish it in the Annual Reports, so as to raise them out of the narrowness to which the words of the Act appeared to confine me, to the position to which those who took an interest in sanitary matters would gladly see them rise. The earlier part, forming the foundation of the whole, was done many years ago, when occasionally making enquiries for the Board of Health, or when stimulated by the ever-ready mind of Edwin Chadwick, C.B., who never loses an opportunity of increasing or causing others to increase our knowledge of matters connected with health.

Chemists who have much to engage them generally learn to lean much on others for their work; and, whilst I have had many assistants, I think it right specially to thank Dr. Theodor Reissig and later Mr. J. W. Gillespie, who confined themselves chiefly to the laboratory, and Mr. George Manley Hopwood, F.C.S., and Alfred Mica Smith, B.Sc., who paid the greatest attention to the calculations and arrangements.

If not on every point able to agree with the illustrious professor under whom I learnt chemistry, and to whom this volume is inscribed, I trust it will be readily forgiven, perfectly aware as I am that if I do hold a different opinion on any subject it is caused in all probability, not by a greater insight on my part, but by a less, which is my misfortune.

Eight years ago a volume was advertised on air and water, to be written by me. The part on Water was completed, but, as I was in the same year requested to enquire into the character of the air of mines, I was obliged to give up attention to water for some time, and to leave the MS. until it was rather too old to be printed without revisal. Being much questioned on the subject of late, I determined to link portions of my papers together; but for the present to confine myself to air, because that subject is freshest in my mind. It was believed that to print the papers altogether would be confusing, and would destroy the natural order of the subject. The method adopted has, however, many objections, as I appear to be so frequently quoting from myself. It is difficult to avoid every fault, and I hope that will be excused. To write a new book from the beginning

seemed to be unnecessary, especially since my work had been scarcely published, the Government blue books being little read.

A small portion on water only was printed in 1865, afterwards more fully in 1868; the chief peculiarity of which was the subdivision of organic matter. I recognised seven different conditions, so to speak, including the nitrous and nitric acids as evidence or measures of old organic matter. The method has been largely used, especially in India, but more exact modes of obtaining the results have since been introduced by others, and for a time I have kept entirely to the study of air.

Some people will look for ozone in this volume, but nothing appears regarding it; I have not been able to make the projected trials; and this work does not attempt to be historic. Many other things are left out in this *beginning*.

Believing that most of our sanitary questions will ultimately be settled by means more or less resembling those treated of here, and knowing how much there is to do, I feel it important that sanitary legislation should not venture too minutely into details until such enquiries have been made more numerous, relieving the subject from the narrowness of merely individual opinion.

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AIR AND RAIN.

AIR.

WHEN we are children, air is to us nothing. A vessel of air is a vessel with nothing in it. Early nations thought in the same way. It was a great day for the world when air was found to be something material, and to be capable of weighing down the scales of a balance. The world had advanced far in other knowledge before it gave up the belief that water could be turned into earth and air and 'everything into everything.' It was fancied that air was spiritual, that it was like the life, and that it was the soul of the world. There was an idea that if solids could only become sufficiently thin and light, they would become like the immortals.

We have learnt that no amount of corporeal tenuity can produce mental subtlety, and thank Descartes for saying, although surprised that he should find it needful to say at such a late period in man's history—'We know that air can be nothing but a collection of particles of the third element, that it is a fluid very rare and pellucid.'

Instead of thinking it nothing, we are now inclined to go nearer to the other extreme. We have learnt that its condition as to warmth, speed, and weight, is so important that we appoint institutions over all the world for the purpose of measuring the frequent variations, whilst the moisture has the most wonderful influence on our lives. We have learnt that there are many bodies invisible as

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air; and of these there are two mainly found in the atmosphere, although there are others in small quantities. Of these latter, one, namely, carbonic acid, influences all the neighbourhood of men and animals, and wherever combustion takes place, whilst hydrogen and its compounds are the consequence of frequent natural phenomena. Besides these, the increasing activity of manufactures brings before us prominently the fact that gases are formed in abundance, and some of them deleterious to plants and animals, being at the same time offensive to the senses.

When we have learnt how numerous these gases are which may be found in the air, we have still to consider the number of vapours. These arise in unknown quantities, and, we may also say, with unknown qualities, from the agreeable scent to the most unpleasant and noxious effluvium.

Even then we have not recounted all the substances in the air. We know that there are solid particles. These consist partly of well-known, but partly of unknown substances. We have common salt and sulphate of soda, we have nitrate of ammonia, and, in times, at least, lime, salts, and iron. We have also phosphates, and, no doubt, in small quantities, iodides, and probably a little of everything at times, which in minute quantities the sea or the surface of the land may contain.

Besides these inorganic substances we have the organic, which have come from animals and from vegetables—the *débris* of living things.

To add to the organic substances, we have those which are organised, living things themselves, and capable of planting colonies wherever they can find food and comfort. These latter may probably be divided into many kinds—the useful and the deleterious, those which promote health and those which bring disease. The idea of any of them bringing health is not founded on anything positive, but we can scarcely imagine these numberless forms to be all

useless. The idea that they bring disease is, I think, one well confirmed, and it ought to be the study of some at least of the medical profession to examine this subject to the utmost. I am not a medical man, but I may quote from a paper read in 1852 to show the purpose I have in view.[1]

PURPORT OF THE ENQUIRIES.

'My object is to show that there are impurities in our atmosphere which may be discovered by chemical analysis, and that the senses and general impressions are not at fault when they speak of the peculiarities of a town atmosphere. I had shown in a former paper that it was not a mere fancy to suppose that the air of crowded rooms was tainted, and that it contained a substance capable of nourishing organic forms, and therefore in itself organic; and although by no means a new idea, as may be shown from old writers, I consider it of importance that these things should not rest merely on ordinary observation, but should be more and more brought under the domain of careful experiment.

'It has often been said that we were unable to tell the difference betwixt good air of the finest mountain-side and the worst of the hospitals (by chemical experiment), or rather, we should now say, of the infected dens of large towns, so well described in various forms of late years to the public. It seemed to many as if the eye had obtained a mysterious power of seeing what was scarcely capable of being proved to be within the domain of substance, and the smell had a power of observing what was more an influence than a positive thing.'

We live in air, and the air flows continually into our blood: no wonder, then, that we are influenced by climate, which means the condition of the air. When we speak of climate we think of the atmosphere in constant motion,

[1] On the Air and Rain of Manchester, *Memoirs of the Literary and Philosophical Society of Manchester*, vol. x.

bringing with it different degrees of temperature and moisture from distant regions. It is everywhere constantly changing, but the changes are of more frequent and of greater amplitude in some places than in others. The average condition is the climate of a place. The changes made by the movements of the air are numerous. The operations of man also are productive of changes so striking and so vital, that we may be said to make a climate for ourselves according to our mode of living. We rush over the world, scarcely considering that the air we inhale must change at almost every step; and we build our houses, not thinking that every field has a climate of its own, unless circumstances are more nearly the same than we can hope for in our country. In extensive tracts, where soil, level, and inclination are similar, such as great prairies or steppes, there will be few changes until the borders are approached, in which case contiguity of other surfaces will produce a variation. In England, which is comparatively a small country, with much variety of soil, it is difficult to find a place where a short distance does not produce some change; and in Scotland, a still smaller and more varied country, the differences of climate are even more striking. Indeed every farmer studies his land in this respect; and the fields are devoted to various purposes, according to climate as well as soil.

We are compelled daily to consider the relation of the size of our rooms to the number of those within them, if we are to live in health; those who neglect this suffer severely. Our towns have to a great extent been built without public recognition of the great fact, and we suffer as a nation by acts that we know to be irrational in private.

We are exposed to great changes of climate arising from the conditions of our civilisation; and although we cannot effect complete alterations, it is possible to do something. To learn the method, we must by careful observation ascertain how we are affected. Who would

have thought that persons living in a swampy district could be cured of ague, and regain their steadiness of muscle, by simply putting drain-pipes under the soil around them? Who would have thought that cold, bracing weather, which is popularly supposed to be healthy, would be so deadly to many, as is shown by the Registrar General's reports? But so it is, and we despise less than before the instinct that shrinks from cold. Who would not be surprised at the meteorologist watching the fluctuations of his barometer, remarking, 'This is a dreadful night somewhere, and wrecks must even now be taking place'? But the admitted correctness of such inferences and their practical utility show the value of observations of the barometer, thermometer, and hygrometer, and of the wind and rainfall.

It was with the desire of clearing the mystery of air to some extent that I have devoted so much of my time to the subject; and now I feel that whilst I have succeeded in doing much of that which I intended to do, I have not got beyond the limits which earlier observers attained by the mere fineness of unaided sense, and by sound reasoning without experiment. Still I hope I shall be found to have put their suspicions into plainer language, proved that which they only imagined, and given in detail that which they only in a general, and we may add, in a vague manner, had attained.

It is not my intention to give much historical matter. The history of our knowledge of air would make of itself a volume, and it might be a very interesting one, and almost amusing. It must stand by itself. It will be enough here if I give conclusions arrived at by my own experiments as a rule, making occasionally very slight deviations in order to bring in the opinions of others when they seem necessary for the elucidation of the subject.

Circumstances led me to approach the subject chiefly

from a chemical point of view, with the occasional use of the microscope, because the condition of our knowledge at the time when I began the study pointed distinctly to the use of chemical experiment, and the microscope as the means of advance.

When the great discovery was made that oxygen was a part of the air and was that which was required for breathing, men imagined the secret of health to be at length attained. Wholesome air was held as that which contained much oxygen or vital air. Many experiments destroyed this hope of happiness to man; he found that no air in nature did contain more than 21 per cent. of oxygen, or at most only a slight trace above this, and that none contained much less, and that if it did contain more, its virtue was limited, since even oxygen itself was no panacea. Infection might exist even where there was the full amount of oxygen, and it might be absent where the quantity was below the average, this state of things occurring in many situations when artificial conditions interfered, such as want of ventilation, and also in natural conditions where gases are formed, which require to be removed by currents. As an example of this, we have the gases from the dying vegetation, and the endless amount formed over much of the earth and in the sea itself by the destruction of life and the incessant activity necessary on water and land, to remove the refuse and to make the way smooth for the living.

Many persons have examined the air, and it will be seen that many results have been attained; but latterly these have been very much alike, and it may be supposed that the question might be allowed to rest. The rougher work was done and done well, but Regnault began to refine it. He first showed the fine distinctions in the amount of oxygen in pure and tainted air. My work has been, so far as the gases are concerned, to carry this out further and to give minute details. These show that

the mountains and great plains have an atmosphere different from that of cities; everybody knew this, but there is shown also the amount of difference. It is true that in figures this appears small, but what is the meaning of small; if we measure size by percentage, it will appear small, but still smaller will appear the strychnine that destroys us if we estimate the amount as a percentage of the weight of our bodies.

The details will be given, but the general result of many analyses will be seen in the following tables. Under the head, constitution of the atmosphere, will be included the experiments of others. Here follows an abridgement of the results obtained:—

Oxygen in the Air.

(Per cent., or, if read as whole numbers, per million.)

	Volume per cent.
N.E. sea-shore and open heath (Scotland)	20·9990
Tops of hills (Scotland)	20·9800
In a suburb of Manchester in wet weather	20·9800
" " "	20·9600
St. John's, Antigua	20·9500
In the outer circle of Manchester, not raining	20·9470
Low parts of Perth	20·9350
Swampy places, favourable weather, France and Switzerland	20·9220 to 20·9500
In fog and frost in Manchester	20·9100
London, open places, summer	20·9500
In a sitting-room which felt close, but not excessively so	20·8900
In a small room with petroleum lamp	20·8400
Ditto, after six hours	20·8300
Pit of theatre, 11.30 p.m.	20·7400
Gallery, 10.30 p.m.	20·8600
About backs of houses and closets	20·7000
In large cavities in metalliferous mines (average of many)	20·7700
In currents	20·6500
Court of Queen's Bench, February 2, 1866	20·6500
Under shafts in metalliferous mines (average of many)	20·4240
In sumps or pits in a mine " "	20·1400
When candles go out	18·5000
The worst specimen yet examined in a mine	18·2700
Very difficult to remain in for many minutes	17·2000

Here it is seen that all the atmosphere in open places has a similar amount of oxygen, and the differences take place in the second decimal place. Still there are differences ; suppose that instead of calling the last four numbers decimals we read them as whole numbers, we shall have 209,990 in a million, and so on.

This table proves distinctly the diminution of oxygen in places where the air is breathed, and it proves also that the analysis of air by estimating the oxygen is an important addition to a knowledge of its purity. The exaggerated amounts of oxygen at one time imagined are not found, but the necessity for attending to small differences is abundantly proved.

Carbonic acid exists in very small quantities in the atmosphere, but it increases around animals, and according to circumstances increases or diminishes around plants. The following is a summary, the particulars being elsewhere given.

Carbonic Acid in the Air.

(Per cent., or, if read as whole numbers, per million.)

	Volume per cent.
In mines—largest amount found in Cornwall	2·5000
Average of 339 analyses	·7850
In theatres, worst parts, as much as	·3200
In workshops, down to	·3000
About middens	·0774
During fogs in Manchester	·0679
Manchester streets, ordinary weather	·0403
Where fields begin	·0369
On the Thames at London	·0343
In the London parks and open places	·0301
In the streets	·0380
On hills in Scotland from 1,000 to 4,406 feet high	·0332
At the bottom of the same hills	·0341
Hills below 1,000 feet	·0337
„ between 1,000 and 2,000 feet	·0334
„ between 2,000 and 3,000 feet	·0332
„ above 3,000 feet	·0336

It would be interesting to know what is the constitution

of the air in various seasons of the year, and over various lands and crops, as well as amongst the crops, and in forests.

It will be observed from the table, that the amount of carbonic acid does not fall below 0·03. Smaller values have, however, been observed in plains. When the oxygen rises high, the amount may be considered correct, even when the percentage is volumetrically wrong. For example, let some of it be as ozone: the condensation of the ozone would produce a result greater than 100. The amount of nitrogen is generally calculated from the remainder, and not directly estimated. It may turn out that, by following this clue, we may obtain a mode of analysis of the air for ozone, if it is condensed oxygen.

Some people will probably enquire why we should give so much attention to such minute quantities—between 20·980 and 20·999—thinking these small differences can in no way affect us. A little more or less oxygen might not affect us; but supposing its place occupied by hurtful matter, we must not look on the amount as too small. Subtracting 0·980 from 0·999, we have a difference of 190 in a million. In a gallon of water there are 70,000 grains: let us put into it an impurity at the rate of 190 in 1,000,000; it amounts to 13·3 grains in a gallon, or 0·19 grammes in a litre. This amount would be considered enormous if it consisted of putrefying matter, or any organic matter usually found in waters. But we drink only a comparatively small quantity of water, and the whole 13 grains would not be swallowed in a day, whereas we take into our lungs from 1,000 to 2,000 gallons of air daily. The detection of impurities in the air is therefore of the utmost importance, and it is only by the finest methods that they can be ascertained in small quantities of air, even when present in such quantity as to prove deleterious to health.

We must remember also that the blood receives the air

and such impurities as are not filtered out in its passage, whilst it is the stomach which receives the water we drink, and that organ has for many substances a power of disinfection and destruction which blood does not possess. If by inhalation we took up at the rate of 13 grains of unwholesome matter per day—half a grain per hour—we need not be surprised if it hurt us. Such an amount is an enormous dose of some poisons, and yet this is not above one two-thousandth part of a grain at every inhalation. It is marvellous what small amounts may affect us, even when, by repeated action, they do not cumulate as certain poisons do. The carbonic acid numbers might have been used instead of the oxygen numbers, with the same result. On the actual effect of carbonic acid there are separate experiments; but its amount, as given, is an important index to the state of the air. The organic matter is the dangerous agent, but not all organic matter; some probably may be wholesome, some neutral, and some putrid, but the most dangerous seems to be the organized, existing as minute germs, and perhaps as full-grown plants or animals also.

We began by assuming very small shades of difference, namely, 190 in a million; but if we examine the table, we find much greater amounts. Take, for example, the pit of a theatre: we have, by subtracting 20·74 from 20·999, a difference of 2,590 in a million, or 14 times more. And so on we may go to the lowest, where we have 17·2 which, taken from 20·999, leaves 3·799, or 37,990 in a million, or 200 times more than the first example. The conclusion to be drawn from all this simply is, that we cannot make the analyses too minutely, and that every deviation from the standard of purity is to be observed and considered.

After finding carbonic acid and oxygen, we have been accustomed to take the remainder as nitrogen. It would, however, be well to know if there is really any tendency

in nature to keep up a definite amount of nitrogen. We know nothing that can diminish or increase its amount directly to any appreciable extent; and when the amount of oxygen is diminished by breathing, the space is filled up by carbonic acid, leaving the same proportion of nitrogen as before. If, however, this carbonic acid is washed out or absorbed, we have at once air with increased nitrogen, but diminished oxygen. This may be supposed to happen when rain washes carbonic acid out of the air of towns, as in a perfectly tight chamber, the carbonic acid formed by breathing is absorbed by lime. Then the nitrogen increases proportionately, the oxygen diminishes really, but the air is kept for a long time in a state pleasant to breathe. It is found, however, that in towns when the rain washes out the carbonic acid, the oxygen is greater than before. In such cases the rain probably makes an exchange; it absorbs the one, and gives out the other.

It is interesting to examine how far this action may be carried. Some of the analyses gave more than 21 per cent. of oxygen. This number is not given among the averages, which form the summary. It is usual to think this a mistake, but it may not be so: as nature has evidently a mode of adjusting differences, certain portions of the air may have at times a greater stock of oxygen for the purpose. The purity of air may be considered as favourably affecting the sanitary state of the more moist counties; but the actual increase of oxygen there is not proved. We may suppose that as pure water dissolves air with a greater amount of oxygen in it than common air, the pure air may give out some in falling. In that case, we require to suppose that the amount abstracted from the upper regions of the air renders the proportion smaller. This would explain the fact that a smaller amount is found on the tops of mountains. We have in vegetation a source of oxygen at certain seasons, and also in animalcular life; but this would not be avail-

able to produce the result on a sudden in a town during rainfall. The formation of ozone in the upper atmosphere does not give us more oxygen; we obtain only a more active condition of that element. The formation of ozone at the surface, and of nitrous gas also, by evaporation, affect, in conjunction with the elimination of oxygen by organisms, the supply of that which may be removed.

Having shown that the atmosphere does differ according to circumstances, and that the chemist can put some of these distinctions into clear language and measure them so finely as to be able to express them by figures, we may proceed to more minute details regarding the gases, and from them to another class of substances still more subtle and difficult to discover.

GASES OF THE ATMOSPHERE.

The air is well known to consist of two gases chiefly, with a third a very small change in the amount of which affects us. It has been found that the air of all places has a similar composition, as we might suppose from the constant mixing which takes place by means of currents always in action, although sometimes to a small extent only. On the first discovery of oxygen by Priestley we could of course have no idea of this uniformity of composition; and even he believed that he had proved the variations 'to be equal in amount to 6 per cent.' At one time he detained 20 per cent. of oxygen, but he did not seize the idea forcibly. Scheele found from 20 to 30 per cent., and others still greater variations; but it was left to Cavendish to show that the differences were very small, and this he did by making five hundred analyses. He arrived at the conclusion that 20·833 per cent. of oxygen is the mean amount, and that the composition is constant.[1]

[1] See Dr. George Wilson's *Life of Cavendish*.

This step made by Cavendish led as to a great and broad landing-place—a place of rest after the toil of mounting a long stair. It was a work of the highest class, and worth the lifetime of a great man, although it by no means occupied all his. It was, however, difficult to believe in absolute identity of composition, and other chemists, from time to time, examined the subject. It will be necessary here to give attention to their labours; we shall learn by what slow stages our knowledge has grown, and how carefully the works of every man have been examined by his successor. Every generation adds to the accuracy, but, as time advances, the errors to be corrected become smaller. Instead of correcting a difference of 6 or even as high as 20 per cent., we now come down to fractions which may be said to have left the first decimal place and taken their stand in the second.

Tables of the Amount of Oxygen in Air. Results obtained by various observers.

	Oxygen. per cent.
Gay-Lussac and Humboldt, after many experiments, which gave from 20·9 to 21·2, settled on a mean of .	21·0
Gay-Lussac himself gave as a mean of the air from mountains and from Paris	21·49
De Saussure examined the air at Chambeisy and found	21·08
	20·98
	21·03
	21·03
	21·13
	21·15
	21·08
	21·09
	20·98
	21·086
	21·006
	21·1
	21·0
	21·04
Mean	21·05

	Oxygen. per cent.
Berthollet found	21·05
Thom. Thomson	21·0
Davy	21·0
Vogel found on the Baltic	21·59
Hermbstädt do.	21·5
Dalton, at Manchester	20·7
,, ,,	20·8
,, ,, (in a N.E. wind)	21·15
,, ,,	20·9
,, ,,	20·73
,, ,,	20·85
,, ,,	20·95

Doyère found			20·5	to 21·5
Regnault gave as the result of 100 specimens in				
Paris			20 913	20·999
9 from Lyons and around			20·918	20·966
30 ,,	Berlin		20·908	20·998
10 ,,	Madrid		20·916	20·982
23 ,,	Geneva and Switzerland		20·909	20·993
15 ,,	Toulon and Mediterranean		20·912	20·982
5 ,,	Atlantic Ocean		20·918	20·965
1 ,,	Ecuador			20·960
2 ,,	Pichincha, higher than Mont Blanc		20·949	20·981
	Mean of all foregoing		20·949	20·988

Mean of the Paris specimens, 20·96.

Bunsen's analyses of air at Heidelberg are as follows:—

Oxygen. per cent.	Oxygen. per cent.
20·970	20·927
20 963	20·919
20·927	20·880
20·914	20·921
20·950	20·892
20·906	20·840
20·943	20·859
20·927	20·925
20·934	20·940
20·928	20·937
20·911	20·952
20·889	20·953
20·928	20·964
20·927	20·960
Average . . 20·924	Lowest . . 20·840

	Oxygen per cent.
R. F. Marchand finds	20·9 to 21·03
Mean	20·97
Graham gives	20·9
Liebig gives	20·9

Of the later men Regnault has made the greatest number of analyses; no man can doubt his power of analyzing well; he is famous for his laborious accuracy. Probably his analyses represent most nearly the true composition of the atmosphere. The analyses made by Bunsen deserve equal respect: he is also a man famous for the minute accuracy of his details, and I would not for a moment put him second to Regnault or to any man; both stand before us as the best specimens of chemical investigators—both living, and in their prime. This is a strong argument for preferring their work to the work of chemists of a past generation. These two have greatly improved the methods of analysis. Still I prefer to take Regnault's results, because they are obtained by a more extensive enquiry. Perhaps the court of the laboratory where Bunsen obtained his specimens may have contained less oxygen than the purest air; at any rate, we cannot doubt Bunsen's accuracy, especially when he gives us these as model analyses.

Analyses made by various Methods.

The earliest analyses were made with gases soluble in liquids. Nitric oxide united with the oxygen of the air; and the resulting compound was absorbed. But, taking all together, they agree with the latest results as well as can be expected. If we examine the results obtained by weighing, we find remarkable differences. It would be difficult to illustrate this difference more clearly than by the analyses of Lewy. When he used the balance he found—

Air of the German Ocean.

	Sky.	Oxygen.
2nd August, 1841	Covered	20·459
3rd do.	Fine	20·423
3rd do.	do.	20·450
4th do.	do.	20·432
22nd May, 1842	Covered	20·884
22nd do.	Fine	20·911
23rd do.	Covered	20·893
24th do.	do.	21·010
24th do.	do.	20·839

Air of Guadaloupe.

			Oxygen in 100 vol.
Petit Canal	20 Nov. 1842	Calm	20·839
do.	20 do.	Calm	20·821
do.	21 do.	Fine	20·848
do.	23 do.	Covered	20·929
do.	23 do.	Calm	20·667
Mangrove on the River Salée	27 do.	Calm	20·839
do. do.	28 do.	Fine	20·504
do. do.	28 do.	. . .	20·522
Petit Bourg	29 do.	Fine	20·802

When he used explosion by hydrogen, his results were as follows:—

Lewy's Analyses of the Air of the Atlantic Ocean. Carbonic Acid in 1,000 *parts.* (*Annales de Chimie et de Phys.* vol. xxxiv. 1852.)

1847.		Each a mean of three Analyses.		
		Carbonic Acid per 1,000.	Oxygen per cent.	
1 Dec.	Cloudy . . .	0·4881	21·05170	We left Havre on the 25th November. The weather was wet, and remained so all the time we were in the Channel.
4 Dec.	Clear . . .	0·3338	20·96321	
8 Dec.	A little cloudy	0·5497	21·05945	54 leagues from Madeira.
17 Dec.	Clear . . .	0·5771	21·06030	30 leagues south of tropics.
18 Dec.	Do. . . .	0·3346	20·96139	Middle between Africa and America.
18 Dec.	A little cloudy	0·5420	21·06099	
19 Dec.	Clear . . .	0·3388	20·96074	Sea phosphorescent.
26 Dec.	Do. . . .	0·5288	21·05889	
28 Dec.	Do. . . .	0·5093	21·05686	
30 Dec.	Do. . . .	0·5143	21·05789	
31 Dec.	Do. . . .	0·3767	21·01114	Entering port of Santa Marta.

Air of New Granada.

Locality.	1848.	Weather.	Mean of three Analyses.		
			Carbonic acid per 1000.	Oxygen per cent.	
Santa Marta .	25 Jan.	Clear . .	0·4616	21·02379	
Mompox . .	7 Feb.	Do. . .	0·3147	21·04936	
Rio Magdalena	18 Feb.	Cloudy .	0·3259	21·03222	
Rio Magdalena	3 Mar.	Covered .	0·4554	20·99826	
Honda . .	29 Mar.	Very cloudy	0·3226	20·99237	
Ambalema .	5 Apr.	Clear . .	1·1203	20·54833	Suffocating hot; wood burning near.
Esperanza .	2 Aug.	Do. . .	2·4475	20·33075	Do.
Guaduas . .	2 Aug.	Cloudy .	0·3068	20·99691	
Santa Ana .	2 Aug.	Clear . .	1·2333	20·54479	Do.
Bogota . .	5 July	Do. . .	0·4994	21·03196	
Montserrat .	8 July	Cloudy .	0·5215	20·98995	Rain beginning.

Lewy's Analyses of the Air of Bogota, 2,645 metres above the Sea.

		Carbonic acid in 1000 parts.	Oxygen in 100 parts.	
1850.				
7 Mar.	Clear .	0·3864	21·02099	Fine.
12 April	do. .	0·3664	21·00382	After rain.
8 May	Covered	0·3609	20·99032	Wet.
9 May	do. .	0·3824	20·99250	Wet.
15 June	do. .	0·4192	20·99506	Less wet.
24 July	Clear .	0·4249	21·01765	
19 Aug.	Cloudy.	0·5043	21·01411	
23 Aug.	Clear .	0·4812	21·01826	Very fine.
1 Sept.	Cloudy.	0·6178	21·02434	
2 Sept.	Clear .	0·7649	21·01700	
2 Sept.	Cloudy.	1·6291	20·96629	The upper part of Montserrat was covered with clouds, and a white veil descended from the mountain. Many persons became ill. Thinks the carbonic acid due to a wind called Las Quemas. After rain it disappeared.
2 Sept.	do. .	1·7040	21·03011	
3 Sept.	Clear .	1·5853	21·01976	
3 Sept.	Cloudy.	4·8963	21·03176	
3 Sept.	do. .	4·9043	21·03197	
4 Sept.	do. .	1·3261	21·02927	
4 Sept.	Covered	0·8648	21·00355	
8 Sept.	Cloudy.	2·2829	21·02097	
9 Sept.	do. .	0·7512	21·03082	
10 Sept.	Clear .	0·4583	21·03199	
12 Sept.	Cloudy.	0·4709	21·02689	
3 Oct.	do. .	0·4751	21·02377	

It would appear as if M. Lewy obtained both gases slightly in excess; but it is not well to attempt to be quite certain on this point.

Dumas and Boussingault together obtain also less oxygen than in the above analyses by explosion; they used weights; and they also are men who stand, like Regnault and Bunsen, in the foremost rank.

	Oxygen per cent.	
Paris . . .	20·810	Dumas and Boussingault.
Brussels . .	20·856	M. Stas.
Genève . .	20·784	M. Marignac.
Bern . . .	20·757	M. Brunner.
Faulhorn . .	20·773	
Gröningen . .	20·793	Verver.
Copenhagen .	20·811	

In looking over the analyses already presented, some of them means of hundreds, and the whole representing many years of labour, we see at once how many give the amount of oxygen to be above 20·9. As a rule those numbers which fall below 20·9 represent air from cities and less pure places or from high mountains, or they have been obtained by weighing the oxygen, a method which seems always to give lower results. Take the conclusion of Cavendish, wonderful at the time, that the average was 20·833, we are surprised at the accuracy of the man who used a method by which no one now seems able to obtain any reliable results. It cannot be supposed to take from the honour of Cavendish, if we add one-tenth of a per cent. to his figures after sixty years of scientific activity has made that apparatus the plaything of boys which in his time a philosopher could scarcely handle. He used acid liquids; and how easy it is to lose a fraction of a per cent. every man who has worked with gas must know and feel most keenly. If, however, anyone shall object to this reasoning, we must put up against him Saussure, a man using the same method; and we find his average much higher, viz. 21·05. This also is strengthened by the labours of Gay-Lussac, Berthollet, Thomson, Davy, and Humboldt, all men to whom we must attend. Cavendish could not decide that the London air differed from that of the country. (See Dr. Wilson's 'Life of Cavendish.')

Dalton laboured long on gases; and although his thoughts were generally greater than his experiments when a theory could be found, few men could so persistently labour out the facts when no theory existed. On the composition of the air of Manchester he is moderate, and, it seems to me, just.

Dumas and Boussingault in conjunction, and also Brunner, gave results by weighing the oxygen. The air was conveyed in bottles, and the bottles themselves were filled by sending them free of air, and allowing the air for

analysis to flow in. There are objections to this method. It is extremely dependent on the accuracy of the apparatus used, and, if I am at all right in my judgment, the tendency is to diminish the amount of oxygen; for although the specific gravity of nitrogen does not differ greatly from that of oxygen, it does differ, and is lighter. At any rate I believe the list of analyses given to be a fair representation of the work done. They may be taken as the evidence of the scientific world on the subject.

Regnault's analyses are very beautiful, and the uniformity of his results predisposes us strongly in their favour. Their number comes in to increase their importance.

Another reason why I admire the results of Regnault is one of a kind which affects all men, and which may be excused. They agree with my own, made in an open part of Manchester. His results are 20·913 to 20·99 for Paris; and all the numbers he made are above 20·9 until he comes to unwholesome places with putrid waters. A similar reason leads me to believe in the analyses by Bunsen, supposing the air he took not to be the best, and those of Dalton made in Manchester, as I found like results when air from close places was examined. Those of Dr. Frankland are also corroborative.

Air deviating from the adopted Standard.

(Air from Heights.)

		Oxygen.
Dalton	Helvellyn	20·64
„	„	20·63
From a height of 9,600 feet		20·7
„ „ 15,000 „		20·62
Brunner	Faulhorn	20·91
Boussingault	548 mètres high	20·7
„	Santa Fé, 2,643	20·65
Air brought by Green from a height of 11,300 feet		21
Frankland	Chamounix	20·894
„	Top of Mont Blanc	20·963
„	Grands Mulets	20·802
„	Jura and other mountains	20·3
Berger	to	21·63
Configliachi	Simplon	21
Mean		20·818

Leaving out some under 20·2, we have still a rather curious result—a lower number on the hills than on the plains. I do not profess to be fully satisfied with these results; we require a few hundred, or at least a few dozen analyses; still these statements are before us, and cannot be removed without much labour. The results of Frankland and Boussingault especially are striking, and require an explanation. Let us again compare results obtained by Dumas and Boussingault ('Annales de Chimie,' 1841).

(Air of Paris.)

	Oxygen.		Oxygen.
By weight,	22·93	Calculated to volume[1] . .	20·729
,,	23·06	,, . .	20·856
,,	23·03	,, . .	20·828
,,	23·01	,, . .	20·810
,,	23·00	,, . .	20·802
,,	23·00	,, . .	20·802
,,	23·08	,, . .	20·826
,,	22·07	,, . .	20·864
,,	22·89	,, . .	20·701
Mean .	23·07		20·864

(From the Faulhorn.)

	Oxygen.		Oxygen.
By weight,	22·96	Calculated to volume[1] . .	20·766
,,	23·09	,, . .	20·882
,,	22·97	,, . .	20·774
,,	22·86	,, . .	20·674
,,	22·97	,, . .	20·774
Mean .	22·97		20·774

Again we find a smaller amount on elevated places.

	Oxygen.
Brunner found	20·867
	20·750
	20·790
	20·842
	20·812
	20·837
	20·818

Although, for reasons given, I prefer the actual numbers

[1] The results are calculated into volumes, taking 1·1057, the number taken by the analysts as the sp. gr. of oxygen.

obtained by Regnault, the comparative numbers in Paris and in the Faulhorn are equally valuable for the purpose of showing differences. These analyses were made by weighing.

Dr. William Allen Miller examined air collected during a balloon ascent, in August 1852, at a height of 18,000 feet, and also a sample collected near the surface at the same time, with the following results:—

	Air 18,000 feet high.	Air near the earth.
Percentage of oxygen . . .	20·88	20·92

Dr. Frankland [1] found at—

	Oxygen.	Carbonic acid.
Grands Mulets	20·802	0·111
Summit of Mont Blanc . .	20·963	0·061
Chamounix	20·894	0·063

He thinks it probable that the carbonic acid is generally, but not invariably, greater in the higher regions of the atmosphere. Messrs. H. and A. Schlagintweit found the carbonic acid to increase up to the height of 11,000 feet.

If the carbonic acid of the higher regions be really greater than in the best air below, and the oxygen less, it will probably be in part owing to the oxidation having been completed more fully. De Saussure considered it to be owing to the action of vegetation decomposing the acid and giving out oxygen at the surface. The organic matter will probably be entirely removed by thorough oxidation in great oceans of air. The process which converts oxygen into ozone would seem very well fitted for removing all organic matter. This, then, might lead to the existence of a smaller amount of oxygen in the air above, and the riddle would be solved. The diminution of the oxygen is probably a disadvantage—although to such a small extent is it so, that there is abundant com-

[1] 'On the Air of Mont Blanc,' *Journal of the Chemical Society*, for 1861.

pensation in the purification consequent on the removal of organic substances. We shall have, then, a distinct variety of air on mountains differing from that of the plains. In the one there would be more carbonic acid and less oxygen, with little or no organic matter—constituting mountain air; whilst the air of the plains would have more oxygen, less carbonic acid, and more organic matter. This is to develope.

It is exceedingly probable that the difference of composition is somewhat connected with the existence of water in the atmosphere; it is less likely to be in part the result of separation caused by the weight of the gases—a result, certainly, which has been much discussed and is well known not to exist in proportion to the weight, and one by no means probable, considering the investigations of Graham: but that the physical cause may after all be struggling so as to make itself in some way felt, it is perhaps rather daring for us to deny absolutely. If the oxygen were diminished without the increase of the carbonic acid, it would be safest to advocate the latter reason. The reasons given in the previous paragraph agree best with the state of our knowledge. However, the discussion on this subject would be better postponed until we become absolutely sure of more facts.

Judging from all these analyses, I was inclined to look on a favourable specimen of air as proved to contain—

	Volume per cent.
Oxygen	20·96
Nitrogen	79·00
Carbonic acid	0·04
	100

In order to be moderate when judging of the air of the mines, I was willing to consider 20·9 as a fair estimate—a number assumed by Liebig and Graham. It must be remembered that this change of oxygen is not intended to affect the carbonic acid.

At present, in consequence of the analyses made, we may view the very pure air as containing not less than

	Volume per cent.
Oxygen	20·99
Carbonic acid	0·030

Air of Impure Places.

Reliable analyses of the air of impure places are less numerous than those given; but decided results have been obtained, showing at least a deviation from the numbers found on analysing fresh air.

Observer.		Oxygen.
Configliachi	Air of rice-fields	20·8
,,	Crowded places	20·3
Regnault	Toulon harbour	20·85
,,	Algiers	20·42
,,	,,	20·395
,,	Bengal Bay	20·46
,,	,, over bad water	20·387
I found in the middle of Manchester, in a place closely surrounded and exposed to smoke		20·868 20·808 20·807 20·613 20·793 20·179
	Average	20·652

The following results have been sent me by Dr. Frankland:—

Air from Laboratory of Owens College, 1852.

	Oxygen.
November	20·883
December	20·898
Ditto	20·868
Ditto	20·876

M. Leblanc, in his researches into the composition of the air ('Ann. de Chimie et de Phys.' t. v., 3rd series, 1842, p. 248), gives the following analyses of inferior and impure air:—

(*The amounts for oxygen were stated in weights, but have been calculated into volumes.*)

	Oxygen by weight, per 1,000.	Oxygen by volume, per cent.	Carbonic acid by weight, per 1,000.
1. Conservatory of Equatorial Plants, Terre de Buffon, 6 o'clock in the evening	230·1	20·81	?
2. Seven o'clock next morning	229·6	20·76	0·1
3. Chemical Theatre, Sorbonne, before lecture	224·3	20·28	6·5
4. Ditto after lecture	219·6	19·86	10·3
5. Bedroom in morning	229·4	20·74	0·4
6. Hall of hospital three hours after shutting the windows	229·1	20·72	0·8
7. Same at 8 o'clock, morning	227·2	20·54	2·8
8. Sleeping-room at the Salpêtrière, atmosphere sensibly bad	225·2	20·36	8·0
9. Another in a similar state	226·0	20·44	5·8
10. Salle d'Asyle, 116 children; smell bad also	227·1	20·53	2·7
11. Salle d'École Primaire, 2nd arrondissement	228·4	20·65	. .
12. Same, incompletely ventilated; no smell	. .	. .	4·7
13. Same, quite closed; a sensation of heat, quickened respiration	. .	. .	8·7
14. Chamber of Deputies, in the ventilating-shaft; no smell	. .	. .	2·5
15. Opera Comique, pit before end of play	. .	. .	2·3
16. Same theatre, centre boxes	. .	. .	4·3
17. Close stable, École Militaire	225·5	20 39	1·05
18. Same stable, ventilated by casements	229·0	20·71	2·2

It was with these results before me that the enquiry for the Mines Commission was begun, and although not desired to speak of the composition of the air generally, it seemed quite necessary to make some experiments, that it might be compared with the air of mines.

Having this great array of analyses, an abstract of the life-labours of many men, we seem to have before us proof sufficient of a decided variation in the composition of the air. The variation is really so great in some places, that

we must admit some powerful local cause. In filthy places, and in marshy spots at a high temperature, the cause cannot be doubted; and how can we doubt the same change to occur in places that are badly ventilated? When carbonic acid increases, is it wonderful that oxygen should diminish? Notwithstanding all this, there is no firmly-founded faith amongst scientific men regarding the subject. The evidence has not been brought fully before them, and probably some links may be wanting. Seeing the question in this state, I was desirous of throwing some light upon it, and indeed had already come to believe strongly in the variations, because of some which had been observed when making occasional analyses for the trial of new apparatus, or for instruction and trial of skill.

Under this impression, specimens of air were collected from the front of the laboratory and from behind, near an ash-pit, each at the same time; and the analyses, along with some others, are given in the following table. Afterwards it will be seen that the carbonic acid of the same spot was also estimated; and the result is that not only is there a diminution of oxygen in the less pure spot, but the carbonic acid, although greater than in the pure air, is not sufficient to make up the vacancy left by the deficiency of oxygen, leading us to look for other gases also that tend to increase the impurity. This is an unexpected result, and, to my mind, one which has in it much value. But, like all other investigations, after leading us onward one step, it shows us that there is another to take.

If emanations arise from foul places, they must occupy space. They are mixed gases and vapours. When we use caustic soda to absorb the carbonic acid, perhaps we absorb some vapours also, and indeed I always find that in a eudiometer we are apt to obtain too much carbonic acid in cases where the quantity is small, unless extreme care is used, when the result has often been correct.

Street and Suburb Air, Manchester, compared with Closet or Midden Air.

Time.	Air from closet or midden behind laboratory.	Air from front door of laboratory.
1863.	Oxygen, vols. per cent.	Oxygen, vols. per cent.
Dec. 1	20·80	20·90
„ 10	20·85	20·96
„ 11	20·79	20·98
„ „	20·72	20·90
„ 15	20·87	20·90
„ „	20·76	20·02
„ 17	20·59	20·96
„ „	20·85	20·78
„ „	20·90	20·83
„ 18	20·21	20·91
„ „	20·58	20·92
„ „	20·74	20·87
„ „	20·40	21·02
„ „	20·77	21·00
„ 19	20·99	20·83
„ „	20·70	20·98
„ „	20·82	20·88
„ „	20·46	21·01
„ „	. .	20·87
„ 21	20·56	20·92
„ „	20·79	21·02
„ „	20·64	20·88
„ „	20·94	20·91
„ „	20·67	21·01
„ 22	20·53	20·96
„ „	20·71	20·92
1864.		
Feb. 26	20·66	21·01
„ 24	. .	21·05
„ 20	. .	20·98
„ „	. .	20·99
„ „	. .	21·01
„ „	. .	20·94
	Average 20·70	20·943

Compare also the specimens from the city of Perth, p. 37.

The meaning of these numbers may be further illustrated. Let us put together all the deviations from 21 per cent. in air from the front of the laboratory and from less pure places (the front may not, after all, give the best air), we have—

Good air.	From backs of houses and impure places.
·10	·20
·04	·15
·02	·21
·10	·28
·10	·13
·02 +	·24
·04	·41
·22	·15
·17	·10
·09	·79
·08	·42
·13	·26
·02 +	·60
·00	·27
·17	·01
·02	·30
·12	·12
·01 +	·54
·13	·44
·08	·21
·02 +	·36
·12	·06
·09	·33
·08 +	·47
·04	·29
·08	
·01	
·05	
·02	
·01	
·01	
·06	
Average . . . 0·065 (subtracting those with +).	Average . . . ·293

This is a remarkable illustration, to my eye, and a conclusive proof.

The results are very distinct; there is nothing exaggerated about them. There are no great deviations to astonish us; and there are so many irregularities, that if two or three analyses only were made the effect would be bewildering or uncertain: by making numerous analyses we are able to show a steady diminution of oxygen, on one side, with occasional risings as the wind may blow

here or there, and a steady rise of oxygen, on the other, with occasional fallings also, as the wind may chance to carry smoke or other gases.

The laboratory stands in an open space, which contains certainly a burial-ground in the centre, but is very much freer from the smoke of the town than the streets are; no manufactories exist beyond or between it and the country. The wind from west and south blows over many houses, but over no large chimneys.

The results clearly show a difference between the air of more and less pure places, and render the oxygen test more valuable than it hitherto has been supposed to be. Unfortunately so many analyses are required, that the test cannot be popular; but as one to be resorted to when the occasion warrants the labour it stands very clear. And indeed how can it be otherwise? We see putrid matter laid on the ground, and find it disappearing rapidly, and yet we are told that it is not accompanied by loss of oxygen; it is not credible, and the results given show it to be incorrect.

It may perhaps be said that, although some of the specimens contain less than 20·9 oxygen, if they came from a clear atmosphere, such air could not be considered very bad. This reasoning cannot hold. Analyses are after all subject to error, and the average is the only number on which we can rely. It may even happen that the small changes are caused by accidents which may give impurity. For example, take gusts of impure air even in the air of a street generally pure.

It is abundantly clear that whenever we leave the region of the uncontaminated or very little contaminated open air we obtain a diminution of oxygen, although that diminution is very small; this small loss is therefore a proof of impurity. In crowded rooms, theatres, cow-houses, stables, and laboratories it is easily proved, and that diminution is enough in decided cases to bring the figures various stages below 20·9.

Air of London.

The examination of the London air for oxygen and carbonic acid shows clearly the value of this plan. It seems indeed that the oxygen gives more striking results than the acid. There is less oxygen in the east, 20·857, and in the rather crowded parts of other districts. In the open places the oxygen rises very decidedly to 20·9500.

In agreement with this the carbonic acid is greatest in the east and least in open places. The range is not great in appearance; we must learn to look on small numbers as of importance.

We must not be violent in our conclusions. The N.W. does not come out very well, but the specimens are too few. We must receive the conclusions in a very general way only.

These analyses show one mode of deciding when a place can be called open. Those numbers which approach nearest those found at Hyde Park are the best.

London Air.—Metropolitan Railway.

Date.	Place.	Time of Day.	Oxygen per cent.
1869.			
Nov. 12 .	Tunnel between Gower Street and King's Cross Stations: specimen taken at the open window, first-class carriage	10 A.M.	20·60
„ 12 .	Tunnel between Gower Street and King's Cross Stations: specimen taken at the open window, first-class carriage	7.30 P.M.	20·79
„ 12 .	Tunnel, Praed Street: specimen taken at the open window, second-class carriage	10.30 A.M.	20·71
„ 15 .	Specimen taken during journey between Gower Street and King's Cross, first-class carriage, windows open	10.15 A.M.	20·66
„ 15 .	Same	3 P.M.	20·70
„ 15 .	Same	11 P.M.	20·74
	Average	. .	20·70

London Air.—Percentage of Oxygen.—November.

	Place.	Oxygen per cent.		Average.
		1	2	
N.	Islington, Duncan Terrace . .	20·86	20·81	20·835
N.	Hoxton, Hoxton Square . . .	20·85	20·82	20·835
N.E.	Dalston, Albion Road . . .	20·90	20·91	20·905
N.E.	Hackney, near Hackney Station .	20·82	20·85	20·835
N.W.	Clarendon Square, Somers Town .	20·90	20·89	20·895
N.W.	Alpha Road and Grove Road . .	20·87	20·80	20·835
N.W.	Near Belsize Park	21·02	21·00	21·010
	Average (Belsize Park left out) . .			20·857 °/₀
S.	Two miles from Clapham Junction .	21·05	20·99	21·020
S.	Lambeth, back street near workhouse	20·85	20·74	20·795
S.	Kennington Park	20·96	20·92	20·940
S.	Pleasant Place, Elephant and Castle	20·97	20·86	20·915
S.	Deverell Street, off New Kent Road	21·00	20·91	20·955
S.W.	Brompton, back street off Sloane St.	20·83	20·89	20·860
S.W.	Brompton Crescent . . .	20·86	20·89	20·875
S.W.	Blackland Terrace, off King's Road	20·79	20·80	20·795
S.W.	Chelsea, Park Lane, off Church St.	20·82	20·80	20·810
S.W.	Near Battersea Bridge . . .	20·85	20·85	20·850
S.W.	Chelsea Hospital, gardens near river	20·91	20·91	20·910
S.W.	South Lambeth, Cavendish Grove .	20·90	20·84	20·870
S.W.	Vauxhall Bridge, near river . .	20·90	20·91	20·905
S.W.	Houses of Parliament, terrace .	20·96	20·93	20·945
S.W.	Westminster Abbey Yard . .	20·91	20·97	20·940
S.W.	Trafalgar Square	20·90	20·87	20·885
	Average (leaving beyond Clapham Junction out) . .			20·883 °/₀
E.	Bethnal Green, Bethnal Green Road and Punderson Street . . .	20·82	20·88	20·850
E.	Stepney, White Horse Street . .	20·87	20·87	20·845
E.	Near London Docks . . .	20·76	20·84	20·800
E.C.	„ London Bridge . . .	20·91	20·84	20·875
E.C.	„ Bank of England . . .	20·91	20·86	20·885
E.C.	„ Holborn Viaduct . . .	20·92	20·87	20·895
	Average			20·86°/₀
W.C.	Woburn Sq., near British Museum .	20·93	20·93	20·930
W.C.	Near Tavistock Square . . .	20·94	20·93	20·935
W.	Regent Street (Great Castle Street)	20 90	20·83	20 865
W.	Oxford Square, near Edgeware Road	20·88	20·89	20·885
W.	Hyde Park, Sloane Street . .	20·91	20·94	20·925
W.	Middle of Hyde Park . . .	21·03	20·98	21·005
	Average			20·925°/₀
	Total average (beyond Clapham Junction not included) .			20·885°/₀

London Air.—Parks and Open Places.

(Oxygen per cent.)

	Place.	Oxygen per cent.		Average.
		1	2	
N.W.	Near Belsize Park	21·02	21·00	21·010
S.	Kennington Park	20·96	20·92	20·940
S.W.	Chelsea Hospital, gardens near river	20·91	20·91	20·910
S.W.	Vauxhall Bridge, near river . .	20·90	20·91	20·905
S.W.	Houses of Parliament, terrace . .	20·96	20·93	20·945
W.	Hyde Park, Sloane Street . .	20·91	20·94	20·925
W.	Middle of Hyde Park . . .	21·03	20·98	21·005

Average . . . . 20·95%

Oxygen of the Air in wet and in dry foggy Weather.

Continuing the subject and going further into detail:

In very wet weather in Manchester, and still before the laboratory, the following results were obtained:—

	Oxygen.
	20·90
	21·01
	21·01
	21·05
	20·96
	104·93
Average	20·98

In dry foggy and frosty weather, when the smoke of Manchester had little exit from the town, the results were—

Near centre of town	20·90
	20·88
At laboratory	20·90
	20·96
At laboratory, afternoon . . .	20·91
„ forenoon . . .	21·01
„ afternoon . . .	20·82
	146·38
Average	20·91

20·82 and 20·89 were found in a dense fog, such as has

rarely visited Manchester. The eyes began to smart, and in walking on the pavement carters were met leading their horses into shops in the day-time—we can scarcely say in the daylight.

Thus we have certified, by experiment as well as the testimony of the senses, the inferiority of the air at certain times, and these senses seem to estimate on certain occasions an amount as small as 0·07; but they do not estimate the loss of oxygen, only the corresponding increase of impurities. In the yard at the back of the laboratory the amount is less than before the laboratory, and is as follows:—

	Oxygen.
	20·80
	21·01
	20·94
	20·84
	21·09
Average	20·936

Thus we have the series—

In very wet weather, in front	20·98
At all times, an average of 32 experiments . .	20·947
Behind, in medium weather	20·936
In foggy frost	20·91
Over ash-pits	20·706

These results surprise me as I write. They come from analyses made some months ago, and without the hope of such a fine gradation of qualities. They seem also to show that we are exposed to currents of good air in the worst, and of bad air in the best atmospheres, in towns like Manchester. This is suggested also by that number, 21·01 or more, so curiously turning up in the analyses of most persons.

In Dwelling-rooms, &c.

If we go to dwelling-rooms, &c., we find the same diminution of oxygen where there is insufficient ventilation:—

Before the door of a house in a suburb of Manchester, the air gave of oxygen	20·96
In the sitting-room, not very close	20·89

In a very small room, with a petroleum lamp burning, a good deal of draught	20·84
After 6 hours	20·83
Pit of theatre, February 13, 1864, 11.30 p.m.	20·74
Gallery, February 15, 10.30 p.m.	20·63

In Cow-houses and Stables.

If, again, we enter cow-houses and stables, the same results are obtained. The following six are so uniform, they seem to be as good as thousands, and are obtained from the only specimens collected. I went in the morning after the cows had been milked and fed, and therefore after the air had been allowed to enter. The houses still had a close smell. The stables were badly closed, and one was open; the specimens were taken near to the horses, as far from the doors as possible, avoiding the direct breath of the animals. I cannot call them fair examples of the air breathed by the horses or cattle alone; but they are very fair specimens of the kind of air breathed by those who work or visit there. Two of the stables were for cab-horses, the doors half open; a third was a gentleman's stable of four stalls, but there was only one horse; the door was shut. The air seemed pretty good, and still the loss of oxygen is visible in the analyses:—

	Oxygen.
Cow-houses	20·70
,,	20·78
,,	20·75
Stables	20·82
,,	20·74
,,	20·74

In such places as have been last described it would not be pleasant to live, and in the atmosphere of the theatre we know how much desire of fresh air is produced. Yet none of these numbers are so low as 20·6, the number assumed as marking the beginning of very bad air. The temperature of the theatre in the pit was 78° F., and this is a common temperature in the mines. By taking Leblanc's analyses a somewhat different number might be arrived at; but even he finds 20·54 only after a hospital window

had been shut all night, and 20·53 in a room with 116 children. Five of his analyses give numbers below 20·6. These are bad cases.

So far, with exceptions stated, I had written on oxygen for the Mines Commission. Since that time many other specimens have been collected by myself and assistants.

Those from Scotland are sufficiently numerous to seek a place for themselves, and, at the risk of diminishing the clearness of the arrangement, they may be here introduced. I may also add that I am unable to persuade myself to write a new and independent paper on the subject, having so lately completed the report; the method of merely inserting the latest matter must therefore be adopted.

This was said in 1864, and I now reprint with addition the paper on the composition of the air.

Specimens from Scotland.

Mountainous Districts.

Top.	Oxygen.	Bottom.	Oxygen.
Ben Nevis	20·91	Ben Nevis	20·93
„	20·96	„	20·91
„	20·94	„	20·89
„	20·88		
„	21·01		
Lochin-y-gair (Balmoral).	20·94	Lochin-y-gair (Balmoral).	20·80
„	20·95	„	21
Ben Ledi.	20·98	Ben Ledi.	21·02
„	20·97		
„	20·97		
Ben Voirlich	21·01	Ben Voirlich	20·87
		„	20·88
Ben-na-bourd	21·03	Ben-na-bourd	21·18
Ben Lomond	20·94	Ben Lomond	20·95
„	21·08		
„	20·91		
Ben Muich Dhu	21		
„	21·07		
„	21·02		
„	20·99		
„	20·93		
„	21·01		
Ochill Hill	21·05		
„	21·07		
Moncrieffe Hill	20·93		
Mean	20·98	Mean	20·94

Districts not Mountainous, or only partly so.

	Oxygen.	Means.	
Shore at Lossiemouth	21·05		
" "	20·95		
" Mean	. .	21	
Inverness, at Moray Frith	20·89		
" "	20·89		
" "	20·86		
" Mean	. .	20·88	
Inverness, behind the Town	20·88		
(Inverness specimens taken in very clear weather.)			
Sea-shore, Oban	20·98		
Edinburgh, Prince's Street	20·99		
" "	20·92		
" Calton Hill	20·94		
" Mean	. .	20·95	
Aboyne	20·94		
"	20·95		
"	21·02		
" Mean	. .	20·96	
Aberdeen, sea-shore	21·05	. .	Wind from sea N.; evening.
"	21·01		
" "	21·07		
" Mean	. .	21·04	
Errol, marshy ground	20·91	. .	Windy and cloudy.
" "	20·96		
" Mean	. .	20·94	
Caledonian Canal (near Inverness)	20·88	. .	Cloudy and windy, S.W.
Balmoral	20·88		
"	21		
" Mean	. .	20·90	
Taynuilt (near Oban)	20·92		
" "	20·86		
" Mean	. .	20·89	
Braemar-on-the-Dee	21·18	. .	Cloudy.
Huntly	21·03		
Mar Forest	21·04	. .	Rain and sunshine.
"	21·02		
"	21·08		
"	20·88		
" Mean	. .	21	
Forest near Braemar	20·87		

Mean of the above, 20·959 or 20·96.

Some impurity rising from the water near Inverness has lowered the otherwise very high average of these analyses, 20·98.

Air from worst places in the City of Perth.

	Oxygen.
Close, 70 South Street	20·87
„ 44 Pomarium	20·92
„ „	20·94
„ „	20·93
Weaver's Close, Pomarium	20·96
„ „	20·94
St. Paul's Close	20·96
„ „	20·99
Long Close, off George Street	20·94
„ „	20·90
Weaver's shop, 44 Pomarium	20·88
„ „	20·93
Close, 28 Watergate	21·02
From a conduit, Athole Crescent	20·93
„ „	20·95
Close, 82 South Street	21·01
From a conduit, Athole Crescent	20·84
„ „	20·89
Hewat's Close, 148 South Street	20·97
„ „	21
From conduit, Stormont Street	20·90
Close, 44 Meal Vennel	20·90
Mean	20·935

Analyses classified.

Air from Scotland, 1863–5.	Oxygen.
Mean of the sea-shore and the heath	20·999
Mean of the tops of hills	20·98
Mean of the bottoms of hills	20·94
Mean of all places not mountainous	20·978
Mean of inferior parts of a town (favourable, *i. e.* windy weather)	20·935
Mean of lower marshy, &c., places	20·922
Mean of the forests	20·97
Mean of all	20·959
	or 20·96

I conclude, therefore, that in order to obtain the assumed mean, 20·96, it is needful to include very inferior air. It is, therefore, the mean composition of air as it is found in wholesome and less wholesome places, not the mean of the finest atmospheres.

It will be seen that here the sea-shore and open places

still command the highest amount of oxygen, although the higher hills are not the most deficient—it may be, because in Scotland really high mountains do not exist, and also because, unlike the great ranges of the Alps and the Himalayas, the Scotch hills have much sea and little land from which to draw their supplies.

It may be remarked that the averages of the hills above and below, viz. 20·98 and 20·94, give exactly the number, 20·96, which was taken as a fair sample of air.

Glasgow.—Oxygen. 1869.

The Closer Parts.—Winter.	Oxygen per cent.		
	1	2	Average.
Buchanan Street, near Western Club	20·92	20·91	20·915
Exchange, front of	20·89	20·90	20·895
Union Street	20·88	20·88	20·880
Miller Street, Argyle Street	20·90	20·93	20·915
Argyle Street, near Queen Street	20·96	20·90	20·930
St. Enoch's Square	20·92	20·91	20·915
Cross	20·90	20·85	20·875
Blackboy Close, Gallowgate	20·90	20·88	20·890
Gallowgate, between Kent Street and Barracks	20·88	20·87	20·875
A Close, High Street	20·87	20·86	20·865
Armour Street, near the Barracks	20·86	20·88	20·870
Kirk Street	20·88	20·87	20·875
Coulter's Lane, Abercromby Street	20·88	20·88	20·880
A small court, Tobago Street	20·85	20·90	20·875
Oswald Street, Dalmarnock Road	20·87	20·89	20·880
Average	20·8890		

Glasgow.—Oxygen.

Chiefly Open Parts.—Winter.	Oxygen per cent.		
	1	2	Average.
Tennant Street, St. Rollox	20·90	20·93	20·915
Charles Street	20·92	20·92	20·920
Middleton Place	20·94	20·95	20·945
Castle Street, near the Cathedral	20·92	20·87	20·895
Dobbies Loan, near Poorhouse	20·93	20·91	20·920
New City Road, near Abercorn Street	20·91	20·92	20·915

Glasgow.—Oxygen—continued.

Chiefly Open Parts.—Winter.	Oxygen per cent.		
	1	2	Average.
Blythswood Square	20·94	20·95	20·945
Renfrew Street	20·90	20·92	20·910
Newton Terrace, Sauchiehall Street	20·95	20·99	20·970
University, Gilmour Hill	20·88	20·92	20·900
Quay, near Broomielaw Bridge	20·95	20·90	20·925
Anderston Quay	20·90	20·90	20·900
Sharp's Lane, Stobcross Street	20·98	20·97	20·975
Finnieston Quay	20·90	20·92	20·910
Pointhouse Pier	20·98	21·01	20·995
Average			20·9293
Total average of Oxygen			20·9092

Marshy or confined Places, Switzerland.

		Oxygen.	Means.
Aug. 1864.	Sion, Upper Valley of the Rhone, Switzerland (morning), over water, marshy grass	20·86	
,,	Sion (morning), over water and brushwood	21·01	
		20·94	
		21·05	
		21·02	
		20·96	
		20·94	
		20·95	
		20·83	
		21	
		20·90	
	Mean		20·95
Sept. 1864.	Reddish, near Manchester, among brushwood	20·92	
		20·98	
		20·95	
		20·90	
	Mean		20·937
Aug. 1864.	Lauterbrunnen	20·94	
		20·97	
		20·95	
	Mean		20·953
,,	Chamounix, Montanvert	21·03	
		20·99	
	Mean		21·01
,,	Verdin, in the Sologne	20·97	
		20·90	
,,	Vouzerou	21·01	
		20·90	
	Mean		20·95

As there is so much cretinism at Sion, and as goître is found in the whole valley, I thought it important to obtain some specimens of the atmosphere from the marshes themselves. The air was taken from the surface of the water or from the brushwood. The time was too favourable, as there was a considerable breeze; but I had not patience to wait, and, as it turned out, it would have been necessary to wait a long time. I consider the matter well worth further enquiry, and should be happy to make the analyses, if specimens were sent, as I may not soon be in the same spot again.

The specimens from the Sologne are very few. They were brought without any hope of a result. Had I known as much as I do now, more would have been collected. The book of Dr. Burdel, entitled 'Recherches sur les Fièvres Paludiennes,' excited a curiosity to see the district; but there seemed such a free air and such an open country, with such a dry sandy soil, that I doubted if anything like gaseous emanations could be found. I saw only few and small ponds. Seeing how careful the search must be, it is well not to be too confident until numerous analyses are made. Dr. Burdel does not believe in any difference of analysis, and seems to refer the unwholesome state to the action of the electricity of the atmosphere and the heat and cold. It is a courageous thing at the present time to refer any phenomenon to electricity; it has been overdone so much, that people now imagine it must never be done, and are afraid to mention it, thus deciding on the other side. It seemed to me when in Switzerland that the frequency of discharges on the mountains, with their absence in the valleys, was itself a proof of difference of condition in the two places. On plains the heavens and earth seem to equalise their electricity more uniformly; in these hilly regions it is done at certain high points. If the flow of this electricity is valuable to us, its loss is

detrimental; and if the discharges are made violently from one point, instead of steadily and slowly from wide surfaces, the condition of these surfaces must be modified. On looking at Mont Blanc for a fortnight, and seeing nightly discharges from its summit, I was naturally led to look on the subject in this way, and to add this to Dr. Burdel's opinion, although I confess I do not understand the part he wishes the electricity to play. All I can see is, that there is a difference in these hilly regions. I do not pretend to see anything affecting animal life that can result from the difference; and in the Sologne there are not the mountainous ranges to account for any similar condition.

It has been shown that air in places where putrefaction may be supposed to be going on has been found by other observers to contain less oxygen than pure air. This enquiry puts the subject in a somewhat clearer light than hitherto, and shows us that those places containing impurities, and which are in or near all our houses, are also subjected to a diminution of oxygen. The diminution is not entirely made up by carbonic acid, and must be made up by other substances. This diminution is very sensible when it comes to 20·75, or even in some places 20·85, being equal to a removal of ·2 to ·1 of oxygen; so that it indicates more clearly in some cases than the carbonic-acid test does. At times we may probably allow for the absorption of oxygen into the soil or elsewhere. We do not require to seek deadly places for air with diminished oxygen; the air of every house is subjected to this diminution, which must of necessity be an indication of the amount of impurity existing in the air, although giving no clue to the quality of that impurity, which may be more or less innocent or noxious.

It is well known that oxygen over putrid substances is

absorbed, whilst carbonic acid and other gases take its place. This reasoning does not touch the question, What is the effect of a loss of oxygen when no impurities take its place?—a condition little known. I wish particularly to say that it is probable that the objection to the air which has a little less oxygen than the normal amount may not arise from this fact itself. The loss of oxygen may only be an indication of the presence of a pernicious body.

Carbonic Acid of the Atmosphere.

Horace de Saussure first paid minute attention to the carbonic acid of the atmosphere, and showed its presence on the mountains of Switzerland as well as on the plains. He used lime-water for analysis. His results are in his 'Voyages dans les Alpes' of 1796. His son Théodore in 1828 published a *résumé* of a much fuller enquiry, and in 1830 the complete account. He used a vessel of 34 litres in volume, and washed the air with baryta-water, collecting the carbonate of baryta precipitated. This is a laborious process; but, considering the great accuracy of the operator, and the long experience which he gained, we may place great reliance on his work. I am disposed to think that there may be a little excess in his results; but this will not affect the comparative amounts found at different times, and which form the most interesting part of the enquiry. He says[1]:—

'The quantity of carbonic acid in the open air in the same place is subject to almost continual change, equally with the temperature, the winds, the rain, and the atmospheric pressure. The observations which I have made since 1816 until the month of June of this year, in a meadow at Chambeisy, three-quarters of a league from Geneva, indicate that the mean quantity of carbonic acid

[1] Annales de Ch. et de Ph. vol. xxxviii. 1828.

in volume which 10,000 parts of air contain is equal to 5, or more exactly to 4·9. The maximum of this gas is 6·2; the minimum is 3·7.

'The observations published ("Bibliothèque univ." vol. i.) show as maximum in the same place a greater proportion of acid; but it is probable that this excess was the result of the imperfection of the experiment.

'The augmentation of the average quantity of carbonic acid in summer, and its diminution in winter, are manifested at different stations—in the country as in the city, upon the Lake of Geneva and upon a hill, in calm and disturbed air. According to an average of thirty observations made at Chambeisy, during seven years, with baryta-water, the quantity of carbonic acid in the months of December, January, and February, at mid-day, is to that of June, July, and August as 77 to 100.

'This ratio is not constant throughout every year. There are times which form exceptions, and in which the quantity of carbonic acid in summer is inferior to that in winter, or *vice versâ*. Thus, after many years of observation, the mean quantity of carbonic acid in the month of January in 10,000 of air is 4·23; but the quantity of carbonic acid in the month of January 1828, which was extraordinary for the mildness of its temperature, rises to 5·1. The average quantity of carbonic acid in the month of August, taken in different years, is 5·68; but after an average taken from four observations (the results of which closely approximate) in the month of August 1828, which was singularly cold and wet, the quantity of carbonic acid at noon was only 4·45.

'The difference in the quantities of carbonic acid contained in the atmosphere in calm weather, during day and night, is one of the most remarkable results of my late observations. The following is the table of experiments which I have made, in open country, at noon and at eleven o'clock in the evening of the same day. (The table in

the original gives the quantities of carbonic acid in 10,000 parts, but for uniformity's sake they are here altered into per-centages.)

	Noon.	Evening, 11 o'clock.
May 22nd, 1827 . . .	·0581	·0623
July 7th „ . . .	·058	·062
Sept. 3rd „ . . .	·0561	·0601
Nov. 6th „ . . .	·043	·0486
May 31st, 1828 . . .	·0475	·0565
June 13th „ . . .	·0506	·0583
June 26th „ . . .	·0539	·0522
Aug. 1st „ . . .	·0432	·0606
Aug. 12th „ . . .	·0429	·0582

'It results from these observations, that the air contains in calm weather more carbonic acid during the night than during the day. The only exception to this result was on June 26th, 1828, during extremely violent wind, whilst all the other observations were made in calm weather or in slightly disturbed air. I have acquired sufficient experience in this kind of work to affirm that the general difference which is found in this table could not result from errors of observation.

'It remains for me to discover if this difference is maintained in the middle of winter, or when vegetation is inactive.

'The air taken at the middle of Lake Leman, opposite to Chambeisy, contains on an average a little less carbonic acid than the air taken a hundred toises from the bank. After eight observations, made at different periods, on the same days at noon the quantities of carbonic acid at the two stations are as 100 to 98·5; but the air of both places follows the same variations relatively to the seasons.

'The air of Geneva contains more carbonic acid than

the air of a meadow at Chambeisy—almost in the ratio of 100 to 92, from six observations made at the same time at both stations. A greater purity in the air of the country could be foreseen. I cite this result only because the other eudiometrical experiments indicate no difference in the air of those two places, and as it shows the utility of the experiment by which this result was obtained.'

At first Saussure was led to believe that rain increased the carbonic acid, but changed his mind on finding, on the contrary, that this acid increased in dry weather even with a freezing temperature.

For the Lake of Geneva and the neighbouring Chambeisy, his numbers are[1]:—

Date.	Time.	Carbonic acid at Chambeisy.	On the Lake of Geneva.
1826.			
December 29	Mid-day	·0421	·0385
1827.			
May 22	ditto	·0540	·0502
July 2	ditto	·0523	·0578
August 9	ditto	·0521	·0542
September 28	ditto	·0495	·0474
1828.			
January 19		·0491	·0446
July 7		·0481	·0441
August 12		·0408	·0392
August 26		·0422	·0410
September 26		·0414	·0320
September 26	Night	·0493	·0430
1829.			
February 5	Mid-day	·0445	·0476
March 7		·0463	·0465
April 18		·0429	·0422
July 7	Night	·0534	·051
July 8	Mid-day	·0435	·0408
October 13		·0354	·0342
October 13	Night	·0416	·0368
	Mean	·0460	·0439

[1] Ann. de Ch. et de Phys. vol. xliv. 1830.

De Saussure found also more carbonic acid in the town of Geneva than outside at Chambeisy. The numbers are :—

Date.	Time.	At Chambeisy.	In Geneva.
1827.			
February 12. .	Mid-day . .	·0358	·0455
May 22 . .	. . . .	·0540	·0569
May 26 . .	. . . .	·0471	·0528
August 9 . .	. . . .	·0453	·0476
1828.			
January 28 . .	. . . .	·0426	·0427
February 19. .	. . . .	·0462	·0482
April 10 . .	. . . .	·0465	·05
July 25 . .	. . . .	·0390	·0445
July 1 . . .	Midnight . .	·0407	·0385
September 4 .	11 at night . .	·0441	·0439
September 5 .	. . . .	·0382	·0420
October 1 . .	. . . .	·0414	·0423
October 2 . .	. . . .	·0367	·0405
	Mean . .	·0437	·0468

In the day the carbonic acid at Chambeisy is ·0445, and at night ·0402 ; in Geneva during day ·0485, and at night ·0414. The difference for the country is ·0043, and for the town ·0073. The diminution at night is greater in the town, where less fuel is burnt and people are shut up.

One of the most curious results of De Saussure's enquiry is, that the carbonic acid on the mountains is actually greater than on the plains. It will be interesting to give these figures, as they are often referred to, but seldom seen. It may be remembered here that there was some reason to believe in a diminution of oxygen in mountain air to a minute extent. The increase of carbonic acid is a corroboration.

Name of mountain.	Height of mountain in mètres.	Carbonic acid in the air of the mountain.	Carbonic acid in the air of the plain.
La Dôle	1267	·0461	·0474
Grand Salève-sur-Crevin .	877	·0557	·0482
Hermitage (Petit Salève) .	331	·0544	·0482
La Dôle	1267	·0491	·0446
Vasserode-sous-la-Dôle .	908	·0481	·0446
Grand Salève-sur-Grange-Tournier . .	945	·0413	·0367* ·0359†
Col de la Faucille . .	963	·0443	·0414
ditto . . .	963	·0454	·0415
ditto . . .	. .	·0369	·0387
ditto . . .	. .	·0360	·0322
ditto . . .	. .	·0422	·0355
ditto . . .	. .	·0395	·0315

He refers the difference to the rain below, and the moisture of the ground, and to vegetation, which diminishes the carbonic acid and increases the oxygen. He finds also that the mountain air does not change at night as the air below does. He finds a minute increase of carbonic acid arising from violent winds, and thinks this may arise from the upper mixing with the lower strata; his evidence on this point may be explained by the fact that he found a decrease on June 20th during a violent wind, probably from the same cause, namely mixing.

De Luna has lately examined the air of Madrid, with the following results [1]:—

Air of Madrid, outside the walls, during month of March.

(1st series.)

Place.	Oxygen.	Carbonic acid per cent.	Place.	Oxygen.	Carbonic acid per cent.
1 .	20·71	·05	7 .	20·70	·06
2 .	20·79	·03	8 .	20·74	·05
3 .	20·77	·03	9 .	20·69	·09
4 .	20·77	·05	10 .	20·81	·02
5 .	20·73	·06	11 .	20·79	·03
6 .	20·75	·03	12 .	20·78	·04

* At foot. † At Chambeisy.

[1] Estudios quimicos sobre el aire atmosférico de Madrid, por D. Ramon Torres Muñoz de Luna. Madrid, 1860.

Air of Madrid, within the walls, during month of April.

(2nd series.)

Place.	Oxygen.	Carbonic acid.
1 . .	20·70	·06
2 . .	20·70	·06
3 . .	20·77	·03
4 . .	20·75	·05
5 . .	20·70	·06
6 . .	20·69	·06
7 . .	20·78	·05
8 . .	20·69	·08
9 . .	20·70	·06
10 . .	20·78	·04
11 . .	20·80	·03
12 . .	20·73	·04

The amounts of oxygen are very small, and of carbonic acid very high. Solutions were used for the oxygen, and permanganate of potash for the organic matter.

Hospitals.

Rooms.	Oxygen.	Carbonic acid.
Hospital general . . .	20·58	·32
	20·50	·38
	20·49	·43
Hospital de la Princesa . .	20·65	·27
	20·52	·30
	20·60	·29

The experiments of Mr. Lewy on the Atlantic Ocean and in America show a great irregularity. They are given on pages 16–18. We do not see clearly why there should be a rise in the carbonic acid from ·0333 to ·0577 at sea. The great inequalities on the land are interesting, and especially at Bogota, where meteorological influences interfere to render the amount great and diminish health.

Pettenkofer gives ·5 per mille, or ·05 per cent., as the amount in the air generally at Munich. This is above the number of Saussure, and both are above the numbers

found here. Munich is 1,690 feet, Geneva 1,154 feet above the sea. In the 'Handwörterbuch der Chemie,' under 'Ventilation,' Pettenkofer gives a summary of the amount of carbonic acid in dwelling-houses, as follows:—

In a dwelling-house, during the day, ·054.

After a while it increased to ·065, ·061, ·064, ·068, ·074, and ·087. Mean ·068.

In a bed-room at night, with closed windows, ·230.

Partly open, ·082.

He found the following amounts of carbonic acid per cent., on examining public places, hospitals, prisons, &c.:—

·232	·143	·223
·226	·307	·247
·334	·261	·131
·186	·278	·495
·362	·429	·536
·317	—	—
Schools . . . ·410	·567	·200
·229	·558	

I obtained the following results in 1864:—

Carbonic Acid in the Air of the Suburbs of Manchester.

Place.	Per cent.	Wind.	Date.	Time.	Temp. Cent.	Bar.
				h. m.	°	in.
Fields in Greenheys . .	·0383	. .	Feb. 22	. .	. .	. .
Higher Broughton . .	·0350	E.S.E.	„ 24	. .	0·6	29·9
Old Trafford . . .	·0432	. .	„ 25	. .	3	29·98
Buxton (*a*) . . .	·0431	E.	. .	11 0		
„	·0459	„	Mar. 19			
„	·0467	„	„ 19			
„	·0443	„	„ 19	1 0		
City Road, Hulme (*b*) .	·0367	„	„ 8	3 0	4	29·2
Rusholme (*c*) . . .	·0295	. .	„ 11	. .	5	29·2
Queen's Park . . .	·0308	. .	„ 14	. .	10	
Old Trafford . . .	·0340	W.	April 1			
„ over canal (*d*) .	·0313	„	„ 1			
„	·0293	„	„ 2			
„	·0291	„	„ 11	5 50		
(14)	·5172					
Mean . . .	·0369					

(*a*) Fine day. (*b*) Strong wind. Little snow. (*c*) Very windy. Hail storm
(*d*) When the hour is not given, it is about the middle of the day.

Carbonic Acid in Close Places.

Place.	Per cent.	Wind.	Date.	Time during day.	Temp.	Bar.
				h. m.	°	in.
Theatre Royal, pit . .	·2734	. .	Feb. 13	. .	22	29·3
,, gallery .	·1358	. .	,, 15	10 50	29	29·7
,, ,, .	·1238	. .	,, 15	,,	,,	,,
Queen's Theatre, pit . .	·1026	. .	,, 16	. .	26	29·7
,, . .	·1019	. .	,, 16	. .	,,	,,
Stables (*a*) . . .	·0833	. .	. .	. .	8·2	30
,, ...	·0875	. .	. .	. .	,,	,,
Cellar of laboratory . .	·0572	. .	,, 17	. .	6·2	30·1
Study, at table (*b*) . .	·1177	. .	,, 25	. .	16	29·98
,, ceiling . .	·1561	. .	. .	. .	22	,,
Schoolroom . . .	·0970	. .	Mar. 15	. .	12	
,, . . .	·0886	. .	,, 15			
Brewery	·1214	. .	,, 16			
,, (*c*)	·180	. .	,, 16			
Mills (*d*)	·286	. .	,, 10	4 0	20	
,, ...	·283	. .	,, 10	5 30	21	
,, ...	·296	. .	,, 10	5 30	21	
,, ...	·300	. .	,, 10	,,	,,	
(18)	2·8883					
Mean . . .	·1604					

(*a*) 4 horses had been there during the night, and 3 were still there.
(*b*) 4 persons were in the room, with 3 gas lights and large fire.
(*c*) Near fermenting vat. (*d*) Taken in weaving-shed which contained 400 people.

Carbonic Acid in the Air of Manchester.

Place.	Per cent.	Wind.	Date.	Time during day.	Temp.	Bar.
				h. m.	°	in.
Churchyard, All Saints .	·0323	N.E.	Feb. 22	. .	1	29·8
Smithfield Market . .	·0446	,,	,, 20	11 0	1·3	29·95
,, ,, . .	·0464	,,	,, 20	,,	1·3	,,
Churchyard, All Saints .	·0437	,,	,, 18	. .	5·2	30·18
,, ,, .	·0521	,,	,, 18	. .	,,	,,
Back yard of laboratory .	·0539	. .	,, 6	. .	1·4	30·8
,, ,, .	·0383	. .	,, 6	11 15	,,	,,
,, ,, .	·0473	. .	,, 6	3 15	,,	,,
,, ,, .	·0515	. .	,, 6	3 35	,,	,,
Philosophical Society (*a*) .	·0677	. .	,, 9	11 20	1	29·55
,, ,, (*b*) .	·0683	. .	,, 9	5 15	,,	,,
Bishopsgate (*c*) . . .	·0734	. .	,, 9	4 30	,,	,,
,, (*d*) . . .	·0727	. .	,, 9	,,	,,	,,
Back yard of laboratory (*e*)	·0523	. .	,, 10	10 30	8	29·4
,, ,, .	·0541	. .	,, 10	11 30	,,	,,

(*a*) Fog, but not very dense. (*b*) Ibid. (*c*) Fog very dense.
(*d*) Ibid. (*e*) Little fog.

Carbonic Acid in the Air of Manchester—continued.

Place.	Per cent.	Wind.	Date.	Time during day.	Temp.	Bar.
				h. m.	°	in.
Deansgate (*f*) . . .	·0578	. .	Feb. 10	2 30		
Small court off Berry Street	·0604	. .	,, 11	. .	4	29·6
Midden	·0724	. .	,, 12	5 5	5·4	29·38
,, ...	·0732	. .	,, 12	,,	,,	,,
,, ...	·0805	. .	,, 13	11 50	7·2	29·3
,, ...	·0837	. .	,, 13	12 25	,,	,,
Churchyard, All Saints .	·067	. .	,, 19	. .	1	30·18
,, ,, .	·0447	. .	,, 19	. .	,,	,,
,, ,, .	·0518	. .	,, 19	. .	,,	,,
End of Market Street .	·0374	. .	,, 23	. .	2·8	29·85
,, ,, .	·0321	. .	,, 23	. .	,,	,,
Blackfriars Bridge (*g*) .	·0495					
Near Printing Lane . .	·0351					
Back yard of laboratory (*h*)	·0327	. .	Mar. 4	4 0	5·6	29·5
,, ,, .	·0319	. .	,, 4	5 30	. .	,,
,, ,, (*i*) .	·0391	S.W.	,, 7	10 15	9·5	29
,, ,, (*j*) .	·0358	,,	,, 7	11 15	. .	,,
,, ,, .	·0343	,,	,, 7	12 15	10	,,
,, ,, (*k*) .	·0311	,,	,, 7	2 15	7·8	,,
,, ,, (*l*) .	·0311	,,	,, 7	2 50	7·4	,,
,, ,, (*m*) .	·0335	,,	,, 7	3 15	7·2	,,
,, ,, (*n*) .	·0367	,,	,, 7	4 15	6·4	,,
,, ,, .	·0333	,,	,, 7	5 15		
All Saints Churchyard (*o*) .	·0319	E.	,, 8	4 45	4	
Stockport, back street off Heaton Street (*p*) . .	·0415	N.E.	,, 9	2 30	4	
Stockport, Chester Gate (*q*)	·0351	,,	,, 9	3 30	4	
,, near Grammar School (*r*)	·0355	,,	,, 9	4 30	,,	
Back yard of laboratory .	·0311	. .	,, 11	4 50	5	
River Medlock, a little above Brook Street .	·0351	. .	,, 17	11 30		
Back street off Gray Street Oxford Street (*s*) . .	·0383	. .	,, 17	2 30		
River Medlock, where the water runs into a basin .	·0367	. .	,, 17	4 15		
Canal bank, Canal Street .	·0375	. .	,, 18	11 0		
Old Garratt, river bank .	·0608	. .	,, 22	11 40		
Granby Row, Crown Street	·0520	N.E.	,, 22	4 0		
,, ,, .	·0544	,,	,, 22	,,		
Scholes Street, London Rd.	·0408	,,	,, 22	4 50		
Devonshire St., All Saints.	·0343	E.	,, 23			
St. Mary's Gate, Exchange	·044	W.	,, 24			
,, ,, .	·0375	,,	April 1	11 0		
,, ,, .	·0344	,,	,, 1	5 0		
,, ,, .	·0333	,,	,, 2	11 0		
,, ,, .	·0346	,,	,, 2	,,		
,, ,, .	·0364	S.E.	,, 5	12 10		

(*f*) Fog. (*g*) Taken immediately over the water. Very bad smell. Fine day.
(*h*) Rain. (*i*) Very fine. (*j*) These experiments, March 7, after 3 days' rain.
(*k*) After more rain. (*l*) Rain. (*m*) Ibid. (*n*) Fine.
(*o*) A little snow; strong wind. Snowing fast. (*p*) Ibid. (*q*) Ibid.
(*r*) Very stormy. (*s*) Rather windy.

Carbonic Acid in the Air of Manchester—continued.

Place.	Per cent.	Wind.	Date.	Time during day.	Temp.	Bar.
				h. m.		
St. Mary's Gate, Exchange.	·0365	S.E.	April 5	12 10		
,, ,, .	·0389	,,	,, 5	5 0		
,, ,, .	·0398	,,	,, 5	5 0		
,, ,, .	·0317	N.W.	,, 11			
,, ,, .	·0329	,,	,, 11			
Back yard of laboratory .	·0399	S.E.	,, 5	2 30		
,, ,, .	·0352					
,, ,, .	·0312	. .	,, 6			
Ditto, over some mud .	·0358	. .	,, 6	3 0		
,, ,, .	·0415	. .	,, 6	,,		
Ditto, over jar of mud .	·0433	. .	,, 7			
,, ,, .	·0404	. .	,, 7			
,, ,, .	·0501	. .	,, 7	3 0		
,, ,, .	·0343	. .	,, 7			
,, ,, .	·0353	. .	,, 7	5 0		
Yard	·0387	. .	,, 7	,,		
,, ...						

The following summary may be made of the Manchester results :—

	Average of Carbonic Acid per cent.
In Manchester Streets in usual weather . . .	·0403
During fogs	·0679
About middens, of which there are thousands . .	·0774
Average of all the town specimens	·0442
Fogs excepted	·0424
Fogs and middens excepted	·0403
Where the fields begin	·0369
In close buildings	·1604
Minimum of suburbs	·0291

When approaching the country the amount seems occasionally very low; probably the lower grounds, with much vegetation, are subject to variations below and above the standard, and such as are not found in exposed or bare regions.

There may be noticed in Manchester a tendency towards increase of carbonic acid as the day advances, as if at times the ventilation could not keep down the increase of acid: this is contrary to the results in a country place. There is less after rain, and less during high winds.

Carbonic Acid on the River Thames, April 1864.

Locality.	Wind.	Carbonic acid per cent.
London Bridge:—		
City side	S.W.	·0354
Middle	,,	·0383
Surrey side	,,	·0383
Westminster Bridge:—		
City side	,,	·0313
Middle	,,	·0344
Surrey side	,,	·0298
Before the Houses of Parliament	,,	·0329
Lambeth Bridge:—		
City side	,,	·0329
Mean	,,	·0343

Carbonic Acid in the Open Places of London.

Locality.	Wind.	1st exp.	2nd exp.
Hyde Park	S.W.	·0334	·0334
The same	S.E. from city	·0306	·0299
Regent's Park	N.E.	·0304	·0304
St. James's Park	,,	·0285	
Duke of York's Column	,,	·0285	·0280
Mean of 5 experiments		·0301	

Carbonic Acid in the Streets of London.

Locality.	Wind.	Carbonic acid present.	
		1st exp.	2nd exp.
Cheapside, Post Office end	S.W.	·0352	·0337
Outside the Exchange	,,	·0398	
Newgate Street	,,	·0413	
Oxford St., above Regent's Circus	E.	·0344	·0344
Lower Thames Street	,,	·0428	
Small Alley, Smithfield	,,	·0337	
Small Court, Smithfield	,,	·0398	
Small Court, Upper Marsh, Lambeth	,,	·0382	
New Cut, Lower Marsh, Lambeth	,,	·0413	
Top of the Monument	,,	·0398	·0405
Mean of 10 experiments	,,	·0380	
Mean of experiments in London 1864	,,	·0341	

With the aid of Dr. Bernays, of St. Thomas's Hospital, I have also obtained the amount of carbonic acid in close places in London.

Carbonic Acid in Close Places in London.

	Per centage by volume.
Chancery Court, closed doors, 7 feet from ground, March 3	·193
Same, 3 feet from ground	·203
Chancery Court, door wide open, 4 feet from ground, 11.40 A.M., March 5	·0507
Same, 12.40 P.M., 5 feet from ground	·045
Strand Theatre, gallery, 10 P.M.	·101
Surrey Theatre, boxes, March 7, 10.3 P.M.	·111
Surrey Theatre, boxes, March 7, 12 P.M.	·218
Olympic, 11.30 P.M.	·0817
Same, 11.55 P.M.	·1014
Victoria Theatre, boxes, March 24, 10 P.M.	·126
Haymarket Theatre, dress circle, March 18, 11.30 P.M.	·0757
Queen's Ward, St. Thomas's Hospital, 3.25 P.M.	·040
Edward's Ward, St. Thomas's Hospital, 3.30 P.M.	·052
Victoria Theatre, boxes, April 4	·076
Effingham, 10.30 P.M., April 9, Whitechapel	·126
Pavilion, 10.11 P.M., April 9, Whitechapel	·152
City of London Theatre, pit, 11.15 P.M., April 16	·252
Standard Theatre, pit, 11 P.M., April 16	·320

Pettenkofer informs us that the air of Munich may be taken as containing about ·05 per cent. of carbonic acid; with us it is certainly below this amount; and if raised to ·05 by breathing, one would perceive it. May we conclude that such a small amount is imperceptible without organic emanations? Munich is very high; the air must sweep over the whole continent to come to it; it may wash up the carbonic acid, and, perhaps, oxidize the organic matter.

Air with a very small loss of oxygen is perceptibly deteriorated if its place is occupied with carbonic acid and exhalations from the person, although we are not able to say how far this is the case when carbonic acid alone is substituted for this small amount of oxygen.

On the Thames it is clearly seen that the open river is purer than the streets when the water is not putrid.

It is purer above, at Westminster, than at London Bridge. London is freer from this gas than Manchester, although not equal to the parks in March and April, when the experiments were made. When the new sewers are complete, the difference will probably be perceptible.

These analyses indicate that a very minute amount of carbonic acid shows deterioration of air sufficient for the senses to observe. The senses observe a difference between Manchester and the outskirts. The difference is ·0034 per cent. The senses observe it in London, where the difference between the streets and parks is ·0040 per cent. They observe it also on the Thames, and in wet weather. But they do not observe it in Munich, which has more carbonic acid than even these towns, and more than the New Cut or Lower Thames Street. The conclusion is, that carbonic acid in these small amounts is not that which annoys us. In some towns it is no doubt sulphurous acid, in others organic matter and gases from putrefaction.

It does not follow that we must therefore neglect carbonic acid; on the contrary, it ought to be examined minutely, so that not the smallest increase be allowed, if possible; not that we know certainly of any positive evil which it can do of itself in these small quantities, but because it almost always comes in bad company.

In the above analyses the air containing ·0774 is really worse than that containing ·1604 and even ·3, because over the middens there is a little sulphuretted hydrogen. It is well, then, in such cases to use a double test. Indeed it is probable enough that other gases besides sulphuretted hydrogen, such as marsh gas and hydrogen, products of decomposition, are issuing from cesspools and middens. I should not say probable; it is really certain. These gases, including the carbonic acid, show the reason why less oxygen should be found in such places.

I believe these analyses are of importance in the enquiry into the state of the air of all places, as they teach us the meaning of a deviation from the normal amount of carbonic acid as well as of oxygen in the air. A deviation of ·02 is not pleasant to us when it is caused by simple want of ventilation. If it is accompanied with gases of putrefaction, it is much more hurtful, as some of these are very deadly.

These analyses teach us to be very careful not to allow the air to become deteriorated even to a very minute extent, and that a figure in the third decimal place is not to be despised; but they teach us more—namely, that in some places, such as high mountains, a slight increase of carbonic acid, such as is found in the third, or even to the length of 2 in the second place, is rather a proof that the oxygen of the air has done its work well and purified the atmosphere, and that this increase is probably owing to pure carbonic acid. It would be well to have the experiments of Schlagintweit confirmed, where ·07 and ·09 are found on high mountains. We all avoid an atmosphere containing ·1 of carbonic acid in crowded rooms; and the experience of civilised men is that it is not only odious but unwholesome. When people speak of good ventilation in dwelling-houses, they mean, without knowing it, air with less than ·07 of carbonic acid. We must not conclude that, because the quantity of carbonic acid is small, the effect is small; the conclusion is rather that minute changes in the amount of this acid are indications of occurrences of the highest importance.

Further enquiry was made in 1869.

London Air.—Carbonic Acid.—November 1869.

	Place.	Carbonic acid per cent.		Average.
		1	2	
N.	Islington, Duncan Terrace . .	·0474	·0474	·0474
N.	Hoxton, Hoxton Square . . .	·0475	·0475	·0475
N.E.	Dalston, Albion Road . . .	·0443	·0443	·0443
N.E.	Hackney, near Hackney Station .	·0408	·0408	·0408
N.W.	Clarendon Square, Somers Town .	·0448	·0448	·0448
N.W.	Alpha Road and Grove Road .	·0419	·0419	·0419
N.W.	Near Belsize Park	·0391	·0360	·0375
	Average (Belsize Park out) . . .			·04445.
S.	Two miles from Clapham Junction .	·0345	·0345	·0345
S.	Lambeth, back street near workhouse	·0471	·0471	·0471
S.	Kennington Park	·0376	·0376	·0376
S.	Pleasant Place, Elephant and Castle	·0457	·0457	·0457
S.	Deverell Street, off New Kent Road	·0457	·0457	·0457
S.W.	Brompton, back street off Sloane St.	·0477	·0477	·0477
S.W.	Brompton Crescent . . .	·0446	·0446	·0446
S.W.	Blackland Terrace, off Kent Road .	·0508	·0508	·0508
S.W.	Chelsea, Park Lane, off Church St.	·0445	·0445	·0445
S.W.	Near Battersea Bridge . . .	·0413	·0413	·0413
S.W.	Chelsea Hospital, gardens near river	·0413	·0413	·0413
S.W.	South Lambeth, Cavendish Grove .	·0421	·0451	·0431
S.W.	Vauxhall Bridge, near river . .	·0417	·0417	·0417
S.W.	Houses of Parliament Terrace .	·0430	·0430	·0430
S.W.	Westminster Abbey Yard . .	·0419	·0419	·0419
S.W.	Trafalgar Square	·0420	·0420	·0420
	Average (two miles from Clapham Junction left out)			·04394.
E.	Bethnal Green Rd. & Punderson St.	·0474	·0506	·0490
E.	Stepney, White Horse Street . .	·0501	·0472	·0488
E.	Near London Docks . . .	·0528	·0508	·0518
E.C.	„ London Bridge . . .	·0407	·0407	·0407
E.C.	„ Bank of England . . .	·0492	·0482	·0487
E.C.	„ Holborn Viaduct . . .	·0457	·0457	·0457
	Average			·04745.
W.C.	Woburn Sq., near British Museum .	·0442	·0442	·0442
W.C.	Near Tavistock Square . . .	·0439	·0439	·0439
W.	Regent Street, Great Castle Street	·0466	·0442	·0454
W.	Oxford Square, near Edgware Road	·0385	·0385	·0385
W.	Hyde Park, Sloane Street . .	·0382	·0382	·0382
W.	Middle of Hyde Park . . .	·0382	·0352	·0367
	Average			·04115.
	Total average (two miles from Clapham Junction not included)			·04394.

London Air.—Carbonic Acid.—Metropolitan Railway.—November 1869.

Date.	Place.	Time of day.	Carbonic Acid per cent.	Oxygen per cent.
1869.				
Nov. 12 .	Tunnel between Gower Street and King's Cross Stations: specimen taken at the open window, first-class carriage	10 A.M.	·150	20·60
„ 12 .	Tunnel between Gower Street and King's Cross Stations: specimen taken at the open window, first-class carriage	7.30 P.M.	·078	20·79
„ 12 .	Tunnel, Praed Street: specimen taken at the open window, second-class carriage . .	10.30 A.M.	. .	20·71
„ 15 .	Specimen taken during journey between Gower Street and King's Cross, first-class carriage, windows open . . .	10.15 A.M.	·338	20·66
„ 15 .	Same	3 P.M.	·155	20·70
„ 15 .	Same	11 P.M.	·150	20·74
	Average . .	. .	·1452	20·70

Carbonic Acid in Scotland.

Having written so far, it was desired to throw more light on the subject by obtaining specimens from purely rural and hilly districts; and for this purpose Scotland was preferred. The uniformity in the numbers is something remarkable. There is no difference in the second decimal place, even in one instance, until we enter a town. I must therefore consider ·0336 per cent. as the amount of carbonic acid in the pure winds of the north of this island. Any amount above that is a deviation from purity. If we have regard to the third place of decimals, we find there nothing to indicate a deterioration; and we can scarcely hope to rely on the fourth place. Still the results in the fourth place are not to be rejected: we find them the highest on the plains, less at

1,000 and 2,000 feet, and more again at 3,000 to 4,000 feet, making differences of three in a million, and so at 3,000 feet beginning to increase, as other observers have stated to be the case at great elevations.

Carbonic Acid—Scotland.—1865.

Place.	Time.	Carbonic acid in 100 parts.		Mean.	
		At foot.	At top.		
Perth (*a*)	Aug. 4, 1 P.M.	·0341	. .	·0340	
"	" "	·0340	. .		
Moncrieffe Hill (*b*)	Aug. 5, 12 noon	. .	·0329	·0340	
" "	" "	. .	·0361		
Kalifountain Hill (*c*)	" 8 A.M.	. .	·0331	. .	
Kinnoull Hill (*d*)	Aug. 6, 12 noon	. .	·0315	·0315	
"	" "	. .	·0315		
Birnam Hill (*e*)	Aug. 8, 11 A.M.	. .	·0300	·0300	
" "	" "	. .	·0300		
Ochil Hills (*f*)	" 12 noon	. .	·0352	·0347	
" "	" "	. .	·0342		
				Foot.	Top.
Ben Ledi (*g*)	Aug. 10, 11 A.M.	·0342	·0323	·0335	·0327
" "	" 5 P.M.	·0329	·0331		
Ben Voirlich (*h*)	Aug. 11, 12 A.M.	·0329	·0321	·0329	·0320
" "	" 6 P.M.	·0329	·0319		
Ben Muich Dhu (*i*)	Aug. 17, 2 P.M.	·0346	·0357	·0344	·0356
" "	" "	·0342	·0356		
Lochin-y-gair (*j*)	Aug. 18, 3 P.M.	·0346	·0337	·0344	·0336
" "	" "	·0342	·0332		
Hill near Braemar (*k*)	Aug. 20, 3 P.M.	·0346	·0327	·0344	·0330
" "	" "	·0342	·0334		
Ben-na-bourd (*l*)	Aug. 23, 8 A.M.	·0344	·0337	·0350	·0337
"	" 1 P.M.	·0357	·0337		
Ben Nevis (*m*)	Aug. 30, 5 P.M.	·0346	·0322	·0346	·0327
" "	" 1 P.M.	. .	·0333		
Ben Lomond (*n*)	Sept. 2, 3 P.M.	·0350	·0344	·0350	·0339
" "	" 12 noon	. .	·0334		
Schehallion (*o*)	Sept. 6, 10 A.M.	·0341	·0335	·0340	·0335
"	" "	·0340	. .		
Mean	. . . .	·0341	·0332		

(*a*) Windy and cloudy. (*b*) Windy and cloudy, S.W. wind. (*c*) Few clouds, sunset, N.W. (*d*) Windy and cloudy, W. (*e*) Windy and clear, N.W. (*f*) Clear, N. (*g*) Clear, N.W., at foot. Cloudy, N.W. by N. at top. (*h*) Clear, N.W. (*i*) At foot, cloudy, N.W. On top, thick mist and drizzling rain. (*j*) On top, cloudy, N.W., as at the foot. (*k*) Cloudy, N., rainy. (*l*) Cloudy, N. (*m*) At foot, rain. On top, cloudy, N.W. (*n*) At foot, rain, N.W. At top, cloudy and misty, N.W. (*o*) At foot, cloudy, N.W. At top, rain, N.W.

Carbonic Acid.

Places not 1,000 feet high.		Places between 1,000 and 2,000 feet high.	
Name of Place.	CO_2 in 100 pts.	Name of Place.	CO_2 in 100 pts.
Perth	·0341	Birnam Hill	·0300
,,	·0340	Ochil Hills	·0347
Moncrieffe Hill	·0340	Braemar (Castletown)	·0344
Kalifountain Hill	·0331	Ballochbuie Forest	·0332
Kinnoull Hill	·0315	Hill near Castletown	·0334
Errol (marshy ground)	·0314	Mar Forest	·0339
Foot of Ben Ledi	·0335	Braemar (on the Dee)	·0344
Foot of Ben Voirlich	·0329	Schehallion	·0335
Aberdeen	·0329		
Elgin	·0347		
Inverness (Moray Frith)	·0341		
,, (above the town)	·0341		
Caledonian Canal	·0341		
Loch Ness	·0329		
Foot of Ben Nevis (Bennavie)	·0346		
Oban	·0348		
Foot of Ben Lomond	·0350		
Forest near Killiecrankie Pass	·0353		
Foot of Schehallion (Tummel Bridge)	·0340		
Mean	·0337	Mean	·0334

Carbonic Acid.

Places between 2,000 and 3,000 feet high.		Places above 3,000 feet high.	
Name of Place.	CO_2 in 100 pts.	Name of Place.	CO_2 in 100 pts.
Ben Lomond	·0339	Ben Muich Dhu	·0356
Ben-na-Bourd	·0337	Ben Nevis	·0327
Ben Voirlich	·0320	Ben Ledi	·0327
		Lochin-y-gair	·0335
Mean	·0332	Mean	·0336

Mean of all the foregoing in Scotland . . . ·0336

Carbonic Acid.

Place.	CO_2 in 100 pts.	Forests.	CO_2 in 100 pts.
Braemar	·0346	Ballochbuie Forest, near Castletown	·0340
,,	·0342	,, ,,	·0325
,,	·0344	,, ,,	·0327
,,	·0344	Mar Forest, ,,	·0336
,,	·0344	,, ,,	·0337
,,	·0344	,, ,,	·0343
Tummel Bridge	·0340	Forest near Killicrankie Pass	·0353
Mean	·0343	Mean	·0337

Carbonic Acid.—Perth City and outskirts.

Name of Place.	Time.	CO_2 in 100 pts.
Close, 148 South Street	Oct. 10, 11 A.M.	·0399
Over a conduit, Athole Crescent	,, 12, 7.25 A.M.	·0401
On North Inch, near the last	,, 12, 8 A.M.	·0352
Craigie	,, 12, 1 P.M.	·0324
,,	,, 12, 3 P.M.	·0332
,,	,, 12, 10 P.M.	·0573
Paul's Close, Newrow	,, 14, 8 A.M.	·0430
Close, 44 Pomarium	,, 17, 9 P.M.	·0508
,, 82 South Street	,, 18, 11.15 A.M.	·0464
,, 44 Pomarium	,, 18, 12.20 P.M.	·0353
Mean		·04136
Weaver's Shop, 56 Pomarium	Oct. 17, 9.40 P.M.	·2674

Glasgow.—Carbonic Acid, 1869.

The Closer Parts—Winter.	Carbonic Acid per Cent.		
	1	2	Average.
Exchange, front of	·0678	·0608	·0643
,, ,,	. .	. .	·0618
A court, Virginia Street	. .	. .	·0468
St. Vincent Street, West Nile Street	·0531	·0561	·0546
London Street, near the Cross	. .	. .	·0478
Blackboy Close, Gallowgate	·0585	·0538	·0561
A close, Wells Street	·0692	·0716	·0704
Open ground behind the barracks	. .	. .	·0398
King Park Place, Glasgow Green	·0453	·0407	·0430
Tennant Street, St. Rollox	·0540	·0589	·0564
Charles Street, St. Rollox	·0540	·0515	·0527
Average			·0539

Glasgow.—Carbonic Acid.

The Opener Parts—Winter.	Carbonic Acid per Cent.		
	1	2	Average.
Stirling Road, near Taylor Street .	·0515	·0496	·0505
Dobbie's Loan, near Poorhouse .	·0458	·0531	·0494
New City Road	·0496	·0441	·0468
St. Vincent Street, Elmbank Street	·0434	·0458	·0446
A green near Elmbank Street .	·0429	·0394	·0411
Park Terrace, West . . .	·0314	·0361	·0337
Anderston Quay	·0458	·0506	·0482
North Street, Main Street . .	. .	. .	·0564
Finnieston Quay	·0434	·0386	·0410
Open ground, end of Stobcross Street, near a foundry . .	·0515	·0466	·0490
Average			·0461
Total Average of Carbonic Acid			·0502

We must carefully study the numbers in the City of Perth. The whole week was windy; but still the amount is higher than round the city at some distance. Although there is much irregularity in the instance at Craigie, above the town the number is high. The analysis was repeated with the same result: the reason is not clear; chimneys may suggest one. On Kinnoul Hill and Errol the amount is lower than in any case, although the latter was wet and marshy. Such enquiries scarcely end. I must take the amount in the town on a calm day. There is, however, sufficient given to prove that, taking the oxygen and the carbonic acid together, there are indications which, although minute, may be found to correspond to great effects. The close places of the town have been of late very unhealthy.

To the series of analyses of air from various places it may be well to add some of the air from hospitals in London. These cannot be taken as sufficient, since an enormous number are required for proofs in these times, when every word spoken finds at once a ready doubter, if not an opponent. They are, however, specimens, and will serve to make comparisons in time to come.

Amount of Carbonic Acid and Oxygen in the Air of Workhouse Wards, having 500 cubic feet to each Inmate.

Workhouse.	Day.			Midnight.						Morning.					
	Carbonic Acid.		Oxygen.	Carbonic Acid.		Oxygen.				Carbonic Acid.		Oxygen.			
	Reduced.	Un-Reduced.	2	Reduced.	Un-Reduced.	1	2	3	Mean.	Reduced.	Un-Reduced.	1	2	3	Mean.
May 17th and 18th. Lambeth.															
Ward No. 5 . .	·0468	·0437	20·90	·0715	·0682	20·88	20·88	20·87	20·876	·0756	·0722	20·89	20·87	20·85	20·87
„ „ 23 . .	·0755	·0702	20·88	·1044	·0987	20·89	20·84	20·83	20·853	·0866	·0824	20·87	20·86	20·85	20·86
Means . .	·0611	·0569	20·89	·0878	·0845	. .	. .	. .	20·864	·0811	·0834	. .	. .	. .	20·86
May 18th and 19th. St. Luke's (Chelsea).															
Ward No. 73*a* . .	·0614	·0580	20·88	·0828	·0784	20·88	20·87	20·84	20·863	·0983	·0926	20·85	20·85	20·83	20·843
„ „ 68 . .	·0443	·0417	20·93	·0524	·0498	20·90	20·91	20·88	20·896	·0697	·0661	20·90	20·89	20·87	20·886
„ „ 12 . .	·0572	·0539	20·90	·0715	·0682	20·87	20·88	20·86	20·870	·0739	·0702	20·88	20·86	20·84	20·860
Means . .	·0543	·0512	20·9033	·0689	·0654	. .	. .	. .	20·8763	·0806	·0763	. .	. .	. .	20·863
May 19th and 20th. East London (Homerton)															
Ward No. 13 . .	·0641	·0610	20·90	·0815	·0773	20·88	20·88	20·87	20·872	·0856	·0814	20·89	20·88	20·87	20·88
„ „ 8 . .	·0472	·0448	20·91	·0708	·0692	20·88	20·89	20·89	20·886	·0705	·0672	20·89	20·90	20·88	20·89
„ „ 10 . .	·0472	·0448	20·94	·0771	·0733	20·90	20·88	20·88	20·886	·0621	·0590	20·90	20·90	20·87	20·89
Means . .	·0528	·0502	20·9166	·0764	·0732	. .	. .	. .	20·8832	·0727	·0692	. .	. .	. .	20·886
Total Means .	·0560	·0531	20·9033	·0777	·0743	. .	. .	. .	20·875	·0781	·0766	. .	. .	. .	20·869

The quantity by day in one of the rooms at each of the workhouses was greater, and the quantity of oxygen less than in the other rooms, and shows that the arrangements for ventilation were not equally good in all.

The least and greatest amounts thus obtained by day were :—

Carbonic acid.	Per cent.	Oxygen.	Per cent.
Minimum . . .	·0443	Minimum . . .	20·88
Maximum . . .	·0755	Maximum . . .	20·93

And the mean of all the experiments gave—

·0560	20·9033

The quantities at about midnight were—

Minimum . . .	·0524	Minimum . . .	20·853
Maximum . . .	·1044	Maximum . . .	20·866

And the means of all were :—

·0777	20·875

The quantities at about 5 A.M. were—

Minimum . . .	·0621	Minimum . . .	20·843
Maximum . . .	·0866	Maximum . . .	20·890

And the mean of all was :—

·0781	20·869

As the object of the enquiry was to show the composition of the air in well-ventilated rooms, it would be more correct to refer to results obtained in other hospitals ; but of these no store presents itself, and I can only place the mean of the above with the minimum of carbonic acid, and the maximum of oxygen side by side.

	Carbonic acid.			Oxygen.		
	Day.	About midnight.	About 5 A.M.	Day.	About midnight.	About 5 A.M.
		Per cent.	Per cent.		Per cent.	Per cent.
Means . .	·0568	·0780	·0802	20·893	20·875	20·869
Minimum .	·0443	·0524	·0621			
Maximum .	. .	. .	. .	20·93	20·886	20·89

When, however, the four best ventilated wards are selected, the means are as follow:—

Showing the Composition of the Air in the best Ventilated Wards.

	Day.		Midnight.		Morning.	
	Carbonic acid.	Oxygen.	Carbonic acid.	Oxygen.	Carbonic acid.	Oxygen.
	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.
Lambeth:—						
No. 5 .	·0468	20·90	·0712	20·876	·0756	20·87
Chelsea:—						
No. 68 .	·0443	20·93	·0524	20·896	·0697	20·886
Homerton:—						
No. 8 .	·0472	20·91	·0703	20·886	·0705	20·89
No. 10 .	„	20·94	·0771	20·8866	·0621	20·89
Means . .	·0463	20·92	·0677	20·886	·0694	20·884

Carbonic Acid and Oxygen in the Air of three Bedrooms of a Private House; one Person in each.

Room.		Carbonic acid.		Oxygen.	Capacity of room in cubic feet.
		Reduced.	Unreduced.		
1 . .	[1]1st day	·0606	·0570	20·90	1,221
	2nd „	·0519	·0478		
2 . .	1st „	·0626	·0590	20·89	2,079
	2nd „	·0670	·0621		
3 . .	1st „	·0560	·0529	20·89	2,042
	2nd „	·0738	·0682		
Means .	. .	·0619	·0578		

Hence it appears that the quantity of carbonic acid in the air of open spaces in London during the daytime varies from ·0334 per cent. in Hyde Park to ·0428 per cent. at Smithfield. In the wards of St. Thomas's, ·046; in the law courts, from ·0507 to ·203; in the theatres, from ·0817 to ·320; and in the hospitals at Madrid, from ·27 to ·43 per cent. But in the workhouse wards in question it was on the average ·0463, and at the minimum ·0443 per cent., a quantity scarcely

[1] The two averages for each room were on different days.

exceeding that in the open air and equal to that in the wards of St. Thomas's. This is the quantity which is found during 18 of the 24 hours daily.

I conclude with a short paper, read a few years ago, showing a great contrast between two qualities of air.

As my friend Mr. Alfred Fryer was going to the West Indies and America, I made up a box of tubes to hold specimens of air, adding also apparatus for its collection. He has brought me back some of the tubes filled; the rapidity of his movements prevented him from obtaining many. As Mr. Fryer is known to be a skilful experimenter we may be sure that the specimens are well preserved.

The air from off the Atlantic is seen to contain more oxygen than any of the others. I did not expect that with such a small number any average could be obtained that could be usefully compared with other results; but we find here that the amount of oxygen is almost identical with that found by me in the air on the sea-shore and open heaths of Scotland, and the amounts found by others in places where the best air was obtained. In other words, this air stands in the first class as regards oxygen, and we could expect nothing less.

The amount of carbonic acid could not be taken with confidence in the small quantity of air at command.

When we look at the analysis of the air from the island of Antigua we find some loss of oxygen; this corresponds to the outer circle of Manchester during dry weather, but not quite equal to the same in wet weather. In Antigua the morning was showery when the specimens were taken —April 11, 1865, at 9 A.M.

It is interesting and important to know that we can trace these small changes. It is probable that to them in part is due the character both of body and mind, not merely found in races, but in sections of the same race, separated perhaps by a hill or a stream, or raised from

the ground by a difference sometimes of a few feet only, although at times hundreds or thousands.

Oxygen per cent. in some Specimens of Air.

18 ft. above water. Fine day. 2.30 P.M. Lat. N. 43° 5′, Long. W. 17° 12′.		St. John's, Antigua. April 11, 1865, 9 A.M. Showery morning.	
	21·0100		20·9600
	21		20·9100
	20·9700		21
Mean .	20·9900	Mean .	20·9500

Dr. Thorpe in the Mem. of the Literary and Philosophical Society of Manchester, and 'Chemical Society Journal, 1867,' has given ·03 carbonic acid per cent. for sea air; in tropical Brazil, April and May, ·0328, and ·0308 over the Irish sea in July and August. His details are very interesting. There is no distinct difference at night.

During a visit to London a scientific friend called my attention to a law court which was badly, or rather in no way, ventilated; and I collected specimens.

Law Court, Feb. 2, 1866.		Law Court, from the lantern. 4.30 P.M. just as the Court was closing.	
	20·6400		20·5000
	20·6700		20·4800
Mean .	20·6500	Mean .	20·4900

The court was extremely warm and unpleasant at the moment of entering, and even after some minutes it was not to be voluntarily borne; I therefore did not attempt to penetrate the mass of people, but took specimens of air when perhaps eight feet from the door. On coming out, the feeling of relief was remarkably pleasant. This feeling, as elsewhere explained, is usually accompanied with a restoration of the normal action of the heart, and a calmer respiration.

The amount of oxygen in places not mountainous is given by me as 20·978—an average from many analyses.

London always stands well in examinations of air, and the parks will contain about 20·9800, and sometimes more, judging from the carbonic acid of which estimations have been made, leaving out the oxygen. We have then 209,800 of oxygen in a million, but in the law court only 206,500, or a loss of 3,300 in a million. Examining the tables to which I have already alluded, we find no place above ground with such a small amount of oxygen, except the gallery of an extremely crowded theatre at half-past ten at night, when the whole evening had been spent in spoiling the atmosphere, and those places at the backs of our houses, which we are not expected to name, much less to inhabit. Although by analysis these places were as bad as the court, in reality they were less so, as the court temperature was very high, and the organic matter from perspiration in proportion. The deleterious effects of this we are not yet able to judge of; the other we can to some extent measure. I say deliberately that this court where I took the air was worse than the middens alluded to.

The warmer air rises, and that at the ceiling is generally the worst. This, however, depends upon circumstances; if it has time to cool, from the height and space being great, the carbonic acid may be arrested before reaching a great height.

If from a space filled with warm air in which many persons have breathed we fill a flask and weigh it, we shall find that, unless the carbonic acid is unusually great, the weight is less than the weight of the same bulk of air taken before it was warmed by human beings. If we shut up the space and allow it to cool to its first temperature, and weigh a similar bulk of air, we find that it is really heavier than it was at first. Fortunately the warmth raises the air above us, and it seeks an exit away from our lungs; so that air rendered in this way impure is made lighter, but as soon as it cools, it is

heavier than at first and falls down. To ventilate well, the air must be removed before it cools, and the heating, cooling, and ventilating must work in harmony. It is not easy to bring these agents to act so.

The air raised into the lantern above the court was inferior to that below, and contained only 20·4800 of oxygen, being a loss of 5,000 in a million. Nature never seems to offer us air with a loss of even 1,000 in a million. Comparing healthy places with healthy, the difference is about 200, and perhaps this indicates a similar difference of vital principle in the air.

I need scarcely say that I found no such loss of oxygen in the mills of Manchester, or in any other inhabited place above ground during the day. If we seek air similarly degraded, we must descend the shafts of mines, and there we find oxygen removed in some places to a much greater extent. As an average, however, the currents in a metalliferous mine gallery contain 20·6500 of oxygen, exactly the amount in the court, and the air under the shafts 20·424, almost exactly the amount in the lantern. I certainly am anxious to see legislation in favour of miners; but this is a circumstance rather adverse to my hopes.

Combustion of the Carbon.

According to the best founded data there are burnt in the course of a year in Manchester 2,000,000 tons of coals. Now, supposing the district in which it is burnt to be 4 miles square, or 16 square miles, and the height of the atmosphere which is used by the inhabitants to be 60 feet, and the amount of carbon in the coal to be 75 per cent., we have introduced into this region in the course of a day 15,066 tons of carbonic acid, or 1·6499 per cent. of the air. Now, it may be said that the region is not correctly laid out; that 16 miles is too extensive, and 60 feet is not sufficiently high. In fine weather I

believe that the atmosphere is influenced by the smoke at least to the height of 600 feet. If that extent were taken, we should find the amount of carbonic acid equal to ·16499 of the air. But as a medium will probably be correct, let us say 300 feet high, and the amount will then be ·33 per cent. of carbonic acid thrown into the air.

The several cases will stand thus :—

	Per cent.
On a space of square miles, 60 feet high, there is an amount of carbonic acid from coals equal to. . .	1·6499
To this add the amount already in the air . . .	·06
Now, supposing 400,000 inhabitants give out 266 cubic feet of respired air with 6 per cent. of carbonic acid, there will be 330 tons or	·0362
Total carbonic acid . . .	1·7461

If this were changed ten times a day, the result would be, at any given period—

Carbonic acid from coal	·16499
Carbonic acid from breath	·00362
Usual amount of carbonic acid in the air . . .	·06
Total at a given time	·22861

If the air were changed twenty times a day, the result would be—

Carbonic acid from coals	·08248
Carbonic acid from expired air of inhabitants . . .	·0018
Carbonic acid of the atmosphere	·06
	·14428

But as a medium state is common, let us suppose the height to be 300 feet, changed ten times a day, then the result is—

Carbonic acid from coals	·033
Carbonic acid of expired air	·00072
Carbonic acid of the atmosphere	·06
	·0937

The average speed of the air at Liverpool is equal to 12·62 miles per hour by the observations of Mr. Hartnup,

F.R.A.S., of the observatory there; let us say twelve miles at Manchester. It would sweep over the four miles three times an hour, or thirty-six times in twelve hours.

This would give with the height of 300 feet—

Carbonic acid from coals	·0091
Carbonic acid from expired air	·0002
Usual amount	·06
	·0693

So that states of the atmosphere occur when the amount is very small when put into figures. Allowing the air outside the town to have ·03 of carbonic acid, and allowing the conditions in other respects to be the same as the last mentioned, we have—

Carbonic acid from coals	·0091
Carbonic acid from expired air	·0002
Natural carbonic acid	·0300
	·0393

When we examine the subject in this manner, we perceive how small is the actual amount of effect which the accumulated efforts of art can produce on nature, and we are inclined to look on the result as not to be regarded. These calculations agree so far with the results obtained that I am disposed to think that each is nearly true in its turn.

The above calculations, taken from a paper in the 'Journal of the Chemical Society,' written when groping my way in a place rather darker than it is at present, may be taken as an introduction to the tables of carbonic acid for this country.

Mode of Reading the Numbers.

When we read of a fraction per cent. we imagine the amount to be very small; and if the fraction is in the second decimal place we have no faith in it. We are

quite wrong. The amount of sulphuric acid in the air is still less than the carbonic acid, and yet a greater effect is produced on the atmosphere than is indicated by the amount shown in the oxygen or carbonic acid volumes. In speaking of these gases, we have been accustomed to disregard minute differences, and one-tenth of a per cent. of oxygen more or less is indifferently given or taken. We may reconsider this question of fractions. It is common to read the carbonic acid as so much in 10,000; if we read the oxygen column in the same way we shall find that the difference between 21 per cent. of oxygen and 20·90 is really 10 in 10,000 of the air and ·47 per cent. of the oxygen. This amount in the case of some gases is simply intolerable; and although not perceptible as regards oxygen, it may not be less efficient. I conclude from these enquiries that we must pay attention to the number in the second place for oxygen: perhaps at a future time we may arrive at the third.

In the case of carbonic acid we must attend to the third even now, as I believe, and for scientific purposes even to the fourth, or one part in a million. The numbers are all put down as fractions, and may be read as fractions per cent.; but they may all be read as whole numbers, disregarding the decimal point. Thus ·0314 will mean 314 in a million. Between this and ·0400 we have 86 in a million, which is no trifling amount even to the senses in the case of many gases, and we must learn that the effect of poisons on health is not in proportion to the effect on the sensations.

Let us consider what is meant by this 86 in a million. A room twice the size of one not unusual, or two 30 feet long, 24 wide, and 15 high, will contain 21,600 cubic feet, or 37,324,800 cubic inches. If we introduce ·0086 per cent., we bring 3,209 cubic inches of carbonic acid into the room, which will be considered a

large amount if put together in vessels: it is nearly 12 gallons. During fogs there will be an addition of nearly five times the amount = 60 gallons, or 405 in a million. If we take the numbers found in a very moderately close building, we add 1,235 in a million, or 14 times the first amount, or 168 gallons. If we make the room as close as a crowded theatre, taking the number given for one in London ·320, we add 2,831 in a million, or 377 gallons. And when we come to the state of air in mines, we have various numbers, in a few instances rising to more than 2 per cent.—nearly a million cubic inches, which would be 578 cubic feet, for the supposed room, which does not differ far from this in which I now speak [the Meeting-room of the Literary and Philosophical Society of Manchester].

In order to read off the amount in a million, there ought always to be four figures after the decimal point.

THE AIR OF MINES.

In addition to the analyses already given, I think it will be interesting to many to read a portion of an enquiry on the air of mines, published in 1864. It was hidden in a folio blue book, and although I think it is calculated to be useful, it has not become known either to chemists or practical men. It will show the various means resorted to for the examination of air for the gases and for the salts and the dust, several specimens of which were lithographed.

Gases.

Before giving analyses of the air in mines, it is well to enquire into the work of our predecessors; and this, it must be confessed, is a laborious undertaking, daily becoming more oppressive for scientific men because of the want of any full arrangement of scientific facts. As time presses, and the report must be ready, it may be sufficient

to mention the reasons for believing that the analyses quoted are the principal ones made public. Gätzschmann in Freiberg, and Combes in France, are men who, of all others, may be expected to know the subject; and I feel inclined to throw on them the blame if any important collection of the analyses of air from metalliferous mines has ever been made and neglected. On the first especially I have learnt to depend, not merely on account of the greatness of his own knowledge as evinced in his writings, but also from the experience of that knowledge in conversation and by correspondence. I know he was desirous of giving all aid, from the impulses of his own nature, as well as on account of the introduction to him from Baron F. C. von Beust, who is the first officer over the Saxon mines. To add to these, I might mention many of the most distinguished professors of Germany who would certainly have known enquiries of this kind had they existed. And no one will doubt the extensive knowledge of such men as Bischof of Bonn.

Without going into details, it may be said that modern writers speak of bad air very much as the mining writers for three centuries have done; but where very late times are arrived at, we have the analyses of Moyle of Cornwall, and of Mr. Hunt, and to these must be added the still more recent enquiries of Dr. Brockmann of Clausthal, which seem less known, although most searching and valuable.

The only separate volume on mining air which has come before me, after advertising and enquiring, is that of Dr. Alois Wehrle, of Schemnitz, published in Vienna in 1835. But analyses are few. When speaking of the candle, there may be more occasion to quote from him. These are some of his opinions on the air: —

'That air in which the mining candle burns dull and dark, but in which the workman feels no oppression, is called (*Matteluft*) dull, flat, or stale air. But when the

workman cannot keep his candle burning, it is called bad air (*schlechte Lüfte* or *Wetter*). A man may live in this bad air, and an Argand lamp may burn in it; but when this also goes out, and the workman feels confined (*beengt*), or when he is suffocated, then it is bad or poisonous air (*bad weather or schwaden*).

'When the quantity of oxygen falls under 13 per cent., and is too small for the process of respiration, or when the carbonic acid amounts to 7 per cent., with several per cent. of sulphuretted hydrogen and miasmas of a peculiar kind, they communicate to the atmospheric air a property which is often very dangerous.' (Wehrle 'Ueber die Grubenwetter,' p. 6.)

Wehrle says: 'The first consequence of a disturbed relation of the oxygen is that the mine candle burns darkly, and will only burn by enlarging the wick, until it at last goes out.' (By enlarging the wick is meant putting the candle to the side and melting more grease, when more of the wick is laid bare, or by spreading it out; an actually larger wick is not used.)

'As oxygen has a greater affinity to the ingredients of the burning material according as they are more decomposable, it follows that this phenomenon occurs sooner or later according to the quality of the substances burnt; and it is well known that tallow goes out when oil burns well, and that the common mine lamp goes out when the Argand lamp with the same oil burns clear.'

'The venous blood also possesses a greater affinity for the oxygen than the carbon of the combustible substance, and a man can replace the loss of oxygen by frequent breathing, and therefore can live where candles and mine lamps go out.'

P. 10. 'As neither the combustible material nor the venous blood can remove all the oxygen from the air, but rather diminish it to 18 and at most to 12 per cent., it follows that atmospheric air, the oxygen of which is to

a certain degree diminished, acts towards men and candles like pure nitrogen.'

P. 12. 'In an atmosphere poor in oxygen, there is felt, not so much as a consequence of the presence of nitrogen, as of the absence of oxygen, contraction of the chest, tickling of the eyes, fatigue, weakness, and anxiety; we breathe more heavily and frequently, and are compelled to make more exertion at work, whilst perspiration and thirst ensue. With continued breathing, Humboldt observed paleness, hardening of the glands, herpetic eruptions, laming of the extremities, and early asthma.'

Mr. P. Moyle examined the air of the Cornish mines. His results were published in the Report of the Royal Cornwall Polytechnic Society in 1839, and the 'Annales de Ch. et de Phys.' vol. iii. 1841. The following is a list of mines from which air was taken, and the analyses of the specimens, taken from the 'Annales de Chimie,' and afterwards supplemented from the original.

			Oxygen.
1.	Wheal Vor	250 fms. from the surface, and 15 from the shaft, two men working in the cavity; air taken a few min. after explosion . .	18·416
2.	do.	16 fms. from the shaft, 10 min. after firing	16·69
3.	do.	24 fms. from shaft, 30 min. after firing	18·95
4.	do.	22 fms., 30 min. after firing . .	17·282
5.	do.	21 fms., „ „ . .	16·762
6.	do.	Four men at dead end . . .	14·76
7.	do.	After firing	17·01
8.	Great work mine	30 fms. from a shaft, 15 min. after firing	15·15
9.	do.	20 fms. directly after firing . .	16·45
10.	do.	23 fms. from shaft, 30 min. after firing	17·544
11.	Binner Downs	18 fms. from shaft, 2 men . .	16·764
12.	Carn Brea	25 fms., 2 men	15·69
13.	do.	45 min. after firing . . .	14·51
14.	Tresarvean	After firing, 65 fms. from shaft .	16·35
15.	Wheal Ann	16 fms., 2 men, no gunpowder .	16·72
16.	do.	End of a level, powder not employed for some time, 45 fms., 2 men	18·22
17.	Consol's Mine	. . . . .	17·78
18.	do.	. . . . .	18·425

These figures are all the averages of several analyses. The specimens were collected by emptying a bottle of water or mercury at the spot. Water would appear to have been used in all cases, as there is a disproportionately small amount of carbonic acid, from ·06 to ·15.

Supposing we recalculate, and give to each specimen an amount of carbonic acid equal to the oxygen removed, we should have different numbers. This amount would not be quite correct, but very nearly. Allowing the amount of nitrogen to be 79 per cent., a number which must be within a small fraction of the truth, we may find the following changes in Mr. Moyle's results:—

	Oxygen per cent.
No. 1	18·80
2	17·44
3	19·34
4	17·95
6	15·74
7	17·71
8	16·09
9	17·23
10	17·86
11	17·55
12	16·57
13	15·51
14	17·14
15	17·46
16	18·74
17	18·37
18	18·91
Mean	17·55

Mr. Moyle says that the lights burned with difficulty in No. 6 specimen; I have not found that candles could burn in such air.

These analyses are the only series found to have been made in this country, and the largest hitherto made anywhere on the air of mines. Hausmann obtained in one of the Harz mines 13 per cent. of oxygen. It is, however, an extremely laborious operation to seek out the literature of any scientific subject, and I doubt if I shall have time

to exhaust this branch before the date when this report must be written.[1] It certainly will be no pleasure to me to neglect the labours of any man. I believe we must come to very late years, viz. 1851, to find another enquiry of equal authority with that of Moyle's, although not of equal extent. This was made by Dr. Brockmann, with the aid of a young chemist, deceased, named Bodemann. Brockmann, who lives at Clausthal, in the Hanoverian Harz, quoted largely in the original report as well as here, and is especially a student of, and an authority on, the condition of mines and the diseases of miners.

These analyses, by 'Bodemann, assayist,' were published in the 'Bergwerke Freund' ('Miner's Friend'), vol. viii. No. 11, and by Dr. Brockmann in his work on the diseases of the Upper Harz.[2]

First Analysis.

Air from Eleonora mine. Place, 6th level, 220 fathoms deep, 1 fathom high, and 1 broad; 120 from shaft, 80 from the nearest winze; the rock not a very hard clay-slate. Two men had worked from 4 to 9 in the morning, and at 6 o'clock 3 holes were bored, and at 9 o'clock 2.

The air was taken about 3 to 4 minutes after firing the last of the holes, and at the spot. There were 6 men with 6 candles present at the time. There was a west-north-west wind, a favourable one for the ventilation of the Upper Harz mines. The average of four analyses is:—

1·86 less oxygen than normal air.
1·8042 carbonic acid by volume, or
2·7496 by weight per cent.

Second Analysis.

Air from pit Queen Charlotte, 80 fathoms from the nearest inside shaft or winze, 500 fathoms from the

[1] This was written in 1863, but it might be repeated in 1871.

[2] *Die Krankheiten des Oberharzes*, von Dr. Karl Heinrich Brockmann, p. 30.

nearest shaft, 60 fathoms from a draught of air. The place was 200 deep, very moist, and with water on the ground. No ventilators used. Tallow candles were not extinguished, but oil-lamps burned badly. Two men had worked since 4 in the morning. At 7.30 o'clock two holes were fired, then two more; and half an hour after, that is at 8.15 o'clock, the air was taken.

1·94 per cent less than normal oxygen.
1·7781 carbonic acid by volume.
2·6975 „ „ weight.

Third Analysis.

From Queen Charlotte pit, 135 fathoms deep, 40 from draught; wet ground; may be taken as the representative of an ordinarily well-ventilated place.

·73 less than normal oxygen.
1·0418 carbonic acid by volume.
1·5874 „ „ weight.

Fourth Analysis.

Duke William mine, 289 fathoms down, 100 fathoms from draught; wet ground, 3 to 4 inches water. The ventilator was removed for 4½ days.

Air taken at 10 o'clock, after four holes had been fired, two men having worked since 4 o'clock. On the previous day two men had worked from 4 to 12, and fired 6 holes. The day before that, two men had worked from 6 to 12, and fired 4 holes, each having 2 ounces of powder.

2·29 per cent. less than normal oxygen.
2·3794 carbonic acid by volume.
3·6262 „ „ weight.

Fifth Analysis.

Air from Wilhelm's shaft, 5 fathoms from bottom, or 310 deep: active circulation of air.

·45 less than normal oxygen, carbonic acid wanting.

Sixth Analysis.

Air from Caroline shaft, 157 fathoms deep, before beginning the week's work.

·29 less than normal oxygen.
·64748 carbonic acid by volume.
·9867 „ „ weight.

Seventh Analysis.

After the week's work.

·21 less than normal oxygen.
·7241 carbonic acid by volume.
1·1035 „ „ weight.

Eighth Analysis.

From the same, after a week's work.

0·51 less than normal oxygen, wanting carbonic acid.

From these results Dr. Brockmann says we have—

Carbonic acid by volume . . 1·1964 per cent.
„ weight . . 1·8214 „

This calculation of the average does not quite agree with mine.

The mean of oxygen is 19·785, and taking the amount in the atmosphere at 20·815 we have a diminution of 1·030 per cent.; this is where ventilators have been used. Averaging these analyses in the manner adopted with the former we have:—

	Oxygen.	Carbonic acid.
No. 1	18·955	1·8042
2	18·96	1·7781
3	20·085	1·0418
4	18·525	2·3794
5	20·365	?
6	20·525	·6474
7	20·605	·7241
8	20·305	?
Mean	19·790	1·396

Were I writing this again, the amount of oxygen in the air would be taken at not less than 20·98, but it is unnecessary to recalculate the table.

Each line gives the mean of from 2 to 6 analyses; making the whole number 29.

Moyle's figures are also means of several trials. It would have been better in both cases to have had the separate items.

These analyses show a very bad condition of the atmosphere in the Harz mines, but a still worse in the mines of Cornwall. I confess myself inclined to think that the figures given by Mr. Moyle for oxygen are rather too low; but the reasons, founded on numerous results obtained by myself, must not be taken as conclusive that Mr. Moyle has made any error; it may be that he has been more choice in his spots. One circumstance, however, speaks strongly against his analyses. A candle will not burn with less than 18 per cent. of oxygen, when there is 3 per cent. of carbonic acid present at the same time. Twenty-five years have greatly changed our ideas of the purity of air, and I was informed by Cornish miners that they have worked in spots where it was impossible to remain above ten minutes at a time, and when nearly every man in his turn fell down overcome, and the candles went out. Nevertheless, they went to their work and took their turns. This occurred when working upwards to meet a winze which was also being worked downwards. Mr. Moyle has no doubt sought such extreme cases, not to be obtained at will, and he had a right to choose such places, because the men were at any time liable to be exposed to their influence. Such, at least, would explain a part; but the candle difficulty remains. We must not forget that the habits of men have changed much in many places; and to look at the holes through which some of the older miners occasionally burrowed in the Harz, prepares one

for a worse state of things at a previous period. Looking now at Mr. Moyle's results, from the experience gained in the lead chamber to be afterwards mentioned, there is no difficulty in concluding that they represent very rare conditions of the atmosphere, even in mines. A man may live awhile in air such as most of the specimens alluded to, but long, I believe, he cannot live. I feel strongly inclined to doubt if he can exist in air such as Nos. 6, 12, and 13. But with great lungs and great strength, perhaps, it may be possible for a while. Healthy people of various ages were found unable to bear such a loss of oxygen in the lead chamber. We may read in a lecture by W. Mackworth, Esq., R.E., that Mr. Robert Hunt, F.R.S., found 17·6 of oxygen in the average air of the Consolidated Mines, whilst the average of 18 samples of air from different Cornish mines yielded 17 per cent., and of 6 others 19 per cent. of oxygen. I fear I have not seen all the labours of Mr. Hunt on this subject.

Method of Analysis.

It has already been shown why especial importance was attached to the oxygen and carbonic acid of the air, although it was not believed that their presence or absence to the extent of a half per cent. or even one per cent. would have a direct action which could be at once perceived by the senses.

To obtain specimens for analysis tubes of about $\frac{3}{4}$ of an inch (nearly 2 centimètres) in diameter were used, the length being 6 inches. They are drawn out at each end, and a fine tube $1\frac{1}{2}$ inch in length is left attached. Near the junction of the fine and the wide tube is a very narrow neck. They are in fact made after Professor Bunsen's model. The air is drawn through the tube until it is filled; and by the use of a candle and blowpipe the neck is melted and the specimen of air hermetically

sealed up, so that it may be preserved for ages unaltered, as far as we know. Fig. 1. shows the tube with the pump attached, ready to be filled, and the tube before sealing. Beside it is the tube sealed. Fig. 2 shows

Fig. 1.

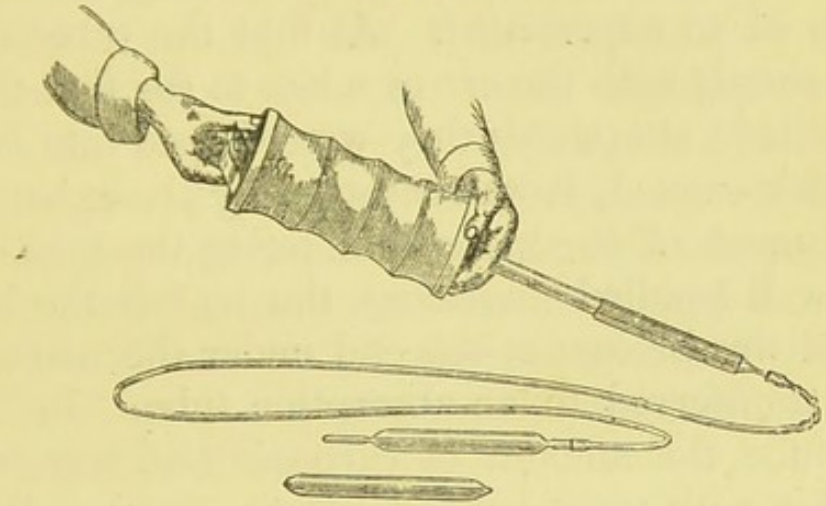
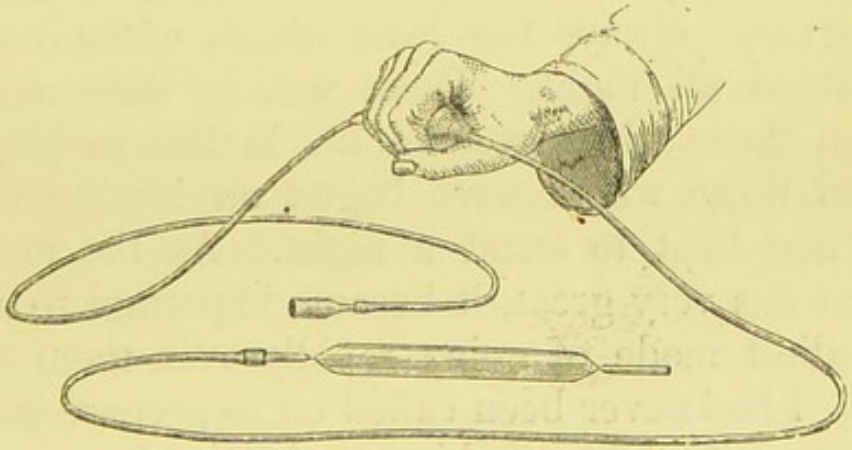


Fig. 2.



another method with an elastic ball pump. The candles in many instances burned very badly and gave little heat, so that the act of sealing was difficult, and sometimes a failure. For this reason it will be seen that there are many numbers which have not the analyses attached, as it was found that the tubes leaked. In these cases the air may have been equal to that mentioned by Mr. Moyle, so that the worst would be left out. It may even happen that this is the reason for some specimens receiving

a better character than they deserve, as the leak may not have been discovered. Most of the specimens were kept a few weeks before examination, some a few months; and minute leaks would be effective in that time in rendering the inside air equal to that outside, although that small leak might not be discovered during the short continuance of an experiment. At first the tubes were examined simply with the eye or a lens to see that the points were melted; afterwards they were dipped into mercury; if any leak existed, it was betrayed by the existence of a drop or more of the mercury entering the tube. If the tube is well handled in sealing, the lens is the best test. The tube was broken at one end under the mercury, and the air transferred to an absorption tube. In this graduated tube the amount of carbonic acid was taken, by absorption with moist balls of caustic potash. When the balls were moist it did not require quite an hour to complete the absorption in wide tubes; an hour and a half, however, was always the least given, whilst one set of tubes stood all night. There was no difference found between those which were done in the middle of the day and those which were begun on leaving the laboratory and kept to stand a night. As the number of analyses was very great, it became important to ascertain the readiest mode of going rapidly over them with accuracy. I had never been called on to prepare an amount of analyses at all approaching to the number now before me, and was a little alarmed. I began with the method of my master, Liebig, and obtained in many cases good results; but I confess to being very much annoyed with the endless washings required in order to remove the caustic solution of pyrogallic acid, a strongly-coloured liquid, from the tubes and from the mercury. Besides these troubles there was observed occasionally an unaccountable obstinacy in the solution, as it appeared extremely unwilling to rise, and wearied the hand by

constant shaking and watching; nor was there any distinct mark to show the limit of the absorption or end of the experiments, except the exhaustion of the patience. I had therefore for some time given up the plan as a rule. Dr. Calvert objects to the method on account of an error of ·4 per cent. caused by the formation of carbonic oxide. I may say, however, that I have sometimes and frequently obtained results far more accurate than the formation of ·4 per cent. of carbonic oxide (See Dr. Calvert's paper, 'Proceedings of the Lit. and Phil. Soc. of Manchester') would indicate, so that the amount formed cannot always be so great. However, I need not mention these results of mine as Liebig himself and Bunsen, I believe, have both performed with pyrogallic acid very accurate analyses of air.

The method of explosion was preferred, and Bunsen's eudiometer used. Frankland's apparatus might have been taken if anyone could have been found to mend it in time. It would be interesting to know by which method the greatest number of analyses could be produced. With Frankland's apparatus one gas only is analysed at a time, but it is done very rapidly. Five or six eudiometers were used at once. For obtaining a spark various methods had been frequently tried. At one time for a while I was satisfied with the rosin electrophorus, but from the moisture of the climate it frequently became extremely difficult to carry to another room as much electricity as would cause an explosion, and no spark could be had except before a fire. Bunsen's tube was greedily sought, as a very simple method, and in dry weather acted well, but became at times quite useless, so that whole days were spent in obtaining a spark. A small electric apparatus was got, and then a large one was used, but both required peculiarly warm rooms, and sometimes in wet weather the mere opening of a door would render useless the machine for a considerable time, although one had a

plate of about 18 inches diameter. It was then clear to me that to work with any comfort a battery and a Ruhmkorff's coil were necessary. With this apparatus the amount of work was multiplied by five at least, and made into a pleasure instead of a pain. The battery used was made of cast iron and zinc. It was, perhaps, unnecessarily large; there were 12 plates of cast iron, and 24 of amalgamated zinc of 6 inches square. This battery has been some years in my possession; the plates have never been renewed, and they are as smooth as at the beginning, although now becoming thin. It is lifted up and down into the cells by a pulley, so that the plates are only a few seconds at a time in the acid when a gas is to be exploded. After doing the usual experiments of a laboratory for a few years, it was set down to the induction coil about three years ago, and in that period, without special cleaning, and merely by a change of acid once in six months, it has exploded perhaps from two to three thousand gas mixtures, besides being used occasionally for decomposing water. It makes us independent of weather.

My assistant at the time, being little accustomed to the use of this apparatus, preferred pyrogallic acid, and I was willing that he should take his experience, which ended in results similar to my own; at the same time I am bound to confess that quietness and patience produced better results. Still that quiet and that patience could not be found with 500 analyses waiting before us. It was in fact necessary to work on a scale and by a method where the feelings would be completely excluded, and the mechanism of the operation be responsible for the exactness. This point, I believe, was attained, with gain in time, in comfort and in accuracy; when I say in accuracy, I do not mean that there is an actual increase of accuracy, but in the number of experiments which were not failures. The analyses spoilt by the explosion

method are very few, and were in reality at the latter part of the enquiry reduced to almost nothing.

Measure of the Accuracy of the Analyses.

At this stage it is necessary to enquire into the character of the results from which the conclusions are drawn.

I shall cite all the duplicate analyses from one volume of notes, containing those of air from Welsh mines, and a few others, taking them exactly as they come, leaving none out.

Mark.	Oxygen, 1st.	Oxygen, duplicate.
W 53	18·60	18·74
25	20·89	20·96
77	20·82	20·79
64	20·52	20·61
76	20·96	20·85
16	20·49	20·48
60	20·73	20·67
52	19·58	19·60
43	20·25	20·17
37	20·64	20·76
39	19·96	19·86
W 55	20·47	20·52
W 58	20·36	20·29
121	20·79	20·90
116	19·70	19·54
W 16	20·44	20·34
W 17	20·42	20·38
D	20·73	20·56
D 10 top	20·09	20·50
D 47	19·09	19
D 8	20·55	20·55
	22) 446·50	22) 446·54
	20·29	20·29

Here we have the average of both columns, absolutely the same to the 2nd decimal place, the farthest distance to which it is attempted to go, and farther after all than with absolute certainty we can at all times pretend to go. Of course it is clear that in individual analyses some

errors must occur, as in D 10 top; but these errors equalise themselves, and unless they were small and very regular they would not do so. That the two sets of analyses should be so near as in the columns above is the utmost that can be desired, especially as it is after all true that we are apt to be more careless about a duplicate, although it is far from wise to be so; the first may be considered as the real analysis, and the second merely to show that no gross error has been made.

Pursuing further the enquiry into the accuracy of the numbers, we may put the question in this form. How great is the departure from 21 per cent. shown in specimens of good air, and inferior air such as is found at ash-pits? 21 is used here simply as a convenient whole number, without any theory to exalt it. The figures were put down for the illustration of this question on p. 28, to which reference may be made.

The numbers in that form, when bared of all their superfluous bulk, show more strikingly the change from pure to impure air. It must, however, be remembered that this is such air as all persons are at times compelled to breathe. The impure air, such as is found about middens, and in waterclosets, actually contains less oxygen than the pure air. The pure air deviates from 21 per cent. by ·065; the air of these places spoken of by ·295. This illustrates further my reason for using the oxygen as an indication of pure air, although not the only one.

In all analyses, and in all work generally, tendency to error is in one particular direction more than in any other; this tendency may depend on peculiarities of the individual or of the conditions of the work. The tendency in these is towards an increase of the oxygen. If the hydrogen which is mixed with the air is not free from oxygen, the air has the benefit of the mistake.

Some of the analyses, about 80, beginning from about

No. 60, were made with hydrogen obtained by throwing sodium amalgam into dilute sulphuric acid.

This gas was very pure. The amalgam was made so as to be a pretty firm paste, and sulphuric acid with about three times its bulk of water was heated and cooled. The first bubbles were allowed to pass away, after throwing the amalgam into the water lest some air might be taken. 76·22 of this gas exploded with water-gas still gave on calculation 76·22, and with time the manipulation becomes pleasant. The gas stood in a tube ready for use. It was not found practicable to prepare by galvanic methods so much hydrogen as was required. Afterwards, more for the covenience of others than myself, zinc was used, a quantity of unusual purity having accidentally been found some time ago. And whilst I place the fullest confidence in the oxygen as being correct to a small fraction, the same minute accuracy cannot be expected of the carbonic acid, although on taking the average the amount of the gas agrees so well with the amount + the carbonic acid from the powder smoke, that the error must be trifling. The estimations of carbonic acid made elsewhere than in mines are not subject to any error except that of the operator ; and this has been reduced so as at any rate to be very small. The reason that the carbonic acid in these tables is in some cases not so minutely ascertained as the oxygen is this: the amount being extremely small and the total amount of air being extremely small, there was more risk of error. Up to this point all the carbonic acid on my lists has been got by Pettenkofer's method, by which any extent of accuracy may be attained. After seeing the best way, I gave all the work into the hands of others : for a long time Dr. Reissig. In no other way could I make an investigation. I suppose all chemists gradually find the necessity of this, the study of the question becoming quite as much as time will permit to one.

Analyses, Published in 1864.

Current means distance from current.

Air normal or nearly so, represented by Old Style figures, e.g. . 21·9

Air less pure or decidedly impure „ Antique „ . . **20·6**

Air extremely impure below the above „ Roman „ . . 30·2

Old Style in the 'Carbonic Acid' column means calculated amount, not actual analysis . . . . . ·6

Carbonic acid of pure air not above ·04 per cent.

No. or Mark on Sample.	Name of Mine, and Depth from surface in fathoms.	Description of Place where taken, and time when taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
1	Rampgill (*a*) .	End, 6½ ft. wide, 4½ ft. high; current, 300 ft.; time, 2 hrs. aft. shot . . .	° 68	2	20·4	·6
2	„ . .	Large cavity, 18 ft. high and 22 ft. wide . .	60	8	**20·60**	·73
3	Long Clough . .	Large cavity, 20 ft. high .	60	5	**20·84**	·16
4	„ . .	In vein, 6 ft. wide, 20 ft. high; current, 42 ft. .	61	4	**20·62**	·27
5	„ (*b*) . .	In driving at an end, 16 ft. high; time, 15 min. aft. shot	61	5	19·94	·76
6	Rodderup Fell (*c*).	Sinking shaft 10 fms. deep from level, 9½ ft. diam. bratticed . . .	58	4	21 04	·48
7	„ (*d*) . .	End, 9 ft. high, 6 ft. wide; current, 24 ft.; time, 15 min. aft. shot . . .	57	4	20·9	·1
7a	„ . .	. . . . .	—	—	**20·66**	·34
9	„ (*e*) . .	End of level; 2 hrs. aft. shot	62	4	18·80	2·06
10	„ . .	In draught, stope above level	64	4	20·28	·72
11	„ . .	Do., in level 6 ft. wide .	59	—	**20·66**	·19
12	„ . .	Top of rise, level above not communicating. .	60	6	**20·76**	·24
13	Cashwell (*f*) .	42 ft. from end, level 4½ ft. wide, 5 ft. to 9 ft. high .	61	4	20·19	·70
14	„ . .	In current . . .	57	4	20·34	·66
15	„ . .	78 ft.	61	4	20·95	·05
16	„ . .	In current . . .	55	2	**20·64**	·25
17	Garrigill . .	End; in current . .	54	4	**20·79**	·21

(*a*) Ventilated by fan blast. (*b*) About 4 lbs. of powder had been fired that morning.
(*c*) Here were men working in water; temperature of water 54°. (*d*) Branching crystals.
(*e*) Faggots of little prisms. (*f*) Heaps of mineral in level.

Mark on Sample.	Name of Mine, and Depth from surface in fathoms.	Description of Place where taken, and time when taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
			°			
18	Allenhead's Mine, West Henry's Vein	120 ft. from rise . .	66	6	Broken	—
19	„ . .	30 do. . .	66	—	19·01	2·35
20	„ (*a*) . .	In direct current . .	63	—	20·20	·80
21	„ (*b*) . .	In a rise 30 fms. high, bratticed: top 6 ft. above brattice . .	66	2	19·09	1·91
22	„ . .	Current, 72 ft. from winze .	66	—	Broken	—
23	„ . .	Current, 90 ft. from winze .	78	—	20·22	·78
24	East part Henry's Vein	End in large cavern 18 fms. wide and 8 ft. high; current 72 ft. from rise .	66	—	20·292	·708
25	„ (*c*) . .	Do. . . .	69	—	**20·8**	·2
26	„ (*d*) . .	Do.	67	—	**20·87**	·747
27	„ . .	Rise, 18 ft. high; time, 1 hr. aft. shot	70	2	18·27	2·73
28	„ . .	In level	67	—	19·3	1·7
29	Greenlaws (*e*) .	240 ft. from rise, 6½ ft. high, 4½ ft. wide	57	4	21	
30	Settlingstone (*f*) .	End of level, 36 ft. from rise, 6½ ft. high, 4½ wide	59	6	20·47	·53
31	„ .	End of level, 150 ft. from rise, 6 ft. high, 4 wide	58	—	Broken	—
32	Grinton Moor .	End, 150 ft. from shaft	51	3	21	—
33	„ (*g*).	Opening at bottom of sump, 3 fms. below level	53	7	**20·85**	·15
33a	„ (*g*).	. . . . .	—	—	20·23	·75
34	Surrender . .	In a sinking stope below level, 30 ft. long, 18 ft. wide	51	—	**20·79**	—
34a	„ . .	Do. do. .	—	—	20·23	·64
35	„ (*h*) .	End, 240 ft. from rise. .	57	—	20·17	1·04
36	„ . .	Just under shaft in adit level	48	—	20·89	·13
36a	„ . .	End of level, 72 ft. from shaft	—	—	20·36	·6
*37	Botallack Crowns Lode (*i*) 190	In direct current . .	75	—	**20·84**	·53
*37a	„ (*i*) .	Do. . . .	—	—	**20·64**	·22
*38	„ . 190	End, 60 ft. from rise .	78	2	**20·88**	·11

(*a*) Furnace ventilation. (*b*) Furnace ventilation. (*c*) No shots for several ays. (*d*) No shots for several days. (*e*) Air must have been blowing here. f) Very dry. (*g*) Two samples from this end. (*h*) Air machine. (*i*) Smells f tallow. Two samples taken.

* These samples, 37 to 39, show the difference of the air when a long distance from current.

No. or Mark on Sample.	Name of Mine, and Depth from surface in fathoms.	Description of Place where taken, and time when taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
			°			
*39	Botallack Crowns Lode (*a*) . 190	End, 84 ft. from rise .	87½	2	19·96	1·04
40	„ . 166	Stoping in level . .	77	2	**20·82**	·18
41	Huel Cock . 125	Stoping, back of level, good current	65	2	Leaking tube	—
42	„ . 142	Do. do. .	71	6	**20·83**	·17
43	„ . 180	In current . . .	—	—	Broken or lost	—
44	„ . 180	End, 180 ft. from rise .	76	2	20·40	·90
45	Huel Hazard 85	In current . . .	62	—	21	
46	„ 85	120 ft. from rise, dry .	67	—	20·11	·50
47	„ 85	360 ft. from rise, wet .	—	1	**20·84**	·31
48	Huel Call 30	End, 42 ft. from rise, 6 ft. high by 2½ wide, hard granite, wet	62	1	**20·73**	·42
49	„ 85	End of level, 42 ft. from rise, granite, dry	67	2	20·96	·04
50	Wheal Owles Buzza Mine(*b*)50	End of adit, 480 ft. from rise, wet	63	2	20·04	1·24
51	„ 50	End of cross cut, 420 ft. from rise, wet	63	1	19·93	1·43
52	„ 50	End, 96 ft. from rise, dry .	60	2	20·47	·92
53	Levant . 150	End, 690 ft. from current, 7 ft. high, 3 ft. wide, hard ground, killas; time, 1 hour aft. shot	75	2	**20·75**	·42
54	„ . 150	End, 660 ft. from current, 7 by 3 ft.	73	2	20·11	1·25
55	„ . 150	End, 1,080 ft. from current, 8 by 3½ ft.	78	2	21	
56	„ . 170	Stopes, 180 ft. from winze	82	8	20·53	1·05
57	„ . 170	End, 540 ft. from winze, 9 by 5 ft.	83	2	Broken	—
58	„ . 170	Stopes 48 ft. from current	73	2	21	
59	Balleswidden 130	Stopes	72	2	20·14	1·59
60	„ (*c*) 140	End, 7 ft. by 4 ft., smoky .	73	2	19·83	1·03
61	„ . .	Rise, 6 ft. high; current 140	73	2	20·93	·06
62	Wire Gill . .	Stopes	—	—	20·40	·64
63	„ . .	End	55	2	20·96	·017
64	„ (*d*) . .	End of level, 780 ft. from current. In shale; time, 20 min. aft. shot	60	2	**20·82**	·59

(*a*) Smells of perspiration. (*b*) Levels nearly choked with deads, only 2 ft. clear.
(*c*) Smoky. (*d*) Ventilated by water blast.

* These samples, 37 to 39, show the difference of the air when a long distance from current.

No. or Mark on Sample.	Name of Mine, and Depth from surface in fathoms.	Description of Place where taken, and time when taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
			°			
65	Hurst . . .	End, 300 ft. beyond a rise, 9 ft. high, 7 ft. wide	—	2	—	1·99
68	Old Gang . .	End of level . . .	—	2	20·58	·48
69	„ . .	End of level . . .	—	2	—	·28
70	„ (*a*) .	Rise, 7 ft. high, 132 ft. from current	—	2	20·25	·39
71	Grassington (*b*) 60	End of cross cut, 480 ft. from rise	—	2	20·94	·06
72	„ . .	End, 480 ft. from rise .	—	2	19·53	1·59
73	„ . .	Rise, 60 ft. high: in shale	—	2	19·52	1·72
74	„ . .	End, 60 ft. from rise .	—	2	20·47	1·06
75	„ (*c*) .	End, 840 ft. from rise .	—	2	20·08	·94
76	Habden Moor .	End, 150 ft. from rise .	—	2	**20·79**	·21
77	Cononley . .	Pitch in back of level, 7 ft. high, 15 wide; time, 15 min. aft. shot	—	3	**20·84**	·59
78	„ . .	Winze, 15 ft. below level; time, 40 min. aft. shot .	—	2	20·54	·37
79	„ . .	Pitch in back of level, 18 ft. high, 18 ft. wide	—	4	20·05	·66
80	„ . .	Rise, 42 ft. high . .	—	2	19·27	1·84
99	. . .	. . . . .	—	—	} Not come to hand	
100	. . . .	. . . . .	—	—		
101	South Francis 104	End, 90 ft. beyond winze .	—	2	20·95	·05
102	„ . .	Pitch, 12 ft. high, 3 ft. wide	—	2	20·12	·99
103	„ . 114	End, 180 ft. from shaft, 7 ft. by 4 ft.	—	2	19·96	·75
104	„ . 124	End, 120 ft. from winze, wet	—	2	20·05	·95
105	„ . 50	End, 180 ft. from shaft, choked	—	2	20·4	·42
106	„ . 60	End, 90 ft. beyond rise, 7 ft. by 3½ ft.	—	2	20·54	·45
107	„ . 70	End, 12 ft. from current; time, 1½ hr. aft. shot	—	2	20·37	·72
108	„ (*d*) . 80	End, 120 ft. from winze .	—	2	21·06	—
109	„ (*e*) . 94	End, 42 ft. from air . .	—	2	20·36	·84
110	„ (*f*) . 94	End of cross cut, 300 ft. from rise; time, 1½ hr. aft. shot	—	2	**20·65**	·46
111	Dolcoath . 314	Bottom of shaft . .	—	—	20·99	·05
112	Huel Harriet (*g*) 130	End, 36 ft. from shaft, 7 ft. by 4 ft.	—	3	**20·75**	·21

(*a*) Air machine. Hand fan, 4 in. pipes. (*b*) Unusual amount of dust.
(*c*) Crystals were chiefly hexagons. (*d*) Ventilated by double duck machine.
(*e*) The men had left work. (*f*) Double duck machine. (*g*) Hard granite.

No. or Mark on Sample.	Name of Mine, and Depth from surface in fathoms.	Description of Place where taken, and time when taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
			°			
113	Huel Harriet (*a*) .	End of cross cut, 18 ft. beyond winze	—	2	20	·97
113a	. .	. . . . .	—	—	—	—
114	. .	Stopes above 100 fm. level	—	16	21	·10
115	,, . .	End cross cut, 240 ft. in .	—	2	broken or leaking	—
116	Carn Camborne 13	End, 150 ft. from winze .	—	2	19·70	1·37
,,	. . . .	. . . . .	—	—	19·54	1·37
117	,, . 13	End, 36 ft. from rise, 7 ft. by 4 ft.	—	—	**20·72**	·29
117(2)	. . . .	. . . . .	—	—	—	—
118	. . . .	. . . . .	—	—	broken or leaking	—
119	. . . .	. . . . .	—	—	broken or leaking	—
120	Tolcarne . .	. . . . .	—	—	19·81	·91
121	,, . 40	End, 78 ft. from shaft .	—	2	**20·79**	·21
121(2)	. . . .	. . . . .	—	—	20·90	·1
122	,, . 40	End, 240 ft. from winze .	—	2	20·49	·69
123	,, . .	. . . . .	—	—	broken or leaking	—
124	,, . .	. . . . .	—	—	broken or leaking	—
125	. . . 30	End, 126 ft. in . .	—	2	20·04	·97
126	,, . .	. . . . .	—	—	broken or leaking	—
127	,, . 20	End, 120 ft. from winze .	—	2	**20·88**	·06
128	Wheal Friendship	. . . . .	—	—	19·32	1·15
128(2)	Tolcarne . 20	End, 36 ft. in . . .	—	—	20·29	·51
129	. . . .	. . . . .	—	—	—	—
130	. . . .	. . . . .	—	—	20·90	·03
D 1	. . . .	. . . . .	—	—	broken or leaking	—
D 2	. . (*b*) . .	. . . . .	—	—	**20·60**	·38
D 3	. . . .	. . . . .	—	—	broken or leaking	—
D 4	Crelake . 52	End, 144 ft. from rise .	—	2	19·82	1·05
D 5	,, . 52	Rise, in current . .	68	—	20·96	·04
D 6	,, (*c*) . 40	End, 576 ft. from shaft .	66	—	broken or leaking	—
D 7	. . . .	. . . . .	—	—	No account of it	
D 8	,, . 24	End, 96 ft. from winze .	—	2	20·55	·23
D 9	,, . 12	Rise, 24 ft. high, no current	—	2	19·98	·97
D 10	,, (*d*) . 12	End, 24 ft. from shaft .	—	—	20·09	·60

(*a*) Winze choked. (*b*) Has a smell of mouldiness. (*c*) Ventilated by waterfall. (*d*) Top.

No. or Mark on Sample.	Name of Mine, and Depth from surface in fathoms.	Description of Place where taken, and time when taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
			°			
D 11	Sortridge Consols 30	End of level . . .	—	2	19·86	·92
D 12	„ 40	End, 240 ft. from shaft .	—	4	19·54	1·26
D 13	„ 40	End	—	—	**20·82**	·86
D 14	„ 40	End, 660 ft. from air .	—	4	broken or leaking	—
D 15	„ 50	Cross cut . . .	—	4	**20·72**	·13
D 16	„ (*a*) 50	End, 660 ft. from air .	—	—	**20·70**	·10
Do.	„ (*b*) .	. . . . .	—	—	**20·87**	·14
D 17	„ (*c*) 50	End, 420 ft. from current .	—	4	19·43	1·30
Do.	„ (*d*) .	. . . . .	—	—	20·27	·80
D 18	„ .	. . . . .	—	—	No tube corresponds to this number it is believed	
D 19	„ .	. . . . .	—	—	Do.	do.
D 20	„ .	. . . . .	—	—	Do.	do.
D 21	„ .	. . . . .	—	—	Do.	do.
D 22	Wheal Friendship	Cross cut, 300 ft. from current	66	6	**20·67**	·22
D 23	„ 100 East (*e*) 100	End, 240 ft. from draught	60	6	19·99	·95
D 24	„ 70	End, 120 ft. from winze .	61	4	Cannot find it	
D 25	„ 80 East 80	End, 240 ft. from winze, 180 ft. from trapdoor	65	6	19·40	1·50
D 26	„ 80	Cross cut, 72 ft. from current	65	2	19·55	1·39
D 27	„ . 100	End, 60 ft. from winze .	66	6	18·80	1·37
D 28	„ 100	End, 57 ft. from winze .	66	6	**20·80**	·27
D 29	East Gunnislake (*f*) 36	End, 468 ft. from winze .	—	—	19·57	1·33
D 30	„ 12	Bottom of shaft . .	—	—	20·08	·89
D 31	„ 12	End, 42 ft. from shaft .	—	—	20·30	·80
D 32	„ .	Adit end, 96 ft. from shaft	—	—	**20·76**	·34
D 33 See at end of D.	Okel Tor . 80	End, 300 ft. from winze .	—	2	19·22	1·79
D 34	„ . 65	End, 24 ft. from winze .	—	2	**20·76**	·74
D 35	„ .	. . . . .	—	—	20·51	·31
D 36	Huel Edward 98	Bottom of shaft . .	—	3	**20·73**	·34
D 37	„ (*g*) 91	End, 240 ft. from shaft .	—	2	20·45	·42
D 38	„ (*h*) 81	End, 372 ft. from shaft .	—	—	20·17	·72
D 38a	„ .	. . . . .	—	—	20·30	·63
D 39	„ 71	End	—	—	**20·66**	·29

(*a*) Top. (*b*) Bottom. (*c*) Bottom. (*d*) Top. (*e*) Ventilated by door and pipes. (*f*) Air-pump worked by engine-rod. (*g*) Smell of mouldiness. (*h*) Waterfall and pipes.

No. or Mark on Sample.	Name of Mine, and Depth from surface in fathoms.	Description of Place where taken, and time when taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
			°			
D 40	Huel Edward .	. . . . .	—	—	20	1·31
D 41	„ 45	Tribute pitch . . .	—	—	19·91	1·01
D 42	„ .	End. Side level 180 ft. from main level	—	—	20·37	·68
D 43	Frank Mills 60	End, 252 ft. from winze .	—	—	20·44	·43
D 44	„ 60	Stopes, 30 ft. long, 15 ft. wide, 12 ft. high	—	—	20·55	·28
D 45	„ (*a*) 70	End, 210 ft. beyond winze	—	—	**20·68**	·31
D 46	„ .	Bottom of shaft . .	—	—	**20·82**	·06
D 47	South Exmouth 30	End, 150 ft. from current .	—	2	18·87	1·88
„	„	. . . . .	—	—	18·80	1·37
D 48	„ (*b*) .	Rise, 24 fms. from air .	—	2	19·46	1·15
D 49	„ 45	End, 120 ft. from rise .	—	—	Broken, it is supposed	
D 50	„ .	Bottom of shaft . .	—	—	19·34	1·75
D 51	. . . .	. . . . .	—	—	Not known if sent	
D 52	. . . .	. . . . .	—	—	Do.	Do.
D 53	„ 45	End, 330 ft. from winze .	—	—	18·67	2·26
D 57	. . . .	. . . . .	—	—	No such number sent it is believed	
D 58	. . . .	. . . . .	—	—	Do.	Do.
D 59	. . . .	. . . . .	—	—	Do.	Do.
D 60	. . . (*c*)	. . . . .	—	—	18·87	1·97
D 61	. . . .	. . . . .	—	—	No such numbers known to be on the tubes	
D 62	. . . .	. . . . .	—	—		
D 63	. . . .	. . . . .	—	—		
D 64	. . . .	. . . . .	—	—		
D 65	. . . .	. . . . .	—	—		
D 66	. . . .	. . . . .	—	—		
D 67	. . . .	. . . . .	—	—		
D 68	. . . .	. . . . .	—	—		
D 69	. . . .	. . . . .	—	—		
D 70	. . . .	. . . .	—	—	19·86	1·12
East. 47 D	. . . (*d*)	. . . . .	—	—	19·07	1·90
30 S	. . . .	. . . . .	—	—	—	—
35 D 35	„ 50	End, 360 ft. from winze .	—	—	20·10	·75
E 1	Dolcoath . 314	End, 30 ft. from winze .	82	12	20·94	·06
E 2	„ . 268	Winze, 24 ft. deep . .	—	2	20·92	·08
E 3	„ . 268	Level at mouth of winze .	—	—	19·77	1·17
E 4	„ . 266	End, 150 ft. from sump .	84	3	20·41	·4
E 5	„ . .	Stopes between 242 and 256 fm. levels	—	—	20·50	·28

(*a*) Trapdoor and pipes. (*b*) Air-pipes. (*c*) Peculiar organic smell.
(*d*) Smells of tobacco.

No. or Mark on Sample.	Name of Mine, and Depth from surface in fathoms.	Description of Place where taken, and time when taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
			°			
E 6	Huel Ruth 130	End, 150 ft. from rise, 7 ft. by 3 ft.	72	2	19·33	1·70
E 7	„ (*a*) 180	End	75	2	20·56	·43
E 8	„ 190	End, 120 ft. from rise .	76	2	20·13	·93
E 9	„ 190	End	77	3	19·96	·98
E 10	St. Ives Consols 160	End, 90 ft. from rise .	80	2	**20·70**	·39
E 11	„ 177	End	77	2	**20·81**	·02
E 12	„ 147	End, 120 ft. from winze .	75	2	20·50	·39
E 13	„ .	Carbons, 70 ft. by 20 ft. to 40 ft.	72	—	20·49	·63
E 14	„ 87	End, close to winze . .	65	—	20·48	·36
E 15	Carn Brea . 155	Stopes close to shaft .	64	2	**20·60**	·43
E 16	„ . 175	End, 36 ft. from winze .	70	2	20·28	·51
E 17	„ (*b*) 197	End, 102 ft. from winze .	77	2	20·20	·70
E 18	Condurrow . 140	End, 108 ft. from shaft .	68	2	**20·71**	·29
E 19	„ (*c*) 155	End, 120 ft. from winze .	71	2	20·27	·72
E 20	„ . 166	Stopes	66	5	20·47	·43
E 21	„ . 165	End, 168 ft. from winze .	—	2	20·52	·43
E 22	„ . 177	End, 132 ft. from shaft .	73	2	20·29	·62
E 23	„ . 180	Bottom of shaft . .	—	3	20·93	·08
E 24	„ . 188	End	70	2	**20·63**	·27
E 25	East Pool . 150	End, 60 ft. from winze .	75	2	20·27	·79
E 26	„ (*d*) .	Under a rise . . .	68	—	20·41	·51
E 27	„ . 140	Stopes	66	—	20·95	·08
E 28	West Cheverton .	Stope close to shaft . .	64	2	20·90	·03
E 29	„ 70	End, 120 ft. from air .	68	2	19·56	1·43
E 30	St. Day United 154	End, killas, 7 ft. by 4 ft. Dry	77	2	20·45	·51
E 31	„ (*e*) 174	End, 12 ft. from winze .	82	2	19·42	1·37
E 32	„ 174	Bottom of winze, 30 ft. deep	88	2	**20·72**	·15
E 33	„ .	Stopes	80	—	19·31	1·73
E 34	West Basset (*f*) 75	Cross cut end, 6·30 ft. in. 7 ft. by 4 ft. Dry. Granite	—	2	19·55	1·46
E 35	„ (*f*) .	Mouth of cross cut . .	—	—	**20·70**	·19
E 36	„ 85	End, 120 ft. from winze .	—	2	19·63	1·4
E 37	„ 94	End, 90 ft. in, dry . .	—	—	20·37	·48
	Botallack (*g*) 160	¼ mile from shaft, under sea	—	3	**20·63**	·68
	South Francis (*g*)	Air at surface coming from shaft	—	—	20·54	·82
	„ (*g*) 29	24 fathoms from a winze, 90 from shaft	—	—	18·69	2·22

(*a*) Peculiar specks, very numerous, apparently crystalline, or at least transparent. (*b*) Wet. (*c*) Dry. (*d*) Smell of mouldy substances. (*e*) Choked. (*f*) An aromatic smell, such as we may read of chiefly in German writers as existing in mines. (*g*) These specimens were taken by myself, and the crystals first seen on them. The relative amount therefore not attempted to guess at.

No. or Mark on Sample.	Name of Mine, and Depth from surface in fathoms.	Description of Place where taken, and time when taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
			°			
	Dolcoath Mine (*a*) 233	80 fms. from shaft . .	—	—	**20·77**	·74
W 1	Minera (*b*) 60	End, 105 ft. from sump, 6½ ft. by 4½ ft.	—	2	19·91	1·16
W 2	„ (*c*) . .	End adit, 210 ft. from sump	—	2	20·50	·29
W 3	„ . . .	Level in current . .	—	—	broken or	leaking
W 4	South Minera (*d*) 54	End, 90 ft. from shaft, 7 ft. by 5 ft.; time, several hours aft. shot	—	2	19·73	·92
W 5	Park (*e*) . 125	Level, 150 ft. beyond winze, 6½ ft. by 5 ft.	—	2	19·72	1·32
W 6	„ . . .	From air-pipe . . .	—	—	20·32	·82
W 7	Maes-y-safn (*f*) 125	End, 90 ft. from vent .	—	—	19·40	1·51
W 8	„ (*g*) 155	End, 90 ft. from shaft .	—	6	19·26	1·86
„	„ (*h*) .	Do. do. .	—	—	20·47	·49
W 9	„ . .	Bottom of shaft . .	—	—	**20·69**	·27
W 10	Rhoses Mawr .	Winze, 15 ft. deep . .	—	2	20	·89
W 11	„ . 60	Level between shafts .	—	—	**20·67**	·38
W 12	„ . 70	End, 45 ft. from shaft .	—	—	19·54	1·26
W 13	„ . 70	End, 75 ft. from shaft .	—	2	20·35	·40
W 14	Talargoch (*i*) 90	End, 72 ft. from shaft, 6 ft. by 4 ft.; 1½ hours aft. two shots	—	2	20·11	·81
W 15	„ . .	. . . . .	—	—	broken	
W 16	„ (*j*) 90	End of great cross cut, 600 ft. from level	—	—	20·44	·57
2d	„ . .	. . . . .	—	—	20·48	·36
3d	„ . .	. . . . .	—	—	20·49	·36
W 17	„ . .	From mouth of pipes .	—	—	20·44	·46
W 18	„ . 90	Level; in current . .	—	—	**20·76**	·24
W 19	„ . 110	Level; in current . .	—	—	20·58	·39
W 20	„ . 110	End, 90 ft. from rise .	—	—	—	—
W 20 a	Coed Mawr Pool 15	End, 72 ft. from air . .	—	2	20·25	·46
W 21	„ 10	End, 102 ft. from shaft .	—	2	20·27	·56
W 22	Symmdde Dylluam (*k*) 40	End, 60 ft. from shaft; 20 min. aft. 2 shots	—	2	19·90	1·10
W 23	„ .	Winze, 18 ft. deep . .	—	2	**20·87**	·26
W 24	„ .	Rise, 30 ft. high . .	—	2	**20·77**	·09
W 25	„ 35	End	—	2	20·92	·04
W 26	„ 70	End	—	—	20·47	·55
W 27	„ .	Winze, 12 ft. deep . .	—	3	20·42	·49

(*a*) This specimen was taken by myself, and the crystals first seen on it. The relative amount therefore not attempted to guess at. (*b*) Hard, dry, siliceous, with shale. (*c*) Level choked. (*d*) Ground hard. Wet. (*e*) Ventilated by hand fan. (*f*) Smell of tallow and perspiration. (*g*) Roof of level. (*h*) Floor of level. (*i*) Limestone. (*j*) Ventilated by door and air-pipes. (*k*) Aromatic smell formerly alluded to.

No. or Mark on Sample.	Name of Mine, and Depth from surface in fathoms.	Description of Place where taken, and time when taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
			°			
W 28	Pary's Mine 100	Stopes from winze . .	—	—	**20·72**	·10
W 29	„ 112	End, 210 ft. from shaft .	—	2	19·81	1·18
W 30	„ .	Winze, 24 ft. deep, 8 ft. by 4 ft.	—	—	19·16	1·80
W 31	„ .	. . . . .	—	—	—	—
W 32	„ 90	End	—	—	20·34	·49
W 33	Lisburn (*a*) .	. . . . .	—	—	—	—
W 34	„ (*a*) .	. . . . .	—	—	—	—
W 35	„ (*a*) .	. . . . .	—	—	—	—
W 36	„ (*a*) .	. . . . .	—	—	—	—
W 37	„ . .	. . . . .	—	—	**20·76**	·22
W 38	Loggy Lass .	. . . . .	—	—	20·30	·63
W 39	„ .	. . . . .	—	—	—	—
W 40	Cwm Erfin . 30	End of cross cut, 300 ft. from shaft	—	—	20·35	·61
W 41	„ . 32	End, 150 ft. from winze, 7 ft. by 5 ft.	—	2	20·12	·79
W 42	„ . .	Rise, 12 ft. high, 102 ft. from winze	—	2	20·04	1·74
W 43	„ . 20	End, 312 ft. from winze; time, soon after shot	—	2	20·25	·72
W 44	„ . 20	Stope	—	4	—	—
W 45	East Darren 104	End, 270 ft. from air, 7 ft. by 5 ft.	—	2	20·12	·66
W 46	„ 80	End, 180 ft. from winze .	—	—	19·72	1·24
W 47	Bryn Gwiog (*b*)75	End, 120 ft. from rise, 6½ by 5 ft.	—	2	19·75	1·22
W 48	„ (*b*) .	Pitch 12 ft. up. . .	—	—	20·11	·9
W 49	„ (*b*) .	Under a winze. Supply air	—	—	20·92	·02
W 50	„ (*b*) .	Bottom of shaft . .	—	—	**20·76**	·12
W 51	. . . .	. . . . .	—	—	20·04	·83
W 52	Mona (*c*) . 60	End, cross cut, 240 ft. from current	—	2	19·58	1·32
W 53	„ . . .	Pitch, 150 ft. from current	—	2	18·62	2·26
W 54	„ (*d*) . .	End	—	2	20·20	·76
W 55	„ . . .	Do. do. after blower had been worked	—	—	20·52	·41
W 56	„ . . .	Supply air to blower .	—	—	20·90	·07
W 57	„ (*e*) . .	End of cross cut, 360 ft. deep, 120 ft. from shaft	—	2	20·37	·87
W 58	Snailbeach . 171	End, 450 ft. from sump; time, ½ hour after shot	—	2	20·36	·61

(*a*) Cannot find these. (*b*) All taken before any shots had been fired. A good deal of dust, scarcely crystalline in any part. (*c*) Air-pipes not much used. (*d*) Candles burn badly. (*e*) Aromatic smell, very distinctly that of apples; German miners frequently speak of this.

No. or Mark on Sample.	Name of Mine, and Depth from surface in fathoms.	Description of Place where taken, and time when taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
			°			
W 59	Snailbeach . 171	Under sump . . .	—	—	20·07	·83
W 60	„ . 186	End, 270 ft. from sump .	—	2	**20·73**	·26
W 61	„ . .	In voog . . .	—	—	—	—
W 62	„ . 201	Bottom of shaft . .	—	2	**20·72**	·14
W 63	„ . 171	End, 132 ft. from sump .	—	—	**20·74**	·12
W 64	„ . 156	End, 315 ft. from sump .	64	—	20·56	·31
W 65	„ . 126	Level; in current . .	—	—	19·44	1·45
W 66	„ (*a*) . 126	End, 300 ft. from sump .	—	2	20·29	·57
W 67	„ . .	Tribute pitch between two shafts	—	—	19·61	1·33
W 68	Roman Gravels (*b*) 6	End, 120 ft. from winze .	—	2	**20·81**	·26
W 69	„ (*c*) 20	End, 290 ft. do. .	—	4	19·58	1·42
W 70	„	Sump, 36 ft. deep . .	—	3	19·90	·92
W 71	Dyliffe (*d*) . 25	Cross cut, 360 ft. from air	—	—	19·70	1·20
W 72	„ (*e*) . 75	End, 480 ft. from winze .	60	2	19·45	1·70
W 73	„ (*f*) . .	Do. . . .	—	—	20·07	·88
W 74	„ . 85	End, 210 ft. from shaft .	—	2	18·88	2·21
W 75	Foxdale . 72	End, 150 ft. do. .	—	—	**20·73**	·12
W 76	„ . .	Stopes between 72 and 86 fm. levels	—	6	20·90	·05
W 77	„ . 86	End of cross cut . .	—	2	**20·80**	·18
W 78	„ . 100	End, 210 ft. from shaft .	—	2	19·79	1·3
W 79	„ . 86	End, 180 ft. from rise .	—	2	—	—
W 80	„ . 72	End, 90 ft. do. .	—	2	19	1·00
W 81	Laxey . .	Stopes	—	4	**20·84**	·05
W 82	„ (*g*) 165	End	—	4	**20·72**	·16
W 83	„ 212	End	—	2	19·92	1·16
W 84	„ .	Bottom of shaft . .	—	—	—	—
W 85	„ (*g*) 110	End	—	2	19·62	1·53
W 86	Coniston (*h*) .	End, 900 ft. from draught	55	2	20·26	·88
W 87	„ .	Sump, 84 ft. deep . .	64	3	19·58	1·40
W 87b	Silberthwaite (*i*)	End, 3,300 ft. in . .	—	2	—	—
W 88	Goldscope . .	Stopes in 60 fm. level .	—	2	20·07	·91
W 89	„ . 90	End, 36 ft. in, 6½ ft. by 4¼	—	2	20·55	·3
W 90	„ (*j*) .	Bottom of shaft . .	—	3	20·42	·6
W 91	„ (*k*) 60	End, 132 ft. from winze .	—	2	19·77	1·05
W 92	Castlenook . 28	Bottom of shaft . .	—	3	19·62	1·13
W 93	„ (*l*) 10	End, 54 ft. from rise, 6½ ft. by 4 ft.	—	2	20·46	·54
W 94	Greenside . .	Engine sump, 23 ft. wide .	—	2	**20·62**	·47

(*a*) Peculiar organic odour. (*b*) Fatty smell; butyric acid very clearly amongst others. (*c*) Air-pipes, level choked. (*d*) Ventilated by duck machine attached to engine-rods. (*e*) Blower idle. (*f*) After machine at work. (*g*) Air-pipes. (*h*) Door and pipes. (*i*) Air-pipes. (*j*) Black dust chiefly; not in great quantity. (*k*) Men just going to fire. (*l*) Level choked.

No. or Mark on Sample.	Name of Mine, and Depth from surface in fathoms.	Description of Place where taken, and time when taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
			°			
W 95	Greenside . .	Rise, 18 ft. high . .	—	2	—	—
W 96	„ . .	End, 30 ft. from air . .	—	2	**20·84**	·08
W 97	„ . .	Sump	—	4	19·55	1·39
W 98	Hutton . .	. . . . .	—	—	20·90	·06
W 99	Upleatham . .	. . . . .	—	—	20·22	·80
W 100	„ . .	. . . . .	—	—	20·91	·1
	Northumberland:					
K	Derwent . 120	95 fm. level, forehead west; current 220 ft.	66	2	—	—
L	„ . 91	50 fm. level, forehead east; current 120 ft.	64	2	—	—
	Cumberland:					
M	Stow Craig 7	7 fm. level, forehead north; current 24 ft.	48	4 2 per shift	21	—
N	„ 12	12 fm. level end, north; current 180 ft.	56	2	20·03	1·53
O	Lady Vein . 30	Rise in forehead of adit, east; current 240 ft.	53	2	—	—
P	Guttergill . 12	High level, east driving; current 60 ft.	54	2	20·44	·72
Q	Dowpot Syke 15	South cross cut end; current 66 ft.	55	2	20·19	1·02
R	„ 10	Top level end; current 42 ft.	56	2	20·56	·29
S	Brownley Hill 90	South driving on 'jug' vein; current 12 ft.	56	2	**20·77**	·99
T	„ 93	Sump below adit; current 72 ft.	62	2	**20·69**	·41
U	„ 83	Ore workings above adit; current 50 ft.	63	4	20·98	·02
V	„ 108	South driving, '4 fm. limestone;' current 100 ft.	56	4 2 per shift	20·33	1·49
W	Nenthead, about 58	Small cleugh level, end going east; current 150 ft.	62	6 2 per shift	—	—
X	„ about 58	Small cleugh level, end going west; current 150 ft.	66	6 2 per shift	—	—
Y	„ „ 58	Ore workings, roof of level; in current	66	18 6 per shift	—	—
Z	Nattrass . .	End driving south; current 600 ft.	62	2	—	—
A A	Honey Bee . 7	End driving east; current 420 ft.	60	2	—	—

No. or Mark on Sample.	Name of Mine, and Depth from surface in fathoms.	Description of Place where taken, and time when taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
	Cumberland:		°			
B B	White Bee 25	End driving east; current 24 ft.	56	2	—	—
C C	Clargill Burn 40	Bottom of sump in low drift; current 160 ft.	59	2	—	—
D D	Fullgrove . 10	Adit, level end, west; current 42 ft.	51	2 per shift	—	—
E E	Crossgillhead 25	Adit, level end, going south; current 700 ft.	52	2 per shift	—	—
F F	Dosey . . 25	Slopes in sole of adit; in current	46	2	**20·62**	·48
G G	Calvert . 60	Clearing adit, south-west; current 120 ft.	50	2	**20·79**	·20
H H	Dougang . 90	Fourbreast of adit level, going west; current 720 ft.	60	6	**20·68**	·38
I I	„ . 60	End of driving going east; current 60 ft.	58	4 2 per shift	19·09	1·84
	Yorkshire:					
J J	Stang, Arkendale . . 20	South cross cut forehead; current about 1,000 ft.	55	4 2 per shift	19·63	1·11
	Cumberland:					
J J	Birchy Bank .	End of east drift; current 80 ft.	52	2	20·37	·77
J J 2d	„ (*a*) .	. . . . .	—	—	—	—
	Yorkshire:					
K K	Stang, Arkendale . . 15	Rise in north driving; current about 400 ft.	58	2 per shift	18·77	2·38
L L	Foregill, Arkendale . . 30	End south, bottom of sump; current 90 ft.	55	3 per shift	19·04	2·51
L L 2	. (*b*) .	. . . . .	—	—	—	—
M M	„ 30	Drift end going towards sump; current 50 ft.	55	4 per shift	19	2·40
M M 2	. . .	. . . . .	—	—	—	—
N N	„ (*c*) 20	'Slack,' cross cut end; current 500 ft.	58	2 per shift	20·13	·96
N N 2	. (*d*) .	. . . .	—	—	19·62	1·39
O O	Fellend, Arkendale . . 40	Gutter's level, drift end east; in current	56	3 per shift	—	—
P P	Hurst. about 30	Pryes level, low drift end; current 180 ft.	58	2 per shift	20·92	—
P P 2	. . . .	. . . . .	—	—	20·37	·58
Q Q	Copperthwaite 45	Low drift forehead, east; current about 200 ft.	54	2 per shift	**20·81**	—

(*a*) A slight but disagreeable smell. A friend considered it putrescent. I could not decide. Found no sulphuretted hydrogen. (*b*) Fatty matter and perspiration.
(*c*) After first explosion in level forehead. (*d*) After second explosion.

No. or Mark on Sample.	Name of Mine, and Depth from surface in fathoms.	Description of Place where taken, and time when taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
	Yorkshire:		°			
Q Q 2	. . . .	. . . . .		—	20·56	·25
R R	Grinton . 6	Low level end; current 300 ft.	52	2	20·52	·43
S S	Old Gang . 90	Bunting, south cross cut end; current 210 ft.	63	2 per shift	19·72	1·28
T T	„ . 60	Bunting, top drift end, west; current 300 ft.	58	4 2 per shift	19·82	1·17
U U	„ about 50	Barberry main level, forehead; current about 300 ft.	55	4 2 per shift	20·55	·22
V V	„ „ 50	Barberry cross cut, no forehead; current about 200 ft.; time, immediately aft. a shot	57	4 2 per shift	19·60	1·11
W W	Hurst . . .	Queen's level, drift 12 fms. above it; in current	55	6 2 per shift	—	—
X X	Arkendale . 70	Faggergill, forehead of adit level; in current	56	4 2 per shift	**20·78**	—
X X 2	. . . .	. . . . .	—	—	20·59	·40
Y Y	Keldheads . 65	Main level forehead; current 210 ft.	51	8 2 per shift	19·77	1·41
Y Y 2	. . . (*a*)	. . . . .	—	—	—	—
Z Z	„ . 81	Low level forehead; current 54 ft.	59	8 2 per shift	19·55	1·64
A A A	West Burton 20	Stopes over main level forehead; current 180 ft.	59	10 5 per shift	20·90	—
2d	. . . .	. . . . .	—	—	—	—
B B B	„ 18	West stopes above level; current 20 ft.	58	6	20·91	·09
C C C	Keldheads . 35	Ash bank, main forehead; current 120 ft.	58	6 2 per shift	19·24	2·08
2d	. . . .	. . . . .	—	—	20·26	·76
D D D	„ . 31	Ash bank, rise; current about 90 ft.	58	4 2 per shift	20·29	1·21
2d	. . . (*b*)	. . . . .	—	—	—	—

(*a*) A strong smell as of cinders burning, sulphurous evidently. (*b*) Aromatic and also fatty.

No. or Mark on Sample.	Name of Mine, and Depth from surface in fathoms.	Description of Place where taken, and time when taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
			°			
	Westmoreland:					
EEE	Dupton Fell 50	'North Dobron's' forehead; current: ventilated by fissures in limestone	53	6 3 per shift	20·13	·78
FFF	„ . 50	'South Dobron's' forehead; current: ventilated by fissures in limestone	55	8 4 per shift	20·21	1·03
GGG	„ . 100	Threalkeld or deep level forehead; current: ventilated by fissures in limestone	53	4 2 per shift	19·84	1·37
HHH	Silverband . 80	Stope vein forehead; current: ventilated by fissures in limestone	49	4 2 per shift	**20·82**	·23
HHH2	. . . .	. . . . .	—	—	20·47	·63
III a	„ . 80	Remington; current: ventilated by fissures in limestone	53	4 2 per shift	**20·79**	·33
2d	. . (*a*) .	. . . . .	—	—	—	—
JJJ	„ . .	Silverband forehead; current: ventilated by fissures in limestone	50	4 2 per shift	20·44	1·02
SSS	. . (*b*) .	. . . . .	—	—	—	—
No. 1	Clifford Mine, the hot mine (*c*) .	120 ft. from shaft, 2 from winze	105	—	**20·87**	·13
No. 2	. . . 220	From stopes at eastern end of 220	95	—	20·30	·68
No. 3	. . . 220	From stopes . . .	98	—	**20·80**	·2
No. 4	. . (*d*) .	From a cross cut; men resting themselves	—	—	**20·62**	·25
No. 1	. . . 40	. . . . .	—	2	—	—
No. 2	. . . .	. . . . .	—	—	—	—
	Analyses not included in the Average.[1]					
1	Parr Consols 80	. . . . .	70	—	20·44	·42
2	„ 100	Cross cut . . .	74	—	19·90	1·18
3	„ 150	Stopes	77	—	**20·60**	·27
4	„ 180	End	84	—	**20·75**	·17
5	Fowey Consols 180	N. of union shaft . .	72	—	19·71	1·23

(*a*) A smell somewhat like candles burning—far from pure air. (*b*) A smell as of burning oils. (*c*) A smell as of burning oil; water running at the rate of two gallons per minute, 120° F. (*d*) Men resting; 17 men in a high state of perspiration, and 7 candles burning.

[1] The carbonic acid is given in old style in a few cases. These numbers were got merely by subtracting the oxygen from 21. They are, therefore, only approximative; but, as a rule, they are a trifle too small, Those, for example, with 21 of oxygen are put as having no carbonic acid, whereas they must have ·03 to ·04 per cent. from the atmosphere.

No. or Mark on Sample.	Name of Mine, and Depth from surface in fathoms.	Description of Place where taken, and time when taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
			°			
6	Fowey Consols 200	Stopes	74	—	20·96	·04
7	„ 220	End	84	—	20·22	·67
8	„ 270	Stopes	81	—	19·82	1·09
9	„ 270	Stopes on south lode .	82	—	**20·24**	·69
10	„ 280	End	86	—	19·70	1·19
		GERMAN MINES.				
G 1	Der alte Elizabeth, Freiberg .	End	—	—	19·80	1·08
2	„ .	Ditto, another end . .	—	—	20·48	·39
3	Mordgrube, near Freiberg . .	. . . . .	—	—	18·93	2·03
4	„ .	A stope	—	—	18·52	2·42
5	Samson Mine, Andreasberg .	End of long level . .	—	—	20·39	·49
6	„ .	Stope	—	—	20·51	·45
7	Elizabeth Mine, Clausthal (*a*) .	Top of a rise . . .	—	—	**20·65**	·23
8	Samson, Andreasberg . . .	End of a long level . .	—	—	20·90	·15
9	„ .	Ditto, farther from shaft .	—	—	20·25	·73
10	„ .	Large wide space . .	—	—	**20·78**	·10

COAL MINES.

No. or Mark on Sample.	Name of Coal Mine, and Depth from surface in yards.	Amount of Air, and place where taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
			°			
1	Cowper Colliery (*b*) 200	65,000 cubic ft. per. min., workings 12 ft. by 7; 100 yards from the downcast shaft	52	100	**20·85**	·06
2	. . (*c*) .	1,000 yards from the downcast shaft	—	—	**20·71**	·44
3	. . . . .	A side working; 2,000 yards from the downcast shaft	65	1 and 1 candle	**20·87**	·13
4	. . . . .	End of workings connected with upcast shaft	—	—	20·57	·39

(*a*) A blowing machine in this mine. (*b*) Current very strong. (*c*) Half-way between downcast shaft and end of workings.

No. or Mark on Sample.	Name of Coal Mine, and Depth from surface in yards.	Amount of Air, and place where taken.	Therm. Fahr.	No. of men working in it.	Oxygen per cent.	Carbonic Acid per cent.
6	North Seaton Colliery (*a*) 248	40,000 to 45,000 cubic ft. per min. of air	65	—	—	—
1	. . . .	100 yards from the downcast shaft	—	—	20·97	·03
2	. . (*b*) .	Average air of mine to all appearance	—	—	**20·71**	·29
3	. . . .	Air from workings after passing over 53 horses and 88 men and boys	—	—	**20·80**	·16
4	. . . .	Air after passing over 250 men and 58 horses, and through 2 miles of workings	—	—	20·44	·42

(*a*) 250 men and 58 horses. (*b*) After passing over 40 men.

Mr. Charles Twite collected nearly every specimen. I began only so as to try the best method.

Summary of the Analyses.

Oxygen, average of the whole 339 specimens . . .	20·26
of ends	20·18
of other parts	20·32
in currents	20·65
in large cavities	20·77
just under shafts	20·424
in sumps	20·14
Carbonic acid	·785

35 or 10·67 per cent., normal or nearly so.
81 or 24·69 per cent., decidedly impure.
212 or 64·63 per cent., exceedingly bad.

Here the division into three classes is made as before explained. Other analyses made after the above 339 give similar proportions.

It is remarkable that the carbonic acid found by the method explained should so nearly approach the amount of oxygen deficient, and also that the difference should be in excess of the oxygen as was anticipated on account of the use of gunpowder, bringing in a disturbing quan-

tity. It turns out as we might expect, that the sumps are the very worst and the ends nearly as bad, and that currents and large cavities have an amount of oxygen equal to that found in a well-ventilated coal mine. The value of the oxygen test is perceived here, and the accuracy of the method of taking the carbonic acid is also confirmed.

I need only observe that if ·1 or ·2 deficiency of oxygen indicate bad ventilation in houses, the state of these mines must exceed all that we who live above ground can comprehend. We enter gradually and cannot well judge; if we leapt from the pure air into a close end in an instant we should recoil with horror.

GENERAL IMPURITIES OF THE AIR OF MINES, AND CALCULATION OF EXTENT.

When a mine opens into the free air, and when there is sufficient light at the entrance, we are apt to suppose, in many cases, that fires are burning within it, because a constant flow of dusty and smoke-like vapour issues in an endless stream. This is the impure air of the mine which escapes with whatever solid matter it can support, the powder and candle smoke mixed with the exhalations of the workmen. When you enter that smoke yourself you are not aware of any impurity—at least after a few minutes are gone; and as the stream is pretty rapid, it may leave even a somewhat cooling and refreshing sensation, if we come from a less pure part of the mine where the air has been warm; this we frequently do when we have gone some distance from the shaft or draught of air.

If we pass from the air of the shafts to the sidings, galleries, or long levels where the air is stagnant, the change is very striking; and when we return to the shaft we are again comparatively refreshed. That this

is really a comparative feeling, and to some extent therefore a delusion of the senses, may be proved, because we experience the same feeling of pleasure in breathing as soon as we leave the shaft itself and enter the pure air; and this is found even on escaping from the largest and best ventilated shafts. Our senses adapt themselves with marvellous rapidity to changes. True, when passing rapidly from one atmosphere to another, they give correct notice of the change, but discrimination appears to them much less useful where there is no choice.

In passing through the long levels, between air currents, or removed from close ends, my small experience did not show that anything occurs to render the air oppressive; the impression given is that of want of freshness. As a rule, the long passages appeared so ventilated that although the air is inferior to that above ground, it is still such as does not strike us with alarm. But it is when we come to the end of the levels that the trial of the air begins, that is, when we leave the direct influence of draughts. It may be, perhaps, looked on as a matter of necessity that most of the borers should work at such ends.

The first mine in which I made experiments was that of Dolcoath. The air which was analysed was taken at 1,320 feet from the surface at a place half a mile from the main shaft at which we entered, but, contrary to anticipation, only 80 fathoms from another shaft, and one by which the air entered as being the lower; the distance from the nearest winze was 40 fathoms. The height of the level was 7 feet 5 in., the broadest part being below, but narrowing above. The air was taken an hour and a half after an explosion for blasting, in which five ounces of gunpowder were used. A blast such as this was made every $2\frac{1}{4}$ hours. The men informed me that after a blast they stay away for about half an hour to let the air clear, but they sometimes, in their anxiety to

know the result, run in, after five minutes, just for a moment, to see if anything remarkable has happened; especially if they have taken the spot for themselves, when they have hopes of a load of treasure. They generally retire to some nook about 20 fathoms from the *blast*. The temperature was 70° Fahrenheit (21·1° Cent.) and in cooler corners 65° (18·3° Cent.)

In South Francis Mine the depth entered was 1,056 feet, at about 100 fathoms from the direct air shaft, and 24 from a winze; this winze, however, giving only air which had previously passed through other levels where the height was 6½ to 7 feet, and the breadth about 3. Five blastings were made in 8 hours, which is a shift, each blast consuming on an average 3¼ ounces of gunpowder. Two men worked together with two candles.

At Botallack Mine the air was taken at about a quarter of a mile out from the shore and the same distance from the shaft. This mine was remarkably dry, whilst the previously-mentioned were remarkably wet. Mr. Couch considered this mine as very well ventilated, but I was not aware of its peculiarities.

It was after viewing the condition in which the men in these mines worked that the following calculations were made. After seeing other mines I am still inclined to look on these figures as fair approaches to an average state of things, or, in other words, a statement of the influences at work in the air at an average of these 'ends,' whilst the analyses show that the calculations represent fairly the state of the worst parts.

The actual condition of the air must of course be obtained from the analyses; there the result is found after all the influences at work have performed their part. The calculations are made for a space represented by 50 feet in length from a 'close end;' if a larger space were assumed the results would correspond to a greater number of cases, but the close ends are the most

important, and for the theoretical part I am not inclined to make a change. We must not forget that explosion, diffusion, and inequalities of temperature and moisture are continually at work to cause variations, and that air which would have had the calculated composition may on account of these influences be found to have fewer impurities. Small as the value of these calculations might be alone, they throw a great light on the condition of the mine, when taken in conjunction with the analyses, to which they are intended specially to act as illustrations.

Amount of Carbonic Acid from Breathing.

Let us take the amount of carbon given out in a day by the lungs at 7·55 ounces :—

This will be equal to carbonic acid—

15·6 cubic feet at 70° F.
Or 844·59 grammes.
441714·6 cub. c.

Let us consider this given out in a space equal to 50 feet in length, 5 feet in height, and 4 feet in breadth : $50 \times 6 \times 4 = 1{,}200$ cubic feet, similar to a close end.

It is equal to 35·3166 cub. mètres.

Let us double the amount of carbon, as there are always at least two men working, and let us divide it by three, giving them eight hours per day :—

15·6 cub. feet × 2 = 31·2 ÷ 3 = 10·4.

This is ·86 per cent. of the 1,200 cub. feet.

Amount of Carbonic Acid from Candles.

During this time there is burnt about half a pound of candles. These may differ, and do differ, in quality, some having more and some less rosin. But we may

suppose them formed of stearine, and if they are not entirely so we shall at least be near enough for the present purpose by supposing it. It is impossible to work out the numberless suppositions that might be made; I must be content with one. Let us suppose the candle grease to contain 114 atoms of carbon, 110 of hydrogen, and 12 of oxygen :—

½ lb. of stearine will contain	2689·89	grains	carbon.
„ „	432·58	„	hydrogen.
„ „	377·53	„	oxygen.
	3500		

These 2689·89 grains of carbon will make an amount of carbonic acid equal to 9862·92 grains. Taking the weight of 100 cubic inches at 60° F. to be 47·339 grains, we have a volume of carbonic acid of 12·044 cubic feet, or at 70° F. of 12·2765 cubic feet.

This, in French measure, is—

Carbon	174·18 grammes
Carbonic acid	638·69 „

Or 347596 cub. centimètres.

The amount on the 1,200 cubic feet is 1·023 per cent. of carbonic acid. Add this to the amount by breathing, and we have carbonic acid—

By breathing . .	·86	per cent.
From candles . .	1·023	„
	1·883	

introduced into the space of 1,200 cubic feet in eight hours by two men and two candles.

Powder Smoke.

During the same period of time as that just mentioned, namely, eight hours, there are used 12 ounces of gunpowder=340 grammes. The composition of gunpowder varies, but one not unusual contains :—

Nitre	74·8 per cent.
Sulphur	13·3 „
Charcoal	11·9 „
	100 „

Or, in equivalent parts—

Nitre	101
Sulphur	16
Carbon	18
	135

These are converted by explosion into—

Carbonic acid	66
Nitrogen	14
Sulphide of kalium	55
	135

The weight, therefore, remains the same after explosion, or in other words the smoke weighs exactly the same as the gunpowder. This is not so with smoke generally, which weighs much more than the wood or coal consumed: but powder burns without air; it supplies its own air in the nitre employed. This powder gives another increase of carbonic acid equal to 2·803 cubic feet, or ·233 per cent.—

making by breathing	·86
candles	1·023
gunpowder	·233
per cent.	2·116

Besides this amount of carbonic acid there are 2,139 grains of sulphide of kalium or potassium, or an equivalent in sulphate of potash, floating in the atmosphere—a portion of the smoke. This is not constant, but exists in great amounts for a short period of time, and in small amounts for a longer time. It is, however, seldom entirely absent. It exists for a while after the explosion in such excess, that sight and breathing become nearly impossible for $\frac{1}{5}$ or 20 per cent. of the time; that

is, above three half hours out of the eight hours. During that time the men are not present above a few minutes, but they breathe the less dense smoke much longer, perhaps some of them constantly.

About one-third of the powder is fired off at a time. There are 713 grains of sulphide of potassium, or its equivalent in sulphate of potash, sent into the air at once, leaving ·59 grains in a cubic foot, or still more of the oxidized salts and other substances, as will be seen in the last table of this chapter. This amount, small as it seems, is intolerable, and the men can neither work nor live in it. They wait till it diminishes. But it is important to remember this amount to be able to understand the enormous influences which are to be ascribed to substances existing in quantities which by our modes of description appear to be extremely minute.

The decomposition attributed in the above calculation to gunpowder is theoretically correct in this sense, that the arrangement of the elements is fitted to bring about such a result as nearly as we can reckon, but the actual result is very complicated. It is extremely probable that no mixture could be made sufficiently intimate to produce this purely theoretical result. We find that on coming from a mine our mouths and nostrils are extremely black; this blackness is much greater in those organs than over the face. The reason is that the current of air entering by the mouth or by the nostrils is strained and purified to a great extent, and charcoal from the powder is one of the substances strained out. The charcoal is not wholly burnt during the explosion of the powder; the sulphur is not wholly as a sulphide of kalium or potassium, and the nitre is not wholly robbed of its oxygen. This sulphide is believed to be converted rapidly into sulphate of potash. Professor Bunsen seems to think that the conversion of part is instantaneous; and as he has examined the combustion of gunpowder

so carefully it will be needful to consider what would be the composition of the air of the 1,200 cubic feet when we have decomposed the gunpowder according to his formula.

Besides these substances mentioned there are 544·4 grains of nitrogen, or 1,784 cub. in. at 70° F. set free from the saltpetre, being ·86 per cent. increase on the nitrogen of the atmosphere in the space alluded to.

The changes in the 1,200 cubic feet will now be more fully understood.

The 1,200 cub. feet of air consisted of—

Oxygen	250·8 cub. feet.
Nitrogen	949·2 „

This will be increased by—

	10·32	carbonic acid from breath.
	12·276	„ „ candles.
	2·796	„ „ gunpowder.
	1·03	nitrogen from gunpowder.
Addition	26·422	

Supposing the oxygen lost and the carbonic acid found in amount to be the same, which for the man is not exactly true, the volume will be diminished by an amount of oxygen equal to the—

Carbonic acid from breath	10·32
and that from candles	12·276
Oxygen united to the hydrogen of the candle . . .	4·929
Or together	27·525

We then have the new state of the air in the 1,200 cubic feet space—

Oxygen	250·8 — 27·525 = 223·275	
Nitrogen	949·2 + 1·03 from gunpowder	950·302
Carbonic acid from breath	10·32	
„ „ candles	12·276	
„ „ gunpowder . . .	2·796	
	25·392	25·392
The 1,200 cubic feet are now		1198·897

A little of the air from the level will now enter to fill up the space left.

I do not here calculate the amount of oxygen burnt by the hydrogen of fatty matters during respiration. Let us put the above into percentage and total amounts:—

Original oxygen in	1,200	cubic feet	250·8	after work .	223·48
Nitrogen	„	„	949·2	„	951·10
Carbonic acid	„	„		„	25·42
					1200·000

The same calculated in volumes per cent.:—

Oxygen . . .	20·9	after work . .	18·62
Nitrogen . . .	79·1	„ . .	79·26
Carbonic acid . .		„ . .	2·12
			100·000

The carbonic acid before work is left out as being too small to affect materially the result of this calculation.

We here see a diminution of 2 per cent. of oxygen, and an increase of rather more than 2 per cent. of carbonic acid; and this may be said to be the rough result, as far as these two gases are concerned with this calculation, and sufficient for all who do not require to go far into details.

If we wish to know with still greater exactness the condition of the air as caused by gunpowder, we must take one of the following analyses. J. Linck gives gunpowder as [1]—

Saltpetre		·7470
Sulphur		·1245
Charcoal	Carbon	·0905
	Hydrogen	·0041
	Oxygen	·0279
Water		·0060
		1·0000

From these result on explosion—

[1] *Jahrbuch der Chemie*, 1858, p. 649.

Solids.

	Grammes.
Sulphate of potash	·2891
Carbonate of potash	·1537
Bisulphide of potassium	·0959
Hyposulphite of „	·0374
Sulphocyanide of „	·0116
Nitrate of „	·0120
Charcoal left	·0183
Sulphur	·0031
Sesquicarbonate of ammonium	·0204

Gases.

	Grammes.	Cubic C.
Carbonic acid	·2239	113·86
Nitrogen	·0952	75·72
Sulphuretted hydrogen	·0238	16·67
Carbonic oxide	·0118	9·45
Hydrogen	·0003	3·56
Oxygen	·0001	·09
	·3551	218·35

Messrs. Bunsen and Schischkoff, in the year 1857 ('Jahrbuch,' p. 626), give—

Gunpowder.

Saltpetre		78·99
Sulphur		9·84
Charcoal	Carbon	7·69
	Hydrogen	·41
	Oxygen	3·07
		100·00

On explosion resulting in :—

Solids.

Sulphate of potash	62·10
Carbonate of potash	18·58
Hyposulphite of potash	4·80
Sulphide of kalium	3·13
Sulphocyanide of kalium	·45
Nitrate of potash	5·47
Carbon	1·07
Sulphur	·20
3/2 Carbonate of ammonia	4·20
	100·00

Gases in Volumes.

Carbonic acid	52·67
Nitrogen	41·12
Carbonic oxide	3·88
Hydrogen	1·21
Sulphuretted hydrogen	·60
Oxygen	·52
Binoxide of nitrogen	·00
	100·00

Or in actual quantities :—

Solids.

Sulphate of potash	·4277
Carbonate of potash	·1264
Hyposulphite of potash	·0327
Sulphide of potassium	·0213
Sulphocyanide of do.	·0030
Nitrate of potash	·0372
Carbon	·0073
Sulphur	·0014
Sesquicarbonate of ammonia	·0287
	·6856
Add gases	·3138
	·9994 from 1 gramme of gunpowder.

Gases by Weight.

	In grammes.	In C.C.
Carbonic acid	·2012	101·71
Nitrogen	·0998	79·40
Carbonic oxide	·0094	7·49
Hydrogen	·0002	2·34
Sulphuretted hydrogen	·0018	1·16
Oxygen	·0014	1·00
	·3138	193·10

Although the result here is much more complicated than by the first calculation, and the proportions differ considerably, it also turns out that the resulting gases and solids which make up the smoke are equal to the weight of the original powder used. This simply means that the amount of nitre has been very carefully calculated. No such accuracy is found in the powder of the miners, or in that which I used for experiments.

The 12 ounces used per day in a close end, or let us say 340 grammes, of gunpowder will give—

As Gases.

	Litres.
Carbonic acid	34·5799
Nitrogen	26·9969
Carbonic oxide	2·5473
Hydrogen	·7944
Sulphuretted hydrogen	·3939
Oxygen	·3414
	65·6538

As Solids.

	Grammes.
Sulphate of potash	144·710
Carbonate of potash	43·311
Hyposulphite of potash	11·189
Sulphide of potassium	7·296
Sulphocyanide of potassium	1·049
Nitrate of potassium	12·751
Carbon, charcoal	2·494
Sulphur	·466
3/2 Carbonate of ammonia	9·790
	233·106

Taking 1,200 cubic feet, or 33,978 litres, we have—

	Litres.
Carbonic acid from breathing	251·0592
" half a lb. of candles	324·7711
	575·8303
" from the gunpowder	34·5799
	610·4102

The nitrogen from the gunpowder must also be added to the total nitrogen; and when this and other operations are performed we have—

	Litres.
Oxygen	6,326·7036
Nitrogen	26,903·5949
Carbonic acid	610·4102
Carbonic oxide	2·5474
Hydrogen	·7944
Sulph. hydr.	·3939
	33,844·4444

By weight in actual amounts—

	Grammes.
Oxygen	9,048·957
Nitrogen	33,726·7
Carbonic acid	1,200·44
Carbonic oxide	3·18800
Hydrogen	·07186
Sulph. hydr.	·59869
	43,979·94855

By weight the numbers would be in per cents.—

For Pure Air.	
Oxygen	23·1
Nitrogen	76·9
	100·0

For the Enclosed Air.	
Oxygen	20·575
Nitrogen	76·686
Carbonic acid	2·72958
Carbonic oxide	·00725
Hydrogen	·00016
Sulph. hydr.	·00136
	99·99935

This will give a percentage constitution of the atmosphere in the enclosure as follows :—

	By Volume.
Oxygen	18·6935
Nitrogen	79·4920
Carbonic acid	1·8036
Carbonic oxide	·0075
Hydrogen	·0023
Sulphuretted hydrogen	·0011
	100·0001

If we put together all the substances thrown into the air of the 1,200 cubic feet space, we have as air and impurities together—

	Grammes.	Grains.
Oxygen	9048·957	139737·7945
Nitrogen	33726·7	520470·4
Carbonic acid	1200·44	18525·2
Carbonic oxide	3·188	49·2
Hydrogen	·07186	1·1088
Sulph. hydr.	·59869	9·23898
Sulphate of potash	144·760	2233·936
Carbonate of potash	43·311	668·375
Hyposulphite of potash	11·189	172·668

	Grammes.	Grains.
Sulphide of potassium .	7·296	112·592
Sulphocyanide of potassium	1·049	16·186
Nitrate of potassium . .	12·751	196·773
Carbon	2·494	38·482
Sulphur	·466	7·191
3/2 Carbonate of ammonia .	9·790	151·079
Organic matter . . .	—	—
Sand	—	—
Sulphurous acid, or sulphite of ammonia . . .	—	—
Arsenious acid . . .	—	—

All these substances actually are breathed by the miners. All except carbonic oxide, hydrogen, sulphocyanide, and arsenic have been actually found in the air. These four have been proved to enter it, and would be found if sought. We know assuredly that they exist, and search is therefore voluntary.

Every gas, with the exception of the first two, is injurious.

Every cubic foot contains—

	Grammes.	Grains.
O	7·540797	114·802599
N	28·105583	433·8182
CO_2	1·036666	16·0086
CO	·002656	·041015
H	·000059	·000911
HS	·000498	·007690
$KOSO_3$	·120633	1·862865
$KOCO_2$	·036092	·557316
KOS_2O_2	·009325	·144000
KS	·006080	·093897
KCy_2S_2	·000874	·013476
$KONO_5$	·010625	·164075
C	·002078	·032089
S	·000388	·005989
$2NH_4O3CO_2$ . . .	·008158	·125779
Solids alone . . .	·194260	2·999486

The first visit to the mines in Cornwall suggested that the probable condition of the air would be obtained by these calculations; fatiguing as they may be to read, and

still more so to find, they approach so nearly the exact truth in the case of those ingredients which can be ascertained by experiment, that I believe they may be taken as actually representing the true condition of the air, with reference also to those substances which are not determined by analyses. Here are the impurities of the air; how far are they removed by the means already in use? If the specimens have been taken from proper places they will show how much purification is due to such influences as position and natural changes or the use of mechanism.

There are some of the ingredients of the air at the end of the table not given in their quantity. The organic matter has been proved to exist, but its actual amount may be variously stated, and will be examined separately.

The sand is meant to include the fine particles of the rocks and the ores which are driven about as dust. The sulphurous acid and the arsenic are both in minute quantities, and both probably have little influence. The arsenic, however, cannot be considered as entirely without effect. The pyrites of Cornwall contains a large amount of arsenic, and when struck with a hammer or pick the smell is very strong. It is not a smell which a chemist only may trace, but one well known and used by the men as a test of the materials. This is a circumstance which may signify good or evil to the miners, most probably evil, but it cannot be entirely avoided, and is counteracted only by draughts of air. To this long list of impurities must now be added the products of distillation of tar and of canvas from the burning fuses. These are in amounts sufficient to affect the senses, and are of themselves extremely complex.

SOLID IMPURITIES IN THE AIR OF MINES.

It has already been said that the air issuing from a mine is not an invisible fluid, but contains solid material enough to make itself distinctly perceived, if not in all cases, still in cases of even well-ventilated mines; using the expression well ventilated in its ordinary acceptation, namely, compared with other mines. When I first observed this, it seemed to me that the sides of the shaft would be continually receiving accumulations of solid matter from these floating particles, and some of the surface of the wood was taken home for examination. But when it was considered how many solid particles in the form of stones and mud are continually moving up and down the shaft, it appeared, on the other hand, not at all probable that any conclusion could be drawn regarding those which were only to be caught by the sides out of the moving air. It was needful, then, to seek another method. In order to obtain floating particles in the air various methods have been used by Pasteur and others. Dr. R. D. Thomson used a screen, and others have used liquids. But an easier method, for a portion of this enquiry at least, presented itself, when Mr. P. H. Holland, on looking at one of the tubes which had been filled with air from an end in South Francis Mine, asked why it glittered so much. A careful examination of every tube showed that the floating crystallized bodies of the mine air had deposited themselves on the glass.

The microscope showed that the solid products of the combustion of gunpowder are to be recognised in the specimens of air, and that there is a considerable variety in the form of crystal. In some cases the crystals of saltpetre may be traced, in most a sulphate of potash. The internal space of the tubes is only from two to three cubic inches, and the substances occurring in smallest

amounts will not be readily detected. The percentage of sulphide of potassium in powder smoke is 3·13, of the solids, and of sulphide of hydrogen ·6; whether from the quantity being so small or from the rapid oxidation of these sulphides, they have not yet been traced with any certainty in the air collected for analysis. In nearly all cases the tubes were examined by the microscope before they were broken open.

The drawings of the substances speak for themselves; the power used for magnifying was 85 diameters, a higher power being found unmanageable on account of the thickness of the glass.

Besides the crystalline bodies there are substances transparent but shapeless, some looking like pieces of quartz, some perhaps looking more like glass.

There are also opaque substances, not very dense but shapeless and small, although at times one is found much larger than the others. I suppose these constitute the dust or broken-down portions of the rocks. Sometimes portions are blacker than others; these are the charcoal of the gunpowder.

Again, there are occasionally filaments of evident organic origin; these also are to be counted amongst the numerous substances that make up that complicated material so vaguely called dust. Portions of our own selves are to be found in it no doubt, and portions probably of everything existing around us.

Having found, then, that of a certainty numerous soluble and insoluble substances and bodies of various shape and texture are to be found in that air which the miners breathe, we naturally enquire what is the effect on the lungs. As this question belongs properly to a medical man only, I shall not presume to enter fully upon it, but it is certainly my part to prepare the evidence, and in fact to say as much as I have the power of learning.

One complaint I have made against the air, viz. that

it is visible, when there is a layer of as little as one foot in thickness. The purest air is probably itself visible in the thickness of many miles. The atmosphere of all our towns is visible in the thickness of about half a mile, but there are portions of it readily seen in the small space of workshops, and even in those where numerous strong men are employed, who have not been found to suffer in an equal degree with the miners. In all these cases it may be said that there is a reservoir of purer air at the door, which may be considered equal to the existence of a shaft within a few feet. If such a convenience existed at every working spot of a mine, this enquiry would have been less necessary.

In those workshops in towns the most apparent impurity is that of smoke and the ashes of coals. It will be well to compare this with the substances found as solids in the air of mines.

Analyses of the Ashes of Coals (*Watts's Dictionary of Chemistry*).

	Newcastle Coal after deducting Sulphuric Acid.	Average of 5 Welsh Coals.	Average of 5 Scotch Coals.
Silica	62·44	42·67	49·63
Alumina	31·22	—	—
Sesquioxide of iron and alumina	—	43·56	38·21
Sesquioxide of iron	2·26	—	—
Lime	·75	6·65	3·18
Magnesia	·85	1·08	1·41
Potash	2·48	—	—
Soda	—	—	—
Sulphuric acid	—	4·46	6·26
Phosphoric acid	—	·66	1·03
Percentage of ash	1·36	8·15	—

Here the ashes are seen at once to differ very far from gunpowder ash, and the smoke, as is well known, differs far from gunpowder smoke. Let us consider the differences.

The coal dust consists almost entirely of insoluble salts; the gunpowder entirely of soluble.

The coal dust is shapeless; the crystals of powder smoke and dust from explosions sharp.

The coal is oxidized by a great current of air, and sulphides are therefore forbidden. The gunpowder smoke contains sulphides. If a few grains of gunpowder are exploded in a large vessel moistened on the side by acetate of lead, the whole vessel becomes black instantly. There is sulphuretted hydrogen in small quantities, but there is sulphide of potassium in comparatively large quantities, and that salt it is which gives the peculiar odour to powder smoke, in my opinion. I cannot but believe that the action on the lungs of this salt must be equal to that of sulphide of hydrogen, although from acting more slowly, perhaps the effect is more easily overcome. The slow action is a great advantage; the body has time to renew frequently the attack against its enemy; but although there may be power to resist actual death, the effect of slow action may be, and most probably is, to distribute over a long period of time that death which might ensue instantly, so that, if a chemist may be allowed to use the expression, the effect is dissolved in health and becomes disease. Probably the sulphide will deoxidize the blood and act in other ways not yet known. A powerful agent in the lungs it must be.

People who love the smell of gunpowder may be surprised at this, but we need not go to the soldier or the sportsman for examples either on one side or the other. The amount of gunpowder which they inhale is a mere trifle compared with that taken into the lungs of the miners. The soldier may have as much in battles, but these are the rare occasions of his life. So far as air forms the condition of existence a miner lives on a battle-field his whole life; he has the smoke, the dust, and the heat. Read the description of the air of a battle-field

and you have already that of a mine. The sportsman with the fine air around him has at the most only a short whiff of the powder smoke.

Let us endeavour to find how much really is inhaled. Let us say there are taken into the lungs in a minute 320 cubic inches of air, an amount considerably below Vierordt's estimate. We have, then, in 10 hours, which is longer than the men must be in the mine air, 111 cubic feet. Let us say that 30 of these cubic feet contain the full amount of solid matter estimated, or, in other words, that the 111 feet breathed contain an amount equal to the full dose contained in 30 feet. We have then inhaled in one foot the amount of—

·19426 grammes × 30 = 5·8278 in 30 feet.
Or in grains
89·9145 in 30 feet.

We cannot expect that a man will inhale so much, but it is at times possible, and to this fully he is at least liable to be exposed during a part of the day. A full dose of gun-powder smoke exists in the air only a portion of the time.

A few drawings are given of the substances found in air, as they were found attached to the sides of the tubes. An investigation into the small bodies attached to surfaces offers to give much that is new. In this case it is not intended to go into details. The result of the general analysis of dust from the tubes has been given. The crystals on No. 5 must, however, be mentioned separately. They were found even on tubes perfectly fresh from the glass-blower's, and always at the ends. The action of the heat on the glass has produced some changes, diminishing perhaps its soda; and at any rate either causing an efflorescence or a general deterioration. As a rule the greater number of all the crystals was found near the extremities of the tubes; I suppose these places were quieter and less exposed to currents which would exist even in the small space contained in a tube.

Dust and Crystals collected from the Air of Mines.

No. 1.—Dust collected on the sides of a tube after gunpowder had been exploded in it.

No. 2.—Dust collected from the air of the lead chamber after explosion of gunpowder in it.

No. 3.—Specimen of an unusually large piece of saltpetre found in the air after explosion.

No. 4.—Dust collected on all objects in the chamber after an explosion of gunpowder.

No. 5.—Figures at the end of the glass tubes after much heating.

No. 6.—Figures on the tubes along with crystals from the air of a mine after an explosion of gunpowder. V V Barberry.

No. 7.—From H H H Dufton (Silverlead), Yorkshire.

No. 8.—N N Arkendale, Fregill, No. 2, immediately after an explosion.

No. 9.—V V Barberry, after an explosion.

No. 10.—V V Barberry, after explosion.

No. 11.—28 D 28.

No. 12.—South Francis Mine, 11th September, 1862, 1,140 feet.

No. 13.—South Francis Mine at surface, 9th September, 1862.

No. 14.—South Francis Mine, 1,140 feet, 10th September, 1862.

No. 15.—T T Bunting.

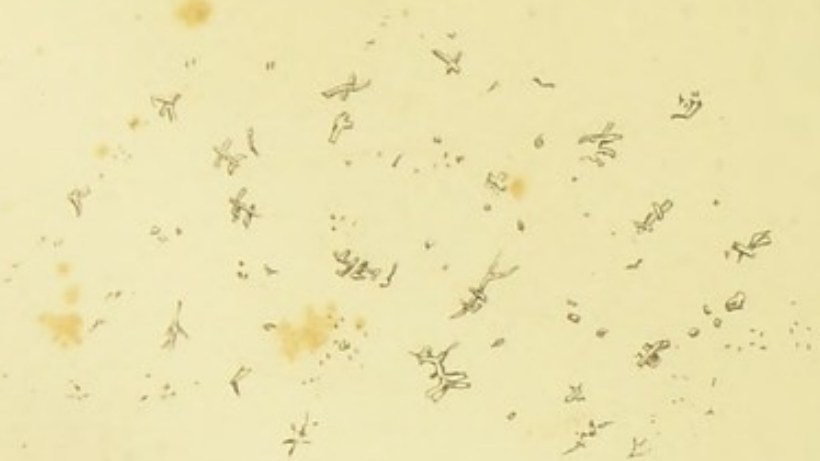
As examples 1, 5, 7, and 10 are copied from the original papers.

No. 1 gunpowder specimen. This was taken by drawing the air as usual into a tube and sealing it When examined by the microscope, and even by the naked eye, crystals of saltpetre were distinctly seen. There was clear matter like a crystallised salt, but with no regular form, and in conjunction with black spots. The tube, which before was clear, had a fine dust over it, distinctly

showing that even the formless matter observed was brought into the tubes at the time of the explosion.

When examined more narrowly (and such results compel us to this fuller examination) we find that the diminution of oxygen in the air of a mine has no exact connexion with the solids contained in the air. Although both must frequently be found together, they are independent defects. For example, it happens that men work long in boring holes for blasts, but making no blast until several are ready. In this case the air deteriorates, but there can be no powder crystals. Next it happens that the powder smoke rolls out upon the purest air of the mine; it does not necessarily carry a diminished amount of oxygen along with it. The solid particles separate from the gaseous. In reality there must be cases where an explosion will purify a portion of the atmosphere, whilst it deteriorates another, and be the most violent agent for evil in the air of mines. For example, it will drive out air from a level at a long distance from the explosion, and other air will enter as the heated gas cools. To some extent this will be of service, but it is one of those things which serve to complicate the conditions to be considered.

In a recess of small extent, an explosion will drive out all the existing air, substituting air with powder smoke in it from another place. This being very different from the atmosphere, will rapidly change by its own action, and fresh air will come in. This increased rate of change would go on no further than the point of impurity in the air before the explosion; but the organic matter and the excessive moisture would be removed. If the carbonic acid were washed out there would be a very fair purification, although not a perfect one. When the cavity is long the same result will not take place. The impure air of the end will be driven forward, it is true, but after the explosion most of it will come back mixed.



Dufton (Silver lead) Yorkshire.

Thermometer 49. Barometer 29. 94



Barberry.

Thermometer 57. Barometer 28. 6.



From Gunpowder (coarse.)



West Burton. Yorkshire.

Thermometer 59. Barometer 28.58.

There will be various modifications of the constitution of the air, and there will be a very gradual entrance of purer air. If in this case the carbonic acid and solid matter were washed out there might remain a deficiency of oxygen, an atmosphere not desirable to breathe. It is interesting to know if this deficiency of oxygen along with purity in other respects would be perceptible, allowing that it did not go beyond one per cent. or perhaps two. This was determined by an experiment in the lead chamber to be spoken of. Meantime it may be said that the air will certainly be better without the carbonic acid and solid matter than with it, so that something at least would be gained. This leads to an enquiry as to the advantage of washing the air.

It is not my intention to say much on the effects of breathing so much solid matter. To judge from my own experience, it must be very hurtful. I must leave medical men to describe the effects, but will here remark that twelve hours after breathing powder smoke to an unpleasant amount, phlegm with taste of nitre appeared after a slight cough, entirely different from all previous experience.

AIR OF CONFINED PLACES.

Enquiry for the Mines Commission—continued.

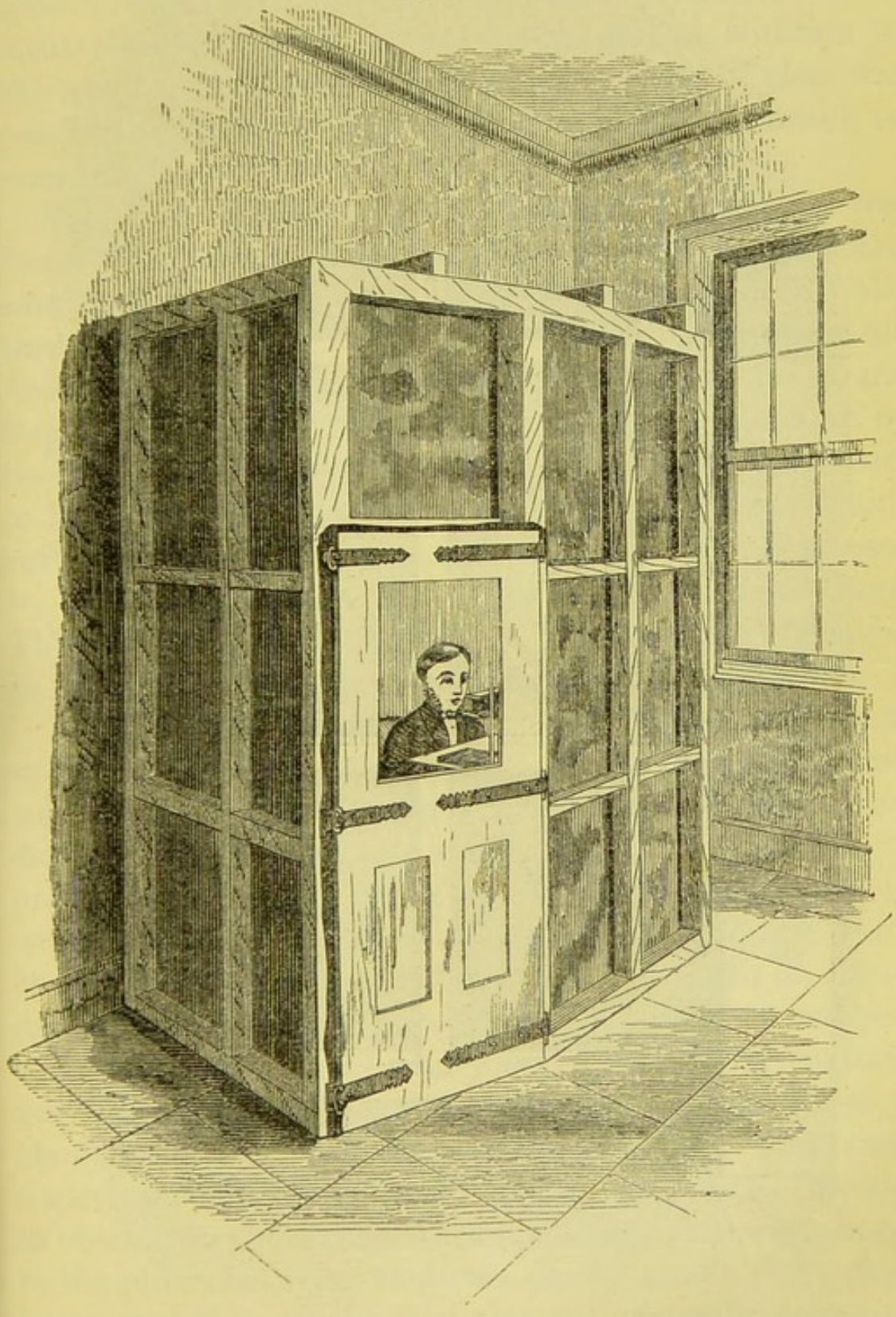
The Lead Chamber.

It was found necessary to enquire into the state of the air after it had been rendered impure by candles alone, as well as by breathing alone, and by gunpowder. Besides the bulky impurities, there were the small amounts of organic substances from tallow, from tobacco, from the human skin and from putrefaction; all these mingled in such a manner as to defy separation, but nevertheless were present, causing unmistakeable unpleasantness, and in some instances a positive feeling of disgust, even when a small portion only of a cubic inch of the air was inhaled.

In order to obtain some idea of the effect of the principal agents separately, it was necessary to make the experiments not in the mine but in the laboratory. When examined before the Commission I mentioned my intention of making experiments in a close chamber, of which I expected to have received the use for a short time, but when it was considered that experiments of the kind alluded to can never be performed by a stroke, that they are in reality never really finished, but at best are only made less and less unsatisfactory, according to the frequency of repetition, it seemed essential that a close room should be procured to be at hand for ready use. Accordingly a chamber was made of lead, 6 feet long, nearly 4 feet unequally broad, and 8 feet high, the cubic contents about 170 feet —it is unnecessary to describe the irregularities of the shape, partly caused by the door, the windows, and the position. The whole lead of this chamber was made into one piece by having the edges melted together by the hydrogen blowpipe, as is now done in making chambers for sulphuric acid. As it was important that no one should be left in the place without having the power of

escape, it was determined that there should be sufficiently large windows which in any emergency could be broken through; were it otherwise, the mere idea of being confined and at the mercy of the treacherous memory of

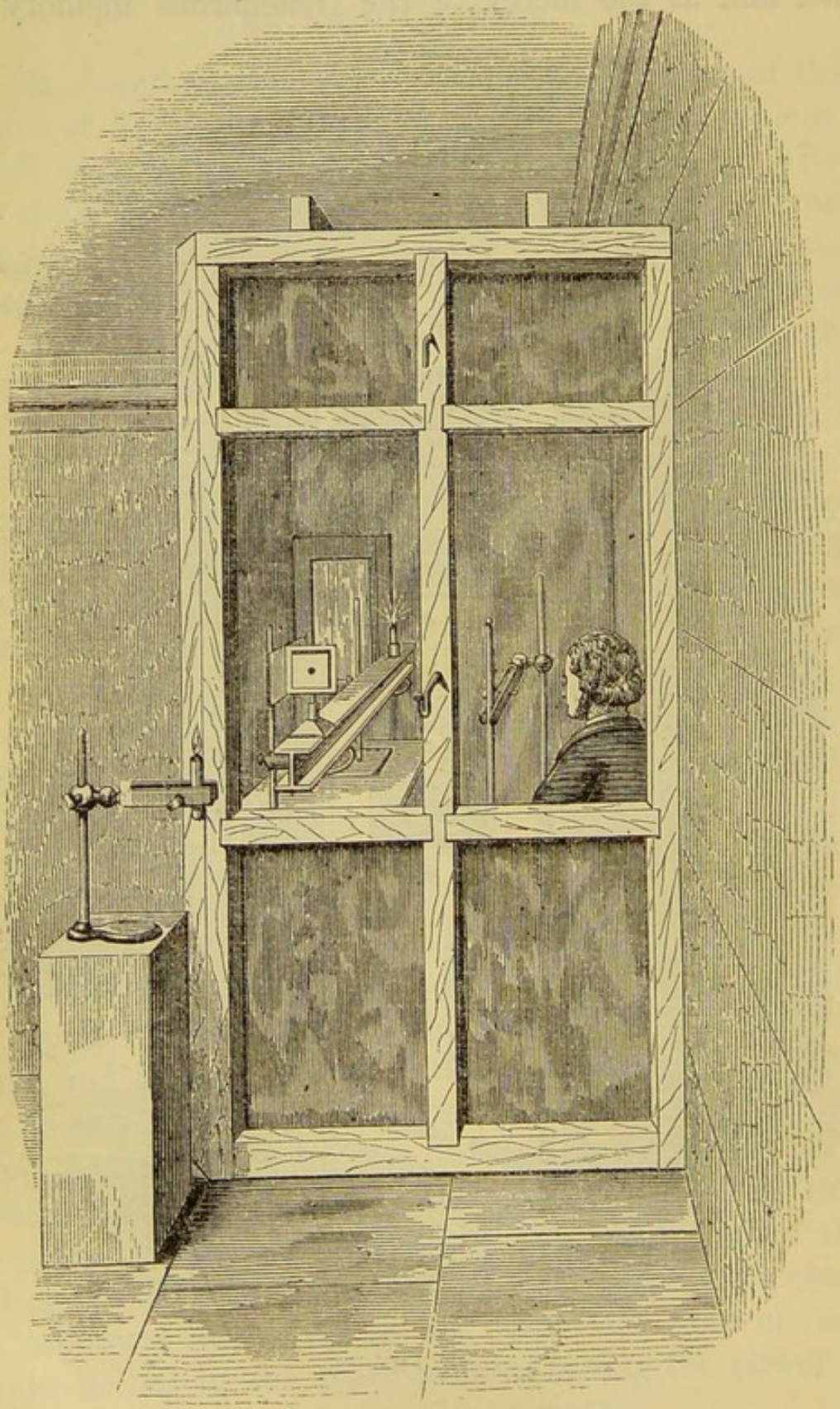
Fig. 3.



Lead Chamber, used for experiment

human beings would be enough to cause much discomfort and render the experiments void. The half of one end was

Fig. 4.



Lead Chamber—end view.

made of glass, chiefly looking to the window, sufficiently large, of the apartment in which this chamber was placed. This apartment had been a scullery, and had a strong floor sufficient to bear the weight of the lead, which was considerable. The room was lighted so well that experiments could be made in it readily, and in addition to the windows at the end, another large pane of glass was put into the door, so that at each end any one could be seen, and his wants instantly attended to.

The door was of lead except where of glass, and was hung on heavy pivots; strong bars crossed it, projecting over the sides. In these projections there were holes through which iron pins on the lintels passed when the door was shut. These pins had screws upon them, and large nuts were used to bolt them down. The door by itself did not fit very closely; it was made so in order to leave room for a tube of vulcanised caoutchouc, which surrounded the lintels; against this the door was pressed by the force of the screws, of which there were three. This arrangement suited exceeding well from beginning to end, and no change of the plan was necessary.

It was not needful that the chamber should be able to sustain a great pressure of air. All the experiments were to be made in a short time, and if minute crevices did exist through which a small amount of air might be pressed, it would not be of consequence, as that process would require much time to affect the air of the whole chamber. When the chamber air was analysed on Saturday, and again on Monday, the difference was not perceived. As there was nearly two per cent. of carbonic acid in it at the time, this will be considered proof of abundant accuracy in the workmanship. In some cases a specimen of air was drawn out for analysis. In order to obtain this from above and below, holes were bored in the pillar between the two panes at the end; these holes went through the lead also, and generally contained

glass tubes, which were stopped by having caoutchouc tubes fixed on them, which tubes again were closed by glass rods. These holes were the weak points of the chamber with respect to air-tightness; when powder was fired a slight whistling sound was heard, which showed that the joinings would not stand much pressure. It was not easy to keep the joining of the lead and wood complete, as the tubes on being moved caused a disturbance of the luting.

In this room was a small table and a chair, and when these and one person were present there were 170 cubic feet of air. Of course this cannot be given to a few inches, and even if it were a foot wrong, which is far from the case, the result would not change the analyses to any important extent. For example, when there are two per cent. of carbonic acid with 170 feet, there are altogether 3·4 cubic feet of carbonic acid, and if there are 171 feet there are 3·42 of carbonic acid; ½ per cent. will be 0·850 in one case and 0·855 in the other. Sometimes there was a person in the chamber, sometimes not; this would make a difference of above two cubic feet; even one cubic foot would require to be accounted for if total quantities were measured, but the experiments as conducted are little liable to be disturbed by this cause. This would not be the case with all experiments.

Bad Air and the Sensations.

Here I am describing feelings, and to some persons they may simply be fancies, but I shall describe them nevertheless, as I believe man has learnt nearly all he knows of ventilation by attention to these feelings, whilst chemical analysis is attempting to struggle after him, and is continually finding itself behind in the race. The feelings are uncertain, it is said. This is not quite correct; they are to us most certain; but they register so many

phenomena at once that they become uncertain guides when one only is sought after. For example, we may imagine that they tell of increased carbonic acid, whereas they may simply be telling us of diminished digestion and vigour. As to personal qualifications for such experiments, I may merely say that I am not peculiarly sensitive to bad air; many of my friends are more so; but having my mind more directed to the subject I obtain a certain advantage. Perhaps my love of fresh air is beyond the average, as I have an actual pleasure in east wind. If these two conditions are contradictory it certainly is not my fault; but they are not so; experiments in the lead chamber showed that even when nothing unpleasant was perceived, the pleasure of coming out to the air of the laboratory was like that which we have on the sea-shore or the mountains, so that the love of pure air is not in exact proportion to our incapacity to endure bad air. Again, I may say that all my fancies lead me to imagine I can bear anything that others can in the way of air, and although this may not be true, it entirely kept away all imaginings of evil during these experiments, and led me to points where actual and long-continued discomfort was the result. I think it important to mention this entire freedom from any illness or tendency to illness caused by the imagination.

The first trial of the chamber was made by simply sitting down for an hour and forty minutes. This produces about one per cent. of carbonic acid. The day was clear and the air pleasant; the temperature 45° F. No difference was, to a certainty, perceptible for twenty-five minutes. Then when the air was drawn from the top by means of an umbrella, it seemed like a soft wind, and had to some extent a pleasant feeling, but was entirely devoid of a faculty of cheering.

A dull, cheerless air is well known. Here we had it produced at once. The air was very moist, and deposited

water when drawn out through a tube on taking a specimen.

After an hour the unpleasant smell of organic matter, such as is so well known in a crowded school, was perceptible on stepping rapidly from one end to the other or on moving the air rapidly. Here we learn that when a current of air blows on us, the chemical actions accumulate, and although if continued for one instant only they may be imperceptible, if repeated for many they culminate in a sensation. A sensation, such as smell or taste, is evidently the result of a chemical action. This may be infinitesimally small; in that case the mind does not take cognizance of it; but if many of these small actions occur, either all at once, or so rapidly one after another that they cannot be separated, the mind observes them, and a sensation is the result. How many atoms or molecules must be moved to cause a sensation it is not possible for us to tell, but we can very readily tell that for some persons one, two, or twenty times more must be moved than for others. These chemical actions are constantly going on in the body. They may take ten years to gather impetus to make their movements strong enough to produce sensations, and disease may be generated unknown to the individual, although it may kill him in the eleventh year. But if the chemical action began at the first so violently as to produce decided sensations, he might be able to avoid it at once before it produced any abiding impression. For this reason, a bad climate is more dangerous than the fumes of vitriol, when we are at all able to move out of the way. Concerning the climate, we reason from very distant premises frequently, as no smell is perceived, but the vitriol explains its character at once, and tells its intentions.

Exactly in the same category as the bad climate are all places in which the air is inferior, without containing any peculiarity which the senses can detect. Now although

the mine air is all or nearly all such that the senses could perceive it at once, if we were introduced into it at once, we enter so gradually that we are scarcely conscious of the increasing deterioration. As it takes a long time to enter, and a long time to come out, we are incapable of comparing the air of mines by the use of our senses in the same way that we can compare the air of any place above ground, out of which we can come suddenly. This being the case, we are obliged to pay more attention to those phenomena that are only perceptible to the senses in their results after years, and also to chemical experiment, which two methods after all must turn out our most certain guides.

It was very decidedly perceived, after remaining an hour, that the air was soft when made to move in this chamber. This arose from the moisture, and shows us at least that a soft air may be a very impure one. Soft air, air with a good deal of vapour, is very soothing; it calms the mind and the body and the burning of a candle or a fire. In this state it cannot be very cold, as the warmth is essential to the existence of the vapour. This air has a tendency to leave the skin and its action unchanged; it causes little evaporation, and perhaps an influence is due to this, that the amount of oxygen introduced into the lungs is diminished, whilst no injurious ingredient is added. I think I hear the question, Will not the air in the lungs decide for itself at once how much vapour there shall be, as there is such an abundant moist surface? The entrance to the lungs, that is, the nostrils and the mouth, feel the moisture with great clearness, and when the air is dry they are dried up. But the lungs seem to feel it also, and it seems a very common thing to know the difference in the respiration. Dry air stimulates the skin, because it removes moisture, and the skin must set to work to renew it. Dry air, therefore, would in this respect be in its first action cheer-

ing, and in its last irritating. Moist air would from this point of view be calming in its action, and often at once calming to languor, probably preservative of the vital powers which are not frittered away by constant irritations. I speak only as a chemist.

After staying in the chamber for 100 minutes, the air had an unpleasant flavour or smell, and I came out; three persons entered at once and pronounced it very bad; I entered after a minute and found it extremely bad. It seemed to me, however, that we are frequently exposed to air equally bad, although I have not found any in daily life so much deprived of its oxygen as this must have been, reduced, that is, to twenty per cent.

I was extremely glad of the escape from this impure air; this gladness not arising from any previous discomfort. I was not uncomfortable. I chose that time of coming out, as it was the moment when the organic matter was most distinctly perceptible; still to perceive it when quiet required attention. The pleasure on coming out was one wholly unexpected; although I now recognise it as exactly that which one has when walking home on a fine evening after leaving a room which has been crowded—it was the reassertion of the rights of oxidation; the blood was evidently in active change desirous to take up a position that was lost, else why was this feeling of unusual delight in the mere act of breathing, which feeling continued for four hours? Dinner seems to have first removed it. From the long time required to bring the functions of breathing to their former state, we may of course argue that they had been much disturbed. If to this it is replied, that after all it was merely the memory of the chamber still remaining, it may be said that if the mind is fixed upon the subject, such a memory may exist, without, as far as we know, any equal corresponding prior affection of the body; but if the condition be unexpected and unthought of, the

result seems to indicate distinct chemical or physiological action. In about four hours the lungs recovered their tone. By the tone is meant their unconscious working. Food seemed to be more than usually required, and was followed with unusual rapidity by the feeling of refreshment. Now as there was no unusual bodily exertion, the demand could not arise from an unusual wear of the system, and indeed the peculiar feeling was rather a need of support than actual hunger demanding food. We cannot suppose that much wear and tear was going on. We seem left to suppose only that the decompositions which are needful to repair the body had not been made, although material was present, and that it took four hours and dinner to make up lost time. Although oxygen does not directly repair, it takes the initiative in any union of labour for the purpose.

The second stay in the chamber, the day after the above, was continued for 160 minutes. At two hours and twenty minutes it was observed that very long inspirations became frequent and more agreeable than usual. The air about that time gave a very decided feeling of closeness. Standing on a chair it was found less agreeable than below. The amount of oxygen was found to be 19·61.

Immediately on opening the door two or three persons entered, and again perceived how uncomfortable it was.

After the experiment on the combustion of candles (Tables 3 and 4), we entered with candles and a spirit lamp. The lights were soon extinguished, and it was found impossible to rekindle them with matches: wooden matches were used, they refused to ignite. Still we breathed without difficulty at first, but a gradual feeling of discomfort appeared of a kind which is not easily described; it was restlessness and anxiety without pain, whilst the breathing increased in rapidity.

Afterwards gas was lighted, and it burned with brilliancy.

On entering after the gas had gone out candles were extinguished as rapidly and completely as if they had been thrust into water; nevertheless we still breathed, and although every one was anxious to go out, no very correct description of the feelings could be given. I stood on a chair, and then a feeling of incipient fainting began; but the senses were not annoyed by anything beyond a feeling of closeness, by no means so unpleasant as a school-room or close end. This is a very important fact, as it points again to the organic matter, of which there was little here, and of which there is much in the school-room. The lungs seemed to refuse expansion, without the senses being able to indicate a reason. The actual amount of oxygen when the gas went out is not known; but a specimen taken from the room after the door had been opened long enough to allow three persons to enter contained 17·45 per cent.

All these experiments tend to diminish our faith in the senses as guides, under certain conditions. The senses are quite unable to measure degrees of closeness and raise as much alarm at a state which may be represented by 0·1 per cent. of carbonic acid, as they sometimes do when there is nearly 4 per cent. with a diminishing pulse and a quickening respiration, or incipient gasping for breath.

After a while the air really becomes by no means proportionately worse to the feelings, but the approach of fainting in the case mentioned showed that the lack of oxygen or presence of carbonic acid was telling on the moving vital act.

It is proved that badly ventilated rooms containing less than 20·7 per cent. of oxygen are very unwholesome. The present enquiry is believed to establish this point without affirming that the oxygen is much more than the index; and the late sanitary movements have really been operating on such a condition of things, however unexpressed. I say this because such an amount

of oxygen represents what would be bad ventilation in the worst workshops. Now there is a faintness produced when the oxygen is at 17·4, but is there a corresponding injury when the numbers range between these? and if a man lives where a candle will not, is it a proof that he is little affected?

It is not possible to obtain exact results with the human being at all these varying stages without going into the region of statistics, and the argument may be considered by some as dependent on that wider range of facts; but for most persons it will appear clear that if the loss of 0·2 of oxygen and its replacement by 0·2 of carbonic acid, be found to make the air disagreeable and injurious to the extent which is attributed by overwhelming evidence to bad ventilation, double the loss of oxygen and double the carbonic acid will produce still worse consequences. The senses begin the argument, but it takes months or years to show the bad effects of that air which they dislike. At the other extremity the vital powers give way, and between these two we must have a gradation of effect.

We cannot and dare not experiment on human beings beyond near limits; but one experiment made with two per cent. of carbonic acid without intention shows that the amount which persons can bear differs with the strength. A young lady was anxious to be in the chamber when the candles went out. She was extremely fond of pure air, but when she entered was not much struck by the impurity of that in the chamber, although the candles were threatening to go out, so that there could not be quite 19 per cent. of oxygen with 2·1 of carbonic acid. No one had been breathing in it, so that the organic matter from the person was absent. She stood five minutes perfectly well and making light of the difficulty, but suddenly became white and could not come out without help. She was remarkably healthy, never

was ill, and was troubled with no fear of the air in which she stood. Several of the specimens of air from the mines had almost the same amount of oxygen as the air on this occasion, and some had less. This is one of the gradations between 21 and 17·4 per cent., the two measured extremes in which breathing was tried.

On another occasion a still greater amount of carbonic acid was present in the chamber, but it was not accompanied with a corresponding loss of oxygen, as the gas was driven in upon pure air. The oxygen, therefore, was 20·19 with 3·84 of carbonic acid. On this occasion Dr. Reissig and Mr. Higgins got headaches instantaneously on entering, and were unable to stay above seven or eight minutes. I stayed about twenty minutes, still felt very anxious to get out, as all my movements were made with great haste, and both mind and body betrayed symptoms of feverish activity. There was also a rush of blood to the head, the face was flushed, and the lungs acted more rapidly than usual, the inspirations being 26, whilst the average of waking hours is as nearly as possible 20. Similar feelings, but not to quite so great an extent, were observed when the experiment on table 7 was made, carbonic acid being then also poured into the chamber. On that occasion only do I think that the sensations could in any way be heightened by the imagination. At that time I had not the slightest experience of carbonic acid, and it was pouring rapidly into the room, requiring some time to mix it. I had no certainty that the results might not be very bad, and the interest might have been mixed with a little anxiety not to go too far with the experiment. In the case more fully described above there was not that feeling; I knew the circumstances, and was satisfied that the condition of body and mind was caused entirely by physical agents, not by the imagination. There was a burning haste to live, as if life were afraid of being put out.

Sense of Smell—Sensitiveness towards Muriatic Acid.

Muriatic Acid was evaporated in a closed space, and observations taken until the senses could perceive it.

	Muriatic acid.	Acid. Vol. in the chamber.	Acid. Amount in 1 vol. of inspired air.	
1	·1036 grms. 1·6 grs.	·00128	. . . .	No smell.
2	·1625 grms. 2·509 grs.	·00202	·0000086 grms. ·0001328 grs.	Slight smell.
3	·2157 grms. 3·33 grs.	·00269	·0000114 grms. ·000176 grs.	Distinct smell.
4	·25 grms. 3·86 grs.	·0031	·0000133 grms. ·000205 grs.	Strong smell.

For sensitive persons probably two of muriatic acid in 100,000 would be a strong smell, and perhaps even one in 100,000, but three may be taken as distinct for all persons.

Here with 262 cubic cent. or 16 cubic inches of inspiration we perceive the smell strongly when 0·0031 per cent. is present.

A silver solution required 1,000 cubic cent. or 61 inches of this air to show a cloudiness. But in reality we can smell the acid with at least four cubic inches of inspiration, or 65 cubic cent. The accuracy of smell as compared with this method of using the silver test is as 4 to 61 inches, or 262 divided by 4=65 to 1,000 cubic centimètres; in other words, one sniff of the acid is equal to a careful experiment with a litre bottle of 61 cubic inches. We must remember, however, that although the smell seems thoroughly to keep the ascendancy over the chemical experiment, the latter may in its turn entirely overcome the smell, viz. when we use a great excess of the air to be examined. We cannot accumulate sensations so very readily, although it is no doubt true that we can to some extent. For example, a substance may not be offensive at first, but by repetition of the attack on the senses it may become extremely so.

EXPERIMENTS IN THE LEAD CHAMBER.

TABLE No. 1.—*Experiment to ascertain the General Effects on the Feelings and on a Candle.—January* 26, 1864.

Time.	Temp. of Floor. Fahr.	Temp. of Table level. C.	Temp. of Table level. Fahr.	Temp. of Top. Fahr.
h. m.				
12 45	52·5	10·3	50·54	52·5
After 10	53·25	12·2	53·96	54·5
„ 20	53·8	12·4	54·8	55
„ 30	54·7	13·2	55·7	55·25
„ 40	54·9	13·4	56·1	55·7
„ 50	55·2	13·78	56·8	55·9
„ 60	55·5	13·9	57·05	55·25
	Carbonic acid . .		·38	
	Oxygen . . .		20·54	
„ 70	55·6	14	57·2	56·4
„ 80	55·7	14·05	57·29	56·49
„ 90	55·75	14·1	57·47	56·6
„ 100	56	14	57·2	56·6
„ 110	55·8	14	57·2	56·6
„ 130	56·2	14·2	57·56	56·8
„ 140	56·3	14·2	57·56	57
	Carbonic acid . .		1·130	
	Oxygen . . .		19·73	
	It seemed pleasant here to take long breaths.			
„ 150	56·6	14·25	57·65	57·2
	Begins to feel decidedly close.			
„ 155	56·5	14	57·2	57
	Carbonic acid . .		1·26	
	Oxygen . . .		19·61	

TABLE No. 2.—*Four Miner's Candles in the Lead Chamber. No person inside.—January* 27, 1864.

Time.	Below. Temp. Cent.	Below. Temp. Fahr.	At Table. Temp. Cent.	At Table. Temp. Fahr.	Above. Temp. Cent.	Above. Temp. Fahr.
h. m.	°					
1 50	—	53·5	10·4	—	—	51·7
—	Outside	53	—	—	—	—
After 10	12	—	—	59	—	59·5
„ 20	12·7	—	—	63·5	—	—
„ 30	13·5	—	—	65·25	—	—
	Windows dim with moisture; difficult to read the thermometer.					

TABLE No. 2—*continued.*

Time.	Below. Temp. Cent.	Below. Temp. Fahr.	At Table. Temp. Cent.	At Table. Temp. Fahr.	Above. Temp. Cent.	Above. Temp. Fahr.
m.						
After 40	13·5	—	—	65	—	—
„ 45	14·3	—	—	65	—	64
Outside	—	53°	—	—	—	—
	Carbonic acid		1·78 } Top.			
	Oxygen .		19·14 }			
	Carbonic acid		1·81 } Middle.			
	Oxygen .		19·02 }			
	Carbonic acid		1·38 } Middle.			
	Oxygen .		19·31 }			
	Carbonic acid		2·28 } When the candles went out,			
	Oxygen .		18·80 } which was at 7 o'clock.			

TABLE No. 3—*Eight Candles, one Paraffin Lamp, and one Spirit Lamp.—January* 29, 1864.

Experiments to ascertain the differences of Candles and Lamps, and also the effect of difference of position of the Lights.

Time.	Bottom. Temp.	Bottom. Fahr.	Middle. Temp.	Middle. Fahr.	Top. Temp.	Top. Fahr.
	Outside	50	—	—	—	—
h. m.						
12 30	Dry .	53	—	60·25	Dry	76
	Wet .	52	—	—	Wet	51
After 10	Dry .	56	—	—	Dry	70
	Wet .	56	—	66	Wet	66·5
	Appeared already to burn worse.					
„ 15	Dry .	59	—	68	Dry	73
	Wet .	57	—	—	Wet	69
	Carbonic acid	1·13 } Windows dim.				
	Oxygen .	19·87 } Three candles, inclined, burnt badly.				
„ 50	The candle nearest the roof went out.					
	Carbonic acid	2·27 }				
	Oxygen .	18 50 }				
„ 58	Nearly at the same time a candle at the window as high as one on the table; one on the floor near the side; one on the corner of the table, near the chair, and window candles and one on the opposite corner went out.					
„ 75	Another on the table . . went out.					
„ 90	One on the chair . . . „ „					
„ 93	One on the floor . . . „ „					
„ 98	Paraffin lamp . . . „ „					
	Carbonic acid	2·32 }				
	Oxygen .	18·48 }				
„ 150	The spirit lamp . . . „ „					
	Carbonic acid	2·45 }				
	Oxygen .	18·40 }				

HEAT IN CLOSE PLACES.

The heat of a mine is very often increased above that of the ground in which the mine exists. The effects are greater than the theory of internal heat will explain. This has been supposed to arise from the oxidation of pyrites; and miners generally testify to the greater warmth of such spots as contain sulphur compounds, which are readily oxidizable. It is of interest, and perhaps of use, to know how much of this depends on the men and the candles; and a few experiments were tried in the lead chamber. The results are not in reality of much value in themselves, although they may lead to other ideas and enquiries. This part was very incomplete.

The experiments were made by simply remaining in the chamber and observing the temperature, or by placing a paraffin candle in the chamber and observing, and by both together.

The amount of heat can be better calculated when a paraffin candle is used. Messrs. Field, of Lambeth, were good enough to make into candles some of the purest paraffin obtained from my friend Mr. Young. It was taken as containing 20 atoms of hydrogen and 20 of carbon. Of this 7·75 grammes burnt in an hour=6·85 carbon and ·9 hydrogen:—

One of carbon burning into carbonic acid will give 8,080 units of heat, or will raise 8,080 of water one degree C.

One of hydrogen heats 34,462 of water.

6·85 × 8,080 units of heat	55,348
·9 × 34,462 „	31,015
Grammes of water heated a deg. C. . . .	86,363
Or the air in the chamber 150[1] cubic feet, raised	65·8° C.
Or	118·4° F.

[1] This has been calculated for 150 instead of 170.

A man burns carbon variously. It will be seen that (10·27 ounces) 302·4 grammes per day was the rate at which the carbon was burnt in some experiments mentioned, or 12·6 grammes per hour (194 grains).

12·6 × 8,080 = 101,808	
Or the air in the chamber 150 cubic feet heated	77·5° C.
Or	139·6° F.
100 grammes of gunpowder (about 3½ ounces)	
containing	13·3 sulphur,
	11·9 carbon,
would give 13·3 × 2,221	29,538
11·9 × 8,080	96,152
Units of heat	125,961
Or the air of the chamber 150 cubic feet, raised	95·8° Cent.
Or	172·4° Fahr.

If we took 100 grains, and had the same per-centage of sulphur and carbon, we should have 8,138 units of heat.

The actual rise of temperature in the chamber is shown in the following :—

TABLE 17.*—*One Person in the Lead Chamber.*

Time	Temperature outside	Temperature inside, top	Temperature inside, middle	Temperature inside, bottom
m.	Cent.	Fahr.	Fahr.	Fahr.
	5·4	45·	44·	43·5
After 15	5·4	47·	51·	46·5
30	5·6	48·5	52.	48·
45	5·6	49·	52·8	48·5
60	5·8	49·5	53·5	49·2
75	6·	49·6	53·8	49·5
90	6·	49·8	54·	49·5
105	6·	50·2	54·8	50·
120	6·2	50·5	54·8	50·2

* The Nos. of the original tables are given here to prevent confusion.

TABLE 18.—*One Person in the Lead Chamber.*

Time	Temperature outside	Temperature inside, top	Temperature inside, middle	Temperature inside, bottom
m.	Cent.	Fahr.	Fahr.	Fahr.
	5·9	44·8	49·	44·
After 15	6·	47·4	51·	47·
30	6·2	48·5	52·4	48·4
45	6·3	49·4	53·2	49·2
60	6·4	49·9	53·9	49·8
75	6·4	50·5	54·4	50·2
90	6·6	50·5	54·5	50·4
105	6·5	50·6	54·9	50·5
120	6·6	50·8	55·	50·6

TABLE 19.—*One Person in the Lead Chamber.*

Time	Temperature outside	Temperature inside, top	Temperature inside, middle	Temperature inside, bottom
h. m.	Cent.	Fahr.	Fahr.	Fahr.
2 0	5·8	45·5	44·8	44·4
After 15	—	48·1	46·5	47·8
30	—	49·6	48·	49·4
45	—	50·2	48·9	50·
60	—	50·6	49·2	50·4
75	—	50·8	49·5	50·5
90	—	50·8	49·8	50·5
105	—	51·	50·	50·8
120	—	51·	50·	50·8
135	—	51·	50·	50·8
150	—	51·2	50·1	51·
165	—	51·	50·1	50·8
180	6·3	51·1	50·1	50·8

TABLE 20.—*One Person in the Chamber.*

Time	Temperature outside	Temperature inside, top	Temperature inside, bottom	Temperature inside
m.	Cent.	Fahr.	Fahr.	Fahr.
	5·5	44·3	43·4	
After 15	5·7	46·3	46·4	
30	5·8	47·9	48·1	
45	5·8	48·5	48·8	
60	5·8	49·3	49·5	

TABLE 21.—*One Person in the Lead Chamber.*

Time	Temperature outside	Temperature inside, top	Temperature inside, bottom	
m.	Cent.	Fahr.	Fahr.	
	4·6	42·3	41·7	
After 15	4·6	44·3	44·4	
30	4·6	45·9	46·2	
45	4·6	46·8	47·1	
60	4·7	47·5	47·8	

TABLE 22.—*One Paraffin Light in the Lead Chamber.*

Time	Temperature outside	Temperature inside, top	Temperature inside, middle	Temperature inside, bottom
h. m.	Cent.	Fahr.	Fahr.	Fahr.
9 10	5·	43·1	42·7	42·2
After 15	5·	45·3	44·	43·4
30	5·	47·	45·	44·4
45	5·	47·6	45·8	45·
60	5·1	48·2	46·3	45·3
75	5·1	48·5	46·6	45·7
90	5·2	48·6	46·9	46·
105	5·3	48·9	47·1	46·2
120	5·4	49·	47·3	46·4
135	5·4	49·2	47·6	46·5
150	5·5	49·4	47·6	46·7
165	5·6	49·5	47·8	46·8
180	5·6	49·5	48·	46·9

TABLE 23.—*One Paraffin Light in the Lead Chamber.*

Time	Temperature outside	Temperature inside, top	Temperature inside, middle	Temperature inside, bottom
m.	Cent.	Fahr.	Fahr.	Fahr.
	5·9	44·5	43·9	43·7
After 15	5·9	47·1	45·4	44·7
30	6·	48·4	46·4	45·5
45	6·	49·	47·	46·
60	6·2	49·5	47·3	46·4
75	6·2	49·9	47·8	46·7
90	6·2	50·	48·	46·9
105	6·2	50·	48·1	47·
120	6·3	50·	48·2	47·1
135	6·3	50·1	48·2	47·2
150	6·3	50·1	48·3	47·3

TABLE 24.—*One Paraffin Light in the Lead Chamber, and one outside.*

Time		Temperature outside	Temperature inside, top	Temperature inside, middle	Temperature inside, bottom
h.	m.	Cent.	Fahr.	Fahr.	Fahr.
10	0	4·2	42·5	42·	41·5
After	30	4·4	45·6	44·	43·3
	45	4·8	46·5	44·9	43·8
	60	4·9	46·9	45·1	44·2
	75	5·	47·3	45·5	44·6
	90	5·2	47·5	45·8	44·8
	105	5·3	47·7	46·1	45·2
	120	5·4	48·	46·2	45·4
	135	5·5	48·1	46·5	45·5
	150	5·6	48·3	46·7	45·6
	165	5·6	48·4	46·8	45·7
	180	5·6	48·5	46·9	45·8
	195	5·7	48·6	47·	46·
	280	5·7	48·9	47·	46·

TABLE 25.—*One Paraffin Light in the Lead Chamber.*

Time		Temperature inside	Temperature inside, top	Temperature inside, middle	Temperature inside, bottom
h.	m.	Cent.	Fahr.	Fahr.	Fahr.
9	0	4·5	42·	41·3	40·9
After	15	4·4	44·5	43·	42·
	30	4·4	46·	44·2	42·9
	45	4·4	46·7	45·	43·6
	60	4·6	47·3	45·6	44·3
	75	4·6	47·6	46·	44·5
	90	4·8	48·	46·4	44·9
	105	5·	48·2	46·5	45·2
	120	5·2	48·5	46·9	45·4
	135	5·3	48·6	47·3	45·6
	150	5·4	48·7	47·4	45·8
	165	5·4	48·8	47·5	46·
	180	5·5	49·	47·6	46·2
	195	5·5	49·	47·6	46·2
	210	5·5	49·	47·6	46·2

The following table gives the abridged results :—

TABLE 26.—*Increase of Heat caused by a Candle in* 170 *cubic feet of Air in the Lead Chamber.*

	Average Increase of Temperature		
	In 1st hour	In 2nd hour	Total in 2 hours
1	3·4	1·1	4·5
2	4·3	1·2	5·5
3	3·7	·7	4·4
4	4·	1·	5·
	4) 15·4	4) 4·	4) 19·4
	3·85 F.	1·	4·85
	2·14 C.	·55 C.	
	Mean increase in one hour		Mean increase in two hours.

TABLE 27.—*Rise of Temperature caused to the* 170 (4·8 *cubic metres*) *cubic feet of Air in the Lead Chamber by one man during one hour.*

March 1	5·16
„ 3	6·57
„ 4	5·27
„ 8	5·55
„ 9	5·65
	5) 28·20
Mean increase in one hour . . .	5·64 = 3·13 C.

These being the only experiments I know of the kind, they may be found useful if compared with candles of different kinds and with gas. In order to make the table I shall use the results obtained by Dr. Frankland, showing the amount of heat given out by several sources of light. He begins with tallow, which, being 100·, wax and spermaceti, are 82 ; sperm oil (Caral's lamp), 63 ; London gases : City, Great Central, Imperial, and Chartered, 47 ; Western, 22 ; Manchester gas, 32—supposing the same amount of light to be given by each at the time.

Taking the amount obtained for a man and paraffin candle, and measuring the above with them, we have—

A man	100,	represented in the lead chamber by 3·13° C., or 5·64 F. in an hour (it contained 170 cubic feet, or 4·8 cubic metres). Paraffin burning 7·75 grammes per hour; the others burning so as to give the same amount of light.—Men of course differ greatly.
Tallow	80	
Paraffin	68	
Wax } Spermaceti } . .	65·6	
Sperm oil (Caral's lamp) .	50·4	
London gases:		
„ City	37·6	
„ Great Central		
„ Imperial		
„ Chartered		
Western . . .	17·6	
Manchester . . .	25·6	

A room 20 feet long, 16 wide, and 11 high, or 3,520 cubic feet (100 cubic metres would be 3,531·6) would be raised in temperature ·27 degrees F. by a man in an hour, if the law of increase were the same as in the lead chamber, but we could not expect it to be the same. One is air-tight, the other is more or less open to draughts. Again, the lead conducts rapidly, the plastered walls slowly. Such a room as is last spoken of would be heated, at the rate in the lead chamber, 5·4 degrees F. by twenty men, and 3·72 degrees F. (2·07 C.) by twenty candles, or about 1·8 degrees F. (1· C.) by gas equal to twenty candles.

But twenty people would not heat it up merely twenty times more than one; the greater the number the more is the heat retained. Besides, the candles and people are not so high as the gas is usually placed, and the heat being above, the warm air comes down when gas is used, and our heads are least able to bear it. A uniform diffusion does not take place.

Twenty men and twenty candles would raise the room 9·12 F., 5·07 degrees C., at the lead chamber rate, but much more in reality, beginning at the ordinary temperature. The difference of heat at the beginning causes a difference in the rate of course as well as the amount outside. The action is therefore very complicated.

Heat caused by Gunpowder.

First			Rise
Temperature to begin . . (8 grammes gunpowder burnt)	roof 43·7 F.	floor 43·5 F.	
After 13 minutes . . .	47·3	47·3	3.7°
Total heat	. . .	. . .	

We may feel surprise at the enormous amount of heat passing from candles, fires, and animals, as shown in the calculations, and wonder how it is so rapidly removed as appears by the experiments. The surface of the lead was very large, and we can readily understand the mode by which the heat was conducted. The amount of heat given out by a man is not accounted for by the amount of carbon burnt. The estimation of the carbon was not made at the same time with the observation of the heat. Still, I found the carbonic acid very constant, as it was always made immediately after a meal, and it will be found to stand high, although not at the highest point. The amount of carbon not being able to produce all the heat found, we must have recourse to hydrogen burnt in the body for the production of the remainder; and it may be worth enquiring whether this method of working may not produce results of importance to medical men. Some men will burn less carbon and more hydrogen, as some men eat less starch and more fat; and the capacities for food, and the quality of the decomposition, may in this be observed, not forgetting also the actual test of the power of producing warmth.

I afterwards found in some experiments that the man gave as nearly as possible double the amount of carbonic acid given by a sperm or paraffin candle. A man produced in an hour ·6 of that gas to 100 feet of air, a candle ·31. The miners' candles produced more, as is believed, but this was not estimated.

On the effect of heat I have little to observe. We all know how it relaxes the exertions of body and of mind, especially when moisture at the same time saturates the air. Dr. Edward Smith has explained this clearly, by showing that a rise of temperature at once diminishes the amount of carbonic acid formed. The rate of life is arrested, but apparently also the functions of life. When life is allowed to move more rapidly by a lowering of temperature, the rate of living may not be more increased, as the repair seems to depend as directly on the oxidation as the waste depends.

There is at a certain temperature a very decided change in the body. This change is marked on all thermometers at 55° F., 12·7 C., as temperate. Above that we require little artificial warmth, and a little exercise supplies sufficient for most persons. On making experiments on putrefaction, the temperature of 54° was observed to be a marked one, or, perhaps, it ought to be 55°, as it was only obtained by many approximate observations, and was not definite. It was supposed that the oxidation was facilitated by the temperature. When colder it is probable that the conducting power is too great for the amount of heat developed, and action is arrested. The same may be the case with man: below 55° the conducting power of his own person is too great to enable him to keep up his warmth, and he must therefore increase his combustion by exercise, or obtain heat by other methods. Above that point the heat is not required, and the oxidation diminishes. In the case of putrefying blood the disruption increases above 55° in a very marked degree; but it is probable that both depend on the same fundamental fact or facts.

At any rate, the point in question is one to be noted, and it is extremely probable that labour will be diminished as the heat rises above that temperature. There is a difficulty in the analogy, as the temperature of man is not lowered to that of the external air.

The use of the lead chamber leads to a new mode of finding the amount of heat given out by man; here it is done in a way that is merely incipient. We are obliged to judge of the amount of heat after the great amount is actually lost. The chamber might be made with sides less capable of conducting, and a more exact result found.

I was led to consider one of the effects of heat when working on the gases of putrefaction. It was then perfectly clear that the putrefaction proceeded exactly as the temperature rose, not ceasing at a little above 130°, perhaps approaching nearly 140°,—which marks another point in organic substances of importance, as being that at which albumen coagulates. At any rate, according as the temperature rises within any limits natural to this country, the necessity for ventilation is felt. The organic matter becomes especially annoying, even with a small increase of carbonic acid. The heat expands it. The substances that annoy us are often volatile, and we may suppose that they begin to decompose in the air when the temperature rises. Indeed, it is scarcely a supposition; it is a fact that after a time they become entirely changed, whether they be organic or inorganic. Sulphuretted hydrogen becomes oxidized, and the unpleasant odours in the mining galleries become the smell of apples. In a house, the organic odours, if shut up, become mouldy or putrefactive according to circumstances; but if the last, they are more rapidly remedied by abundance of air, which oxidizes and washes them away. At a low temperature they will lie unaltered on every substance, and start out again when they are warmed. This action is more distinctly seen when the quantities of oxidizing matter are large; then the eye can perceive the amount daily diminishing until it is all carried away.

If this result be caused by the action of warmth it is not right to demand as much air for ventilation in a cold day as in a warm; it is not right to break the windows

of patients in winter, and to tell them that fresh air is better than heat. The chemical action, and with it the feelings, demand warmth first above all things. It is the very first demand, as no function can go on without it. You may live hours, days, or years in badly ventilated places with more or less discomfort and danger; but a draught of cold air may kill like a sword, almost instantly. In the railway carriage as well as in the house, the great instinct of man is first to be warm enough; and he is quite right. Such a universal instinct must not be sneered at; and even if it had no good scientific foundation it must be considered as a power strong enough to stand on account of the number of its supporters.

Whatever it be that causes organic substances to be unpleasant, it is very much connected with the lungs, as the body may bear much heat if the mouth only has the advantage of a good current of air. Certainly, this points strongly to want of oxidation and diminished strength on that account. Again, if these minute substances are oxidizing, they will to a slight extent be candidates for the oxygen of the air that the blood is seeking, and act just as sulphuretted hydrogen would act. Moreover, they are taken up more rapidly than mere gases would be, as we know from the capacity they have of remaining so as to make a room unpleasant long after the air has been changed; that is, if time has been given them to settle. This is peculiarly to be observed in some churches, and so far it is true that you may know for some time, and at some feet distant by the smell of the clothes, if people have just lately come from such a church. This will not occur when there is warm air, or when the windows have been sufficiently open. Again, near a great number of the working people the same smell is perceived as in their houses. And this great fact is, after all, one of the main causes of cleanliness, as it has produced in us a dislike which demands its removal.

The intense heating power of the gunpowder is to be remarked, and the sudden change which it produces. It is not easy to imagine any greater advantage to the miner than the removal of this evil. Analyses point to a fresh current on the floor, whilst a warm and less pure current is above. Some such thing must take place to keep the mines even in the condition in which they are, and to explain why they do not become filled with poison. Although gunpowder is said to have been used at Rammelsberg as early as the 12th century,[1] it was not used with great effect till very modern times. We cannot say, however, that its use has made the mines less fitted for life, as before its time it was usual to heat the rocks by burning wood upon them, after which the place was left for a day or two till the smoke became tolerable. Some recommend the miners to wait from Saturday to Monday, some from Friday. This practice is not entirely given up, according to accounts received, as it is found useful for very hard rocks.

If gun cotton could be used of a kind which gives out none of the oxides of nitrogen, an advantage of the most pleasing kind would be gained. The solid matter, the crystals and carbon, would cease to trouble the miner, as well as the sulphur compounds that follow every explosion.

Still the production of heat must be of advantage to the mine; it makes ventilation possible. A mine without workers becomes filled with stagnant air. It is the problem of miners to compel that heat which is formed so to warm the air as to produce a current. If the air is led towards the shafts this effect will be produced. There are two modes of doing this: one is by drawing out the warm and foul air directly, another is by driving in pure air to the men, and so forcing the foul air towards the shafts.

[1] Reitemeier's *Geschichte des Bergbaues.*

COMBUSTION OF CANDLES IN BADLY VENTILATED PLACES.

I cannot quote any experiments on the combustion of a candle in the conditions required, excepting those of Wehrle. At p. 11 he says:[1] 'If a candle continues to burn (meaning thereby even with difficulty), the amount of oxygen is 18 per cent.; but if it goes out and a common mining lamp burns, the oxygen is 16–18 per cent.; but if that goes out, and an argand lamp burns, then the amount is under 16; when this goes out also it is 14. The light goes out sooner or later according to the different substances used for burning, and the construction of the lamp.' At p. 13 he says: 'Candles go out completely when the amount of carbonic acid in the air is only 10 per cent. If it is between 5 and 8 per cent. they burn badly; a small round flame hops round the wick and then goes out.'

These accounts do not quite agree, and there is some indefiniteness. Some time ago I made experiments on this subject, burning the candle in a tin vessel about two feet high on nine inches square, standing over water, an apparatus belonging to Dr. Holland, who suggested some of the trials, and was present when they were made. The vessel was open below and placed in water. A window in the side showed when the light burnt. The results were:

	Oxygen in the Air when the light went out.
With a common dip tallow candle . . .	15·2 to 16·2 p. c.
Paraffin oil lamp with diminished draught	15·5
Same lamp, very low, with a half-inch flame	14·4
Spirit lamp	12
Petroleum very volatile	10·8

In operating in the lead chamber the candles, as a rule, went out with the oxygen between 18 and 19.

[1] Ueber die Grubenwetter. Von D. Alois Werhle. Wien, 1835.

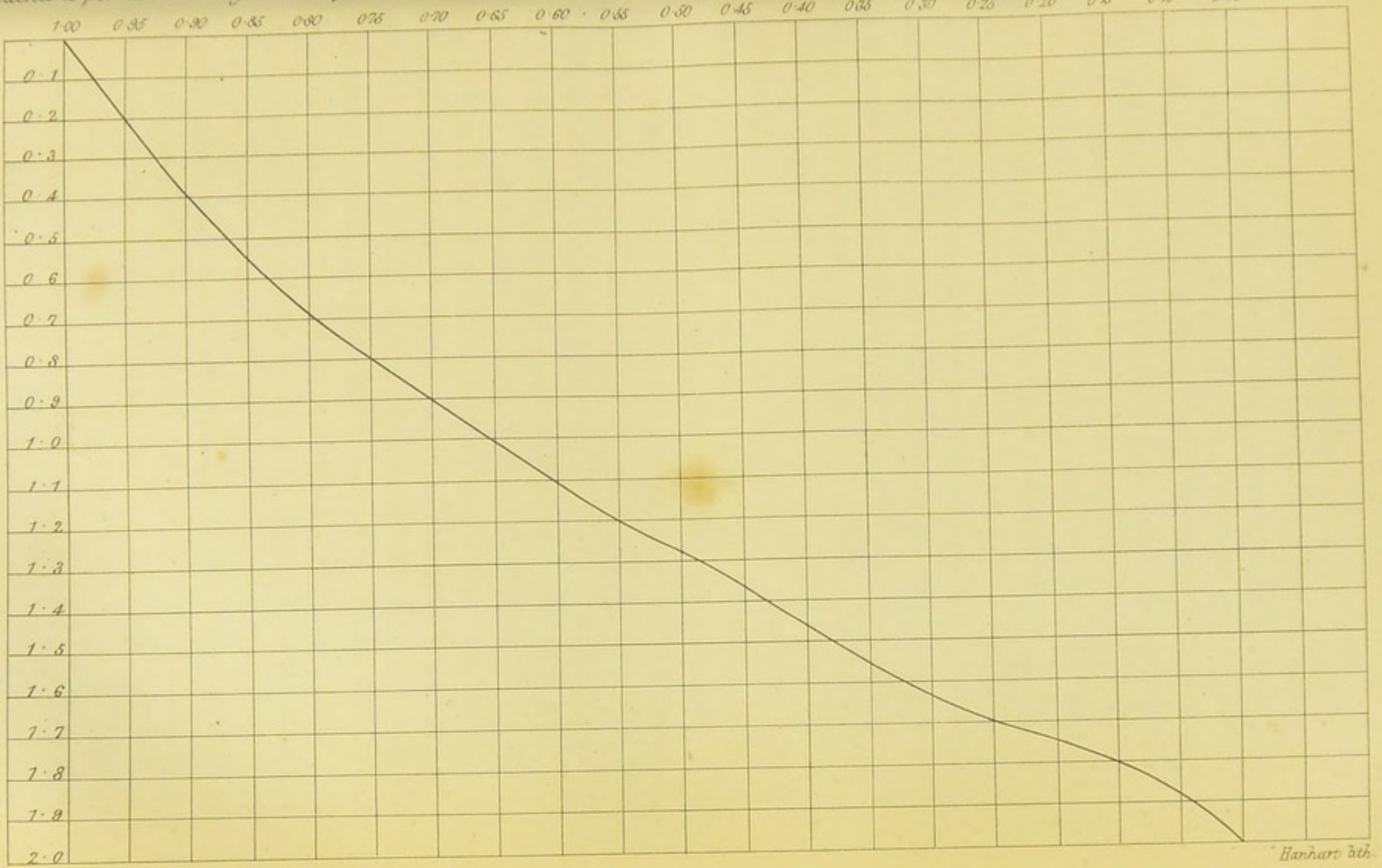
These results seem more decidedly in contradiction than those of Dr. Wehrle, but the cause will appear. It was found in the lead house that before the candle went out the tallow around the wick became gradually less liquid, and at last solid. If, however, it were again rendered liquid by any means, the light rose up and burnt freely even in air which contained only 18 per cent. of oxygen, and in which a candle would not usually burn. For this reason the miners incline the candles in order that the flame may approach more nearly to the grease and melt it more rapidly. Sometimes this is done to such an extent that there is a constant stream of tallow moving past the lower part of the wick and dropping on the floor. When a small vessel is used for the combustion of the candle, such a box as that above described, it is necessary, in order to observe the varying stages of combustion, to let the candle be in full flame when it is inserted; at that time the tallow is melted around it and a considerable supply is at hand. Before this supply is used up the amount of oxygen is reduced to the number spoken of. Such an experiment, therefore, is not satisfactory; it shows only the combustion of a candle of a given size with a given extra supply of oily matter. If, however, the space is large, as in the lead chamber or the mine, the few additional drops of melted fat will not perceptibly affect an experiment lasting for hours, as, after all, their effect is over in a few seconds, at most 30. In this case the combustion ceases whenever the candle is incapable of giving out heat enough to melt sufficient fat for its consumption. If the experiment last for hours, a gradual diminution of the heat takes place and with it a gradual diminution of light. But if the candle be held on the side, or if additional fat be melted, the flame may be made to rise as high as in the ordinary air; still if left to itself the hardening will take place again. As in

this case the flame is to begin with as large as usual, there appears as much heat as usual, and still the tallow is not melted. The tallow is melted by radiant heat, and the hardening seems to prove that there is an obstruction to this radiation. The carbonic acid, the watery vapour, and the fatty matters themselves which exist unburnt in the air, may be these non-conductors. The experiments in Tables 4 to 8 will show that this diminution of light, of heat, and of melted matter takes place with vapour of water alone to some extent, and with carbonic acid without any additional vapour added to the air, but that when both the acid and the vapour are present the action is most rapid.

It will be seen from Diagram 1, that the diminution of the combustion was not caused by a diminished quantity of oxygen merely, because when the carbonic acid was removed the candle burnt very well, although not so well in the remaining air, although this air would only have 18·7 per cent. of oxygen in it, whereas by experiment in Table 7, the light was very much diminished by the presence of carbonic acid, although the air contained actually 20·1 of oxygen with little of additional watery vapour to disturb it, and was otherwise in a condition to support very fair combustion.

When Dr. Frankland was on the top of Mont Blanc he found that a candle burnt with an elongated blue flame without consuming less fatty matter than usual. In air rendered bad by carbonic acid and watery vapour, or by any of the substances floating or volatilised in the air of mines, the candle has no tendency to become blue, but preserves its whiteness to the last. On the other hand, the blue edges, especially at the beginning of the combustion, entirely disappear, thus giving what is called a 'sickly' or a pale yellow appearance to the light. At the last a pale white flame hovers over the wick, which has become short from a process of self-snuffing. The

reaches 2 per cent the light is only 5. whereas it was 100 to begin with, as may be seen by turning to the figure [illegible]



red wick burns short, and, when the small lambent flame escapes, still glows for some time.

When candles burn in fairly ventilated places the unburnt vapours are not always perceived, but when many candles are burnt in close places these vapours become very perceptible to the senses. Perhaps this is owing partly or wholly to a less thorough combustion than in pure air. Acrolein is probably one of these vapours, and, according to Dr. Tyndall's investigations, we must ascribe to them no small influence in preventing the radiation of heat.

When powder smoke is present, the light of the candle is diminished to a considerable extent. The solid matter, as well as the gases which are evolved, prevent the combustion, and I suppose also the radiation. This is clearly seen from Table 8.

By Table 4 the light of the candle is observed to be less, even after only half an hour's delay in the chamber, and with 18·57 of oxygen the candle still retained one fifth of its power. This occurred when the exhalations from a candle and a man rendered the air impure. The exact point of extinction depends on the gases, vapours, and temperature united. It must therefore vary.

If we compare Table 5, showing an experiment in the lead chamber when a small amount of steam was sent into the space where there was one person with a candle, we find the result, after 190 minutes, almost exactly similar to the experiment of Table 4; the candle diminishing to the same fraction after the same time, the amount of vapour from the candle and the person saturating the air towards the conclusion.

But if we examine the change in the candle at the beginning of the experiment, we find that, in Table 4, without steam, the value of the candle had in half an hour gone down to ·933, whereas with a small amount of steam, not affecting the air to the sight, the amount

had fallen to ·875 in 25 minutes; and with a large amount of steam to supersaturation, the candle fell in 10 minutes to ·826, and in 20 minutes was diminished by half. It may here be said that the steam might act as an obstruction to the light if it condensed in the room; but to show that this effect is not that which is observed here, we find that the values of the candle are, after 103 minutes, with steam ·206, without steam ·376. The numbers are thus rapidly approaching each other, and at the point of saturation of the air, when no steam was used, would no doubt have met, or nearly so. As the temperature was raised with the steam, the candle would burn a little better, because the tallow would melt more easily.

In experiments, Tables 4 and 5, at 190 and 195 minutes the values of the candle are ·0108 and ·0104. We may draw then this conclusion, that the effect of the vapour of water in preventing combustion is very great. In No. 5 the amount of vapour sent in at once more nearly approached that which would arise from natural causes in the mines, and there we see that there is a diminution of light of one-tenth of a candle beyond that which is caused when there is the ordinary amount of vapour in the air. This will be the most useful and practical experiment, although the study of experiment, Table 6, affords also some valuable information. In No. 5 no steam was ever visible.

At the end of each of these three experiments the air was saturated with vapour, and all were practically very nearly in the same condition.

The experiment in Table 7 is given to show the effect of carbonic acid on the combustion of a candle. The only value which I can attribute to this experiment is this, that it proves the effect on the candle to be very great. Here there was one person and a candle in the chamber, and therefore the amount of moisture was con-

siderable after one hour, but the effect is visible in ten minutes, or before the moisture could be formed.

At the end of the experiment the candle was nearly expiring with the same amount of carbonic acid as in experiment, Table 4, confirmatory therefore of both, although, from the fact of the carbonic acid having been poured into the chamber, and not formed by the simultaneous removal of the oxygen, some difference might have been expected.

This difference was seen in another experiment when the carbonic acid was thrown in from the outside and no person or candle was present in the chamber; in this case the candles taken in afterwards burnt moderately, when there was 3·9 per cent. of carbonic acid in the room. The reason of this I attribute to two causes: first, there was less vapour of water in the room, and, secondly, there was more oxygen in proportion, as it had not been removed by combustion.

As the combustion of a candle depends very much on the state of the air, it is not a surprising thing that the combustion of a fire should be affected similarly. In cold weather the vapour is removed from the air even if the ground may appear moist, and it is still further removed if the ground is frozen, and this also in proportion to the hardness of the frost. In warm weather the vapour is proportionately increased, and the fire burns proportionately dull. The warmth cannot do otherwise than assist the combustion; we learn this on a grand scale by the effects of the hot blast. In cold weather a fire burns brightly, even although the cold is an agent of an obstructive kind to the combustion. In warm weather the fire burns less clearly, although the heat is an assistance to the combustion. The reason seems to lie in this fact, that the cold air is deprived in a great measure of its moisture. Its effects on the candle are such as the least observant eye may remark, and the changes on a

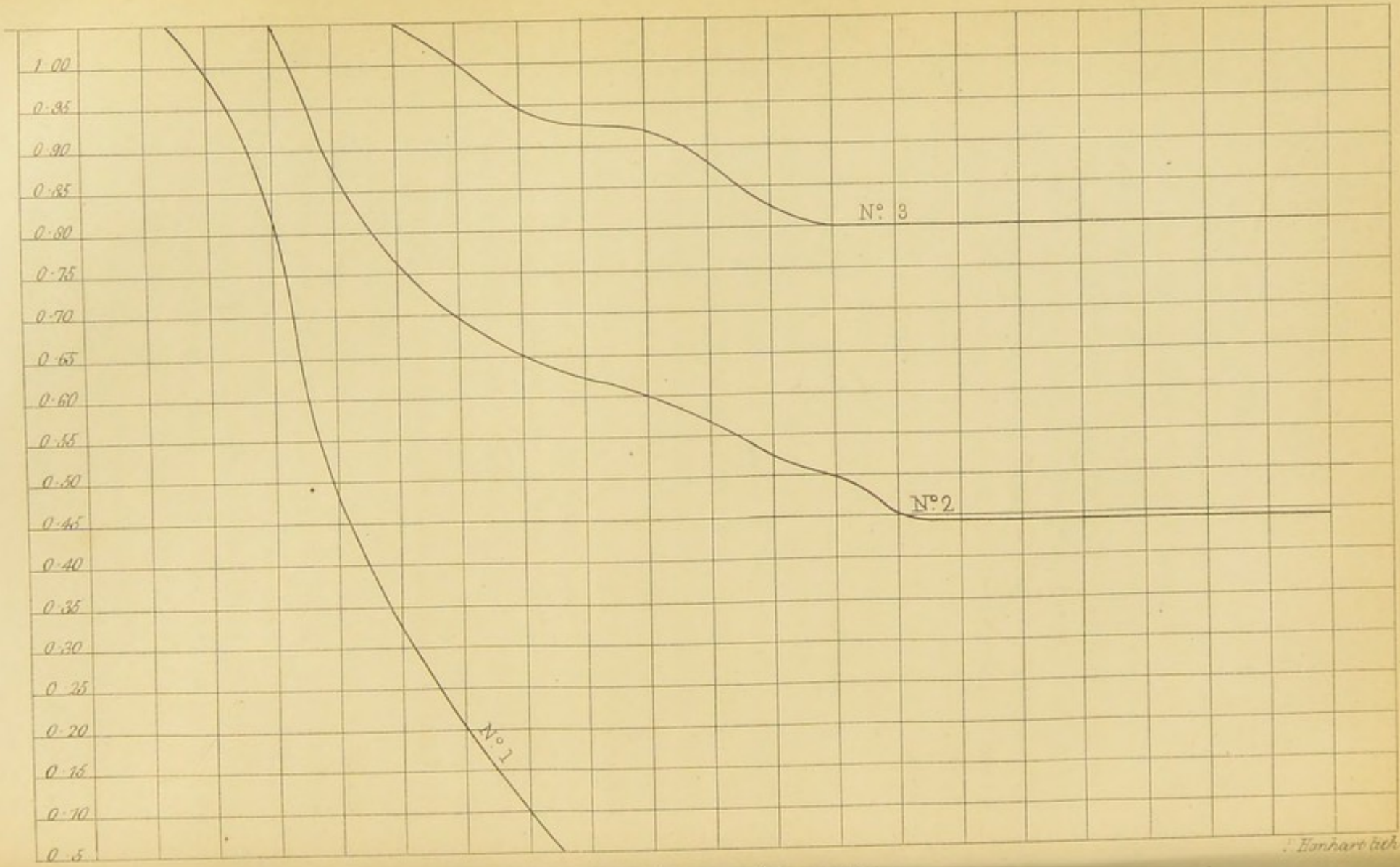
fire are of the same kind. The value of this action is very great in economising the products of nature which are used for heat, and in economising the labour which must bring these products into active use. If this be so, it is a lesson which may be taught to many who will persist in attempting to increase the amount of heat in the fire by pouring in either water or watery vapour.

It may be said that the diminished combustion is due to the imperfect conduction of the heat of the flame to the candle. This appears at first a very remote conception, which will require much reasoning to bring within the range of daily thought, but in reality it is not so. As the candle stands before you, the grease is seen hardening and the flame expiring simultaneously; you add a little heat by a blowpipe or any other means, and the flame starts up. There is nothing wanted but power to melt the tallow; you raise the flame to a height greater even than the average, and still it soon sinks and goes out. The heat does not pass from the flame to the tallow.

The capacity of the atmosphere to absorb the heated rays of the sun has long been known to be great, and we readily believe that after passing through many miles of air, as these rays do toward the evening, the heat might be diminished till it would be very small, but that it should be absorbed in the space of an inch to such an extent as I now mention would scarcely have been conceived before the investigations of Professor Tyndall. He has shown us that the absorptive power of water, carbonic acid, ammonia, and scents or odours, bodies not unlike those distilled from fat, as well as olefiant gas, one of its products, all powerfully absorb heat; and although he has not worked with such small layers of gas as an inch, the principle seems to apply.

In his enquiry into the absorption of heat by gases and vapours, Professor Tyndall says: 'Air sent through the

The air being supplied by a pump and the candle being in a close vessel the light went down in the case of common air to 0·75: in the air from which the 2·2 per cent carbonic acid replacing that amount of oxygen was washed out the light went down to 45, but when the 2·2 of carbonic acid was present it went rapidly down & went out The light in all cases burned better ab first because as explained there was a certain amount of material ready melted



N°1 Air with 2·2 p.c. Carbonic Acid
N°2 The same with the acid washed out

system of drying tubes and through the caustic potash tube produced an absorption of about 1. Air direct from the laboratory, containing therefore its carbonic acid and aqueous vapour, produced an absorption of 15.' The amount of carbonic acid in his laboratory would in all probability not exceed ·1 per cent. even allowing for a very bad state of things, whilst the aqueous vapour would in all probability be far from such saturation as was found necessary in my experiments. I trust I do not misapply his theory.

It is extremely probable that the exhalations of animal matter from the body have also their own peculiar influence on the candle as well as on the breathing.

There is, however, another explanation; when 21 per cent. of oxygen surrounds a burning body, the action may be greater by $\frac{1}{21}$th part than when 20 per cent. only surrounds it. A candle was placed under a receiver and supplied with air containing about 18·5 per cent. of oxygen with a corresponding amount of carbonic acid, that is about 2·5 per cent. This was simply prepared by burning candles in the lead chamber until they went out. A vessel of flexible material was then filled with the air and pressure put upon it, so that the air was driven into a receiver containing a burning candle. The candle went out. The same quality of air was washed with caustic soda and driven into the receiver, and the candle could then be maintained burning, although at an inferior speed, compared with burning in common air. Diagram 2 shows the difference of combustion under the two circumstances.

There are then two influences at work, the active principle of the carbonic acid (a narcotic poison, as W. Müller calls it[1]), and the absence of oxygen. The last would begin to act instantly, and its effects would be visible at the lower part of the flame, the burning being

[1] *Annalen der Chemie und Ph.*, 1858.

feeble. This may be seen well when a paraffin candle burns; as soon as 1·5 per cent. of carbonic acid is reached the melting is so slow that the flame leaves the lower part and goes above to burn the paraffin that has been drawn up; when this is done it seems inclined to go out. The flame leaves the upper part of the wick, which becomes completely burnt; it then passes down. For this reason the heat is communicated again to the candle, more is melted, and the flame rises; this goes on constantly, and we have a candle brightening intermittently, the intervals shortening after a while. That the combustion is rendered more difficult at the lower part of the wick is pretty clear; the flame is entirely changed and its clearness is gone, as well as its blueness. Very probably when there is abundant oxygen that gas mixes with the evaporated oil, and explodes with the hydrogen, forming that pale blueish flame which heats the rest of the vapour and produces whiteness above, but with less oxygen there is a dull external combustion producing less heat and so supplying less melted fat. Whilst it is probable that the diminution of heat is one cause, I cannot see that all the circumstances can be explained without introducing the non-conduction of carbonic acid. Perhaps some would suppose an influence such as is exercised by a colytic agent preventing chemical action. There is room here for much experiment and much theory, but the fact is clear.

For photometric experiments a candle was burnt inside the chamber and another outside. I stood inside, and the disk of the photometer was within also. Two candles giving the same light were used. These candles, I know by experience, seldom vary above two or three grains in the amount burnt per hour.

The photometer disk was about two inches from the glass of the window. The candle was distant 22 inches from the photometer; that is, when the outside candle was $16\frac{1}{2}$ inches from the photometric disk it was just equal

to the inside candle at 22 inches distant, so that the glass destroyed ·438 or nearly half the light of the candle.

According as the candle within gave less light from the deterioration of the air it was gradually moved nearer to the disk of the photometer.

The change of light was sometimes perceptible at an earlier stage than indicated by any figure given in the table, and I can readily believe that much experience might cause the candle in some conditions to be a very sensitive indicator; not in all.

TABLE No. 4.—*The Effect of Impure Air on the Combustion of a Candle.—February* 6, 1864.

Time.	Outside candle.	Inside candle.	Value of the light in candles.	Temp. Fahr. near roof.
	Inches.	Inches.		°
11 h. 20 min.	16·5	22	1	49·5
After 30 min.	,,	21·25	·933	49
Flame observed to be shorter .	. .	. .	. .	52
	Carbonic acid ·59			
Outside 2 inches . . .	Oxygen . 20·38			
Inside 1·6 ,, . . .				
	16·5	18·8	·734	54·5
45 min.		18·25	·688	55
60 ,, ...	. .	17·1	·604	55
75 ,, ...	. .	15·5	·496	55·25
90 ,, ...	. .	13·5	·376	55·25
105 ,, ...	. .	12·8	·338	54·5
120 ,, ...	. .	10·5	·227	54·5
135 ,, ...	. .			
	Carbonic acid 2·31			
	Oxygen . 18·57			
		9	·167	57·5
150 ,, ...	. .	7·75	·124	54
165 ,, ...	. .	Very small flame.		
		6·75	·0941	54
180 ,, ...	. .	2·25	·0104	54
195 ,, ...	. .			

Before Lighting.	At End of Experiment.	
Inside candle 33·82 Grammes.	Grammes. 14·715	= 5·87 per hour. 24·26 diminution when 3 per cent. oxygen were removed from the air.

TABLE No. 5.—*Effect of Steam, one Person and one Candle in the Chamber.—February* 11, 1864.

Time.	Distance of Candle from Photom. Disk.		Candle's value.	Temp. Fahr.	Height of Candle inside and out.
h. min.				°	
10 52	24	. .	1	40	Both 1·5 inch.
After 25 min. .	22·5	. .	·879	41	Still the same.
„ 40 „ .	21	. .	·765	46	
„ 55 „ (*a*)	19·5	. .	·660	48	A difference observable in the height of the flame.
„ 70 „ .	18	. .	·562	52	
„ 100 „ .	14·5	. .	·365	54	Flame bad.
„ 130 „(*b*)	11·5	. .	·229	55	About one-third of its former height.
„ 160 „ .	7	. .	·085	55	Flame changeable, the largest given.
„ 190 „ .	2·5	. .	·0108	55	
„ 200 „ (*c*)	out			5	Flame out.
		Before burning. Grammes.	After burning. Grammes.		
Outside Candle . .		33·946	6·547	used	27·398.
Inside „ . .		31·01	15·584	„	15·426.
			4·63 per hour.	. .	40·26 per cent. diminished.
		2 per cent. CO_2.			
2 spermaceti candles burnt in the chamber and one person present, 180 grammes of steam driven in.					

There was no particular inconvenience felt in the chamber during any of the time.

(*a*) At this point the steam began to enter the chamber, causing a rapid lowering of the light.

Specimen *a* taken . . } . . 1·02 p. c. of carb. acid.
The steam continued to the end } . . 20·03 oxygen.

(*b*) Here the moisture was distinctly observed in breathing.

Specimen β . . . . . . 2·10 carbonic acid.
18·80 oxygen.

(*c*) Specimen γ . . . . . . 2·66 carbonic acid.
18·48 oxygen.

TABLE No. 6.—*Steam in the Lead Chamber sent in from a flask of boiling water. One Person and one Candle inside. Compound Experiment.—February* 16, 1864.

Candle outside, 16½ inches from disk.

Time.	Candle inside.	Value of Candle.	Temp. Fahr.	
12 o'clock .	22 in.	1	59	Although steam was visible.
After 10 min. .	20 „	8·26	63	
„ 13 „ .	16·5 „	·462	65	Sudden fall. Steam collected above and fell. Candle visibly declined.
„ 20 „ .				
Here the steam was interrupted till the air became clear, yet the light diminished rapidly.				
„ 20 „ .	14·5	·434	65·5	Fall rapid. Steam visibly condensed, and thick vapour.
„ 40 „ .	Steam begun; the heat fallen to		62	
„ 50 „ .	12·5	·322	65·5	
„ 60 „ .	*12·5	·322	68·5	
„ 70 „ .	12	·297	70	Flame appears very dull.
Entrance of steam stopped to see if the flame would recover.				
„ 77 „ .	*15	·464	69·5	
* Probably this rise of 15–12 due to the visible steam.				
„ 85 „ .	11	·250	65·5	Supply of fat is suddenly stopped.
„ 95 „ .	11·5	·273	65	
„ 98 „ .	12	·297	62·5	
„ 103 „ .	10	·206	62	
At this point the candle began to take its usual course.				
Specimen of air taken				·85 carbonic acid. 20·01 oxygen.

TABLE No. 7.—*Pure Carbonic Acid poured into the Lead Chamber. One Person in the Chamber.—February* 13, 1864.

Time.	Inside Candle	Candles.	
11 h. 25 min. .	22 in.	1	Feeling here an acid or prickly sensation on the face.
After 10 „ .	14·5 „	3·434	
„ 15 „ .	12 „	·297	
	Carb. acid ·579		

TABLE No. 7.—*continued.*

Time.	Inside Candle.	Candles.	
After 19 min. .	11 in.	·250	
„ 25 „ .	10·5 „	·227	
„ 30 „ .	9·5 „	·186	Breathing, 25½; pulse above average.
„ 45 „ .	5 „	·051	Here came steam, and the candle fell suddenly to ·227.
	3·5 „	·227	
„ 50 „ .	3 „	·018	Candle became very changeable, as if supplied by an intermittent fountain of oil.
„ 55 „ .	2·5 „	·013	
„ 60 „ .	Nearly out .		Breathing very heavy and frequent.
	Carb. acid 2·59		Those who entered immediately after found the air very bad.

TABLE No. 8.—*Gunpowder in the Lead Chamber.—February* 17, 1864.

Time.		Candles.	Temp. Fahr.
12 h. 56 min. . .	22 inches	1	41
After 14 min. . .	20 „	·826	51·5
After 45 grains of gunpowder had been burnt in the chamber, there was no immediate diminution of light; but in a few minutes the candle changed suddenly, as the smoke fell down upon it.			
After 24 min. . .	18 inches	·669	51·5
	15 grains then burnt—60 in all.		
„ 27 „ . .	18 inches	·669	52
„ 29 „ . .	16 „	·529	52·5
Very unpleasant breathing, as if every little crystal was felt sharply; caused coughing and discomfort.			
After 39 min. . .	14·5 inches	·434	53
Distinct taste of salt as it came from the throat, after collecting there and causing much phlegm. Depressing. Breathing worse than after three and a half hours in the chamber without saltpetre.			
After 47 min. . .	Fired other 15 grains.		
„ 51 „ . .	13 inches	·349	53·5
Decided weakening of the flame, as seen by its indistinct edges; it becomes shorter.			
The flame at the bottom becomes whitish; it is usually blue.			
	30 grains fired.		
After 64 min. . .	11·5 inches	·273	54·5
Weight of outside candle on entering, 27·04 grammes; at end of experiment, 12·11 grammes.			
Weight of inside candle on entering, 21·87 grammes; at end of experiment, 8·59 grammes.			
Outside candle burnt 14·93 grammes.			
Inside „ 13·37 „			

TABLE No. 9.—*General View of the Amount of Light from a Candle under various conditions, showing Parts of a Candle, a Candle being* 1.

After minutes burning.	With Person and Candle. Value of Candle.	A little steam and 1 person.	With great excess of steam and 1 person.	With carbonic acid and person.	With gunpowder and person.
10.	.	.	·826	·434	
13.	.	.	.	·562	
14 (45 grms. of gunp.)	.	.	.	.	·826
15.	.	.	.	·297	
19.	.	.	.	·250	
20.	.	.	·434		
24.	.	.	.	.	·669
25.	.	·879	.	·227	
27 (6 grms. burnt)	.	.	.	.	·669
29.	.	.	.	.	·529
30.	·993	.	.	·186	
39.	.	.	.	.	·434
40.	.	·765			
45.	·734	.	.	·051	
50.	.	.	·322	·018	
51.	.	.	.	.	·349
55.	.	·660	.	·013	
60.	·688	.	·322	nearly out	
64 (100 grms. altogether)	.	.	.	.	·273
70.	.	·562	297		
75.	·604				
77 (steam stopped)	.	.	·464		
90.	·496				
100.	.	·365			
105.	·376				
120.	·338				
130.	.	·229			
135.	·227				
150.	·167				
160.	.	·085			
165.	·124				
180.	·0941				
190.	.	·0108			
195.	·0104				
200.	.	out			

TABLE No. 10.—*Burning of a Paraffin Candle in the Lead Chamber, under various circumstances.*

Time.	Amount of Candle consumed inside.	Amount of Candle consumed outside.	Percentage burnt less inside than outside.	Carbonic Acid formed by the burning inside.		Amount burnt in Grammes.	Per Hour.	Less in than outside per Cent.
	Grammes.			Per cent.				
In 1 hr. . . .	7·52					24·700	5·489	29·27
,, . . .	7·456				In 4½ hrs.	26·802	5·956	23·44
,, . . .	7·14	(mean) 7·751	6·2	·312		25·522	5·670	27·09
,, 2½ hrs. . . .	19	20·47	6·5	·80		19·280	6·42	20·19
,, 3 ,, . . .	20·828	22·574	7·7	·96	In 3 hrs.	18·803	6·26	22·16
,, 3⅓ ,, . . .	25·37	27·19	6·69	1·12		20·040	6·64	18·68
,, 4¾ ,, . . .	31·73	36·27	12·5	1·52				
,, 3¼ ,, . . .	19·1	25·19	24·26	2·21				
,, 3 hrs. 20 min., and steam . .	15·426	25·57	40·26	2·80*				
,, 2 hrs. 4 min., with gunpowder .	13·37	15·96	13·80					
Due to 90 grains of gunpowder exploded . . 8·8 per cent.								

* This is ·07 Co_2 for 1 per cent. of diminution of combustion.

WEIGHT OF CANDLE BURNT IN AIR OF VARIOUS QUALITIES.

By Tables 10--13 the amount of sperm and paraffin candles burnt in different atmospheres is seen. The amount burnt is in proportion to the light with remarkable exactness. No. 10 gives the amount burnt in certain times, with the carbonic acid at the end of the experiment. From Tables 4, 5, and 6, the amount of light given at various stages is obtained, and by combining these we have Table 13 which shows the amount of light prevented by given quantities of carbonic acid. Moisture is present in all, but in the lower numbers not to saturation, whilst the saturation will be nearly complete in the higher. Again, it seems to me that it makes a difference if the exhalations other than moisture from a human being are present or not, and I cannot therefore call this table accurate as far as carbonic acid is concerned alone. It is given, however, as a very near

approach to the condition of things actually found in the mines. It will only be made perfect when each agent is separately examined and the effect of each added to make a total. A mode of making these experiments is already devised, but they may not be ready, if ever made, for the Report of the Commission, and are not strictly demanded by the enquiry.

In order to ascertain the amount of light given by a candle at various stages of impurity of the air, it has been said that a photometer was used inside and candles placed outside and inside. The distance was generally twenty-two inches at first, and the experiment ended by the candle almost touching the disk.

When these experiments were making, it was observed that the outside candle was burnt rapidly down, whilst the candle inside the chamber was still long. It will be seen by experiments in Tables 10, 11, &c., that this shortening was in exact proportion to the amount of light given. This would not be the case if the candle burnt with a blue flame as in rarefied air. We may conclude from these experiments that the value of the light of a candle may be measured in three ways: 1st, by the photometer; 2nd, by simply weighing it before and after burning; 3rd, by measuring the length of the candle before and after burning. It has been shown that these three modes of measurement may be used for estimating the condition of the atmosphere in a mine, not with great nicety, but to an extent far beyond any method ever yet used in mines. The plans 2nd and 3rd are actually applicable, and may be said to constitute the best method of applying the candle test, if time is allowed. The amount may be weighed to an accuracy far beyond anything that can be done with the eye only. If, therefore, the candle is valuable as a test, this is one true mode of using it, perhaps the safest; but either a paraffin, spermaceti, or wax candle would be required,

and it would require to burn a long time. This length of time is an objection. Coal mines are *not* alluded to.

The number of witnesses to the value of the candle as a test is so great that to give them all would seem more to be the transcription of a catalogue of books. Among all the writers who in the course of centuries have spoken of the subject, no dissentient voice has been found except perhaps one. Bartels,[1] in his report on ventilation in 1711, says that old air was generally blown back to the places requiring ventilation, and although it was not bad enough to affect the candles much, it was still hurtful to the workmen.

A belief so long and so well established as that concerning the infallibility of the candle is not to be supposed to be entirely unfounded. It will be seen from the experiments given, that, within bounds, it really is remarkably well founded, and by the aid of fine measures or balances may be carried out to an extent far exceeding that which it has hitherto reached. This great value it certainly has, that it indicates impurity of air of almost every kind usual in mines, little heeding whether that impurity be pure vapours of water or poisonous carbonic acid; and to a great extent the effect of the two substances on the candle very much resembles their effect on human life, or at least comfort. A certain amount of moisture, carbonic acid, and powder smoke affect the candle, but this gives no indication whether one or the other is in excess; and although a certain lassitude be produced by the vapour of water in excess we cannot attribute to it the same principal effects which are made apparent by the smoke of powder. There is also a difference if the carbonic acid is brought into the air, and not formed in it by removing the oxygen. As an example the candles will not go out with 4 per cent. of carbonic

[1] *Acta Historico-Chronologico-Mechanica circa Metallurgiam in Hercynia superiori*, by Henning Calvör. Braunschweig, 1763.

acid when the acid is poured in, but they are extinguished by $2\frac{1}{2}$ per cent. if the carbonic acid is formed by burning the oxygen. These two extreme cases illustrate some weak points of the candle as a test. They perhaps illustrate, also, experiments related to me by Professor Bunsen, in which it seemed clear that the candle burnt longer than the human life.

Reference to similar results is found in Professor Miller's 'Chemistry.' In such cases it is probable that other gases were present. If, on the other side, sulphuretted hydrogen had been present our calculations would be incorrect, as a small amount would be deadly.

The candle will go out when the accumulated effects of the vapours and acids amount to a certain point, but a very large portion of this effect may be due to deadly gases, and none to watery vapours, or the contrary. If the large effect be due to watery vapour a man will have the greater chance of living on. If the chief effect is due to carbonic acid, we cannot doubt that the danger is greater, but this cannot be distinctly proved, although it was somewhat illustrated by experiment.

A certain amount of moisture in the air is agreeable, but it diminishes the burning power of the candle; a time then comes when this moisture is prejudicial both to men and candles; but go further again, it is impossible to add moisture enough to the air to do rapid injury to any man, whilst it is very easy to add carbonic acid to that amount. Again, the eye is not quick enough to observe the differences in the early stages, such as when there is a quarter per cent. of carbonic acid, and still less able when the amount is below a quarter per cent. Now this is the only state of things in which men should ever be allowed to work; it follows therefore that the candle as used is valuable only when the air is so bad that no one should be allowed to remain in it.

TABLE No.11.—*The Measurement of the Length of the Candle as a Test of the Air.*

	In 4½ Hours.	Per cent. burnt.	Actual Amount in Inches.	Calculated Length from Amt. burnt.	Difference.
	Grammes.				
Amount burnt in common air . . .	34·987		4·036		
1st . .	24·70	70·73	2·812	2·84	·028
2nd . .	26·802	76·56	3·312	3·092	·220
3rd . .	25·522	72·91	3·	2·94	·060

TABLE No. 12.—*Combustion of Candle in an Atmosphere with Carbonic Acid and Vapour from a Candle and Person.*

Time of burning in minutes.	Carbonic acid in the air.	Differences.	Value of the light and candle being 1.
30			·933
40	·63	·43	
45			·734
60	·95	·32	·688
75			·604
90	1·31	·36	·496
105			·376
120	1·61	·30	·338
135			·227
150	1·76	·15	·166
165			·124
180	1·88	·11	·094
195			·014

TABLE No. 13.—*Amount of Light lost by the action of Carbonic Acid in the Air.—Moisture being present.*

Carbonic Acid.	Light lost, per cent.
·63	26·6
·95	31·2
1·31	50 4
1·61	66·2
1·76	83·4
1·88	90·6

There is here an irregularity, because, as will be seen,

the heat rises rapidly the first hour, and slowly the second, whilst it remains stationary afterwards. The amount of light lost may be reckoned for the present, in a rough way, as nearly 10 per cent. for every ·2 to ·22 per cent. of carbonic acid, whilst it is somewhat less with some conditions of vapour more nearly approaching ·25, although both these numbers are disturbed by the temperature. The experiment is not a simple but a very complex one.

Diagrams 1 and 2 illustrate this question. See also under ‘ Carbonic Acid and Ventilation.’

Methods of ascertaining the State of the Air by the Amount of Candle burnt.

These methods are obtained from 11 and 13.

If a candle burns 120 grains per hour in pure air, and is found in any place to burn only 110, there is a loss of 9·1 per cent. Let 10 per cent. be equal to ·25 of carbonic acid, the 9·1 will be equal to ·22 per cent. of carbonic acid. Or let us say, if a candle burns 26·6 per cent. less than its normal amount estimated either by weighing or measuring, the amount of carbonic acid is ·63, and so on.

I have not applied this method to such small quantities, but the principle is found correct in experiments in the lead-chamber, and I hope to make use of it a little further.

The method by measuring the candle is similar, but less exact; a longer time must be allowed for burning, but the edges being unequal, great accuracy in estimating short lengths is prevented.

Temperature affects all plans: there must be no great deviations in this respect, or the rudeness increases.

Effects of Moisture.

It is probable that the amount of moisture in the chamber during the experiment alluded to in Table 6 is seldom found in the mines, but the amount in Table 5 is, I believe, found. Not having made in the mines any observations of the kind, the comparison is incomplete. In both the experiments the moisture was at last brought to saturation, in one at an early period. It is true that moisture at a high temperature is oppressive, but when the temperature falls it is a great agent of purification. The organic matter was wonderfully well washed out after the temperature had fallen; it was, in fact, dissolved in the dew. Even when the temperature was high the organic matter was less sensible: this, no doubt, because there was during the whole time a deposit of dew on the substances in the chamber.

The same thing occurs on a summer evening: the organic matter is removed, but unfortunately it is brought down for removal, so that in places where it has been abundant and impure the results are disastrous. After the air is cleared of this deposit, it must be much more wholesome. This effect of moisture in purifying the air is observed in the tables of carbonic acid for Manchester and other places; it is also perceived in the oxygen tables; and the difference was also found several years ago when examining the air before and after rain, by means of permanganate of potash.

This purification of the air by rain is so readily seen to be a matter of course, that it is rather to be wondered at that no one has more fully examined the subject. When we look at the percentage of deaths in different countries in Europe, we are glad to see England stand so high—according to Oesterlen and Baudin, before any of the Continental States, except Norway and Sweden and

parts of Switzerland; and yet England has a proverbially moist climate. (Norway and Sweden have more dry air. We may see there the silvering of the trees on the hills when the sun is shining behind, and the view is from the shaded part of the hill. This a friend tells me he saw in Scotland in the winter 1869-70, during intense cold, which I suppose dried the atmosphere.) When we leave England and go to west Scotland, a much moister climate, we find the death-rate lower. Does the rain cause this increase of vitality by washing out the impurities in one case and freezing them in another?

CARBONIC ACID AND VENTILATION.

The carbonic acid of the air increases with the activity of life and with artificial warming and lighting, as well as with animal and vegetable decay. It follows us everywhere, living and dead, and we can only get rid of excess by remaining in the natural currents of the air. We know it in certain conditions by a peculiar smell and taste which it apparently has when in conjunction with other substances, but of itself it has none, or at least very little. We enter into an atmosphere with $2\frac{1}{2}$ per cent. of carbonic acid, and we smell nothing; we enter into one with 4 per cent., and the candles are long ago out, and we smell only products of combustion or life. It has been a question if it really does any active harm, or is only negative to life. I have always considered it as remarkably innocent in small quantities, and been accustomed to look on the organic substances as the real evils; but the experiments in the close chamber have so far changed my mind, that I am inclined to think that carbonic acid has an injurious influence in small amounts. To prove this absolutely is not easy: we cannot readily

obtain an atmosphere for a long period containing different amounts of carbonic acid without organic matter existing in it at the same time. Such atmospheres do exist in nature, and in all probability there are persons living in them the statistics of whose lives would give us the required information; but this has yet to be collected. We can prove the bad effects of the minuter quantities of the organic matter, but the bad effects of minute quantities of carbonic acid exist as yet only on suspicion.

We can, however, reason in this way:—The carbonic acid increases constantly and regularly in a close place where men breathe. The organic matter does not do so; it deposits with moisture, it covers every surface, and we smell it when all the carbonic acid is washed out by fresh air. The air is not limited in its capacity of holding carbonic acid; it seems to be so with regard to organic matter. Breathing in a closed space, the capacity of the air to support life diminishes regularly, and the capacity to support a candle diminishes also, in a manner more consistent with the idea of the influence of the acid air than the absence of oxygen. There is much oxygen still left when the candle goes out, and it will be seen by Diagram 2 that the air when freed from carbonic acid is at once able to support a candle with much more vigour.

We do not know enough about the absorption of oxygen by the blood independently of oxidation, but supposing the laws of absorption by water to have some influence, although the proportions absorbed by the two liquids are very different, we see that the amount of carbonic acid absorbed increases with great rapidity when the amount of gas present is increased.

For example, in water let there be ·1 of carbonic acid, 20·9 of oxygen, 79· of nitrogen; using Bunsen's tables for calculation.

The amount of these gases absorbed by water at 68° F. or 20° C. will be ·09914 carbonic acid, ·59142 oxygen, 1·10837 nitrogen; but let the carbonic acid be 4 per cent., as was nearly the case in one experiment, the amount absorbed would be 3·6056 carbonic acid, ·4824 oxygen, 1·1083 nitrogen. It is to be observed how rapidly the carbonic acid rises, whilst the oxygen diminishes slowly. The numbers will not be exactly the same at blood heat, and blood may act differently independently of its power of retaining oxygen far beyond that amount which water can retain.

It is to be remembered, however, that oxygen is not absorbed by the blood merely according to the laws of absorption regulating the amount taken up by water; that is, the amount absorbed is retained chemically, and the blood is then able to take up another portion. The first act is probably physical, and although the next is chemical, the ordinary law of absorption will in all probability have a very great influence on the rapidity of the act of supply. Liebig does not reason entirely in this method, but his words on this subject are of great weight and value, and explain it more fully and clearly than has elsewhere been done. His chapter in the 'Letters on Chemistry' has great beauty and power. I shall here quote a portion of it, remarking only that it does not seem well proved that the increase of oxygen has little influence. Liebig says: —

'Moreover, if the quantity of oxygen that can be absorbed depends, according to a certain law, on the amount of the carbonic acid to be expelled, it is clear that the increase of the amount of oxygen in the air must be altogether without influence on the respiratory process. This remarkable fact has been satisfactorily ascertained by Regnault and Reiset in their admirable researches. They found that animals living for 22 to 24 hours in an atmosphere containing twice or thrice as much oxygen as

the air, experienced no kind of uneasiness; and that the products of respiration, in their quantity and relative proportion, were exactly the same as when the same animals lived in common air.

'It is further evident that the amount of carbonic acid in the air is one chief obstacle to the separation of that gas from the blood, and, therefore, an obstacle to the absorption of oxygen. When the amount of carbonic acid increases, the absorption of oxygen is impeded, even when its quantity remains the same. It is only by a corresponding addition of oxygen that this injurious effect of carbonic acid can be counteracted. Such an increase of oxygen never occurs under ordinary circumstances; but Regnault and Reiset have observed that animals could live in air containing one and a half to twice as much oxygen as common air, even if the amount of carbonic acid were so great as from 17 to 23 per cent., that is, about 400 times greater than in common air; and this, without any injurious effect, after 22 to 26 hours. Such a proportion of carbonic acid in common air is absolutely incompatible with life.

'The fact that men and animals die very rapidly from inhaling pure carbonic acid, while they live comparatively much longer in nitrogen or hydrogen gases, is explained by this—that in an atmosphere of carbonic acid the blood cannot give off any portion of that gas, but, on the contrary, absorbs more of it, by which the small proportion of oxygen in venous blood is expelled from the blood, and consequently its vital functions are much impeded, nay arrested.

'The condition most favourable to a rapid and perfect formation of arterial blood, and a more accelerated expulsion of carbonic acid from the venous blood, is, consequently, a rapid change of air in the air-cells of the lungs.

'When the inspired air has the same composition as

that which is exhaled, the object of respiration is no longer attained. The expired air is used air, which cannot a second time perform the same functions in the lungs. The venous blood is no longer changed into arterial; difficulty of breathing, and finally suffocation, soon come on, just as if the mouth and nose had been closed.

'In this case, death is determined by two causes. One is, beyond doubt, the deficiency of oxygen; the other is the presence of carbonic acid, by the presence of which the absorption of oxygen is impeded. In one of the experiments of Regnault and Reiset, a dog, three years old, in an atmosphere—the amount of oxygen in which had fallen to $4\frac{1}{2}$ per cent., while that of carbonic acid was $9\frac{3}{4}$ per cent.—fell into death-like convulsions; but he soon recovered in pure air, and in half an hour was as lively as before. In these experiments the carbonic acid given out from the lungs was for the most part removed, in the confined space in which the animal breathed, by solution of potash, introduced along with the animal.

'If we reckon, in the state of rest, 15 respirations per minute, and for each 31 cubic inches of air (English measure), and in the expired air 5 per cent. of carbonic acid, and 15 per cent. of oxygen, we easily find that a man in 24 hours produces 540 litres or about 19 cubic feet (English measure) of carbonic acid, consuming in the same time 10,800 litres, or about 380 cubic feet, of air.

'In a closed space, 8 feet high, 9 long, and 8 wide, a man could not breathe 24 hours without uneasiness. At the end of that time the air would have the composition of expired air; and if the patient remained longer in the same air, a morbid state, and finally death, would ensue. Lavoisier and Seguin found that the carbonic acid of expired air when again inspired, may be raised to 10 per cent., but not beyond that quantity, even when

respiration was continued, which it could only be for a very short time. This proportion of carbonic acid may be regarded as the limit at which life is endangered in man.' (p. 360, *et seq.*; 4th ed.)

Here I would remark that the experiment in Table B, showing that the amount of carbonic acid given out was as much when the air had been breathed for five hours as at first, is a companion experiment to that of MM. Regnault and Reiset, when it was found that twice or thrice the usual amount of oxygen did not hurt animals. But I may be allowed to remember that the experiments here recorded were performed on human beings who could register their sensations. All those made with animals take cognizance only of extreme results, such as spasms or approaches to death. The uneasiness comes on before the air has the composition of expired air; when it reaches that point the discomfort is very great indeed; and if the air may be raised to the point of having 10 per cent. of carbonic acid, it must surely be by breathing small quantities; at least it seems to me impossible to endure 4 per cent. for any length of time. I believe this may be done by breathing air from a bladder, blowing in and re-breathing for a short time; but we cannot try this plan long, and such experiments are of little value.

Liebig also says, alluding to the subject, 'These variations in the density of the air from temperature, evaporation, or pressure, exert no perceptible influence on the quantity of oxygen taken up by the blood in every second of time.'

M. Em. Fernet,[1] speaking of the organic substances in blood, says, 'The volume of carbonic acid absorbed remains equal to one and a half that which pure water would absorb under the same circumstances. On the contrary, in the absorption of oxygen there is observed a very small diminution of the co-efficient of solubility,

[1] *Comptes Rendus*, vol. 46, p. 623.

and a notable augmentation of the volume absorbed, independent of the pressure. The absorption of oxygen by the serum is on the whole regulated by the law of simple solution.' On p. 675, 'The volume, chemically combined, is almost five times the volume dissolved under atmospheric pressure.'

Meyer and Fernet found that blood took less air up at low temperatures than at high. The contrary is the case with water; but it is clear that if the blood does take up oxygen according to pressure, it retains it by another power, and this is exercised more at a temperature equal to or above animal heat.

Muller [1] has done interesting work on this subject, and has shown that an animal can reduce the amount of oxygen almost to disappearance before dying. But his experiments were made on animals confined in very small spaces or bottles, the air not being equal to the bulk of the animal itself. The results, although interesting, do not bear on the subject of health. He concludes that the removal of two-thirds of the oxygen does not affect life, and explains thereby the possibility of living in the rare atmosphere of mountains. But that which he called no effect was an observation of ten minutes; and I can bear witness with myself and others that such a short experiment is not sufficient. That which may be borne for many hours, yet depresses deeply, and changes are taking place during the time in the body and mind, which many hours afterwards of fine air are required to obliterate.

That the difference of a small amount in the proportion of oxygen should be perceived in the burning of a candle, whilst the difference of a great amount in absolute quantity is not equally perceived if this difference is caused by rarefaction, points rather to the importance of the proportion of the elements being preserved. The

[1] *Annalen der Chemie und Ph.* vol. 108.

23rd conclusion of Vierordt seems to be the true one: 'The quantity of carbonic acid given out into the lungs increases or diminishes with the amount in the lungs. If the lungs were to be cleared entirely of carbonic acid by the addition of fresh air, the freshest air would clear them most rapidly; but use air with carbonic acid, and near the end of the process the amount of air required would be very great to make any change.' This is easily seen by Table 16, showing the very great amount required to ventilate. For a similar reason the time required to change the composition of the mixture of gases in the blood, granting it to be changed, would be considerable, and impure air, which would finally kill, might be borne with for a time, although at first scarcely perceived.

When the air has been reduced to 18 per cent of oxygen, and the carbonic acid removed by caustic soda or lime, leaving then an atmosphere with 18·5 per cent. of oxygen, the difference is great both in the candle and in man; the breathing becomes pleasant, and the candles burn better. (See diagram.) Dr. Frankland's experiments on Mont Blanc show that, by diminishing the oxygen to the extent of the increase of carbonic acid by means of rarefaction, the combustion is not lessened.

I do not know that the passage of carbonic acid through moist membranes has been applied to any condition found in the lungs. The absorption may be again referred to, in order to explain the action of carbonic acid, supposing it to keep in contact simply with a liquid such as water which will absorb it. We find that with ·1 per cent. carbonic acid gas, the absorption is ·09, and of oxygen ·533, but how rapidly the rise is made on the side of the carbonic acid. When we reach ·2 the amount of carbonic acid is doubled, and that of oxygen scarcely altered. With ·1 carbonic acid the absorption is 15 per cent. of the absorption of the oxygen; with ·2 it is 30 per cent. The tables for this in

the original have no value except to show how careful we ought to be. The great capacity the blood possesses of absorbing oxygen, puts these calculations aside. On the actual amount of absorption I do not enter.

Knowing as we do the necessity that blood should give out carbonic acid, it is easy to believe that a constant exposure to much of it would prevent this action.

If we shake a volume of common air containing ·04 per cent. of carbonic acid with water the amount of oxygen dissolved is much greater than that of the carbonic acid; but let the carbonic acid rise only to ·6, and the amount dissolved would be greater than the amount of oxygen; when we arrive at 2 per cent. the amount of carbonic acid dissolved is actually more than three times the amount of oxygen; at 4 per cent. it is eight times. This estimation is for water, whilst the oxygen in respiration is absorbed by blood; and this is for 68° Fahr., whilst that of respiration is nearly 30 degrees higher; but although there are experiments on the absorption of oxygen by blood, the knowledge is not in such a state as to admit of decisive replies. It does, however, admit of illustration. R. F. Marchand, and after him Fernet, found that the absorption of oxygen by the blood did not produce instantly carbonic acid,[1] the oxygen is carried into the blood and gradually acts upon it. Oxygen has been found along with carbonic acid in the blood, and in greater quantity. Meyer's experiments in 'Watts's Dict. of Chemistry' give from two to three times more oxygen than carbonic acid; Bernard found in a dog's arterial blood 19·46 of oxygen.[2] Much dispute has arisen whether the oxygen was combined or absorbed; without inquiring into the mode by which it is retained, it seems probable that the act of absorption is first physical. The blood has in reality no pure air

[1] Kopp's *Jahresb.* 1858.

[2] Ibid. p. 260.

exposed to it, as Vierordt has shown that in the deepest portions of the organs of respiration there is more carbonic acid. This explains why death does not at once occur; with a moderate increase of carbonic acid, such would immediately be the result if the absorption were exactly as in water. Still it is fair to draw this conclusion, that the more carbonic acid there is in the air, the more difficult will it be for the blood to keep its excess of oxygen.

The Table B shows that the formation of the normal amount of carbonic acid is no proof that there is a sufficient supply of oxygen, or that a place is healthy. There is evidently power lost in supplementing a deficient supply by great exertions. I must not attempt to speak as a physiologist; the chemical fact was well proved.

It would of course be much better if we could reason in a more direct way, and by experiment in a laboratory prove that the smallest amount began the mischief. It would be better, because people have more faith in that which goes on before their eyes rapidly; when the operation is extended through years they begin to lose sight of the cause; and besides this, other influences cause the argument to be more mixed.

In brewhouses the carbonic acid is evolved nearly pure, and I am inclined to think from enquiries made, and from a little more trial of it myself, that even when pure it is not well to have an amount so great as ·15 per cent., although ·19 is not perceptible to me. (See under 'Organic Matter.') This is going farther in that direction than I expected to go; even ·25 will scarcely show on a common photometer, and it is certainly not perceived by the senses in a direct way.

If we look at the diagram showing the effect of bad air on the burning of a candle, we shall see that the beginning is very rapid. The whole bearing of the experiment leads us to think that there is no point at

which the effects of the carbonic acid can be called null, but that the effect shades out as finely as the acid itself.

The proportion of the gases in the atmosphere is adjusted so that we can keep combustion freely under control. If we use very little more oxygen the rate of combustion is perceptibly increased; if we take any less than one per cent. (·8) the rate of combustion is diminished at least a third. If ·8 per cent. will have such a great effect, is it a strain on our reasoning to attribute a corresponding influence to one tenth per cent.? And if the absence of so little oxygen be felt, how much more will this be the case when it is replaced by an equal amount of carbonic acid?

It is extremely probable that the carbonic acid and oxygen are adjusted to the condition of the blood and lungs, or, as some would prefer it, that the blood has adjusted itself to the oxygen and lungs; the slightest excess of oxidation might in a few years destroy the smooth action of the organs; and it is not to be wondered at that some persons are more fitted for working as it were under low pressure, and if put into pure air give way, whilst others again require all the stimulus of the purest air and exercise to set them in harmonious action.

It has been said, and probably with truth, that animals seek inferior air when sleeping, and man also dislikes then a bracing air. It may be well to breathe less oxygen for a short time—such air, for example, as is got by putting the head under the bed-clothes. It is probable that it removes the stimulus caused by pure air, and calms one down to sleep; but this impure air by degrees becomes more and more unwholesome, and when the sleep has begun it seems more natural that the lungs should themselves adjust the amount of air required, by respiring more slowly rather than moving with rapidity

in impure air. But as few persons seem to regulate themselves so well, and as the stimulus required in active life is not required in sleep, it is probable that for many persons, if not all, the air may be with propriety diminished in oxygen to the extent of two or more parts in 10,000. If this is not the case, it will be difficult to explain many of the habits of both men and animals.

Viewing the subject from another point, it becomes in fact practically necessary to have this diminution. We cannot during sleep bear so much cold; we breathe less, and must be kept more covered, and probably cannot therefore bear equal ventilation. In conformity with this, it is universally agreed that the temperature shall rule the ventilation in domestic life—a law not [at all times] very bad, and in many circumstances quite sufficient. Could this measure be applied in mines? Could we use a fine thermometer so as to indicate the rise [of the temperature, and the need of more ventilation]? Mr. P. H. Holland proposes a differential thermometer with one bulb in the earth [of the mine]. But the value of this method would be limited to places in constant occupation. It would give no idea of the value of air which, although bad, had become cool and produced *cold damp*. [Air which has been rendered impure by combustion or breathing is heated, and rises; after a while it cools. The advantage of the heating is that it becomes lighter, and rises to escape; if this is not allowed it cools and falls, making, I suppose, what miners call 'cold damp.']

But let us ask the question—Will a loss of oxygen, amounting to ·1 per cent., make any change in the respiration if its place is not supplied by a hurtful substance (such as carbonic acid)? According to quotations already made, the blood seems to have a reserve of oxygen. It is limited in its power of absorbing that gas, but if you give it more oxygen it can bear more carbonic

acid, as Liebig's quotations from Regnault and Reiset prove. Every addition, therefore, of oxygen gives a corresponding amount of resistance to the carbonic acid poison. Every removal, therefore, takes away the power of resistance to some extent.

On the other hand, the respiration is not affected sensibly by a small, or even by a considerable, diminution of oxygen, when the place of that gas is not taken up by others of a noxious character. The question,—Would the respiration or the health be affected if we lived continuously in air with a diminished quantity of oxygen? is not yet answered. It is against all our experience to believe that such an active agent would remain without character in the presence of organic matter. The only dispute that can be on the subject will turn on the amount of effect, whether, we may ask, will it be perceptible in days or years? If the oxygen should be ozone, I am inclined to believe in the power of very minute quantities, and that they will be perceptible at once. I think most chemists will agree to believe this, whatever their theory of the substance called ozone may be. If, however, the oxygen is the common gas, the proof of any rapid effect is wanting, although it is clear that there is no rapid injury. The question is not of much practical value, but it is very interesting theoretically.

The value of air with a diminished amount of oxygen was discussed, because of suggestions to wash out the carbonic acid and other impurities, instead of trusting to ventilation alone in far recesses of the mines. If the oxygen which moisture supplies were more than is required for breathing, might we not wash out the air of the mine with lime or soda, and have air again as good as at first. Liebig advocates the use of lime in sleeping apartments. It occurred to Dr. Bernays and myself to wash the air, and I made some experiments with that beautiful jet made by Sir Goldsworthy Gurney for

washing the air supplied to the Houses of Parliament. The water passes from a narrow tube and strikes a plate of metal, from which it is driven on every side in the finest dust, as it is to appearance, or, as the inventor styles it, 'Scotch mist.' Caustic soda was supplied in this manner, but it was found that very small pieces of floating matter stopped up the tube, and that very clear liquids only could be used. Such could not always be found. However, experiments were made in the lead chamber. The practice was sufficient to condemn the system; the floor and walls were covered with caustic soda or with lime, according as each was used, and the place was rendered too uncomfortable for daily life. The lime was used as milk of lime, and everything it touched was whitened. Lime-water was found to be too weak.

The lime became carbonated at the mouth of the tube and stopped progress, but cloths saturated and covered with alkaline substances were used for an experiment.

So far the question was answered without any uncertainty. *A candle burnt better*, as the diagram shows, and *the air was rendered pleasant to breathe.* Still it did not seem advisable in mines to manufacture air in this way, and I concluded that when it was bad it was better to replace it by that which was good and fresh.

SIMPLE TESTS FOR THE CARBONIC ACID IN HOUSES.

The Method called Minimetric.

TESTS FOR CARBONIC ACID AND OF VENTILATION.

Although the only impurity in air is not carbonic acid, as a rule the most convenient chemical test for ventila-

tion of rooms rendered impure by exhalations from the person is the presence and quantity of this gas. It will be seen on another page that baryta- and lime-water were tried for a long time in various ways, and after various stages became accurate in the able hands of H. Saussure; simple, and in theory completely accurate, in the hands of Dalton and Mr. Hadfield; and at last, with the greatest refinement, used as scientific instruments by Pettenkofer.

It is not pleasant to speak of the history of discoveries, as we so often find that much that seems new appears for the second time and not for the first; but it is extremely improbable that Pettenkofer knew of Hadfield, and it is very probable that Hadfield, whom I long knew, could not have carried out the refined experiments of Pettenkofer. Besides, the use made of the instrument by the Munich professor is more important than the instrument itself.

It was one of my duties, in connection with the Royal Mines Commission, to examine into the subject of tests, in order to find a simple method of determining the value of the air in mines. It was clear that my early test for oxidizable-matter was valueless in such places, and Pettenkofer's could not be used comfortably, or at least would not be used. More simplicity was required. There must be little to carry, little to do, and little to think of. Nothing better than baryta or lime suggested itself. The comparison of precipitates of lime, as Dr. Boswell Reid recommended, failed long ago, because the precipitates changed in physical appearance; but his mode of keeping the extent of the precipitate in the memory did not exactly fail, and was to be considered correct or otherwise, according to the memory, and according to the frequency of the experiment.

Equal quantities of baryta-water were poured into two bottles; air was blown into them from the lungs until a

decided precipitate formed, equal in both cases. The amount of precipitate was estimated by testing the amount of baryta still in solution. When this was done several times by two persons, the results were almost absolutely the same. Next day, these same two performed the experiment, relying on the memory of the precipitate of the previous day; and the results were that the oxalic acid required was 23·7 cub. centims., 23·2, and 23·2. The difference in one case is ·0005 gramme of carbonic acid, as every cub. centim. of the oxalic acid solution was equal to ·001 gramme of carbonic acid. This was repeated times without number, and served as a basis for a new mode of using the baryta- and lime-water tests. To this method of analysis I have given the name *Minimetric*. We ascertain the smallest amount of air required to produce a precipitate of a given density.

The same method can be employed to determine hydrochloric acid, sulphuric and sulphurous acids, sulphuretted hydrogen, &c.

Estimation of Carbonic Acid by Minimetric Analysis.

1. *For Definite Amounts of Carbonic Acid.*—If we shake a bottle containing 644 cub. centims. or 23 ounces of common air, we obtain a precipitate such as that described above. Now, if air containing twice as much carbonic acid were to be put into the bottle, the precipitate would be twice as great, but we could not ascertain its value by the eye. We cannot even make a probable approach to it. If, however, we used a bottle just half the size of the first, the air being still twice as bad as the first specimen, we should have a precipitate exactly the same, because in fact the amount of carbonic acid would be exactly the same. If the air were four times as bad, we should then use a bottle four times smaller, and obtain a precipitate also exactly the same as the first; and so on

down to the smallest dimensions. I go here in the belief that, although we cannot approach at all closely when endeavouring to obtain the comparative value of two precipitates, *we can retain in the memory with great exactness the character of one precipitate of a given density*.

If, then, we wish the air of a place to be kept at any one given state of purity, we should require only to have a bottle corresponding to the amount of carbonic acid, and the trial could be made at once. This plan would not suffice for estimating the amount in any given air ; it would estimate only one amount ; but it would show clearly when there was more and when less.

When it was found so easy to remember a certain bulk of precipitate, it became important to know what bulk would be the most easily remembered. Must it be a minute quantity, such as a chemist would call a trace, or must it be a quantity such as we should call milky? Neither suffice. The first is too small for certainty ; the second has no translucency, or so little that we cannot judge of the amount that lies behind. The quantity will be expressed most clearly by saying that the liquid is turbid and still translucent ; but not so that you could read through it. Anyone may obtain it exactly by shaking a clear 23-ounce bottle with half an ounce of baryta-water in air containing ·04 per cent. carbonic acid ; and this may easily and frequently be done to aid the memory. To be more precise, it is a precipitate obtained by throwing down baryta with ·2515 cub. centim. of carbonic acid, or ·00224 gramme carbonate of baryta freshly precipitated in half an ounce of liquid.

Some may prefer less ; any amount will do if it is uniform.

TABLE I.—*To be used when the Point of Observation is the Precipitate described, page* 195. *Half an Ounce of Baryta-Water, containing about* ·08 *Gramme Baryta.*

Air at 0° C. and 760 millims. bar.

Carbonic acid in the air, per cent.	Volume of air in cub. centims.	Size of bottle, in cub. centims.	Size of bottle, in ounces avoirdupois.
·03	838	853	30·
·04	629	644	23·
·05	501	516	18·
·06	419	434	16·
·07	359	374	13·
·08	314	329	12·
·09	279	294	10·
·10	251	266	9·
·11	228	243	8·55
·12	209	224	7·88
·13	193	208	7·32
·14	180	195	6·86
·15	167	182	6·40
·16	157	172	6·05
·17	148	163	5·74
·18	139	154	5·42[1]
·19	132	147	5·17
·20	125	140	4·92
·21	119	134	4·71
·22	114	129	4·54
·23	109	124	4·36
·24	104	119	4·19
·25	100	115	4·04
·26	96	111	3·90
·27	93	108	3·80
·28	90	105	3·70
·29	87	102	3·59
·30	84	99	3·48
·40	63	78	2·74
·50	50	65	2·28
·60	42	57	2·00
·70	36	51	1·79
·80	31	46	1·61
·90	28	43	1·51
1·00	25	40	1·40
2·00	12	27	·95

In Table I. all the information actually necessary is given. Column 2 is for fine measurements in cubic centimetres, indicating the amount of air which will contain the carbonic acid necessary for producing the precipitate of

[1] This size of bottle gives no precipitate in air with ·04 per cent. carbonic acid.

baryta when the proportion is according to any number in the first column. Column 3 is the same number, with the addition of 14·16 cub. centims., or half an ounce, which is the space occupied by the liquid. This, then, gives the size of the bottle to be used. Column 4 also gives the size of bottle to be used, the numbers being avoirdupois ounces; fractions are not in all cases given, and are not required so minutely as they are given.

Perhaps in some cases it may be found more convenient to use those sizes of bottles which do not give any precipitate or milkiness when half an ounce of baryta-water is shaken up with the air in them. The sizes corresponding to various percentages of carbonic acid are given in Table II.

TABLE II.—*To be used when the Point of Observation is 'No Precipitate.' Half an Ounce of Baryta-Water, containing about* ·08 *Gramme Baryta.*

Air at 0° C. and 760 millims. bar.

Carbonic acid in the air, per cent.	Volume of air, in cub. centims.	Size of bottle, in cub. centims.	Size of bottle, in ounces avoirdupois
·03	185	199	7·06
·04	139	154	5·42
·05	111	125	4·44
·06	93	107	3·78
·07	79	93	3·31
·08	70	84	2·96
·09	62	76	2·69
·10	56	70	2·46
·11	51	65	2·29
·12	46	60	2·14
·13	43	57	2·01
·14	40	54	1·90
·15	37	51	1·81
·20	28	42	1·48
·25	22	36	1·29
·30	19	33	1·16
·40	14	28	1·04
·50	11	25	·89
·60	9	23	·83
·70	8	22	·78
·80	7	21	·75
·90	6	20	·72
1·00	5·5	19·7	·70

In order to use this Table, first in its application to ordinary circumstances in life, we may assume that a bottle holding 5·42 ounces will not give any precipitate in the air around houses if we live in a tolerably fair atmosphere. To try the experiment the bottle must be very wide-mouthed, so that we can put into it a rod covered with clean linen, and rub the sides dry and clean; we must then fill it with the air of the place, either by blowing in air with a bellows, or, better, drawing the air out with a pump, allowing that of the place to enter, or putting a glass or caoutchouc tube into the bottle, and inhaling the air out of the bottle, so that fresh may enter. No way is more exact than this, if care is taken not to breathe into the bottle. This care is not at all difficult to take, and no amount of apparatus can be more accurate than this method, if done intelligently. If the slightest amount of breath goes into the bottle, the process of rubbing clean and drying must be undertaken anew.

When the bottle is filled with the air of the place to be examined, add the half ounce of baryta-water, put on the stopper, and shake. If there is no precipitate, the air is not worse than ·04 per cent. When it is desired to ascertain if it really contains as much as ·04, then a bottle holding 7·06 ounces must be used.

We may be satisfied when the air around contains no more than 0·04, or it may be decided that a sitting-room shall not be allowed to contain more than ·06, ·07, or ·10 per cent. If the first, then a bottle holding 3·78 ounces is taken; if the air does not contain above ·06 per cent., there will not be any precipitate in the liquid. If it is allowed to contain ·10 per cent. (1 per thousand), and on some evenings many houses will contain this, then a bottle of 2·46 ounces is enough.

If in workshops ·25 per cent. is allowed, which I hope will never be the case, then a bottle holding 1·29 ounce is enough.

This plan does not enable us to make an analysis of air. The person to whom the care of the atmosphere would be committed would have only one bottle of the proper size, and would only require to see that the air never gave any precipitate with that size of bottle. The order might be given for any required purity, and by this test an uneducated man could tell when the amount of carbonic acid was too great.

For a private house the rule would be not to have the air above ·07 at most, better to have less.

The baryta-water need not be of any particular strength; a weak solution is sufficient. The strength used is given; but the precipitate does not differ when the water is stronger.

If, however, the water should be extremely weak, several times weaker than the above, there is a difference. The carbonate of baryta dissolves in the water to a very perceptible extent. The first precipitate made in baryta-water even by oxalic acid, although very white at the surface where there is much acid and before mixing, disappears on shaking to a perfectly transparent and brilliant liquid. This applies to very weak solutions; a solution five times weaker than the one given as an example would give incorrect replies on account of its weakness.

Hitherto baryta has been spoken of, and it may well be asked why lime should not be preferred. The same precipitate to all appearance may be got with lime-water. Tables III. and IV. are constructed for lime-water, on exactly the same principles as the former ones. It will be seen that lime is so soluble or so transparent that it requires three times more space or air from which to collect its equivalent of carbonic acid needful to produce the required opacity. This is, of course, an objection. Still, lime is to be had everywhere, and lime-water has not the poisonous properties ascribed to baryta-water.

TABLE III.—*To be used when the Point of Observation is the Precipitate described, page* 199. *Half an Ounce of Lime-Water, containing* ·0195 *Gramme Lime.*

Air at 0° C. and 760 millims. bar.

Carbonic acid in the air.	Volume of air, cub. centims.	Size of bottles to be used, cub. centims.	Size of bottle, in ounces avoirdupois.
·03	2566	2581	91·
·04	1925	1940	68·
·05	1540	1555	55·
·06	1283	1298	46·
·07	1100	1115	39·
·08	963	978	34·
·09	856	871	31·
·10	770	785	28·
·11	700	715	25·
·12	642	657	23·
·13	593	608	22·
·14	550	565	20·
·15	513	528	18·59
·16	481	496	17·42
·17	453	468	16·48
·18	428	443	15·60 [1]
·19	405	420	14·78
·20	385	400	14·08
·21	367	382	13·45
·22	350	365	12·85
·23	335	350	12·32
·24	321	336	11·83
·25	308	323	11·37
·26	296	311	10·95
·27	285	300	10·56
·28	275	290	10·21
·29	266	281	9·89
·30	257	272	9·58
·40	193	208	7·32
·50	154	169	5·95
·60	128	143	5·03
·70	110	125	4·40
·80	96	111	3·90
·90	85	100	3·50
1·00	77	92	3·22
2·00	38	53	1·86

[1] This size of bottle gives no precipitate in air with ·04 per cent. carbonic acid.

EASIEST PROPOSED HOUSEHOLD METHOD.

TABLE IV.—*To be used when the Point of Observation is 'No Precipitate.' Half an Ounce of Lime-Water, containing* ·0195 *Gramme Lime.*

Air at 0° C. and 760 millims. bar.

Carbonic acid in the air per cent.	Volume of air, in cub. centims.	Size of bottle, in cub. centims.	Size of bottle, in ounces avoirdupois.
·03	571	584	20.63
·04	428	443	15·60
·05	342	356	12·58
·06	285	299	10·57
·07	245	259	9·13
·08	214	228	8·05
·09	190	204	7·21
·10	171	185	6·54
·11	156	170	6·00
·12	143	157	5·53
·13	132	146	5·15
·14	123	137	4·82
·15	114	128	4·53
·20	86	100	3·52
·25	69	83	2·92
·30	57	71	2·51
·40	43	57	2·01
·50	34	48	1·71
·60	29	43	1·51
·70	25	39	1·36
·80	22	36	1·25
·90	19	33	1·17
1·00	17	31	1·10

It is worth observing that the proportion of lime and baryta is nearly as their atomic weights. Perhaps more minute observation would make it quite the same. It was supposed that lead might give a similar proportion, but the texture of the precipitate was entirely different, the particles much larger. This prevented it being used in a similar way, and obstructed the theory as well as the practice so far.

One of the main advantages of this process is that it requires no weighing and no measuring, and we may almost say no thinking; this idea is, perhaps, more fully carried out with the lime than with the baryta-water.

Lime-water may be prepared of the same constant strength so closely, that we may neglect the difference. Burnt lime is slacked with water and dissolved by shaking. It is then kept in a bottle to stand till it is clear. With baryta we are apt to make the solution unnecessarily strong, and so waste it, but the experiment will still be the same. Lime-water is common, but baryta-water could also be prepared cheaply if there were a demand for it.

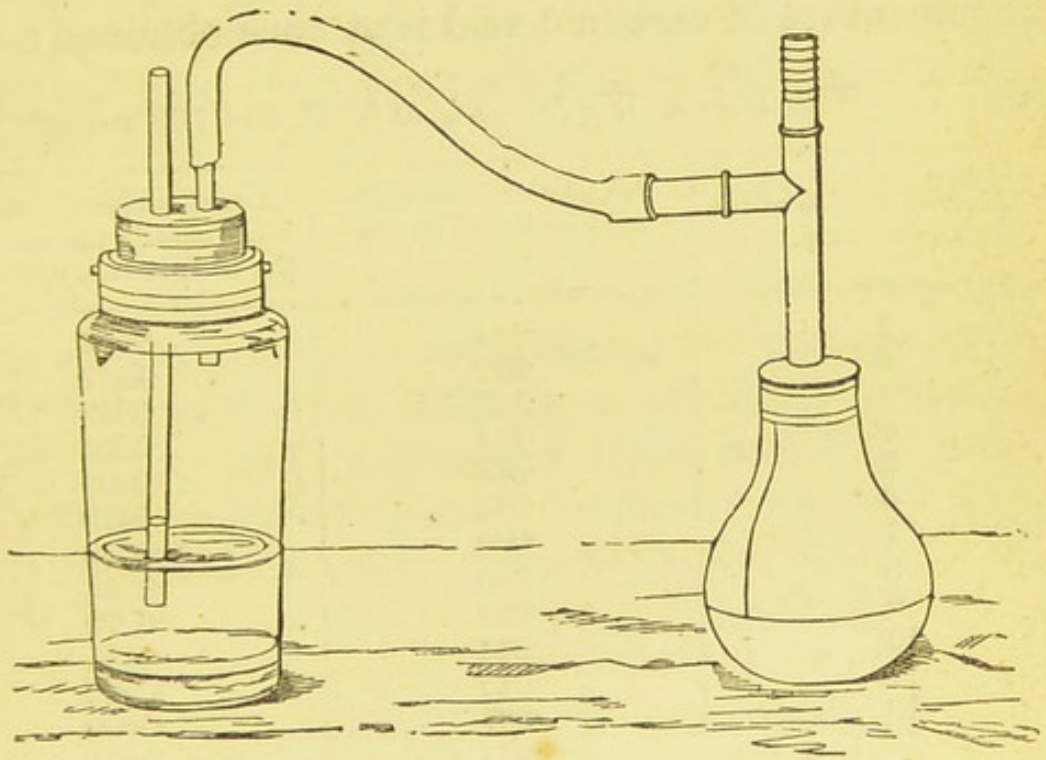
The use of this method would enable us to distinguish air in this manner; we could say, this is 6-ounce air, that is 4-ounce air, that is 2-ounce air; meaning that 6, 4, or 2 ounces of it cause a precipitate in baryta-water; and any person would understand it, and prove it easily, whilst vials so often seen in cottages might be converted into scientific instruments for sanitary purposes.[1] The clear lime-water ought to be drawn out of the bottle by a pipette holding the quantity.

2. *For all Amounts of Carbonic Acid, the Bulk of Air varying.*—If it be desired to ascertain the amount of carbonic acid in air of which we know nothing, the following less simple apparatus is proposed:—

An elastic india-rubber ball may be made to contain any given amount; let us suppose two ounces. When we press it in the hand we can drive out the whole air, or at least nearly the whole; we let it go, and it fills again. If a tube be fitted to it we can drive the air which has gone into the ball through baryta- or lime-water, and obtain the precipitate spoken of at the beginning. It is, however, preferable to cause the air to pass through the baryta- or lime-water before entering the ball; and this can easily be done. If the air be pure, it will require so many more balls to be emptied; if impure, a smaller number.

[1] The rule may be so stated: *Let us keep our rooms so that the air gives no precipitate when a* 10½*-ounce bottle full is shaken with half an ounce of clear lime-water.*

If a ball with a simple tube is used for blowing into a liquid, that liquid is drawn up as soon as the ball expands, and many fillings cannot be made without inconvenience. If, however, a valve be placed between the liquid and the ball, there can be no return. This is the case in the tube A in the annexed figure; connected with the ball, there is a valve such as is used in air-pumps, preventing all entrance of air, but allowing it to pass out. Then, again, on tube B there is a valve of the same kind, preventing all egress of air, but allowing it to enter. The little instrument is therefore in reality an air-pump and a condenser. The air passes in at B, and goes out at A.



It is well to use always the same amount of solution, which may be, as in the previous method, half an ounce. If this liquid is put into an open vessel there is an escape of the carbonic acid, which ought to be retained, and also a collection of some from the atmosphere, which ought not to be absorbed. It is better, therefore, to use a bottle as at C, with a small entrance-tube.

The bottle should have the same capacity as the ball + the space required for the liquid. Previous to commencing the experiment, it must be filled with the air of the place; this is done by one or two pressures of the ball. The liquid and cork are then put in their places, and the whole shaken up. That counts for one ballful of air. The ball is then emptied by pressure, and allowed to fill itself again through the bottle. The fresh air gives up most of its carbonic acid in passing through the liquid; but as a little remains unabsorbed, the bottle is well shaken, so that the liquid may absorb all the carbonic acid. The operation is repeated until the desired precipitate is obtained. The number of ballfuls being counted, on referring to a Table such as the following, the percentage of carbonic acid is at once obtained:—

TABLE V.—*With Baryta-Water. Air at* 0° *C. and* 760 *millims. bar.*

Number of strokes of the finger-pump.	Per cent. of carbonic acid indicated in the air.	Actual amount of carbonic acid in the air of the ball, in cub. centims.
1	·444	·2515
2	·222	·1257
3	·148	·0838
4	·111	·0629
5	·088	·0503
6	·074	·0419
7	·063	·0359
8	·055	·0314
9	·049	·0279
10	·044	·0251
11	·040	·0229
12	·037	·0209
13	·034	·0193
14	·032	·0180
15	·029	·0167

This Table is constructed for a ball of 2 ounces capacity; but of course any size of ball may be used, and a table constructed to suit it. For inferior air, above ·07 per cent., it is found advisable to use a ball of half the size.

We might call the apparatus a finger-pump, if no better name is suggested.

In using this ball it is well to observe the method by which the points of the fingers press into the centre. If this is followed, the whole of the air may practically be driven out. The ball is thus divided into two parts: one part is pressed between the palm of the hand and the finger; the other is pressed between the surface of the nails and the first joint of the thumb.

This apparatus requires a little experience to produce confidence. Assistants have tried it along with Pettenkofer's method, and obtained remarkably accurate analyses, with fewer errors. I have not yet used it in scientific investigations, and scarcely even practically, although with observing persons it is worthy, as I believe, of all confidence, and especially if used daily, as in certain proposed inspections would be the case.

For practical purposes the state of the barometer may be neglected.

3. *The Bulk of Air fixed, the Lime-water varying.*—There are many ways of ascertaining the amount of carbonic acid in the air with great precision. An experimenter may make any one of them perfect, if he will only continue to use it until familiarity ensues. Lime-water may be used according to Table VI.

TABLE VI.—*Neutralisation with Lime-Water.*[1] *Capacity of Bottle, 50 ounces.*

Air at 0° C., and 760 millims. bar.

Carbonic acid in the air, per cent.	Quantity of lime-water required.	Carbonic acid in the air, per cent.	Quantity of lime-water required.
	grs.		grs.
·03	120	·15	599
·04	160	·20	798
·05	200	·25	998
·06	239	·30	1197
·07	279	·40	1596
·08	319	·50	1995
·09	359	·60	2394
·10	399	·70	2793
·11	439	·80	3192
·12	479	·90	3591
·13	519	1·00	3990
·14	559		

Here it is proposed that a bottle of 50 ounces capacity should have attached to it a flexible ball filled with a known quantity of lime-water. A little is squeezed into the bottle, and shaken about until it becomes neutral; again a little more; and when there is no more carbonic acid to render more lime neutral, the operation ceases. But how are we to know when the liquid is neutral? Of the many substances tried for this, perhaps turmeric was the best—a little bit of turmeric paper floating on the liquid itself. One of my assistants, Mr. Clement Higgins, tried the turmeric in this way, and became very familiar with its use. The operation, however, was slow, and not satisfactory to me, although it can be made excellent with patience andattention.

The plan is not correct for large quantities of carbonic acid, because the liquid takes up so much of the vessel. This could be avoided by emptying it into another elastic ball; but I have not cared to employ it so.

The solution used by this method was lime-water ten

[1] The lime-water used is the ordinary lime-water diluted with 9 volumes of pure distilled and fresh boiled water.

times diluted. The manufacture of lime-water is a very good method of obtaining a pretty exact strength of a liquid without weighing. The lime-water which has plenty of lime at the bottom remains much the same. There is a little change occasionally; it would be well to determine the exact cause, and we might perhaps be able to start from the point of saturation, even for the most exact researches. The following results were obtained: —four bottles of lime-water took of oxalic acid—

	Cub. centims. solution.			
	1st	2nd	3rd	4th
April 7, 12° C.	19·5	19·6	19·5	19·6
After two days, 6° C. . .	19·4	19·3	19·3	19·4
" " 8°·4 C. . .	20·0	19·8	20·1	19·9
" " 15° C. . .	19·9	20·05	20·1	20·1
" " 13° C. . .	19·9	19·9	19·9	19·9
	19·9		19·9	19·9

This shows that for the precipitate, on the plan of Tables I. and III., lime-water is more than necessarily constant, and even enough for Table VI., and may be used in some cases to save the purchase of a balance.

Some years ago I proposed rosolic acid as an agent for ascertaining neutrality. It shows the neutral point with extreme sharpness when oxalic or any liquid acid is used, and we can tell to a small drop when to cease pouring. If all caustic solutions were coloured with it, it would help to point them out, and serve also instead of litmus or turmeric. I did not obtain such successful experiments as I could have wished when trying it with carbonic acid lately. The last traces of colour are difficult to remove; I hope to bring it on further some day. It is curious that lime- or baryta-water take up carbonic acid much less readily when rosolic acid is present. The latter portion of the experiment is affected chiefly when the solutions become weak. It is as if the resinous

character prevented absorption; when a liquid is used, such as oxalic acid in solution, the rapidity of action is very great, the same resinous quality causing the rosolic acid to shrink into its shapeless and colourless state with great suddenness.

It may, however, be remarked that weak solutions of baryta and lime take up carbonic acid very slowly of themselves, although rosolic acid renders the absorption still slower.

MANGANATES AND FERRATES AS TESTS FOR CARBONIC ACID.

In a former paper I mentioned that the carbonic acid of the air was capable of being estimated by observing its action on manganates. The green manganate was prepared in the usual way, or by adding caustic alkali to the so-called permanganates, which, however, from this action appear rather as bi-manganates. The amount of acid required to convert the green to red may be found readily by a solution of test acid, and the equivalent for carbonic acid calculated. The plan already described, with a ball from which the manganate is pressed, suits the test very well when the ball contains nothing which affects the salt. If the ball is objected to, we may adopt another method. This plan consists simply of a graduated tube like a pipette fitted into the cork of the bottle; a small quantity is allowed to flow down when required, and when the liquid begins to become purple, there being no more carbonic acid to render it red, the experiment is finished; the amount used is read on the graduated tube.

At Mr. Hutchinson's alkali works I obtained from Mr. Powell a salt of ferric acid accidentally formed. It was analysed by Dr. Roscoe. This salt was expected to be still more sensitive to organic matter than chameleon.

It was sensitive certainly, but it decomposed rather rapidly in light. It can, however, be used both for carbonic acid and oxidizable matter, in the same manner as the manganate. The action on light is, however, not quite sufficient to make the salt a good photometer. The objection to it for carbonic acid is that it does not keep well.

The aspirator may be used for drawing the air through any of these solutions, and either the ball apparatus or two small bottles for not very refined experiments. Even one bottle may be used with great safety, if it is covered from the air, and the speed not very great; the exact measurement of the air must be obtained. This proposal appears like a return to Dr. Reid's carbonometer, made somewhat more convenient and elegant, although not exactly quantitative, as I once thought.

There may be varieties of tastes, but I expect the first and second, or third and fourth, methods to be very much used; they are very simple. It is quite possible to add many other methods and modifications, but I know of none so simple as these.

ON SOME PHYSIOLOGICAL EFFECTS OF CARBONIC ACID AND VENTILATION.

In a report on the air of mines and confined places, there was given a chapter on the action of the pulse when carbonic acid accumulated in the air. It is proposed to repeat that chapter, and to supplement it with additional experiments. The experiments, when not otherwise explained, were made in an air-tight lead chamber described in the report alluded to. It may be well first to show the amount of carbonic acid exhaled. This will be done by giving the amount per cent. in the air of the chamber. This experiment was the beginning of the inquiry. I

expected that the amount of carbonic acid exhaled would diminish, and with it the amount of strength in the muscles; but the last point could not be reached by the methods employed. The amount of oxygen used is for the time the same, although there is less in the atmosphere. I shall not pretend to say how the health is affected further than this, that a change is observed in the respiration and the pulse. I must leave physiologists to find what mischief this will ultimately cause; but I cannot doubt that the circulation is diminished, and that the lungs endeavour to compensate for this by more rapid action. How much each person can bear of this change will depend on circumstances which, it appears to me, cannot at present be estimated.

TABLE B.—*One Person in the close Chamber.*

	Carbonic acid. 1st day.	2nd day.
After 20 minutes	·18 per cent.	·19 per cent.
„ 40 „	·32 „	·36 „
„ 1 hour	·49 „	·49 „
„ 80 minutes	·62 „	·64 „
„ 100 „	·74 „	·75 „
„ 2 hours	·88 „	·89 „
„ 140 minutes	1·06 „	1·03 „
„ 160 „	1·20 „	1·22 „
„ 3 hours	1·25 „	1·34 „
„ 200 minutes	1·52 „	1·48 „
„ 220 „	1·54 „	1·60 „
„ 4 hours	— „	1·81 „
„ 260 minutes	— „	1·98 „
„ 280 „	— „	2·10 „
„ 5 hours	— „	2·25 „

I have not had time to attend to the full explanation of each experiment, and some require a continuation of the enquiry; but this one must not be passed over without special notice. The amount breathed every hour is the same—no matter whether there be ·04 or 2 per cent. of carbonic acid in the air, and no matter although

there be 20·94 or only 18·8 of oxygen. This is strongly corroborative of the views taken by Liebig, but other circumstances tend on the other hand to show that this state of things is kept up by some alteration, to say the least, of other vital functions. There must of course be a limit, but I was afraid to go farther than I went. In one experiment the breathing was changed from 16 inspirations per minute to 22, the pulse fell from 76 to 55, whilst it was so weak that it was difficult to find. My assistant was in the chamber this time; I requested him to attend to his pulse and breathing, as on another occasion when there was still more carbonic acid in the air, namely, 3·9 per cent, my breathing rose up to 26 inspirations, and my pulse became so weak as to cause alarm. This had happened so regularly that it must be put down as the result of poisoning with carbonic acid. On one occasion there was a comparatively large amount of oxygen in the room, viz. 20·1. The carbonic acid had been driven in upon fresh air, and no oxygen removed. Even here the pulse was weak, although the breathing was not very difficult, and the candles burnt moderately well.

The conclusion is, that in the air containing an increased amount of carbonic acid, this gas alone, even without the other hurtful ingredients, such as organic matter, begins to poison in the manner indicated, and men exposed to it are really gasping for breath without knowing it.

As I came on this result at the end of the enquiry into the composition of the air of mines, it is not easy to do it justice. We learn much from it. We learn that the blood can take its oxygen out of very impure mixtures; but we learn also that some functions are meantime suffering greatly. It is, to my view, a most important thing to show that with an amount of oxygen not less than is found in the air of some mines, and an amount

of carbonic acid actually less, such extraordinary changes should result in the functions of a healthy man. We want no other experiment than this to prove great evil arising from impure air, either in mines or elsewhere.

In order to make similar trials in a shorter time, five persons entered the lead chamber, expecting to have in one hour effects similar to those obtained by one person in five hours. The figures are here given; it is seen that they are not exactly the same as previously. Time causes us to yield, although we may struggle against the evil influences for an hour or so. The pulse begins to be irregular very soon, and certainly when the air contains ·4 per cent. of carbonic acid, in three cases only ·2. It rises and falls, but at last begins to fall. The weakness is to be remarked.

With the younger it rose rapidly at first; this seems to indicate the more rapid struggle for life; with the more advanced it was a steady determination not to be changed by external circumstances, although they gradually caused a change at last.

These figures will probably induce many other persons to continue the enquiry.

May it not be useful to lower the pulse by this method in some cases? If so, must the experiment be tried with pure carbonic acid? How much was due to the carbonic acid, and how much to organic matter? All these are interesting questions, the answers to which are begun, and in them, so far as they go, we see the effects due to the want of ventilation.

TABLE C.—*Beats of the Pulse. Five Persons in the Chamber. Observations every 5 minutes.*

	A	B	C	D	E	Temp.
To begin . . .	60	78	84	70	73	68° F.
After 5 minutes . .	60	70	90	70	72	
„ 10 „ . .	59	76	90	75	72	
„ 15 „ . .	72	74	91	74	70	
„ 20 „ . .	70	74	89	74	72	
„ 25 „ . .	79	77	91	71	74	
„ 30 „ . .	74	81	89	70	71	
„ 35 „ . .	78	79	87	74	68	72° F.
„ 40 „ . .	73	76	89	76	70	
„ 45 „ . .	70	70	90	73	72	
„ 50 „ . .	74	72	89	72	71	
„ 55 „ . .	70	73	89	72	70	
„ 60 „ . .	66	73	88	73	72	
„ 65 „ . .	66	74	88	72	70	
„ 70 „ . .	69	73	86	71	69	
„ 75 „ . .	70	70	85	71	70	
„ 80 „ . .	73	70	86	70	69	
„ coming out 5 minutes	66	68	89	68	68	
„ 3 hours . . .	63	74	84	74	73	
„ „ . . .	61	75	85	74	73	
Number of Respirations.						
Normal	20	15½	22	20	20	
After 33 minutes . .	24	16½	25	20	25	
„ 58 „ . .	23	17	25	22	24½	
On coming out after 5 min.	20	16	23	19	21	

After 5 *minutes.*

Organic matter not pleasant.

After 15 *minutes.*

A. Pulse stronger and quicker.
B. Irregular pulse, but strong.
C. Weaker, and already difficult to feel.
D. Same to the feeling.
E. Much weaker.

After 25 *minutes.*

A. Stronger.
B. Irregular.
C. Irregular and weak.
D. Irregular.

After 45 *minutes.*

Organic matter less sensible than at first to the majority.

D feels air to be bad.

After 50 *minutes.*

A can scarcely feel his pulse; several attempts made to count it. Still feels quite well.

B begins to fell head uneasy.

C feels his heart beat more than usual.

D. Pulse weak.

E. Pulse very weak.

Here every one was observed to be sighing, although all were cheerful.

After 60 *minutes.*

B. Flushed.

C and D. Headache began slightly.

The effect of company perhaps prevented the lowering of the pulse by keeping the mind cheerful.

The force of the pulse is shown to be lessened; to what extent has not been measured.

This experiment differs from that on Table D. The impure air was formed five times more rapidly, but the results were not so perceptible. It would appear that we can resist for a short time when we cannot resist for a long time.

The irregularity of most of the pulses is apparent.

A was the youngest, being about 17, and having a naturally low pulse; his was raised.

B was about 21 years old; his pulse went lower, then higher, then finally lower.

C, about 24; his pulse went higher, then sank to nearly its usual point, but he was the most affected in sensation.

D, 27; his pulse went higher and then lower.

E, 47; his pulse went lower, higher, and lower, but he felt no discomfort; forehead began slightly to warm.

It is remarkable that the breathing increased in all cases, and that it went back to its normal amount very rapidly.

TABLE D.—*One Person in the Lead Chamber. Respiration and Beats of the Pulse taken every* 10 *minutes.*

Time.	Pulse.	Respiration.	Temperature, Celsius.	Carbonic acid in the same periods.
h. m. 10 55	73	15·5	18°·2	·04
min.				
After 10	73	16	18°·2	·114
„ 20	72	16	18·2	·187
„ 30	71	17	18·4	·261
„ 40	71	16	18·4	·335
„ 50	70	16	18·5	·408
„ 60	68	16	18·6	·482
„ 70	67	16·5	18·7	·556
„ 80	67	17	18·8	·629
„ 90	66	17	18·9	·703
„ 100	65	18	19·0	·777
„ 110	65	18·5	19·0	·850
„ 120	64	19	19·0	·924
„ 130	63	19	19·2	·997
„ 140	62	19·5	19·1	1·071
„ 150	62	20	19·1	1·145
„ 160	62	20	19·1	1·218
„ 170	61	20	19·1	1·292
„ 180	60	21	19·1	1·366
„ 190	60	22	19·2	1·439
„ 200	59	23	19·2	1·513
„ 210	58	24	19·4	1·587
„ 220	57	24	19·4	1·661
„ 230	57	24	19·4	1·734

TABLE E.—*When the door was opened.*

Time.	Pulse.	Respiration.
After 10 minutes	59	22
„ 20 „	59	19·5
„ 30 „	60	19
„ 40 „	60	18
„ 50 „	60	17

TABLE F.—*Sitting quiet for an hour in the Lead Chamber in pure air.*

Time.	Pulse.	Respiration.
4h 50m	75	17
After 10 minutes	76	17
„ 20 „	76	17
„ 30 „	76	17
„ 40 „	77	17
„ 50 „	76	17
„ 60 „	76	17

From this we learn that the same quiet condition in pure air produced no change.

Experiments B, C, and D, on the beats of the pulse, seem decisive. The air affects the pulse when the ventilation is such that the amount of carbonic acid reaches ·18. The question of carbonic acid and organic matter, viz. which is the most hurtful, must be decided by other experiments. My belief is that much is due to the carbonic acid, because the progress of the pulse downwards is so regular, and I believe that the organic matter does not increase so regularly. This may not be true at the temperature given, and is another point to be ascertained.

But, leaving out all the details, the great broad fact remains, that carbonic acid and other emanations from the person diminish the circulation, and hasten the respiration, and that the effect is perceptible in a very short time when the percentage of carbonic acid reaches ·18, or say one-fifth of a per cent. certainly. If, however, we do not wish to infer too much from one beat of the pulse, let us, for rough practice, say $\frac{1}{4}$ per cent. We may infer also that smaller quantities will show their consequences after a longer time.

EFFECT ON THE PULSE AND BREATHING.

Artificial carbonic acid being inhaled along with the organic exhalations of the body.

1 per cent. of Carbonic Acid.

	Pulsations.
	68.
After 5 minutes	68.
„ 12 to	68, 70, 70, 70, 69, 70.
„ 22 to	70, 70.
„ 30 to	68, 68, 66.
„ 34 to	65, 65, 66, 66, 66.
„ 42 to	67, 68.
„ 51 to	66, 66.
„ 60	64, 63, 63, 63, 63, 63.

2 per cent. of Carbonic Acid.

	Pulse.	Inspirations.
		18
After 5 minutes . .	64	19
„ 10 „ . .	66	19
„ 15 „ . .	65	20
„ 20 „ . .	64	20
„ 25 „ . .	63	21, pulse very weak.
„ 30 „ . .	62	21
„ 35 „ . .	63	21
„ 40 „ . .	64	21¼
„ 45 „ . .	63	22
„ 50 „ . .	62	22¼
„ 55 „ . .	60	23
„ 60 „ . .	61	23
„ 65 „ . .	60	23¼
„ 70 „ . .	60	23¼

2 minutes after coming out 68.

Here the pulse was very much affected even in the number of beats, but the effect was observed principally in its great weakness: it sometimes tried to recover its number, but this was not observed to take place with regard to the strength.

3 per cent. of Carbonic Acid.

	Pulse.	Inspirations.	
	67	17	
After 10 minutes .	67	21	Acidity perceptible
„ 15 „ .	65	21	to the smell.
„ 20 „ .	63	22¼	
„ 27 „ .	62	23	

Here the pulse became so weak that it was difficult to count the beats. There was also a very unpleasant feeling. The door was opened, and two other young men entered. Of course a good deal of carbonic acid was removed, but not more than from $\frac{1}{4}$ to $\frac{1}{2}$ per cent. In ten minutes the pulse of the eldest, B, fell from

79	Inspirations rose from 18
to 75	to 22
Unpleasantness felt.	

Here, as in the experiment recorded previously, the pulse of A rose.

At first it was 63	Inspirations 21
It rose to 69	rose to 25

A's pulse very feeble. There is always a slight rise at the beginning. This rise was very decided in the case of A. It always results in a fall, and would no doubt have done so in this case had A remained longer. This, however, would not have been safe, as, even in these two minutes, his pulse was almost imperceptible, and he could not count it himself.

In the above cases the persons who breathed sat in the lead chamber, and of course the organic matter from their bodies escaped into the air around them. Still we know that the organic matter would not produce these effects without the carbonic acid, simply because when we remain in the chamber much longer without pouring in carbonic acid the pulse does not become so weak, whilst the organic matter is of course accumulated to an extent much greater than it could have been with artificial carbonic acid.

Whilst I gave abundant credit to the organic matter for doing evil, I could not refuse to blame the carbonic acid; but as a friend was still dissatisfied with the argument if applied to smaller amounts of carbonic acid, the following experiments were made. In them the organic matter is entirely excluded. For the first, in which I

per cent. of carbonic acid was mixed with the air, several aspirators of flexible material were filled with the mixture, and the air was inhaled from the aspirators by the mouth, whilst it was exhaled from the nostrils. The carbonic acid was made from bicarbonate of soda, and passed through a solution of bicarbonate of soda to remove mineral acids.

With 1 *per cent. of Carbonic Acid.*

	Pulse.
	66
After 2 minutes	67
„ 4 „	67
„ 6 „	68
„ 8 „	67
„ 10 „	68
„ 12 „	67
After 14 minutes	66
„ 16 „	64
„ 18 „	64
„ 20 „	64
„ 22 „	63
„ 26 „	63

In this experiment the difficulty of supplying air was felt to be considerable; and the respirations having become less agreeable and regular, they were not counted.

In order to remove all difficulty, the lead chamber was charged with the mixture to be breathed, and the operator sat outside inhaling the air through a wide tube with ease. Of course a similar amount of air entered, and this was supplied through some small chinks, which were not carefully filled up. The change taking place in the air of the chamber from this latter cause only would scarcely be perceptible in half an hour, and then it would be against the success of the experiment. The uniformity of results is therefore very remarkable.

With ·5 *per cent of Carbonic Acid.*

	Pulse.	Inspirations.
	76	17
After 5 minutes	76	
„ 10 „	76	
„ 15 „	75	20
„ 20 „	73	
„ 25 „	71	
„ 30 „	71	22
„ 35 „	71	
„ 40 „	71	24

With ·25 per cent. of Carbonic Acid.

	Pulse.	Inspirations.
	70	17
After 5 minutes	72	
„ 10 „	73	
„ 15 „	72	19
„ 20 „	70	
„ 25 „	69	
„ 30 „	69	21

Here a disturbance is seen at once, more fully in the breathing. Owing to the mode of draining the reservoir of air breathed, it would not have been fair to proceed further.

With ·1 per cent of Carbonic Acid.

	Pulse.	Inspirations.
	73	18
After 5 minutes	72	
„ 10 „	73	
„ 15 „	73	19
„ 20 „	72	
„ 25 „	71	
„ 30 „	72	
„ 35 „	73	19
„ 40 „	73	
„ 45 „	72	19
Average	72·4	18·75

Here there is a disturbance perceptible of two on the pulse; and I may say that the experiment is scarcely fair after 25 minutes. The disturbance on the inspirations is more uniform. It is, for example, more perceptible than in the next case.

Pure air was breathed in the same position as in the previous cases, D sitting outside the lead chamber, which had been well ventilated. This experiment was made in order to ascertain the influence of breathing through a tube, as it was feared lest some mechanical difficulties might have interfered with the value of the trials. The result shows that no such difficulties occurred. There is a little diversity of one above and one below the average of the pulse, and the breathing is a little lower in one

case, instead of being resolutely higher as in every other case given, even when so little as one-tenth of a per cent. of carbonic acid was used.

Ordinary Atmosphere in the Lead Chamber; breathing through the tubes as before.

	Pulse.	Inspirations.
	74	18
After 5 minutes	74	
„ 10 „	75	
„ 15 „	74	18
„ 20 „	74	
„ 25 „	75	
„ 30 „	74	8
„ 35 „	74	
„ 40 „	74	
„ 45 „	73	17
„ 50 „	74	
„ 55 „	74	
„ 60 „	75	18
Average	74·1	17·8

In a report on the air of mines I discussed questions relating to the absorption of oxygen and poisoning by carbonic acid, quoting several opinions of eminent chemists. The important point is this: How can the blood be influenced by a diminution in the amount of oxygen to the extent of ·1 or even 1 per cent.? Liebig says, 'In a closed space 8 feet long, 9 feet high, and 8 wide a man could not breathe 24 hours without uneasiness.' This is equal to living about 5 hours and a half in my lead chamber; and in that time, by sitting quietly, we may avoid uneasiness; but the air will be very bad, candles will scarcely burn, some will go out, and any person entering suddenly will feel unwell. When the air gradually deteriorates, nothing striking is observed; the senses fail to guide us well. If we look at the important total acts, the circulation of the blood and the respiration, we find that death has begun, so to speak, and the life is going out as quietly as the candle.

Nearly all the usual experiments on breathing in

impure air have been made violently and not with small amounts of impurity, or during a very long interval of time; a rabbit has been killed in a few minutes, and the same air has been breathed until it has attained its maximum impurity. (See p. 179, &c.)

The danger to life of any amount less than 4 per cent is not immediate when the person is healthy, as trial showed. The constant lowering of the pulse, even in much less impure air, must have a gradual effect on the vitality; which effect will be seen in some persons in a few hours, in some after days, and in others perhaps years. It is probable that to live during the whole 24 hours of the day in any air containing above 1 per cent. would bring results on the health very rapidly; but no men are exposed to this, so far as I know; the usual exposure is only for three or four hours, seldom during the whole working time; and even with this the pulse is kept permanently low, as will be seen in Dr. Peacock's report.

Now comes the question: If the oxygen of the air is taken into the blood by chemical affinity, why should the presence of carbonic acid affect it, and why should it be a matter of importance whether the amount of oxygen be small or great in the air breathed?

1st. The absorption cannot be wholly chemical; it must follow the physical laws of absorption, if we may so call them. It will be in proportion to the bulk of the two gases presented to the liquid. The smallest increase of either gas will make a difference.

2nd. If the absorption were purely chemical, knowing as we do that the work of absorption must be done rapidly, the amount absorbed must still depend on the amount presented.

3rd. In either case it will require a certain quantity of oxygen to drive out the carbonic acid.

If blood contains 10 per cent. of oxygen and 5 of carbonic acid, add one per cent., or one-tenth, or one-

hundredth per cent. of oxygen more, and a certain amount of carbonic acid will be removed.

Viewing blood as a liquid like water, this would be the case, I suppose, if we gave it time. Viewing it as a chemical solution, it would be still more the case. If we add oxygen to protocarbonate of iron in water, the carbonic acid is driven out in proportion to the rapidity with which the oxygen is absorbed, and of course the oxygen is absorbed with greater rapidity if the liquid contains less carbonic acid.

If, again, we view the blood and the membranes rather as porous bodies, we have the question still more clearly answered; and there are reasons why we should believe the action somewhat to resemble the action of these bodies. Whenever charcoal, a porous body, is filled with one gas and is put into another, a certain amount of the first is driven out with great force; the result is not a mere mixture taking place quietly, but an instant forcible diffusive and absorbent action. If we view the carbonic acid as driven out by the oxygen, taking any of the three views, the actual amount of the gases present must be of the greatest importance.

There is room for a rediscussion of the subject.

If we consider the effect of even one beat of the heart in a minute in a mechanical point of view, we need not be surprised at a change of result in the health. If the amount of blood sent by the heart is three ounces, we have, for every beat of the pulse lost per minute, a diminished circulation of many gallons of blood per day; for every beat less, the corresponding amount is taken from the circulation. But even this is not the whole difference, because the beats become excessively feeble in breathed air. In one case especially the pulse was raised, not sunk; and in most cases it was raised a little for a short time at first, as if an inferior blood were endeavouring to do equal work by moving more rapidly.

Medical men have objected to the argument that any evil result can arise from these effects, saying that man is formed so as to resist such influences, and is not so weak as to be confined within such small limits. When the ground gives way under a man he cannot resist, he can generate no force contrary to gravitation; he gives a few movements or leaps from the ground itself if the sinking is not too rapid. When the heart ceases beating man cannot resist, as he needs the beating heart itself to generate his power. If the heart is feeble, he may breathe fast to supply it rapidly with oxidized blood, and to a certain extent succeed; but he must take this compensation force from some place. I cannot pretend to give an opinion on the result of an unnatural slowness of pulse, and an unnatural rapidity of breathing; but that they are evil omens is true, or we have long been deceived. In mines and such places the evil is complicated, because the exertion required to climb the ladders leads to an increased activity of the heart.

If the gas by which the oxygen in air is diluted were insoluble, the result might be very different, and we might probably remain in air with less than 10 per cent. of oxygen. In one condition, namely, in high regions, something similar to this occurs; the amount of oxygen is diminished by rarefaction. But even if no rarefaction took place, we could breathe in air having much less oxygen than in the worst metal mines we know, if the carbonic acid was removed. Nitrogen and some other gases, marsh gas for example, not uniting chemically, and not being driven out from any compound in the blood, either by the addition or otherwise of oxygen, do not produce effects so violent as carbonic acid.

Whatever the explanation be, my conclusion from the experiments is, that the smallest diminution of oxygen in the air breathed affects animal life, if its place is supplied by carbonic acid.

RAIN.

Rain has generally been considered as water simply, and its beneficial effects on man have been indefinitely summed up in the idea of refreshing. This, again, connects itself in our minds with temperature and moisture, whilst the purification of the air by washing has had little attention. If there is life and death in the air, we must believe the same of the rain, which collects solids and liquids, not omitting gases and vapours. These contents show themselves to chemical analysis and the microscope, so that distinctions between the air of different places may be known without the dangerous test on health. As this volume is a collection of early work in great part, it will be consistent to begin with my ideas when first finding practically how complicated the substance we call rain really is. It does not appear that I considered them new in principle, although new in some of their relations.

'As I said before,[1] I have nothing which I can call actually new to bring forward here, but it does still present some novel feature. The air was not examined as such, because I had not proper convenience for the experiments, and I was compelled, therefore, merely to examine the rain. All the rain was found to contain sulphuric acid in proportion as it approached the town, and with the increase of acid the increase also of organic matter.'

'The existence of albuminous compounds may be traced

[1] 'Mem. Lit. and Phil. Soc.' vol. x. 1852.

in the rain, however carefully collected, and the still further vestiges of living creatures, minute animalcules, may be found also. These creatures are sufficient of themselves to show the existence of phosphates, whilst sulphates and lime may be readily obtained. In examining the Thames water, I often found that the readiest way of collecting the phosphates and magnesia was to wait for the animalcules to do it. When the residue of the rain is burnt, an abundant evolution of ammonia may be obtained; but I have not ascertained the amount, because it varies much, and I do not well feel able to collect all the ammoniacal salts which may have existed in the rain, as so much loss is caused by evaporation, even if an acid is present. All results hitherto obtained must have been approximative and too low.

'This organic matter, however, is capable of decomposing and of forming ammonia when it falls upon the ground, and of furnishing food to all kinds of plants. There is enough therefore to grow plants scantily, although experience shows that there is not enough to produce a crop of any value. I do not regard it, however, as the object of nature to manure the land by rain (*this and other remarks would be modified were I to give present opinions*); one more important and practical is to purify the air; and there is enough of evidence to show us that places entirely without organic matter may become covered with it, and also to show us that plants nourished even by rain-water only may be made to grow (*that is, without the soil to feed them*). This shows also the possibility of large quantities of impure matter being kept afloat in the air; indeed, it is scarcely possible to obtain the vapour of water without some such impure matter. The organic matter found in the rain seems to be in perfect solution, and no doubt the more decomposed portion of it at least is entirely so, but an exception must be made of that which is alive.

'It becomes clear from the experiments that rain-water in town districts, even a few miles distant from a town, is not a pure water for drinking; and that, if it could be got direct from the clouds in large quantities, we must still resort to collecting it on the ground in order to get it pure. The impurities of rain are completely removed by filtration through the soil; when that is done there is no more nauseous taste of oil or of soot, and it becomes perfectly transparent. The presence of free sulphuric acid in the air sufficiently explains the fading of colours in prints and dyed goods, the rusting of metals, and the rotting of blinds.

'It has been observed that the lower portions of projecting stones in buildings were more apt to crumble away than the upper; as the rain falls down and lodges there, and by degrees evaporates, the acid will be left and the action on the stone much increased.

'I do not mean to say that all the rain is acid; it is often found with so much ammonia in it as to overcome the acidity; but in general, I think, the acid prevails in the town. But, even if alkaline when it falls, it becomes acid on standing, and especially on boiling down, as the ammonia in these cases is separated from its acid.

'A specimen taken in Greenheys fields, half a mile from the extreme south-west of Manchester, wind blowing west, had a peculiarly oily and bitter taste when freshly caught. A person to whom I gave some of it to taste supposed it had been put into a glass in which castor oil had been put. I had collected the water in a large meat-dish, which had been very carefully cleaned, and was then set on a stand about two feet from the ground during the rain. Thinking it possible that some fatty matter might have been adhering to the vessel in spite of all my care, and not being inclined to believe that such an amount of impurity could be found in that place, I used a platinum basin, which was carefully cleaned, and, to prevent all mistakes

as to organic matter, kept red-hot for some time. There was, however, no difference to be perceived from that collected in the larger vessel. The rain was very alkaline and contained scarcely a trace of carbonic acid.

'Boiling removes all taste, and standing alone removes the taste of the oily matter and leaves only the taste of smoke.

'The smoke here shows that it was not out of the range of chimneys, although the wind was west.

'The taste was that of the flattest and most insipid water, which could not be drunk with pleasure, independently of the nauseous taste.

'The water was very clear, but on standing it produced and deposited a number of organic bodies of the monad kind, small enough certainly when seen by themselves, but in clusters large enough to be seen lying at the bottom of the vessel.

'The clear water above was a solution of organic and inorganic substances, giving the following results:—

Organic matter . . .	2·625 grs. per gallon.
Inorganic matter .	·875 1·33 2·100 } in three experiments.

'By boiling the carbonate of ammonia is driven off; at least this seems the only way of accounting for the loss of alkalinity.

'On burning the residue after evaporation ammonia is given off, and a strong smell of feathers, characteristic of albuminous compounds.

'The ash is alkaline, with fixed alkalies, like the ashes of plants and other organic matter.

'*Cavendish Street*, June 8, 1851.—The taste of this water, collected of course directly into the vessel, was the same as that which comes from the roofs of houses. The taste is nauseous, and chiefly of smoke.

'It contains many of the green monads, singly and in

groups. These increased immensely, and were in numbers much greater than in the field rain. There were also some lengthened bodies, somewhat resembling the gallionella; but I cannot speak with certainty of the species.

'This water is acid, and on boiling becomes very acid, until it may be readily tasted as sour.

'The residue, when burnt, gave off ammoniacal vapours, and showed also the presence of albuminous compounds. The sides of the vessel were covered with an oily or tarry substance.

'The ashes were neutral, and consisted of sulphates, sulphate of lime and soda being amongst them.

'These two specimens are characteristic of the places; these are not extreme points by any means. One is not very far in the town, and the other is not very far out of the town. I am sorry I have not obtained extreme cases, but the difference is sufficient as a point to start from. The one leaves alkaline ash, the other neutral ash. In the fields the amount of acid is not sufficient to neutralise the bases which are in union with the organic matter, and the residue is therefore alkaline; but in the town the amount of acid is equal or in excess; what is in excess is driven off, and enough remains to saturate the bases, which become then neutral salts.

'The increase of amount of organic matter is not so apparent from the table of quantities which I have drawn up; but I rely more on observing these living creatures, which are the sure indications of its presence. And they show also that, if there was not a great increase of it in the town, it was at least in a state peculiarly organisable.

'Again, *Greenheys Fields*, on the same day.—This water has a blackish deposit; a few monads may be seen at once; taste, when first got, very greasy; after standing a while this taste becomes bitter, like rotten leaves; flat, like all the specimens; sickly taste begins when the greasy and bitter tastes are gone.

'Alkaline also; alkalinity lost by boiling. Nitrogenous fumes obtained on burning the residue.

'Residue as before, alkaline.

'*Timperley*, six miles distant.—Abundance of green matter at the bottom of the glass; an immense amount of green monads, mostly separate, but some in clusters.

'Gave off alkaline fumes when the residue from evaporation was burnt.

'Ash then strongly alkaline.

'This water was strongly alkaline, and was farthest from the town; it had, however, a great deal of organic matter in it—as much as any—so that the acid seems so far to be a surer guide to the neighbourhood of the town.

'*Park Street*, outside of the town, south-west.—Matted confervæ appeared in this specimen, on standing, with many green spots, stationary and in motion.

'The water alkaline, but acid on boiling.

'The ashes neutral.

'We are here, therefore, still within the town influence, but it appears that in the outskirts of the town the acid is neutralised in a great part with ammonia, as the rain does not become acid until that is driven off. (*I do not now find this state of things*, 1871. *To say that the acid was neutralised is not enough; there was less acid.*)

'We may therefore find easily three kinds of air—that with carbonate of ammonia in the fields at a distance; that with sulphate of ammonia in the suburbs; and that with sulphuric acid, or acid sulphate, in the town.

'I need not minutely describe each specimen which I collected; there is much similarity when from the same district.

Amount of Inorganic Matter in a Gallon.

Greenheys	·875	2·100
Cavendish Street, June 8		1·050
„ „ 9		5·6
Park Street		·21
Timperley		3·937

Greenheys Fields	2·1
Cavendish Street again	2·8
Moss-side	·8
Greenheys Fields	2·333
Greenheys again	1·33
Cavendish Street, 5,000 grs. used	3·010

Organic Matter.

Greenheys	·56
Cavendish Street, June	1·960
Park Street	4·200
Greenheys Fields	2·799
Moss-side	1·45

Chlorine in a Gallon.

Greenheys	·47712
Cavendish Street, June 8, 1851	·3976
„ „ 9, „	·5300
Moss-side	·896

Sulphuric Acid in a Gallon.

Greenheys	0·3840
Cavendish Street	1·0752
„	1·0752
„	·5972
Greenheys Fields	·4480
Park Street outskirts	·5376
„ „	·5376
„ „	·6740
Timperley[1]	2·2400
Moss-side Fields	·8960.'

It is preferred in this place to give a summary of the work done up till lately, as seen through the eyes of another and not by myself. It is taken from a very careful little volume by J. I. Pierre, called 'Chimie Agricole.' This is done in order to avoid all charge of carelessness as to the work of others; and it may serve as a means of separating my own work from that of others. It does also save a good deal of trouble in compiling.

[1] There is an unaccountable quantity at Timperley. This requires explanation. The wind was from the west and violent. Did it receive its impurity from an upper current?

The quantity of acid was determined by comparing prepared solutions.

Different Substances which are generally found in variable proportions in Air and Rain. From 'Chimie Agricole,' by J. I. PIERRE. 2nd edition. No date. Since 1860 I believe.

Dalton found that at Manchester, in the neighbourhood of the sea, 10,000 kilogrammes of rain-water contain more than a kilogramme of common salt.

Brandes (in 1825) found in Salzuffeln, by evaporation of rain collected during the various months, in 1,000,000 kilogrammes the following:—

Month.	Kilogrammes.
January, a residue of	65
February „ „	35
March „ „	21
April „ „	14
May „ „	8
June „ „	11
July „ „	16
August „ „	28
September „ „	21
October „ „	31
November „ „	27
December „ „	35

He found the residue to contain—

Organic substances.	Magnesia.
Chlorine.	Ammonia salts.
Sulphuric acid.	Carbonic acid.
Soda.	Lime.
Potash.	Oxide of iron.

Oxide of manganese.

Brandes found that the average quantity of foreign matter in rain amounted to 26 kilogrammes in 1,000,000 kilogrammes.

In a series of researches made at Caen during 1851, I [Pierre] found 24½ kilogrammes.

Suppose in France an annual average rain-fall of 60 centimètres, a hectare will then receive 6,000 cubic mètres (or 6,000,000 kilogrammes) containing, according to Brandes, 156 kilogrammes solid matter in solution, according to me [Pierre] more than 147½.

On analysis of this residue, I found that in the vicinity of Caen a hectare of land receives annually by rain—

	Kilogrammes.
Chloride of sodium	37·5
„ kalium	8·2
„ magnesium	2·5
„ calcium	1·8
Sulphate of soda	8·4
„ potash	8
„ lime	6·2
„ magnesium	5·9

I have found besides evident traces of ammonia salts, organic matter, and other substances of a nature not yet determined.

M. Barral's recent researches show every hectare at Paris to receive annually by rain—

	Kilogrammes.
Nitric acid	63·6
Ammonia	15·3
Chlorine	13
Lime	31·2
Magnesia	9

How can the solid matter be carried to considerable distances by rain? This is not easily shown by direct experiment. It is known, by the beautiful researches of Saussure, that a part of the water raised into the atmosphere resembles the soap-bubbles that amuse children. In other words, the clouds are composed of small vesicles, of which the water forms the envelope; every vesicle that rises from the sea must contain a small quantity of the solid matter which was dissolved in the sea-water. As these vesicles form not only on the surface of the sea, but also on lakes, streams, and rivers, the proportion of solid matter taken up by the rain in a given space will vary according to the relative proportions of these original different vesicles that enter into the composition of the clouds; that is to say, according to the distance at which they are found from the sea, the direction and swiftness of the winds, the temperature, and a number of causes of which it is difficult to find the proportion. It is therefore extremely probable—one may say it is almost certain—that the amount of saline matter in rain is not the same everywhere; but it is undeniable that these matters are found in all rains, and that they must play in agriculture a part that ought to be studied. The results at which I have arrived lead me to admit that certain saline substances are

furnished to the soil in larger proportions than they are used by the crops. Salt (*common*) particularly so.

Among the substances the existence of which has been proved in rain there are two which deserve special attention, on account of their action on vegetation; these are the *nitrates* and *ammonia*.

We quoted just now the enormous amount of 63 kilogrammes of nitric acid received during one year by a hectare of land in the environs of Paris by rain alone, according to M. Barral.

According to M. Liebig only the rain of a thunder-storm contains nitric acid, but the researches of M. Barral, which take the water collected during the different months of the year, have given for the months of November and December, 1851, numbers as large as those of July and August. Here is the amount of nitric acid in rain during the last six months of 1851:—

Months.	Kilogrammes.
July	5·30
August	4·89
September	8·89
October	2·81
November	4·26
December	5·95

The experiments made by M. Bineau at the Lyons Observatory in 1853 led to similar conclusions. According to him the rain at Lyons contained during the year—

In winter	·3	millig.	nitric acid per litre.
In spring	1	„	„
In summer	2	„	„
In autumn	1	„	„

and the average might be estimated at 1 milligramme per litre, which would correspond to 10 kilogrammes per hectare, if one considered the rain fallen at Lyons for all the year equal to a layer of a mètre in thickness.

We are not discussing here the cause of the production and of the presence of this nitric acid; we only confirm the fact, which is of the utmost importance.

The presence of ammonia in rain has been lately the object of careful and important researches.

Many observers have found the presence of ammonia in rain. Bergmann, Brandes, Liebig, and Pierre have shown the presence

of ammonia salts without determining the proportion. MM. Bineau, Barral, and Boussingault have made this lately the special object of many interesting researches.

M. Barral has found that at Paris, in the six last months of 1851, a hectare of land received from rain—

	Kilogrammes.
July	3·13 ammonia.
August	1·04 „
September	·77 „
October	1·53 „
November	1·01 „
December	1·17 „

The quite recent researches of M. Boussingault and M. Bineau prove that the amount of ammonia contained in rain is really worthy of attention.

Thus the proportion rises sometimes to 7 milligrammes per litre of water, according to M. Boussingault; it rises even to 28 or 29 milligrammes per litre according to M. Bineau.

The first of these observers has found that the proportion of ammonia is much larger at the beginning of a shower than at the end.

Thus on August 5 he has found—

Commencement of shower	4 millig. ammonia per litre.
End	·5 „ „

During another series of observations on August 26 he found—

First part of shower	4 millig. ammonia per litre.
Middle	2 „ „
End	·5 „ „

Another series, August 28—

	Milligrammes.
Beginning	1·15 ammonia.
Further on	·70 „
„	·20 „
„	·10 „
The end	·03 „

After a cessation of several hours in the rain he found at the beginning 1·13 milligrammes.

After a number of days without rain, the proportion of ammonia rose to 7 milligrammes at the *beginning* of the first

shower, but lessened successively to 3, 1·5, and to ·3 milligrammes at the *end*.

We may sum up in the following manner the result at which M. Boussingault has arrived.

When starting from the commencement of the rain the fall reached on an average—

Millimètres.			Milligr.		
		·5 it contained	3·11	ammonia	per litre.
From ·5	to 1	„	1·20	„	„
„ 1	to 5	„	·70	„	„
„ 5	to 10	„	·45	„	„
„ 10	to 15	„	·45	„	„
„ 15	to 20	„	·41	„	„
„ 20	to 31	„	·40	„	„

It is thus in the first part of the rain that falls that we find the most ammonia.

In a *single instance* only M. Boussingault has not found ammonia in rain in a determinable quantity. M. Boussingault has been led to think, according to his experiments, that rain collected in the fields contains notably less ammonia than that collected in towns.

According to M. Martin rain collected at Marseilles contains a little more than 3 milligrammes of ammonia per litre.

The *dew* also contains ammonia.

In a first observation M. Boussingault found in it—

	3	millig.	per litre.
In another	6	„	„
In a third	1	„	only.

But it is worth remarking that that was after a rainy day.

In *fogs* he found one time . .	2·5	millig.
Another time	7	„

Finally M. Boussingault once found in the condensed water from a fog the enormous proportion of 50 milligrammes of ammonia per litre. This fog was very thick, and had lasted two days and a half.

If we admit that on an average rain contains three-fourths of a milligramme of ammonia per litre, and that the annual rain-fall in France is 85 centimètres of water, every hectare

of land will receive from rain about 7 kilogrammes of ammonia, a quantity to which we must add that which comes from dew and fogs.

According to the experiments of M. Bineau, who found that 6·8 milligrammes per litre was the mean of ammonia salts in the rain collected at the Lyons Observatory in 1853, the quantity of ammonia furnished by that water to each hectare of land would be, under similar conditions, represented by 68 kilogrammes, about the fourth part of that furnished by a good manure.

M. Bineau has found as an average—

In winter	16·3	millig.	of ammonia	per litre.
In spring	12·1	„	„	„
In summer	3·1	„	„	„
In autumn	4	„	„	„
Average of year . . .	6·8	„	„	„

If we add to this quantity, already pretty considerable, that which is furnished by dew, which M. Bineau and M. Boussingault have found much richer in ammonia, we get for 1853 the amount of 29 kilogrammes per hectare from dew alone.

It is right to add that, if similar facts were found everywhere, they might and ought to differ as to the quantity; in the variations they might run from one to three times, according to local circumstances. Thus M. Pouriau has found in rain fallen at Saussaie (Ain) during 1854 about 29 kilogrammes of ammonia per hectare, and about 7 kilogrammes of nitric acid.

M. Boussingault has found in the snow itself a notable quantity of ammonia; he has found at Paris in March 1853:

Falling snow ·70 milligramme per litre of water. Thirty-six hours after the fall of snow, a litre of water formed by its melting contained—

	Milligrammes.
On a terrace	1·78
In a garden joining the terrace .	10·34

This increase proves evidently that the ammonia was set free from the soil, and that it had been intercepted and absorbed by the snow.

M. Bineau has found even from 60 to 65 milligrammes of ammonia per litre in water formed by the melting of needles

of ice formed on the border of the roof of a house, and the even larger amount of 70 milligrammes per litre of water formed by the melting of hoar-frost.

M. Boussingault has found that the fogs of Paris have more ammonia than those of the plains of Bechelbronn. In these latter he has found on an average ·3 milligramme; in those of Paris 3 milligrammes. He found one time the enormous amount of 130 milligrammes of ammonia per litre; another time 138 milligrammes.

This large amount of ammonia explains why, under certain circumstances, the fog is endowed with such a penetrating odour as to affect very sensibly the organs of respiration, as ammonia vapours do.

Volcanoes in a state of activity throw out a notable quantity of ammonia salts. They are given off also in calcining organic matter, and in the combustion of coal.

Matters of organic origin, all more or less nitrogenised, decompose, either at the surface of the land or at a very small depth, and among the products of this decomposition we find carbonate of ammonia, a very volatile substance. A portion of this substance remains in the soil, and contributes to the prosperity of the plant; another part is disseminated in the air. Carbonate of ammonia being very soluble in water, we can understand why it is found in rain.

When there is some time without rain, one can again understand why this substance is found in larger proportion in the air than during rain, and how the first rain that falls will carry to the soil a larger proportion than the succeeding.

If this explanation is right, we ought to find ammonia in the air at all times.

M. Grager has found 323 grammes per million kilogrammes of air, being about ·42 milligramme per cubic mètre.

M. Kemp has found 368 grammes per million kilogrammes of air, being about 4·78 milligrammes per cubic mètre.

M. Fresenius has found 133 grammes per million kilogrammes of air, or about ·17 milligramme per cubic mètre.

In an early series of experiments I have been led to conclude that in the neighbourhood of Caen, under the almost constant influence of winds tending to remove from the place of observation the emanations which might come from the town, the air contained, as an average of the winter season of 1851 to

1852, more than 4½ milligrammes of ammonia per cubic mètre. In another series of experiments, which comprise nearly a year's observations, on air taken from a little greater height I have only found ·65 milligrammes per cubic mètre.

If these different results do not agree among themselves for the proportion of ammonia found in the air, they all agree in the fact of the existence of this atmospheric ammonia, and our explanation is not deprived of probability.

The total of the results which thus come to be seen allows us now to give an idea of the sense which we must put upon this old popular saying that 'the fogs and the snow remain to fatten the land.' The fogs then, we see, contain a very large proportion of ammonia, especially those that remain several days; and, as the ammonia and carbonate of ammonia exercise a very energetic and very favourable influence on vegetation, the prolonged presence and deposit of this water charged with ammonia can and does produce very good effects.

We may also say as much of the snow; it arrests and absorbs the liberated ammoniacal vapours which come from the soil, and restores them to the earth when thawing.

Here, as in many other cases, experience is before science; that is to say that the observation of these facts, repeated from time to time, have allowed conclusions to be drawn which have been long called prejudices by *savants*, till they thus found their explanation.

Before going further we will attempt to point out the importance of the results of which we have given an account, and to consider them in a purely agricultural point of view.

It results from the experiments of M. Barral, that rain could give to the crops more than 21 kilogrammes of nitrogen per hectare and per annum, to which we ought to add the amount furnished by dews and fogs; on the other hand, M. Boussingault has found that certain crops contain more nitrogen than the manure can supply them with, and that the difference is comprised between 4 kilogrammes and 31 kilogrammes per 'hectare' and per annum, according to the distribution of crops adopted. If it is proved that, by following such a distribution of crops, the land is not impoverished, it follows of necessity that this difference of nitrogen comes to the crops from some other source, and we see no other than the rain,

the dew, the fogs, and the atmospheric air, otherwise this difference would be perfectly inexplicable.

We can understand, by the aid of these facts, how it is possible to produce in some indefinite way crops without manure, by the system of fallow-land with or without the aid of manure.

The quantity of nitrogen taken from the soil by this mode of agriculture, which conduces to wear it away by reason of these insufficient products, is much smaller than that furnished to it in the same time by the rain.

Fallowing land, by giving rest to the earth, gives it the means of receiving the manure disseminated in the atmosphere, which the rain carries to it, till its fertility be great enough to allow it to bear a proper crop.

Besides nitric acid and ammonia rain contains still other elements, which we have already mentioned, principally saline substances, which are often restored to the soil by the rain in greater abundance than they have been taken from it in the same time by the crops.

The *sulphuric acid* of the *sulphates*, which we find in rain-water, may proceed in part from water which is volatilised from the sea, lakes, rivers, &c., and which is carried away with it.

Another portion might be formed in the air itself; for about a hundred years ago Rouelle said positively 'When one steeps a well-cleaned cloth in potash-lye free from sulphuric acid, and exposes it to the air in a place free from rain and dust, the cloth becomes wet and dry alternately a great many times; at last it becomes dry, and does not again become wet; the potash with which it was impregnated is found changed into sulphate.'

M. Dumas has discovered that hydrosulphuric acid mixed with air and moisture, by the concurrence of a porous body, and under the influence of an elevated temperature, is converted slowly into sulphuric acid. We can thus see how the elements required to form sulphuric acid are carried to a distance in the air, till their absorption by a porous body causes their transformation.

According to Chevreul, Vogel, and Lewy, the sulphates of the alkalies in contact with organic matter can become the sources of hydrosulphuric acid, sources to which we may add the putrid decomposition of organic matter.

The porous bodies in the air will be without doubt the fine dust nearly always found in suspension.

It is thus we see produced before our eyes a number of providential movements, which co-operate successively to the production and the nutrition of animals and plants succeeding each other on the surface of the globe. Man can, indeed, by his power, modify these results within certain limits.

After what we know of the production of dew it was easy to foresee that it would contain not only ammonia, as we have already seen, but also the other matters we have found in rain. So it is that Julia de Fontenelle found, in 1819, chloride of sodium, chloride of potassium, sulphate of lime, and carbonate of lime in dew-water.

Besides the matters whose existence we have already mentioned in atmospheric air in notable amounts there are also others, found in much smaller proportions, and to which we are disposed to attribute an important *rôle*, not so much from an agricultural point of view as from a hygienic.

I shall not speak of the different 'dusts' which are constantly in suspension in the air, and of a nature varying greatly according to places and seasons. Their study is yet entirely to be made, and will furnish curious and perhaps very useful information.

The presence of *iodine* in the air and in rain and in running waters has been known for some years. It is admitted that this iodine must exercise a very decided influence on the health, as it is understood that in the country, where the 'goitre' and 'cretinism' are most frequent, iodine is wanting, or is found in much smaller quantities in the air and in the rain and running water which serve for drink to the inhabitants.[1]

There has also been recognised for a long time the existence in the air of substances whose nature is little known, in quantities almost indiscernible, to which the name 'miasm' has been given.

The existence of these 'miasms,' whose disastrous effects are happily confined to certain localities, is often a great obstacle to the progress of agriculture in certain naturally fertile countries. There still remains, therefore, much to be learned regarding the causes which produce these miasms.

[1] This is one of the points not believed to be proved sufficiently.

It is generally known that the border of marshes, of ponds, of places where the water stagnates and dries up, especially if the water results from a mixture of fresh and sea-water, are unhealthy; that there men are subject to painful intermittent fevers, sometimes even to yellow fever, &c.

Entire countries are unhappily notorious for the maladies which rage during one part of the year among their unhappy inhabitants. The winds, and above all the wet winds, extend the radius of their unhealthy action.

It appears without doubt that it is through the medium of the air that these pernicious emanations are transmitted. But what is their nature? What are the precise causes of their production? What is their manner of acting? In what proportion can they be found mixed with air? These are questions which science is far from having settled. We are scarcely more advanced in the knowledge of proper means for preserving us from the action of these singular substances.

We have already made on this respect some interesting observations; thus, we have for centuries understood that the interposition of a curtain of moistened canvas was sufficient to preserve the inhabitants of a house; that a thick curtain of trees even could limit the sphere of action of a centre of miasms.

Certain persons have doubted the existence of 'miasms' as material substances, and have attributed their effects to abnormal conditions of temperature, of humidity, &c.; but experiments many times repeated do not allow us to accept this opinion.

Thus towards 1808 MM. Thenard and Dupuytren made the following comparative experiments:—

They shook several times with perfectly pure water some *carburetted hydrogen gas obtained from mineral substances*; this water, left at rest with the air, is deprived little by little of its gas, and is not at all turbid.

A similar experiment was made with the same water and with *carb. hydrogen resulting from the putrefaction of animal matter*; the water is then turbid, there are flakes formed which are deposited at the bottom of the bottle, and the liquid putrifies. The flocculent matter that is deposited appears to be of an animal nature, and exhales when burnt the odour of burnt feathers or horn.

To the physicist, carburetted hydrogen gas got from these two

sources is the same. However, there existed in the latter, in some unknown form, a small quantity of matter of 'miasmatic' nature.

About 1809 the celebrated doctor Moscati made analogous experiments on the air of hospital rooms. He suspended over the beds of several patients spherical vessels full of water cooled by ice, and collected with care the water that condensed by cooling upon the external surface; this water putrefied as animal substances do, and gave a residue of analogous matter.

He suspended also on the border of a rice plantation in Tuscany flasks filled with ice, and collected the water which was deposited on their surface. This water, at first limpid, soon gave birth to flakes possessing analogous properties to those of animal matter, and the liquid finished by being completely putrefied in a few days.

In 1812 Rigaud de Lisle made in the marshes of Languedoc analogous experiments, which gave the same results. The water collected putrefied, and gave rise to a deposit of flakes of nitrogenised organic matter. This water gave also, with nitrate of silver, a precipitate which became immediately purple, a fresh indication of the presence of organic matter.

In 1819 (August 13) Julia de Fontenelle collected in the marshes of Cercle 4 litres of dew. This water was inodorous, without colour, and clear; in a short time it deposited small flakes of nitrogenous matter.

About the same time M. Boussingault observed that sulphuric acid placed near a pond where hemp is retted becomes black very quickly, while at a distance from the centre of putrefaction it becomes black very slowly. The explanation of these observations is easily understood. When organic matter is brought in contact with sulphuric acid it becomes charred, as if it were put into the fire. A part of this carbonised matter is dissolved in the acid, and gives it a brown colour, becoming darker as it is more abundant.

The experiment of M. Boussingault teaches us that the air in contact with sulphuric acid placed near the pond contained more organic matter than the air found further off.

In 1829 the same *savant* made in America, in the environs of Cartago, the following experiments:—

A little after sunset he placed two watch-glasses on a table

in the middle of a swampy meadow. Into one of the glasses he poured hot distilled water, in order to moisten the surface, and at the same time to make the temperature higher than that of the air.

The cold glass was not long in being covered with an abundant dew; the hot glass evidently could not cause condensation. On the addition of a drop of sulphuric acid in each glass, and evaporating to dryness at the heat of a spirit-lamp, there was then seen a trace of carbonised matter adhering to the glass in which the dew was deposited, while the glass which had none was perfectly clean after the volatilisation of the acid.

These experiments of M. Boussingault allow us to speak of certain practices of those Chinese who are employed in the culture of rice.

They take great care to use a great abundance of drink, which passes to the skin, and avoid with care causes of cooling during their work; in this manner they cause a continual exhalation from their bodies in place of condensation; they are in a similar condition to that of watch-glasses filled with warm water. We often ask how such a small amount of matter can produce in our organisms such grave disorders, and one is led to doubt the reality of their action. An example will suffice to make the possibility comprehensible. When a fly, after having rested on a putrefying animal, comes to us, it can communicate to us carbonaceous affections strong enough to cause death. We also know that the pricks of a scalpel which has been used for the dissection of a putrefying body are often mortal.

M. Boussingault thinks that the miasms are hydrogenised bodies. To assure himself of this he passed through a tube heated to redness about 300 litres of air taken from the marshy atmosphere just mentioned; being deprived of moisture, and then passed through a red-hot tube, there was found in the tube about 50 milligrammes of water; when the air was passed over a vessel of sulphuric acid before passing into the red-hot tube, no appreciable quantity of water was formed.

See also the work by Robinet and Bobierre, given in abstract farther on.

Although I think the little paper previously quoted

gave really the pith of the subject from a fresh source, there was still much to be done, and it was needful to gather rain in many places, and under various circumstances, in order to come to satisfactory conclusions, and such as would be useful to those who attended to the influence of air on health. The following results were attained at a later period, and are to be found in the 'Report of the Proceedings under the Alkali Act' for the year 1868 :—

Composition of Rain.—General Conclusions.

1st. The rain from the sea (Western Islands) contains chiefly common salt, which crystallises clearly.

2nd. The rain contains sulphates in larger proportion to the chlorides than is found in sea-water. This is true from central Germany to the most northern Hebrides.

3rd. The sulphates increase inland before large towns are reached. They seem to be a measure of the products of decomposition, the sulphuretted hydrogen from organic compounds being oxidised in the atmosphere. (In other words, just as I believe chlorides with proper deductions to be a measure of the sewage, however old in water, so I believe sulphates to be a measure of the sewage in air, unless when coal interferes too much to permit allowance to be made).

4th. The sulphates rise very high in large towns, because of the amount of sulphur in the coal used as well as of decomposition.

5th. As sulphuretted hydrogen and sulphide of ammonium oxidise in the atmosphere, the sulphates may be expected to increase in proportion to the amount of decomposing organic matter containing sulphur, such as albuminoid compounds, called conveniently by a name now less used, protein.

6th. When the sulphuric acid increases more rapidly than the ammonia the rain becomes acid.

7th. When the air has so much acid that two to three grains are found in a gallon of the rain-water, or forty parts in a million, there is no hope for vegetation in a climate such as we have in the northern parts of the country.

8th. The acid is calculated as dry sulphuric, but to some extent the agent may be hydrochloric rendered free by the sulphuric acid decomposing the common salt.

9th. Sulphate of soda increases in the rain as coals are burnt; and if the salts are heated, chloride of ammonium comes off and sulphate of soda remains.

10th. Chlorides increase with the burning of coal to a perceptible extent, although not so much as in places where salt is decomposed, whether in alkali or other works.

11th. Free acids are not found with certainty where combustion or manufactures are not the cause.

12th. The chlorides and sulphates may be found neutralised even where there are manufactures.

13th. By attending to these facts it may be found if the plants in any place are hurt by acid, and by which acid. Other acids may probably be found as readily as the two mentioned.

14th. By attending to the amount only of the sulphates and chlorides great injustice may be done. The acidity and the average of the district must be known.

15th. Ammoniacal salts increase in the rain as towns increase. They come partly from coal and partly from albuminoid substances or protein decomposed.

16th. The albuminoid substances may be found in the rain even by the rude experiment of burning the residue, which renders unmistakable their peculiar odour, but they may also be recognised and estimated by the method used by Wanklyn for potable water.

17th. Experiments in the direction here indicated may enable us to study and express in distinct language the

character of a climate, and certainly of the influences of cities on the atmosphere.

COLLECTION OF SPECIMENS OF RAIN.

It is not easy to obtain a good specimen of rain even during a heavy shower. A vessel must be prepared, and it is scarcely possible to keep one clean long enough if it is to be of a large size, such as would collect from one shower a quantity sufficient in bulk for examination. It is needful to clean the vessel at the time, and this is best finished by washing it with some of the rain itself; to that, however, there is an objection, because we learn from the first of the shower the substances previously in the atmosphere. The latter part is very different in quality. The earliest experiments, which really gave the characteristic results and decided the most important questions, were made on specimens collected whilst the vessels were watched. I learnt by this experience what to expect. I adopted, after consideration, as a collecting surface, a simple glass funnel five to six inches in diameter when it was necessary to give much time for collection. This was placed in a glass bottle holding half a gallon, or about $2\frac{1}{4}$ litres. In calm weather this does well. In windy weather the bottle is blown over at once. It is needful, therefore, to have it held down steadily. For a permanent arrangement the outer surface of the funnel ought to be protected from the wind, but for these experiments the funnel itself was generally tied. Each of the gentlemen who were good enough to aid me took his own plan. It was necessary to see that the funnel was not too near the ground, on account of splashing, and that it was not exposed in dry and dusty weather. Some of the specimens were wonderfully free of any floating matter visible to the naked eye; all were very free, except those caught in towns. I gave up the idea of

obtaining from all places mentioned specimens entirely free from the dust visible to the naked eye. Such can be had only by great care and from few places, if at all. In most cases the bottles have stood out a considerable time, and the average rain as it falls, with the dust of intervening dry weather, has been found. In London and large towns this is especially the case. But an examination even of these is necessary, because we wish to know all that we are liable to be obliged to breathe. In Manchester, however, as convenient variations could be made, specimens of both kinds were examined frequently.

Desirous of obtaining specimens when in the north, I applied to Alexander Buchan, Esq., of the Scottish Meteorological Society. He, with great kindness, gave me many names of gentlemen to whom it would be well to write for assistance. All of them undertook to fill the bottles sent to them, and in every way aided the search. The result is, I believe, a proof that great care has been taken in the collection. The distinctions are remarkably fine, and such as might be expected from the conditions. We have not to rely on the gross differences between a pure clear or mountain rain and that of a smoky town, but we have the more or less smoky town, the more or less inland, and the more or less elevated spot, whether town or country.

The rain having been collected by several gentlemen in various parts of the country, I cannot avoid thinking that I put some of them to much trouble; I am only in return able to thank them. To have collected all myself would have been impossible. By reading over the observations of some of those who have aided me, the nature of the precautions will be seen. A merely general description would not fit where there is considerable variety of manner of working. The following was sent to such as agreed to take the trouble of collection :—

'Specimens of Rain-Water.

'It is desired to obtain specimens of rain-water exactly in the condition in which it falls.

'To prevent extraneous matter from entering great care will be required.

'The rain ought to touch nothing but the glass funnel and bottle.

'Splashings from the ground or from any elevations ought to be avoided, also falling leaves, snow, and sea-spray; and in the case of country specimens the collection ought to be made on the wind side of a house, if the house is near.

'The bottle is expected to be sent clean, but it is better that it should be washed out with the rain-water itself. In other words, it is well to throw away the first portion of rain collected.

'The funnel ought to be washed with clear water, and finally with rain-water. Care ought to be taken not to touch the inside of it: when the stopper is removed from the bottle the funnel is inserted, and the stopper may be put into the funnel; this prevents solid matters from going into the bottle; occasionally these are blown into the rain, and it is difficult to prevent this.

'It is desired to have the bottle half full; it holds two quarts.

'It is not necessary that it should be filled at once, but, if dry weather should take place before it is filled, it is well that the stopper should be put on to prevent dust. In this case the funnel would require again to be washed with rain-water, unless it were kept in a very clean place meantime.

'The bottle when filled is to be stoppered, and a piece of strong paper, better calico or leather, put over the stopper, which is to be tied down with string.

'It is not needful to return the funnel, and probably, if well covered at the top, the bottle may come without a box to cover it. For this reason it has been covered with basket-work.

'It will be better to give a general description of the weather during the time of collection, especially in relation to wind. It cannot be supposed to be uniform during the whole time.

'It will also be well to say what is the nature of the ground round the collecting spot, and the distance from dwellings.

'To add also the height of the bottle above the ground, and the height of the place above the sea.

'It is difficult in words to convey an idea of the importance in such experiments of obtaining freedom from all extraneous substances.

'R. Angus Smith.

'*22 Devonshire Street, All Saints,*
'*Manchester.*'

Since the earliest specimens received came from Scotland, I shall begin with them. They are chiefly from places far removed from towns. They must have been well collected, since they all exhibit distinct characteristics, such as we might expect, of their origin.

The N. Uist and Tyree specimens were obtained at the request of Mr. Ed. C. C. Stansford, who has agents there. They are from low levels, as much as possible avoiding high winds, and were expected to be fair specimens of sea-rain, as well as perfect specimens of coast and island-rain.

Kelly, Wemyss Bay, at the mouth of the Clyde, shows the smallest amount of sulphuric acid. I know that remarkable care was taken here; the only difficulty was to keep out flies. After emptying the bottle several times Mr. Young put the stopper into the funnel, and effectually kept out insects. The garden in which this

rain was collected is about 150 feet above the level of the sea, and perhaps a quarter of a mile from the shore, along which there are a few houses.

Auchendrane specimen was collected by Mr. E. Cathcart, of Auchendrane House, on the Doon, at 96 feet above sea-level, three miles from the shore, and five miles from the town of Ayr before December 19, 1869.

Carlesgill, Langholm, Dumfriesshire, in a valley of the Esk.—This specimen was collected by Mr. J. Little, who says: 'Great care was taken to have the water pure. The bottle was placed on a block of wood 2 feet high 18 yards from the front of the house, and clear of drips from trees or shrubs; house, 370 feet above the sea. A pastoral country for many miles around. Wind during collection varied little from SW., occasionally W. Three days of heavy squalls, viz. December 11, 13, and 19. Sharp frost for three days, when for safety the bottle was brought into the house. The hills near are 1,000 to 1,200 feet high.

'The second (from Carlesgill) was collected NW., NE., and SE. Genuine east seldom prevails any length of time, and generally brings less rain than from the opposite quarter. In stormy but dry east winds I have seen salt spray on the windward side of the trees, and the leaves of evergreens *browned*, though we are nearly 50 miles from the nearest point of the German Ocean. I do not know the amount of salt in the rain here. I fancy it must be greater in proportion to our rain-fall than at Pennicuik. I find in my note-book: "Calculating the fall of rain at 31 inches per annum, Dr. Madden has determined by analysis that the rain which falls at Pennicuik brings down upon each acre of land in the neighbourhood 641 lbs., or nearly 6 cwts, of salt." '—'Agric. Gazette,' April 8, 1854. I take this here from Mr. Little's letter, as I may not be able to refer to the volume.

Lanark, Ridge Park.—Mr. Charles Lindsay says: 'The weather during collection, from November 24 to January 6, has been of the most mixed description, the wind when high invariably from the S.W., and when raining S. or W.

Barometer from 30·03 to 27·80.
Correction for sea-level, ·67.
Thermometer in shade from 54° F. to 6·5.

The locality where the bottle was placed is distant from any building 30 yards; height above sea-level, 634 feet. Distance from east and west sea nearly equal, or 30 miles; in the middle of a garden with no trees or shrubs so high as the funnel.'

Wanlockhead.—This specimen was collected by Mr. Gilbert Dawson, there. He says: 'Wanlockhead is situated amongst the Lowther Hills, and is about two miles north-west of the highest summits of the range. The position of the bottle was 1,330 feet above the sea, and the ground sloping towards the south-west. The nearest houses in that direction were 60 or 70 yards distant, and so much lower that the tops of the chimneys were not nearly so high as the bottle. The water was all collected during SW. wind. A board was laid on the ground and covered over with a little grass; the bottle was then tied down with strings and nails driven into the edge of the board.

'The weather during the time of collection was very variable, there being a very intense frost on December 28, and snow fell at intervals on the 25th, 26th, and 30th to the depth of about 5 inches. There were also light falls of snow on the 19th, 20th, and 23rd.

'The water was collected as follows:—

	Rain-fall.
December 18, wind SW.	·82 in.
„ 29, gale SW. for 23 hours	3·19 „ !!
„ 31, light wind W. and SW.	·11 „
January 1, light wind S. and SSW.	·08 „
„ 2, S.	·05 „

	Rain-fall.
January 3, fresh breeze SW.	1·04 in.
„ 4, gale SW.	·59 „
„ 5, light air SW.	·02 „
„ 6, strong breeze	·90 „

'This filled the bottle.

'Every precaution has been taken to prevent extraneous matter from getting in, but, on account of the exceeding stormy and changeable weather which has prevailed during the collection, I regret to say that when the water is shaken some impurities may be seen floating in it.'

These specks are as few as in any, and do not prevent this from being a very interesting water. It is from the highest spot observed.

Cargen, Dumfries.—This specimen was collected by Mr. Dudgeon, lat. 55° 2′ 30″ N., long. 0° 34′ W., between December 29 and January 8. Prevailing winds during the time, SW. and NW. Position of bottle, centre of large park, on grass. Height above sea-level, 70 feet. Solway Frith distant 4 miles S. No house to windward within a quarter of a mile. Town of Dumfries 3 miles N.

Crieff.—This was collected by the Rev. Albert L. T. Morris, at Balwharrie House. This spot is three miles north of the village. There are no towns at all to the west, beginning with SW. round the arc to the north. It is 45 miles from the nearest coast and 245 feet high.

Kirkwall.—Mr. John G. Iverach sent this specimen. He says: 'The place where the rain was collected was in the centre of my garden. The bottle was placed on the grass, and would be about 10 feet above sea-level. I had the bottle almost full on two occasions, but had to reject the contents, as some snow had got into the funnel.'

This shows that much care was taken to collect the specimen pure. I wished to keep the snow and rain separate, in order to be able to examine each separately. A little snow would have done no harm; still the exactness is to be preferred.

Weather during Collection, 9 A.M.

			Rain Inches.
December	30	S.E., 1.0	1·350
„	31	S., 1.0	·096
January	1	SE., 1.0	·300
„	3	S., 1.0	·038
„	5	SW., 1.0	·114
„	6	SW., 1.5	·124
„	7	NW., 1.0	·110
„	8	NE., 1.0	·086
„	14	E., 2.0	·090
„	16	E., 1.5	·030
„	17	SW., 1.5	·180
„	21	W., 1.5	·019
„	23	SW, 1.0	·038
„	24	SW., 1.0	·048
February	1	S. 1.5	·030

Stornoway specimen, collected by Mr. J. Smith, in the Castle gardens.—'On every occasion the wind was blowing S. to SW. when the water was collected, and in case of violent gales the water was excluded. The ground near site where collected is hilly, about 120 feet above sea-level; distance from dwelling, 150 yards.'

East Linton, Haddington.—The specimen was collected by Mr. John Storie, who says: 'It has been about three months in collecting, having only had about 3 inches of rain during that period. Fortunately the weather has been favourable for getting it free of impurities, and it has been all the while damp, and therefore not dusty. On inspecting it you will see that it is remarkably pure. Wind from NNW. to W. and SW. On the 11th inst. (Feb. 1870) we had 7 inches of snow, which dissolved in the funnel and entered the bottle. The height above the sea is 90 feet. I placed the bottle in a small cask, about half an inch higher than the bottle and funnel included, which protected them from the wind, whilst it did not prevent the rain from entering.'

New Pitsligo, Aberdeenshire.—Sent by Mr. David Sturrock, who writes: 'The bottle was set on the grass,

about 500 feet above the level of the sea. My house is on the west of the village, and I have exposed the bottle only when the wind was not blowing the smoke of the village towards it. Part of the rain was collected in the middle of December; wind S. by SW. Part on February 6 and 7, during which two days 1·31 inches of rain and sleet fell. During the time the weather has been rough and frosty, with a few days of thaw now and then.'

Berkshire, Streatley Vicarage, near Reading.—The Rev. John Slatter, who sent this specimen, says: 'Streatley Vicarage is 150 feet above the sea; the bottle with funnel was placed on a grassplot 40 yards SW. of the house. On the NW. and N., at a distance of 400 to 600 yards, lies the village of Streatley, with little more than 70 houses. On the NE., at a distance of 800 to 1,200 yards, the village of Goring, of about twice the size, separated by the river Thames. The rain was collected between January 1 and February 7, 1870; the first half was with the wind from SSW. to WSW., the latter half from the SSE. At a distance of nine miles SE. by E. lies the town of Reading, from SSE. to WSW. extends a country of hill and wood without large towns near, and no mills for 60 miles to the English Channel.'

Tring.—This was sent by the chairman of the Board of Health there.

Agricultural College, Cirencester.—Specimen of rain sent by Professor A. H. Church, who has published an account of the salt in the Cotteswold storm-water.

Rugby.—Specimen sent by Mr. Jas. Arch. Campbell. The situation of Rugby is well known.

Several specimens were collected in *London* by Dr. Stevenson, of Guy's Hospital; one at Eversham Road, NW.; another in Hampstead Road, close to Cumberland Market, Regent's Park, east side, about 115 feet above the sea-level. The bottle and funnel were placed 5 feet above the ground on blocks of clean newly-dressed granite. I

am indebted to Dr. Stevenson for several others, as he has requested some of his friends to send specimens.

Dr. Woodforde, officer of health, sent a specimen collected about 1,000 yards north of the chemical works on *Bow Common*. He says: 'The direction of the wind has varied greatly during the period of collection (about two months), and care has been taken to guard against accidental splashing, dripping, &c., and collection in violent driving wind and rain, so that I hope the sample is a fair specimen of the rain-fall in my locality.'

Dr. Stevenson collected samples at Guy's Hospital very carefully 'during one heavy rain-fall in November and December respectively.' These samples have great value, but even they have a fault as before mentioned, the heavy rain, excepting at first, being pure beyond the average. They will require to be taken along with others to secure the true average.

Berkeley, Gloucestershire.—Mr. John Williams sent a specimen from Whitfield.

Newcastle.—Mr. Robert Foster sent a specimen from a rain-gauge. It was found that the specimens from gauges were not fair representatives, a great quantity of organic matter having accumulated sometimes for years.

Islington.—Dr. Ballard sent two specimens, one from a rain-gauge and one collected specially. The difference was great. He suggests that it be collected in different parts of London on the same days. He is right, and this more detailed examination has to a certain extent been attempted, but it requires to be done often.

Hackney.—Dr. Tripe was so good as to send this specimen.

East Dereham.—By the aid of the Rev. J. Crompton, of Norwich, I received several from that neighbourhood; from East Dereham one was sent by the Rev. J. M. De Port. It was obtained at Mattishall Vicarage, 165 feet above the sea-level. The collection was between May

and November, but the time was of little importance, since the bottle was not left out in Mr. De Port's absence.

Tenbury.—A specimen was sent by Mr. Philip J. Williams, of Stoke House.

Aldershot.—By Mr. James P. Blackburn, of Camp Farm.

Beccles, near Norfolk.—Mr. E. T. Dowson sent a specimen. He specially mentions his great care in putting on the stopper when dry weather intervened. 'The wind was,' during collection, 'partly from SW. to NW. and SE. to E. The two principal falls, which were only ·33 and ·27 inches, were when the wind was easterly.' The bottle was in a terraced garden, on a flower border, with no flowers of its height near. The height of the bottle was 1 ft. 4 in. above the ground, and above the sea about 40 feet.

Bracondale, Norwich.—The Rev. J. Crompton sent a specimen collected in his garden; 'aspect ESE., about 20 or 25 yards from the house. Three lofty factory chimneys were about 150 or 200 yards off, other house and cottage chimneys not far off. Rain has been chiefly collected when the wind blew the smoke from the bottle. Dirty weather all the time; some snow and sleet; wind chiefly SW. and W.'

Abingdon.—The Rev. J. C. Clutterbuck sent a specimen, but it is the only one which seems not to have been traced. I am sorry for it, as he sent also tables of the weather during the time of collection.

Banbury, Warwick Road.—Dr. Garrett sent a specimen. It was collected by planting the bottle on a flat leaden roof.

Baschurch, Salop.—Mr. W. Dickenson Paley collected a specimen here.

Mr. Cresy, of the Metropolitan Board of Works, mentioned to the chairman my wish to collect specimens of rain, and I was introduced to Captain Shaw, who kindly took much trouble in the matter. Mr. Robert H.

Scott, of the Meteorological Office, gave also introductions to gentlemen interested in meteorology, as also did Mr. J. S. Smith and Mr. Morton, of the Rivers Pollution Commission. I have thus put myself under obligations to many personally unknown to me. In some cases the specimens came too late; in some the bottles were broken by weather and other accidents; but a considerable number came safely.

Valentia.—This was sent by the Rev. T. Kerr from the observatory there.

Perth.—Mr. E. W. Phibbs was kind enough to obtain several specimens for me in Scotland, one from Perth, in the town, one from the tower, and one of snow from a neighbouring hill. Some bottles that he put on other hills —Moncrieffe Hill, for example—were broken by the frost.

Mr. A. Macdonald sent a specimen from Ballinluig, and Mr. Ewen Macdonald one from Aberfeldie.

Dundee.—A specimen sent by Mr. Alex. Miller, of the Blyth School, was collected at the height of 30 feet, on the roof of the Blyth School, in the middle of the town.

Braemar.—A specimen sent by Mr. Jas. Aitken, 1,114 feet above sea-level on a green lawn, western exposure.

Nookton, Windygate, Fife.—By Mr. W. M. G. Miller; says: 'Considering the very long time it has been in hand, it can scarcely be considered altogether free from impurities, although every care was taken. In the month of December, 10th to 18th, with a fall of one inch, the first large contribution to the bottle came; we had winds mainly SW. to W. and in considerable force. The other months were rather dry. The ground where the rain was collected was laid out in permanent grass. It has a southern aspect and slope, with trees near. 80 feet above sea-level. A little snow is included.'

Glen Urquhart.—Mr. Ogilvy, in his absence from Corrimonie, gave directions to his gardener, who writes that 'the bottle is placed 4½ feet from the ground, and about

level with the top of the house; 600 feet above the sea,' in an open place free from the smoke.

Islay.—Mr. Ballingall, of Eallabus House, was kind enough to put two bottles on the highest point of Jura, but both were broken by storms. He took a rain sample near his house.

Carnwath, Lanarkshire.—Mr. William Currie has so fully described the circumstances under which the specimen he sent was collected, that it is better to give his account entire. It will show conditions which in other cases were either the same or similar.

'*General Description of the Weather at Carnwath, Lanarkshire, while Specimen of Rain-Water was being collected for Dr. Smith; bottle having been set on November* 13, 1869, *and lifted on January* 9, 1870.

Date.	Rain-fall.		Wind.		Weather.	Temperature.
	No. of hours.	Amount in inches.	9 a.m.	9 p.m.		
1869.						
Nov. 14	18	1·09	WSW.	WSW.	Gale of wind and heavy rain .	
,, 15	·3	·05	S.	SSW.	Do.	Highest, 54·5°
,, 16	4·7	·13	SW.	SW.	Do.	Lowest, 36·8°
,, 17	·5	·01	SW.	SSW.	Gale of wind, afternoon and night; heavy dew, morning	Range, 17·7°
,, 18	·5	·01	SW.	WSW.	Gale of wind . . .	
,, 19	3	·10	WSW.	WNW.	Do.	
,, 20	1·5	·06	WNW.	NW.	Frost and light wind . .	
,, 21	0	·00	SW.	SW.	Thaw and rain, night . .	
,, 22	7	·37	SW.	0	Calm, fog and hoar frost .	
,, 23	0	·00	0	0	Do. do. .	
,, 24	0	·00	0	0	Frost	
,, 25	2	·04	SSW.	SW.	Frost, light wind . . .	
,, 26	5·5	·25	W.	SW.	Rain and heavy snow and sleet showers, stiff breeze .	
,, 27	8	·26	SW.	SW.	Snow showers and frost, stiff breeze	
,, 28	3	·08	W.	WNW.	Frost, snow and sleet showers, stiff breeze	
,, 29	6	·09	WSW.	WNW.	Frost, light wind . . .	
,, 30	0	·00	NW.	NW.	Hard frost, light wind . .	
Dec. 1	0	·00	NW.	0	Haze and hard frost . .	
,, 2	0	·00	SSE.	ESE.	Hoar frost, temperature very low	Highest, 38·4°
,, 3	0	·00	ESE.	NNE.	Dense fog do. .	Lowest, 10·°
,, 4	0	·00	NNE.	ENE.	Thaw, mizzling rain very light	Range, 28·4°
,, 5	4	·05	W.	0	Aurora, hard frost . . .	
,, 6	0	·00	0	E.	Frost	
,, 7	0	·00	ESE.	E·	Frost and hoar frost . .	

'General Description of the Weather at Carnwath, Lanarkshire—continued.

Date.	Rain-fall.		Wind.		Weather.	Temperature.
	No. of hours.	Amount in inches.	9 a.m.	9 p.m.		
1869.						
Dec. 8	0	·00	E.	E.	Thaw, light breeze	
„ 9	0	·00	0	E.	Fog, light airs	
„ 10	1	·02	SSW.	SW.	Gale, afternoon and night	Highest, 48·5°
„ 11	18	1·14	SW.	W.	Gale, continued with heavy rain	Lowest, 29·°
„ 12	0	·00	S.	WSW.	Frost and thaw, hail shower	Range, 19·5°
„ 13	3	·25	SSW.	SW.	Gale, sleet showers	
„ 14	5	·34	SW.	SW.	Gale and rain	
„ 15	3·5	·33	W.	WSW.	Very heavy snow and sleet showers, gale	
„ 16	1	·02	SW.	SSW.	Stiff breeze	
„ 17	2	·15	W.	WSW.	Snow showers and stiff breeze	Highest, 52·°
„ 18	4	·15	SSW.	W.	High wind	Lowest, 17·9°
„ 19	10	·38	SW.	WNW.	Do.	
„ 20	6	·11	SW.	WSW.	Snow and sleet showers, stiff breeze	Range, 34·1°
„ 21	5	·46	0	0	Dense fog and rime, calm	
„ 22	0	·00	0	N.	Do.	
„ 23	0	·00	SW.	N.	Winds, light and variable	
„ 24	0	·00	NNW.	N.	Do. do.	Highest, 39·6°
„ 25	0	·00	WNW.	NW.	Light wind	Lowest, 3·6°
„ 26	1·5	·08	NE.	NNE.	Gale and snow showers	
„ 27	4	·20	NNE.	0	Gale continued, night; light airs, day	Range, 36·°
„ 28	0	·00	0	SSW.	Severe frost and nearly calm	
„ 29	0	·00	SSW.	SSW.	Thaw; gale	
„ 30	12	·35	SSW.	SW.	Gale, sleet showers	
„ 31	14	·41	SW.	SW.	Gale	Highest, 44·5°
1870.						Lowest, 29·°
Jan. 1	0	·00	SE.	NE.	Misty clouds, light air	
„ 2	4	·08	SE.	E.	Do. do.	Range, 15·5°
„ 3	0	·00	ESE.	SSE.	Light airs, aurora	
„ 4	6	·24	S.	WSW.	Heavy showers and stiff breeze	
„ 5	5	·24	SW.	WSW.	Stiff breeze	
„ 6	1	·03	SSW.	S.	Frost, night; thaw, day; stiff breeze	Highest, 44·3° Lowest, 31 1°
„ 7	8	·27	WSW.	S.	Rain, night; high wind	
„ 8	8	·45	N.	W.	Do. do.	Range, 13·2°
„ 9	2	·14	NNW.	NW.	Heavy rain, early morning, and stiff breeze	

'*Note.*—The rain-fall is registered at 9 a.m. and marked as the fall for the day upon which it is recorded.

'The bottle was placed upon a grass-plot in a large garden, and sunk about 6 inches into the ground, the mouth of the funnel being about a foot above the ground. Height above sea-level, 702 feet. There was a house about 100 yards SW. of the bottle, and a vinery about 40 yards north of it. The west end of Carnwath village, containing between 700 and 800 inhabitants, lay about 60 yards NE. from the bottle. William Currie.

'*Carnwath, Lanarkshire.*

'March 9, 1870.'

Barry Village, near Carnoustie.—Mr. J. Procter placed three bottles at the heights of 12 feet, 25 feet, and 45 feet.

Galashiels.—Dr. Somerville obtained a specimen on grass about 30 yards SE. of his house. It was collected from the 10th to the 20th of May, wind being W., S., SE., and SW. This rapid collection must tend to give a favourable result.

Aberfeldy.—This specimen was collected by Mr. Alex. Robertson, of Lurgan, Ballinluig. It was gathered in two days. The height of the bottle from the ground was 4 feet, about 100 yards from nearest houses, and about 500 feet above sea-level.

North Esk Reservoir.—Mr. John Garnock placed the bottle on a grass-plot and sunk it in the ground. The top of the funnel was about 10 inches above the ground; the height about 1,150 feet above the sea.

Greenock.—This specimen was collected at the Shaws Water Chemical Works by Mr. Thomas Robertson Ogilvie. It is therefore from a high part of the town.

Before concluding the description of the places and modes of collection I must repeat my precautions, lest the numbers should be held as truer than it is in our power to make them. One difficulty I have not been able to surmount; it lies in the fact that, if the bottles are much exposed to air, so much dust collects that it may be said to interfere with the exactness of the results. On the other hand, if the bottles are not exposed except during rain, they receive very little of the floating matter of the air, and the result is again unfair. Perfectly aware of these difficulties, I have still gone forward: the first enquiries are seldom perfect. When the season is very wet, and the bottles are filled very rapidly, it may be said that an exaggerated idea of the purity of air is obtained; on the other hand, when the season is very dry, and a long time is required to fill the bottles, an

exaggerated idea of the impurity follows. In order to obviate this difficulty entirely, it is my belief that the air-washings will be resorted to more than the examination of the rain. I give only comparatively few of these, but, as they are not subject to any of the difficulties connected with the collection of rain, they must be increased. I must not, however, exaggerate the difficulty. The specimens from Glasgow were collected by an exposure of the bottles for several months, and, as the season was dry and the town large, I was inclined to leave them out and have fresh samples taken. It turns out, however, that one of the worst is from a height of 82 feet, being collected on a church-tower in Bridgegate. This does not point to any small local accumulation of mere dust, but a complete filling of the atmosphere. The place was above all the houses around. In Manchester one of the worst specimens was collected at the height of almost 30 feet, and a similar was found frequently.

The London specimens, which Captain Shaw, of the Metropolitan Fire Brigade, was so good as to cause to be collected at the various stations, were, as I believe, too long exposed to dry weather to compare with the others, and an excess of matter is obtained in them. This has induced me to leave them out of the general average, not that I look on them as valueless; on the contrary, they are most valuable as showing the changes which take place in cistern-water much exposed, and the necessity of frequently cleaning vessels, whether covered or uncovered, and perhaps we shall learn more from them. The bottles were partially-closed cisterns; the funnels were similar to the roofs often used for collecting water.

The other London specimens from which the average is taken may have been too carefully collected. It is probably better to try London again. It is so very large that it deserves a special enquiry, and of this I was

sufficiently aware, but many seasons are required for the purpose.

The free acids are not increased by the dust unless evaporation takes place.

ANALYSES OF THE RAIN SPECIMENS.

Inorganic Substances.

COMMON SALT. HYDROCHLORIC ACID, OR CHLORIDE.

It has often been observed that chloride of sodium, or common salt, is found on windows far from the sea when a violent wind is blowing. It was interesting to know how far this might be considered the natural mode of supplying that substance to soils. We know that plants use it and that animals require it. Experiments show that it is found in greatest quantities in the air of the coast, but that there is another source of it—namely, the coal burnt in our towns. The rule is that it diminishes as we leave the coast, and increases again as we enter towns; but it does not rise so high in towns as it does during wind from the sea and near the coast. Taking London and the south, it is the distance from the western sea chiefly that governs the amount. There is also an increased amount when we approach alkali-works and such glass-works and potteries as decompose salt. The common salt from the sea is not spray, or at least not spray purely; if it were so, it would have the same relative amount of sulphates to chlorides as we find in sea-water. The amount of hydrochloric acid represented by the salt is given, so as to make comparison easy. To some people it would have been easier to have read the numbers off as common salt, which would be higher than hydrochloric acid, as 100 to 160, leaving out the decimals, which would make it 160·27.

The chlorides become, therefore, a test both of sea-water and of coal-burning, especially as connected with

certain manufactures. When the sea is the source the rain will not be acid, but when the source is coal acidity will arise from the sulphur acids. If alkali and similar works give out the chlorine or chlorides, there will be acidity from the muriatic acid almost certainly.

The practical value of these enquiries can be readily understood at once, since the mode of examining places for sanitary purposes becomes comparatively simple so far as acids are concerned.

Sulphates.

As sulphur compounds are found in the atmosphere where no coal is burnt to account for them, it is necessary to look to an additional source. We know that there is an enormous amount of decomposition constantly going on, and sending its products into the air. The vegetation of the year decays with such speed, that we may probably say that it equals yearly the amount of growth. We know, too, that sulphur is found in all the most important liquids and solids of animals, in fibrin and albumen from the muscle and the blood, and also from the food as prepared for them by vegetation. These albuminoid substances are called by Mulder compounds of protein. This name, although formed in part through an imperfect theory, is worth preserving, because of the prominence these bodies have in all living structures, taking in reality, as that chemist said, the first rank. Now we know that, besides nitrogen, these bodies contain sulphur. When they decay sulphuretted hydrogen is given out, and sulphide of ammonium ; but these in time are oxidised into sulphate of ammonium, or with another base in conjunction, according to circumstances. It is easily seen, therefore, why sulphates should increase as we leave the sea. They increase over the land because of vegetable and animal decomposition.

The amount of the sulphates by weight in the atmosphere is great, but the comparative amount is not high. It is, however, important to estimate it, so that we may be guided to a mode of expressing in chemical and exact language the meaning of a crowded town or room or an imperfect atmosphere and an open place. If we take the atmosphere of the small islands in the western sea for example, we obtain very little sulphate and no free sulphuric acid. If we go inland, we find the first increased, or its proportion to the chlorides raised; if we take the air of high grounds, we find the amount approaching to that at the sea. We can readily believe that sulphates can be carried from the sea exactly as chlorides are; but we can see no mode of their increase when the air is inland, and far from manufactures, except by the accumulation from organic sources or in connection with organic matter. The inorganic sources of sulphuretted hydrogen are few and small. If this be the case, the sulphur is an index of the amount of vegetable and animal matter decomposed whenever we can abstract that which comes from coal. It is not easy to do this well, and many refinements may yet gather around this subject; but the main results are abundantly clear, and the conclusions to a certain extent equally so.

The sulphur compounds arise from the ground in conjunction probably with the ammonia; both will increase together when the source is organic, and we may add also when the combustion of coal is the source, although in that case the ammonia is partly destroyed by the heat.

When they are products of decomposition, they ought to be found in greatest quantities near the ground, and such is the result. They increase in towns in a double ratio, because of the decay of vegetable and animal matter, and because of the combustion of coal.

The sea-water of the British Channel contains chlorides

and sulphates, the former in great excess. If we calculate the chlorine as hydrochloric acid, and compare it with the sulphuric acid in the sulphates, we have hydrochloric acid 1 to ·1134 sulphuric anhydride. This is from Schweitzer's analysis as seen in Professor Miller's chemistry. It was found convenient to put down the acids in this manner since they compare well, whilst the fixed bases were seldom examined. It might have been better to put down chlorine instead of hydrochloric acid, but the fact of examining for the latter in a free state was one reason for the preference.

For purposes of comparison the proportion of chloride and sulphur makes a table by itself. The acidified sulphur includes vegetable, animal, and manufacturing activity where coal is used; it measures density of population and work when the distance traversed is the same.

As a rule rain is not acid far from towns. If it is acid, artificial circumstances must be suspected.

Ammonia.

When organic substances undergo decomposition the nitrogen goes off with hydrogen, forming ammonia, unless strong oxidising influences are present. The ammonia found is in all probability a fair measure of the amount of organic impurity in the atmosphere; it is so of the amount of organic matter which has sent its products into the air. But ammonia itself is not an infection; a small amount, even when constant, may not be hurtful. We like the carbonate in the form of smelling salts, and we know that the alkali acts in such a way as to diminish the hurtful tendency of sulphuretted hydrogen. The ammonia mentioned in the table is not free—or at least it is seldom free—it is united to sulphuric, muriatic, or carbonic acid. Now, although it is not advisable to breathe much of such salts as the chloride, or sulphate,

or even carbonate, I am not aware of any evil done by them in small quantities. If no harm is known, why is it not advisable to breathe them? may be asked. The reason is that in larger quantities they are unpleasant, and produce a stifling sensation; we therefore infer that a proportionate harm will be done by small quantities; the proof is not full, but, considering also that nature furnishes only minute quantities in pure air, the probability is that they are better absent. Still they are to be avoided for a stronger reason, viz., their usual company—their presence indicates objectionable or decaying matter, and that matter may send out into the air worse substances than ammonia. The most dangerous compounds seem to be such as preserve their organic character completely, and of these the worst portion is that which preserves its organised character. This is capable of being decomposed into ammonia and other bodies, and the processes so largely carried out by nature may be effected artificially and the amount of produce measured. Table VI. is therefore added to show the amount of ammonia contained in substances of complete organic character, germs of plants and animals included.

This table indicates the amount of animal and vegetable matter containing nitrogen, and existing undecomposed in the rain at the time of the experiment. We have now this knowledge in our power, and may find the number for any time or place. Similar experiments may be made on the air where no rain falls.

It may be that in some places the albumen will be more oxidised than at others, depending on the amount of sun and of ventilation as well as of soil, not forgetting the times of the year and day. It may be interesting to give the results of the combined amounts of ammonia, inorganic and organic, as we may call the two sources.

Nitric Acid.

When the oxygen of the air acts on the ammonia, or at least on the organic substances containing nitrogen and capable of giving out ammonia by their decomposition, it forms nitric acid, which is a compound of oxygen and nitrogen. It is remarkable that this acid increases in quantity, according to the height from the ground, within certain limits.

In speaking of this it is pleasant to refer to the work done by M. Robinet and M. Ad. Bobierre in the year 1863, therefore before my tables of ammonia and nitric acid were made.

Some Facts Relating to Rain-Water, by M. Robinet. 'Comptes Rend.' 1863, p. 493. Rain-Water collected at Paris between March 1, 1862, and September 1863.

'1. The agitation of the air seemed to have no influence on the amount of fixed substances dissolved in the rain.

'2. Day and night were also without influence.

'3. If portions of a shower are collected one after the other, or if several showers of the same day are tried, it is found that the amount of fixed matter goes on diminishing.

'4. The Paris rain-water contains principally sulphate of lime and an organic substance little known. The amount of sulphate of lime may rise as high as 20 grammes per cubic mètre.

'5. When mixed with nitrate of silver the Paris rain-water becomes of a red colour of various tints, and then forms a deposit of a garnet colour. The garnet-coloured substance contains silver. The nature of this body is not known.'

Researches into the Chemical Composition of Rain-Water Collected at Different Altitudes, by AD. BOBIERRE. 'Comptes Rend.' 1864, p. 755.

'In 1863, I evaporated in an oil-bath 372 litres of rain-water having carbonate of soda present, and determined month by month the amount of nitrogen in the fixed organic matter, the nitric acid, and the chlorine; then, by fractional distillation, by Boussingault's method, I determined the amount of ammoniacal nitrogen.

'Suspended matter was separated by filtration and examined by a microscope. I extract from my Memoir some of the principal figures, which show the nature of the results (at 47 mètres height and down below).

Grammes of Ammonia, Nitric Acid, and Chloride of Sodium, in a Cubic Mètre of Rain-Water collected at Nantes in 1863.

Month.	Ammonia.		Nitric Acid.		Chloride of Sodium (Common Salt).	
	154 feet high.	Below.	154 feet high.	Below.	154 feet high.	Below.
January .	5·225	6·398	5·790	3·200	14·10	8·40
February .	4·610	5·900	,,	,,	15·10	10
March .	1·880	8·620	7 115	5·980	16·10	11·90
April . .	1·840	6·680	: 309	1·813	7·30	9·20
May . .	·747	4·642	3 501	1·998	5	9·40
June . .	2·222	3·970	13 218	10·237	15	17·40
July . .	·272	2·700	,,	16 ,,	,,	,,
August .	·257	2·112	15·520		14·80	19·30
September .	1·432	5·512	9·999	5·720	11·20	14·80
October .	1·688	4·289	4·989	3·198	12	9
November .	·593	4·480	6·278	5·574	22·80	26·10
December .	3·178	15·665	4·890	3·100	21·60	16·30
Mean .	1·997	5·939	7·360	5·682	14·09	13·80

'The following conclusions were drawn:—

'1. The composition of the rain of large towns is very variable.

'2. The variations observed, and which result from the

purification of the air by washing, are particularly remarkable, so far as ammonia, nitric acid, and oxygen are concerned.

'3. The examination of rain-water leaves no doubt as to the relation of the different substances.

'4. In the rain-water collected at Nantes in 1863, at 47 mètres high, the amount of ammonia is 1·997 grammes in a cubic mètre. In the lower and unhealthy parts it is 5·939.

'5. The amount of nitric acid increases as the amount of ammonia diminishes. The cubic mètre of rain contains 7·36 in the upper and 5·682 in the lower parts of the town.

'6. The composition of 372 litres at Nantes shows that the variation in organic matter and alkaline chlorides is more marked than that of other substances. The cubic mètre gave in this place 13·90 as the amount of chlorides expressed as common salt.'

RAIN.

COMPARATIVE RESULTS.

The actual amount of the substances found in the rain and air being of less importance to us than the comparative, I shall give it a secondary place and begin with the latter. The actual weights are so small that we might readily suppose them as representing bodies utterly incapable of influencing the life of the larger animals. The idea of size and quantity must be secondary when considering that force which is chemical, but still more so when organisms are the agents. Chemical action is by equivalents, and these presuppose quantity, but there are some strange anomalies. Perhaps one quantity may act an enormous number of times in a second. Our only method is to compare places known to be healthy with those known to be otherwise.

RAIN. TABLE I.—*Summary of Results.*

COMPARATIVE. HYDROCHLORIC ACID (CHLORIDES).

That of Valentia (Ireland) taken as 100.

Darmstadt—Germany	2
London, 1869	2·6
German specimens	2·7
Birkenhead, near Liverpool	6·5
Scotland—Inland country places	6·9
Near an Alkali-Works (standing alone inland)	7
England—Inland country places	8·2
Manchester, 1870	11·9
„ 1869 and 1870 average	12
Scotland—Towns (Glasgow not included)	12
Manchester, 1869	12·1
Newcastle-on-Tyne	16·7
England—Seven towns, manufacturing	17·9
Glasgow	18·4
St. Helens	19·6
Liverpool	20·9
Scotland—Sea-coast country places, west	25·2
„ „ average	25·9
„ „ east	26·5
Runcorn	52·9
Waterloo, near Liverpool	75
Ireland—Valentia	100
England—Sea-coast country places, three, west	115·4

The best London specimens were collected with remarkable care, the vessels exposed only during rain. Other London specimens were, again, too long out, and are therefore given separately. The German specimens were too few.

It is, of course, not intended to assert that hydrochloric acid exists in the air of all these places mentioned, but rather common salt, its equivalent. There is, however, no common word sufficient to express them both except *chloride*.

It will be seen here that the amount of chlorides

depends on two causes mainly; these are (1st) the distance from the sea and (2nd) the distance from manufactories. Chlorides, as well as sulphur acids, must be considered a product of the combustion of coal. To the distance from the sea as a main cause must be added the amount of wind. Valentia affords, probably, a good specimen of a sea atmosphere; it has, therefore, been taken as 100. The London specimens here given are not numerous, but were carefully collected by scientific men.

The east coast specimens have a little more chloride than the west. This is caused, I believe, by the more sheltered character of the latter. The manufacturing towns of England alluded to are near the coast, and so have a large amount naturally, but this is increased by alkali-works, glass-works, potteries, and many other things independently of the coal itself.

This table, like the others, gives results which must be used with discrimination. An excess of chlorides taken alone is no proof of a bad atmosphere, but the excess in an inland place above the amount in the district surrounding it is a certain proof of impurities being thrown into the atmosphere; and we have here various degrees, and can measure how far they can be endured by the way in which the residents are affected.

RAIN.

SULPHURIC ACID, OR SULPHATE.

The sulphate has been said to be so important that, when coal-smoke is not in question, it may be held to indicate the amount of the sewage of the atmosphere. The most dangerous products of decomposition come from substances containing nitrogen and sulphur. The first we find in ammonia and nitric acid, the second in sulphate—that is in an acid of sulphur, either free or united

with a base. When sulphur has united with oxygen its organic relations cease, and we have then a mineral acid. It is interesting to see how the sulphate increases as we enter on the more inhabited places, and when we leave the sea-shores for inland places. Still greater is the increase among dense masses of people, and especially in the presence of great manufactories burning much coal. Of these places again those have most sulphate where sulphuric acid is manufactured and utilised. This we might expect. It is remarkable, also, how much less it is in places where house-fires only are burnt than where high chimneys consume great amounts of coal in a small space, and frequently also an inferior quality of coal, such as would be too sulphurous for domestic fires.

The word *sulphuric* is used instead of *sulphurous* acid, although it is the second which is most perceived in towns. It does, however, pass rapidly into sulphuric, and the two are found mixed in the rain. They are thrown together as sulphuric anhydride in the table.

RAIN. TABLE II.—*Summary of Results.*

COMPARATIVE. SULPHURIC ACID (SULPHATES).

That of Valentia (Ireland) taken as 100.

Scotland—Inland country places	75·5
Ireland—Valentia	100
Scotland—Sea-coast country places, west	132·2
England—Inland country places	202·2
Scotland—Sea-coast country places, average of E. and W.	206·4
England „ „ west	215·4
Scotland „ „ east	280·6
Waterloo, Liverpool	418·7
German specimens	600 7
Scotland—Towns (Glasgow not included)	604 4
London, 1869	750·5
Birkenhead, near Liverpool	848·2
Runcorn	865·2
Darmstadt—Germany	1068·5

St. Helen's	1215·8
England—Towns	1255·3
Liverpool	1450·2
Manchester, 1869	1526
Newcastle-on-Tyne	1627·7
Manchester, average of 1869 and 1870	1641·9
Manchester, 1870	1757·8
Glasgow	2571
Near an Alkali-Works (isolated)	2685

Glasgow gives a remarkably high number. Its great mortality seems to be explained by the tables as a whole. The table above would not be sufficient, but is an important one nevertheless.

Sulphuric acid, or sulphate, is a measure of manufacturing activity and also of decomposition. It is a part of the oxidised, and therefore purified, sewage of the air. It increases inland, even without smoke, on account of the decomposition of vegetable and animal matter. This is best seen in the table of proportions.

RAIN.

TOTAL CHLORIDES AND SULPHATES.

If we join these two acids, we obtain a very instructive table. It must, however, be read with discrimination, remembering the observations made under the separate heading of sulphate and chloride. The chlorides, it was said, increase as we approach the sea and as the wind rises, blowing from the sea. Such a source of chlorides cannot be called unwholesome. They increase also as the towns are approached, because they exist in the coal smoke, so that they have an innocent as well as a noxious source.

RAIN. TABLE III.—*Summary of Results.*

COMPARATIVE. TOTAL ACID (ACIDS OF CHLORIDES AND SULPHATES).

That of Valentia (Ireland) taken as 100.

Scotland—Inland country places	10·6
England „ „	18·5
Scotland—Sea-coast „ west	30·9
German specimens	34·4
Scotland—Sea-coast country places, average	35·5
„ „ „ east	40
London, 1869	42·3
Scotland—Towns (Glasgow not included)	43·5
Birkenhead, near Liverpool	51·2
Darmstadt—Germany	58·6
St. Helen's	83·1
England—Towns	83·6
Manchester, 1869	92·5
Waterloo, near Liverpool	93·2
Runcorn	96
Liverpool	96·8
Manchester, 1869 and 1870, average	98·5
Ireland—Valentia	100
Newcastle-on-Tyne	102·2
Manchester, 1870	104·6
England—Sea-coast, west, one specimen	120·7
Near an Alkali-Works	149·2
Glasgow	154

This compound table is very interesting and, with precaution, may be used as a guide to purity of air, but the great amount of chlorides in the Valentia specimen, shows that we must carefully eliminate in such cases the sea influence. Valentia, as a pure air, will probably stand at the beginning, as we cannot suppose the pure salt will injure. When, however, this salt comes from coal it is accompanied by other substances, and is a measure of impurity.

RAIN. TABLE IV.—*Summary of Results.*

COMPARATIVE. ACIDITY.

That of the lowest (Scotland, sea-coast country places, west) being taken as 1 or 100.

Ireland—Valentia	None
England—Inland country places	„
„ Sea-coast „	„
Scotland „ „ west	1
„ Inland „	2·27
German specimens	9·22
Scotland—Sea-coast country places, average	9·30
Darmstadt—Germany	12·56
Scotland—Sea-coast country places, east	17·61
„ Towns (Glasgow not included)	22·85
London, 1869	27·97
St. Helen's	28·71
Manchester, 1869	60·13
England—Towns	61·57
Manchester, average of 1869 and 1870	73·44
Runcorn	82·40
Liverpool	83·46
Manchester, 1870	86·76
Glasgow	109·16

This table is a very good measure of the impurity caused chiefly, if not wholly, by inorganic acids. The slight amount in the first two numbers (for Scotland) was not quite certain, the difference was so small; however, there seemed to be slight appearances, and the figures are retained.

Acidity is caused almost entirely by sulphuric acid, which may come from coal or the oxidation of sulphur compounds from decomposition, but it may also be caused in manufacturing towns by other acids, and in country places to a small extent by nitric acid and by acids from combustion of wood, peat, turf, &c. This measures the rapidity of waste in a country by burning

or by decaying matter, a partial measure of activity therefore.

RAIN. TABLE V.—*Summary of Results.*

COMPARATIVE. AMMONIA.

That of Valentia (Ireland) taken as 1 or 100.

Ireland—Valentia	1
Scotland—Sea-coast country places, west	2·69
„ Inland „ „	2·96
„ Sea-coast „ average	4·10
„ „ „ east	5·51
England—Inland „ „	5·94
„ Sea-coast „ west	10·55
German specimens	10·61
London, 1869	19·17
Scotland—Towns (Glasgow not included)	21·22
St. Helen's	25·33
Runcorn	25·72
England—Towns	28·67
Liverpool	29·89
Manchester, 1869	35·33
Manchester, 1869 and 1870, average	35·94
„ 1870	36·54
Glasgow	50·55

The ammonia is one measure of the sewage of the air: it is the result of decomposition. By itself it is not impure, or in these small quantities hurtful, so far as we know. This table is an indication of the processes going forward, and is for that reason important. The ammonia is in no case free, but combined probably with chlorine or sulphuric acid in towns. In country places it may be united to carbonic acid, wholly or partially.

RAIN. TABLE VI.—*Summary of Results.*

COMPARATIVE. ALBUMINOID AMMONIA.

That of Valentia (Ireland) taken as 1 or 100.

Ireland—Valentia	1
Scotland—Inland country places	1·15
„ Sea-coast „ west	3·09
„ „ „ average	3·11
„ „ „ east	3·1
England—Inland „	3·21
German specimens	3·59
Liverpool	4·67
Runcorn	5·59
London, 1869	6·03
Scotland—Towns	6·23
England „	6·29
Manchester, 1869	6·38
St. Helen's	6·76
Manchester, 1869 and 1870, average	7·38
„ 1870	8·38
Glasgow	8·82

This table is a measure of sewage of air not purified, and includes the most dangerous substances, germs of living things, vegetable and animal. The process for determining the nitrogen from them may not include absolutely all; this may be for future enquiry, but the table is no less useful or interesting on that account, as we can make use only of comparative results. One sea-coast country place, England west, gave a number 11·76. I thought it absurd to put such a result among the averages, as it was unique as well as contradictory. That specimen and another quite anomalous one, which was accounted for, are the only specimens the analyses of which are not included in the above.

RAIN. TABLE VII.—*Summary of Results.*

COMPARATIVE. NITRIC ACID.

That of Valentia (Ireland) taken as 1 or 100.

Runcorn	·75
Scotland—Inland country places	·83
Ireland—Valentia	1
England—Sea-coast country place, west	1
Scotland „ „ places, „	1·01
„ „ „ „ average	1·15
„ „ „ „ east	1·29
Liverpool	1·57
England—Inland country places	2·02
London, 1869	2·27
England—Towns	2·33
Manchester, 1869	2·39
Manchester, average of 1869 and 1870	2·79
Scotland—Towns (Glasgow not included)	3·14
Manchester, 1870	3·19
St. Helen's	3·82
Glasgow	6·72
German specimens	7·81

Nitric acid is another measure of purified sewage or oxidised nitrogenous bodies. The amount is a measure not only of the impure matter, but of the progress made in purifying or cleaning it. As before said the German specimens were few; if the average here is at all correct, we have a large amount of nitric acid as the probable peculiarity of a Continental or inland atmosphere. Possibly the greater amount of sulphur in our fuel may diminish the tendency to decay and formation of nitric acid from vegetable and animal matter. These are questions to be answered some day.

RAIN. TABLE VIII.—*Summary of Results.*

COMPARATIVE. PERMANGANATE TEST.

The least amount of Oxygen required in each case taken as 1 or 100.

Rain obtained from	Instantly. ·028 = 1	Total in a few minutes with acid. ·018 = 1
Scotland—Sea-coast country places, west	None	1
Ireland—Valentia	,,	2·72
German specimens	—	11·22
Scotland—Inland country places	1	14·67
,, Sea-coast ,, average	None	18·50
England—Inland ,,	,,	25·89
Scotland—Sea-coast ,, east	,,	36·06
Runcorn	—	60·55
England—Sea-coast country place, west	None	94·77
Scotland—Towns (Glasgow not included)	6·39	104·39
Manchester, 1869	7·03	113·89
St. Helen's	—	126·44
England—Towns	15·89	152·39
Manchester, average of 1869 and 1870	12	179·19
Liverpool	23·64	216·44
Manchester, 1870	16 96	244·50
Glasgow	23·96	557·78

This I consider a very remarkable table. It includes most of the other results, and gives a rough summary like a touch of common sense, simplifying the most tangled figures, and including organic matter and sulphurous acid.

SUMMARY OF THE AVERAGES.

Hitherto the numbers given have been comparative, the lowest as a rule being taken as one. The following tables give the actual numbers, each being itself an average number.

RAIN. TABLE IX.—*Summary of Results.*

AVERAGES. HYDROCHLORIC ACID (CHLORIDES).

Rain obtained from	Hydrochloric Acid (Chlorides).	
	Grains per Gallon.	Parts per Million.
Darmstadt	·0681	·97
London, specimens for 1869	·0872	1·25
German specimens	·0919	1·31
Birkenhead, Liverpool	·2217	3·17
Scotland—Inland country places	·2357	3·37
Near an Alkali-Works	·2380	3·40
England—Inland country places	·2795	3·99
Manchester, 1870	·4055	5·79
„ Average of 1869 and 1870	·4086	5·83
Scotland—Towns (Glasgow not included)	·4102	5·86
Manchester, 1869	·4118	5·88
Newcastle-on-Tyne	·5678	8·11
England—Towns	·6093	8·70
Glasgow	·6282	8 97
St. Helen's	·6670	9·53
Liverpool	·7110	10·16
Scotland—Sea-coast country places, west	·8600	12·28
„ „ „ average of, east and west	·8819	12·59
Scotland—Sea-coast country places, east	·9039	12·91
Runcorn	1·8022	25·74
Waterloo, near Liverpool	2·5550	36·50
Ireland—Valentia	3·4067	48·67
(England—Sea-coast country place, west, only one)	3·9308	56·15

RAIN. TABLE X.—*Summary of Results.*

AVERAGES. SULPHURIC ACID (SULPHATES).

Rain obtained from	Sulphuric Acid (Sulphates).	
	Grains per Gallon.	Parts per Million.
Scotland—Inland country places . . .	·1444	2·06
Ireland—Valentia	·1911	2·73
Scotland—Sea-coast country places, west .	·2529	3·61
England—Inland country places . . .	·3865	5·52
Scotland—Sea-coast country, av. of E. & W.	·3947	5·64
England—Sea-coast (west) country . .	·4116	5·88
Scotland—Sea-coast country places, east. .	·5366	7·66
Waterloo, Liverpool	·8004	11·43
German specimens	1·1481	16·40
Scotland—Towns (Glasgow not included) .	1 1553	16·50
London, 1869	1·4345	20·49
Birkenhead, near Liverpool . . .	1·6210	23·16
Runcorn	1·6537	23·62
Darmstadt—Germany	2·0417	29·17
St. Helen's	2·3232	33·19
England—Towns	2·3988	34·27
Liverpool	2·7714	39·59
Manchester, 1869	2·9163	41·66
Newcastle-on-Tyne	3·1111	44·44
Manchester, average of 1869 and 1870 .	3·1378	44·82
Manchester, 1870	3·3593	47·99
Glasgow	4·9139	70·19
Near an Alkali-Works	5·1310	73·30

RAIN. TABLE XI.—*Summary of Results.*

AVERAGES. PROPORTION OF HYDROCHLORIC TO SULPHURIC ACID (CHLORIDES TO SULPHATES).

	Proportion of Hydrochloric to Sulphuric Acid.
Ireland—Valentia	1 to ·056
England—Sea-coast country places, west	1 „ ·10
Scotland „ „ west	1 „ ·294
Waterloo, near Liverpool	1 „ ·31
Scotland, average sea-coast country places	1 „ ·447
„ Sea-coast country places, east	1 „ ·593
„ Inland „	1 „ ·61
Runcorn	1 „ ·92
England—Inland country places	1 „ 1·38
Scotland—Towns (Glasgow not included)	1 „ 2·82
St. Helen's	1 „ 3·48
Liverpool	1 „ 3·90
England—Towns	1 „ 3·94
Newcastle-on-Tyne	1 „ 5·47
Manchester, 1869	1 „ 7·08
Birkenhead	1 „ 7·31
Manchester, average of 1869 and 1870	1 „ 7·68
Glasgow	1 „ 7·82
Manchester, 1870	1 „ 8·29
German specimens	1 „ 12·49
London, 1869	1 „ 16·45
Near an Alkali-Works	1 „ 21·56
Darmstadt—Germany	1 „ 29·98

RAIN. TABLE XII.—*Summary of Results.*

AVERAGES. TOTAL ACIDS OF CHLORINE AND SULPHUR.

Rain obtained from	Total Acids of Chlorides and Sulphates.	
	Grains per Gallon.	Parts per Million.
Scotland—Inland country places . . .	·3801	5·43
England „ „ . . .	·6661	9·51
Scotland—Sea-coast country places, west .	1·1129	15·89
German specimens, a few	1·2400	17·71
Scotland—Sea-coast country-places, average.	1·2760	18·23
„ „ „ east .	1·4392	20·57
London, 1869	1·5217	21·74
Scotland—Towns (Glasgow not included) .	1·5655	22·36
Birkenhead, near Liverpool	1·8427	26·32
Darmstadt—Germany	2·1098	30·14
St. Helen's	2·9902	42·72
England—Towns	3·0081	42·97
Manchester, 1869	3·3281	47·54
Waterloo, near Liverpool	3·3554	47·93
Runcorn	3·4559	49·37
Liverpool	3·4824	49·75
Manchester, average of 1869 and 1870 . .	3·5464	50·66
Ireland—Valentia	3·5978	51·40
Newcastle-on-Tyne	3·6789	52·55
Manchester, 1870	3·7648	53·78
England—Sea-coast, west, one specimen .	4·3424	62·03
Near an Alkali-Works	5·3690	76·70
Glasgow	5·5421	79·17

RAIN. TABLE XIII.—*Summary of Results.*

AVERAGES. ACIDITY.

Rain obtained from	Acidity calc. as Sulphuric Acid (Anhydride).	
	Grains per Gallon.	Parts per Million.
Ireland—Valentia	None	None
England—Inland country places . . .	,,	,,
,, Sea-coast, one place . . .	,,	,,
Scotland ,, country places, west .	·0097	·140
,, Inland ,, . . .	·0220	·314
German specimens	·0894	1·273
Scotland—Sea-coast country places, E. & W.	·0902	1·290
Darmstadt—Germany	·1218	1·740
Scotland—Sea-coast country places, east .	·1708	2·440
,, Towns (Glasgow not included) .	·2217	3·160
London, 1869	·2713	3·875
St. Helen's	·2785	3·980
Manchester, 1869	·5833	8·33
England—Towns	·5972	8·53
Manchester, average of 1869 and 1870 .	·7124	10·17
Runcorn	·7993	11·42
Liverpool	·8096	11·56
Manchester, 1870	·8416	12·02
Glasgow	1·0589	15·13

RAIN. TABLE XIV.—*Summary of Results.*

AVERAGES. AMMONIA.

	Ammonia. Parts per Million.
Valentia—Ireland	·180
Scotland—Sea-coast country places, west	·484
„ Inland „	·532
„ Sea-coast „ average	·738
„ „ „ east	·992
England—Inland „	1·070
„ Sea-coast „ west	1·900
German specimens	1·910
London, 1869	3·450
Scotland—Towns (Glasgow not included)	3·820
St. Helen's	4·560
Runcorn	4·630
England—Towns	5·16
Liverpool	5·380
Manchester, 1869	6·360
Manchester, 1869 and 1870, average	6·469
„ 1870	6·578
Glasgow	9·100

RAIN. TABLE XV.—*Summary of Results.*

AVERAGES. ALBUMINOID AMMONIA.

	Albuminoid Ammonia. Parts per Million.
Ireland—Valentia	·034
Scotland—Inland country places	·039
„ Sea-coast „ west	·105
„ „ „ average	·105
„ „ „ east	·106
England—Inland „	·109
German specimens	·122
Liverpool	·159
Runcorn	·190
London, 1869	·205

	Albuminoid Ammonia. Parts per Million.
Scotland—Towns (Glasgow not included)	·212
England „	·214
Manchester, 1869	·217
St. Helen's	·230
Manchester, 1869 and 1870, average	·251
„ 1870	·285
Glasgow	·300
England—Sea-coast country place, west	·400

RAIN. TABLE XVI.—*Summary of Results.*

AVERAGES. NITRIC ACID.

	Nitric Acid. Parts per Million.
Runcorn	·278
Scotland—Inland country places	·305
Ireland—Valentia	·370
England—Sea-coast country place	·371
Scotland „ country places, west	·372
„ „ „ average	·424
„ „ „ east	·476
Liverpool	·582
England—Inland country places	·749
London, 1869	·840
England—Towns	·863
Manchester, 1869	·886
Manchester, average of 1869 and 1870	1·032
Scotland—Towns (Glasgow not included)	1·164
Manchester, 1870	1·179
St. Helen's	1·413
Glasgow	2·436
German specimens	2·890

RAIN. TABLE XVII.—*Summary of Results.*

AVERAGES. PERMANGANATE TEST.

Rain obtained from	Parts by Weight. Oxygen required per Million Parts of Rain.	
	Instantly.	Total in a few Minutes with Acid.
Scotland—Sea-coast country places, west .	None	·018
Ireland—Valentia	„	·049
German specimens	„	·202
Scotland—Inland country places . . .	·028	·264
„ Sea-coast „ average .	None	·333
England—Inland „ . . .	„	·466
Scotland—Sea-coast „ east .	„	·649
Runcorn	—	1·090
England—Sea-coast country, west, one specimen	„	1·706
Scotland—Towns (Glasgow not included) .	·179	1·879
Manchester, 1869	·197	2·050
St. Helen's	—	2·276
England—Towns	·445	2·743
Manchester, average of 1869 and 1870 .	·336	3·225
Liverpool	·662	3·896
Manchester, 1870	·475	4·401
Glasgow	·671	10·040

ACTUAL AMOUNTS, OR DETAILS OF THE EXPERIMENTS.

The two sets of tables already given are sufficient to show the nature of the results, and are all that are required for general principles. The amounts obtained by experiment for any place may be compared with them and a position assigned accordingly.

It is, nevertheless, interesting to see the individual cases, and it is necessary to show the figures from which the averages are collected.

RAIN. TABLE XVIII.

HYDROCHLORIC ACID (CHLORIDES).

Sea-coast country places—Scotland.

EAST COAST.

Rain obtained from	Elevation.	Wind.	Date.	Hydrochloric Acid.	
				Grains per Gallon.	Parts per Million.
	ft.				
Kirkwall, Orkney . .	10	Southerly	December 30 to January 8	1·0220	14·60
Preston Kirk, Haddingtonshire	90	Westerly	Dec. 1869 to Feb. 1870	·5110	7·30
New Pitsligo, Aberdeenshire	500	Southerly	Dec. 1869 to Feb. 1870	2·8389	40·56
,, ,,	500	—	April 28 to June 12, 1870	1·0430	14·90
Nookton, Windygate, Fifeshire	80	Westerly	Dec. 1869 to April 1870	·1460	2·09
Barry Village, Carnoustie.	12	—	Dec. 1869 to June 1870	·7303	10·43
,, ,,	25	—	,, ,,	·6387	9·12
,, ,,	45	—	,, ,,	·3010	4·30
Average . . .	—	—	—	·9039	12·91

WEST COAST.

North Uist . . .	Low	—	March 1869	3·5770	51·10
Tyree	Low	—	May ,,	·3650	5·21
Kelly, Wemyss Bay . .	About 150	SW.; NE. short time	June 12 to 15, 1869	·0852	1·22
Auchendrane, Ayrshire .	97	—	Dec. 1869	·4258	6·08
Cargen, 3 miles south of Dumfries	70	SW. and NW.	Dec. 29, 1869, to Jan. 8, 1870	·3194	4·56
Row, Dumbartonshire .	—	—	Nov. 1869	·1277	1·82
Stornoway, Western Isles .	—	S. to SW.	—	1·0220	14·60
Eallabus, Islay . .	60	—	Oct. 1870	·9579	13·68
Average . . .	—	—	—	·8600	12·28

RAIN. TABLE XIX.

SULPHURIC ACID (SULPHATES).

Sea-coast country places—Scotland.

EAST COAST.

Rain obtained from	Elevation.	Wind.	Date.	Sulphuric Acid.	
				Grains per Gallon.	Parts per Million.
	ft.				
Kirkwall, Orkney . .	10	Southerly	Dec. 30, 1869, to Jan. 8, 1870	·5880	8·40
Preston Kirk, Haddingtonshire	90	Westerly	Dec. 1869 to Feb. 1870	·7350	10·50
New Pitsligo, Aberdeenshire	500	Southerly	Dec. 1869 to Feb. 1870	·7350	10·50
,, ,,	500	—	April 28 to June 12, 1870	·2646	3·78
Nookton, Windygate, Fifeshire	80	Westerly	Dec. 1869 to April 1870	·5880	8·40
Barry Village, Carnoustie .	12	—	Dec. 1869 to June 1870	·4683	6·69
,, ,,	25	—	,, ,,	·1999	2·86
,, ,,	45	—	,, ,,	·7140	10·20
Average . . .	—	—	—	·5366	7·66
WEST COAST.					
North Uist . . .	Low	—	March 1869	·5880	8·40
Tyree	Low	—	May ,,	·1176	1·68
Kelly, Wemyss Bay . .	150	SW.; NE. short time	June 12 to 15, 1869	·0588	·84
Auchendrane, Ayrshire .	97	—	Dec. 1869	·0882	1·26
Cargen, 3 miles south of Dumfries	70	SW. and NW.	Dec. 29, 1869, to Jan. 8, 1870	·0882	1·26
Row, Dumbartonshire .	—	—	Nov. 1869	·2262	3·23
Stornoway, Western Isles .	—	S. to SW.	—	·1764	2·52
Eallabus, Islay . .	60	—	Oct. 1870	·6799	9·71
Average . . .	—	—	—	·2529	3·61
Average of both coasts . . .				·3947	5·64

RAIN. TABLE XX.

PROPORTION OF HYDROCHLORIC TO SULPHURIC ACID (CHLORIDES TO SULPHATES).

Sea-coast country places—Scotland.

EAST COAST.

	Proportion of Hydrochloric to Sulphuric Acid.
Kirkwall, Orkney	1 to ·57
Preston Kirk, Haddingtonshire	1 „ 1·44
New Pitsligo, Aberdeenshire, Dec. to Feb.	1 „ ·26
„ „ April to June	1 „ ·186
Nookton, Windygate, Fifeshire	1 „ 4·02
Barry Village, Carnoustie, 12 ft. high	1 „ ·64
„ „ 25 „	1 „ ·31
„ „ 45 „	1 „ 2·37
Ratio of Average Acids	1 „ ·593

WEST COAST.

North Uist	1 to ·16
Tyree	1 „ ·32
Kelly, Wemyss Bay	1 „ ·69
Auchendrane, Ayrshire	1 „ ·21
Cargen, 3 miles south of Dumfries	1 „ ·28
Row	1 „ 1·77
Stornoway, Western Isles	1 „ ·17
Eallabus, Islay	1 „ ·71
Ratio of Average Acids	1 „ ·294
„ Average of both coasts	1 „ ·447

RAIN. TABLE XXI.

ACIDITY (FREE ACIDS).

Sea-coast country places—Scotland.

EAST COAST.

Rain obtained from	Acidity calculated as Sulphuric Anhydride.	
	Grains per Gallon.	Parts per Million.
Kirkwall, Orkney	None	None
Preston Kirk, Haddingtonshire	,,	,,
New Pitsligo, Aberdeenshire	,,	,,
,, ,,	,,	,,
Nookton, Windygate, Fifeshire	,,	,,
Barry Village, Carnoustie, 12 ft. high	·5124	7·32
,, ,, 25 ,,	·5124	7·32
,, ,, 45 ,,	·3416	4·88
Average	·1708	2·44
WEST COAST.		
North Uist	None	None
Tyree	·0683	·97
Kelly, Wemyss Bay	None	None
Auchendrane, Ayrshire	,,	,,
Cargen, 3 miles south of Dumfries	,,	,,
Row	—	—
Stornoway, Western Isles	None	None
Eallabus, Islay	,,	,,
Average	·0097	·14
Average of both coasts	·0956	1·36

RAIN. TABLE XXII.

AMMONIA.

Sea-coast country places—Scotland.

EAST COAST.

Rain obtained from	Elevation.	Wind.	Date.	Ammonia. Parts per Million.
	ft.			
Kirkwall, Orkney . . .	10	S. chiefly	Dec. 1869 to Jan. 1870	2·467
Preston Kirk, Haddingtonshire	90	Westerly	Dec. 1869 to Feb. 1870	1·70
New Pitsligo, Aberdeenshire .	500	Southerly	Dec. 1869 to Feb. 1870	1·45
,, ,, .	500	—	April 28 to June 12, 1870	·36
Nookton, Windygate, Fifeshire	80	Westerly	Dec. 1869 to April 1870	·66
Barry Village, Carnoustie .	12	—	Dec. 1869 to June 1870	·68
,, ,, .	25	—	— ,,	·44
,, ,, .	45	—	— ,,	·18
Average . . .	—	—	—	·992

WEST COAST.

North Uist	Low	—	March 1869	1·20
Tyree	Low	—	May ,,	·40
Kelly, Wemyss Bay . .	150	SW.; NE. short time	June 12 to 15, 1869	·15
Auchendrane, Ayrshire . .	97	—	Dec. 1869	·10
Cargen, 3 miles south of Dumfries	70	SW. and NW.	Dec. 29, 1869, to Jan. 8, 1870	·16
Row	—	—	Nov. 1869	·48
Stornoway, Western Isles .	—	S. to SW.	—	·48
Eallabus, Islay . . .	60	—	Oct. 1870	·90
Average . . .	—	—	—	·484
Averages of both coasts				·738

RAIN. TABLE XXIII.

ALBUMINOID AMMONIA (MEASURE OF ORGANIC BODIES CONTAINING NITROGEN—INCLUDES PUTRESCIBLE SUBSTANCES).

Sea-coast country places—Scotland.

EAST COAST.

Rain obtained from	Elevation.	Wind.	Date.	Albuminoid Ammonia. Parts per Million.
	ft.			
Kirkwall, Orkney . . .	10	Southerly	Dec. 1869 to Jan. 1870	·080
Preston Kirk, Haddingtonshire	90	Westerly	Dec. 1869 to Feb. 1870	·06
New Pitsligo, Aberdeenshire .	500	Southerly	Dec. 1869 to Feb. 1870	·07
,, ,, .	500	—	April 28 to June 12, 1870	·04
Nookton, Windygate, Fifeshire	80	Westerly	Dec. 1869 to April, 1870	·12
Barry Village, Carnoustie .	12	—	Dec. 1869 to June, 1870	·06
,, ,, .	25	—	— ,,	·20
,, ,, .	45	—	— ,,	·216
Average . . .	—	—	—	·106

WEST COAST.

North Uist	Low	—	March 1869	—
Tyree	Low	—	May ,,	·3
Kelly, Wemyss Bay . .	150	SW.; NE. short time	June 12 to 15, 1869	·075
Auchendrane, Ayrshire . .	97	—	Dec. 1869	·018
Cargen, 3 miles south of Dumfries	70	SW. and NW.	Dec. 29, 1869, to Jan. 8, 1870	·060
Row, Dumbartonshire . .	—	—	Nov. 1869	—
Stornoway, Western Isles .	—	S. to SW.	—	·08
Eallabus, Islay . . .	60	—	Oct. 1870	·20
Average . . .	—	—	—	·105

Average of both coasts . . . . . ·105

RAIN. TABLE XXIV.

NITRIC ACID.

Sea-coast country places—Scotland.

EAST COAST.

Rain obtained from	Elevation.	Wind.	Date.	Nitric Acid. Parts per Million.
	ft.			
Kirkwall, Orkney . . .	10	S. chiefly	Dec. 1869 to Jan. 1870	·259
Preston Kirk, Haddingtonshire	90	Westerly	Dec. 1869 to Feb. 1870	·667
New Pitsligo, Aberdeenshire .	500	Southerly	Dec. 1869 to Feb. 1870	·556
,, ,, .	500	—	April 28 to June 12, 1870	·901
Nookton, Windygate, Fifeshire	80	Westerly	Dec. 1869 to April 1870	·345
Barry Village, Carnoustie .	12	—	Dec. 1869 to June 1870	·022
,, ,, .	25	—	— ,,	·345
,, ,, .	45	—	— ,,	·715
Average . . .	—	—	—	·476

WEST COAST.

North Uist	Low	—	March 1869	·556
Tyree	Low	—	May ,,	—
Kelly, Wemyss Bay . .	150	SW.; NE. short time	June 12 to 15, 1869	·185
Auchendrane, Ayrshire . .	97	—	Dec. 1869	·111
Cargen, 3 miles south of Dumfries	70	SW. & NW.	Dec. 29, 1869, to Jan. 8, 1870	·185
Row	—	—	Nov. 1869	—
Stornoway, Western Isles .	—	S. to SW.	—	·482
Eallabus, Islay . . .	60	—	Oct. 1870	·713
Average . . .	—	—	—	·372
Average of both coasts				·431

RAIN. TABLE XXV.

PERMANGANATE TEST.

Sea-coast country places—Scotland.

EAST COAST.

Rain obtained from	Elevation.	Wind.	Date.	Parts by Weight. Oxygen required per Million Parts of Rain.	
				Instantly.	Total in a few Minutes with Acid.
	ft.				
Kirkwall	19	S. chiefly	Dec. 1869 to Jan. 1870	None	·804
Preston Kirk . . .	90	Westerly	Dec. 1869 to Feb. 1870	,,	1·023
New Pitsligo . . .	500	Southerly	Dec. 1869 to Feb. 1870	,,	·463
,, ...	500	—	April 28 to June 12, 1870	,,	·065
Nookton	80	Westerly	Dec. 1869 to April 1870	,,	·368
Barry Village . . .	12	—	Dec. 1869 to June 1870	,,	·975
,, ...	25	—	— ,,	,,	·780
,, ...	45	—	— ,,	,,	·715
Average . . .	—	—	—	None	·649
WEST COAST.					
North Uist . . .	Low	—	March 1869	—	—
Tyree	Low	—	May ,,	—	—
Kelly, Wemyss Bay . .	150	SW.; NE. short time	June 12 to 15, 1869	—	—
Auchendrane . . .	97	—	Dec. 1869	None	None
Cargen, 3 miles south of Dumfries	70	SW. and NW.	Dec. 29, 1869, to Jan. 8, 1870	,,	,,
Row	—	—	Nov. 1869	—	—
Stornoway . . .	—	S. to SW.	—	None	·073
Eallabus	60	—	Oct. 1870	,,	None
Average . . .	—	—	—	None	·018
Average of both coasts . . .				None	·439

RAIN. TABLE XXVI.

Sea-coast country—Charfield, Berkeley, Gloucestershire, England. Jan. 1870.

Acids and Alkalies.	Grains per Gallon.	Parts per Million.
Hydrochloric Acid (Chlorides)	3·9308	56·15
Sulphuric Acid (Sulphates)	·4116	5·88
Total Acid (Acids of Chlorides and Sulphates and free)	4·3424	62·03
Acidity	None	None

Proportion of Hydrochloric to Sulphuric Acid (Chlorides to Sulphates)	1 to ·104

	Parts per Million.
Ammonia	1·90
Albuminoid Ammonia	·40
Nitric Acid	·371

Permanganate Test.	Parts by Weight. Oxygen required per Million Parts of Rain.	
	Instantly.	Total in a few Minutes with Acid.
	None	1 706

RAIN. TABLE XXVII.

Sea-coast—Valentia, Ireland.

Acids and Alkalies.	Grains per Gallon.	Parts per Million.
Hydrochloric Acid (Chlorides) . . .	3·4067	48·67
Sulphuric Acid (Sulphates)	·1911	2·73

Proportion of Hydrochloric to Sulphuric Acid (Chlorides to Sulphates)	1 to ·056
Acidity	None.

	Parts per Million.
Ammonia	·18
Albuminoid Ammonia	·034
Nitric Acid	·370

Permanganate Test.	Parts by Weight. Oxygen required per Million Parts of Rain.	
	Instantly.	Total in a few Minutes with Acid.
	None	·049

RAIN. TABLE XXVIII.

HYDROCHLORIC ACID (CHLORIDES).

Inland country places—Scotland.

Rain obtained from	Elevation.	Wind.	Date.	Hydrochloric Acid.	
				Grains per Gallon.	Parts per Million.
	ft.				
Lanark	634	S. & W.	Dec. 1869	·1825	2·61
Carnwath, Lanarkshire .	702	—	Nov. 1869 to Jan. 1870	·2555	3·65
Carlesgill, Dumfriesshire .	370	SW. & W.	Nov. and Dec. 1869	·2840	4·06
,, ,, .	,,	NW., NE., & SE.	Spring, 1870	·1460	2·09
Wanlockhead ,, .	1,330	SW.	Dec. 18, 1869, to Jan. 6, 1870	·3650	5·21
Dalwhinnie, Invernessshire	—	—	Jan. 1870	·1277	1·82
Aberfeldie . . .	500	E. & W.	My 13 & 14, 1870	·1548	2·21
Snow from the hill behind Cragie, Perth	200(?)	After an E.	Jan. 4, 1870	·1277	1·82
North Inch, Perth . .	20	SW. to SE.	Jan. 1870	·3931	5·61
Craigie ,, . .	—	—	Jan. and Feb. 1870	·3194	4·56
Balgowan, Perthshire .	—	—	Feb. 13 to 16, 1869	·2555	3·65
North Esk Reservoir, Edinburghshire	1,150	—	—	·2493	3·56
Crieff	245	—	Dec. 1869 to Jan. 1870	·2044	2·92
Average . . .	—	—	—	·2357	3·37

RAIN. TABLE XXIX.

SULPHURIC ACID (SULPHATES).

Inland country places—Scotland.

Rain obtained from	Elevation.	Wind.	Date.	Sulphuric Acid.	
				Grains per Gallon.	Parts per Million.
	ft.				
Lanark	634	S. & W.	Dec. 1869	·1470	2·10
Carnwath, Lanarkshire .	702	—	Nov. 1869 to Jan. 1870	·0735	1·05
Carlesgill, Dumfriesshire .	370	SW. & W.	Nov. and Dec. 1869	·1029	1·47
,, ,, .	,,	NW., NE., & SE.	Spring, 1870	·1176	1·68
Wanlockhead ,, .	1,330	SW.	Dec. 18, 1869, to Jan. 6, 1870	·0882	1·26
Dalwhinnie, Inverness-shire	—	—	Jan. 1870	·1470	2·10
Aberfeldie . . .	500	E. & W.	My 13 & 14, 1870	·2450	3·50
Snow from the hill behind Craigie, Perth	200 (?)	After an E.	Jan. 4, 1870	·0441	·63
North Inch, Perth . .	20	SW. & SE.	Jan. 1870	·2058	2·94
Craigie ,, . .	—	—	Jan. and Feb. 1870	·1176	1·68
Balgowan, Perthshire .	—	—	Feb. 13 to 16, 1869	·2940	4·20
North Esk Reservoir, Edinburghshire	1,150	—	—	·2205	3·15
Crieff	245	—	Dec. 1869 to Jan. 1870	·0735	1·05
Average . . .	—	—	—	·1414	2·06

RAIN. TABLE XXX.

PROPORTION OF HYDROCHLORIC TO SULPHURIC ACID (CHLORIDES TO SULPHATES).

Inland country places—Scotland.

Lanark	1	to	·81
Carnwath, Lanarkshire	1	„	·29
Carlesgill, Dumfriesshire, 1869	1	„	·36
„ „ 1870	1	„	·80
Wanlockhead	1	„	·24
Dalwhinnie, Inverness	1	„	1·15
Aberfeldie	1	„	1·58
Snow from the hill behind Craigie, Perth	1	„	·34
North Inch, Perth	1	„	·52
Craigie „	1	„	·37
Balgowan „	1	„	·15
North Esk Reservoir, Edinburghshire	1	„	·88
Crieff	1	„	·36
Ratio of Average Acids	1	„	·61

RAIN. TABLE XXXI.

ACIDITY (FREE ACIDS).

Inland country places—Scotland.

Rain obtained from	Acidity calculated as Sulphuric Anhydride.	
	Grains per Gallon.	Parts per Million.
Lanark	None	None
Carnwath, Lanarkshire	„	„
Carlesgill, Dumfriesshire	·0342	·49
„ „	None	None
Wanlockhead	„	„
Dalwhinnie, Inverness	·1025	1·46
Aberfeldie	None	None
Snow from the hill behind Craigie, Perth	·1025	1·46
North Inch, Perth	None	None
Craigie „	·0342	·49
Balgowan, Perthshire	None	None
North Esk Reservoir, Edinburghshire	„	„
Crieff	·0137	·19
Average	·0220	·314

RAIN. TABLE XXXII.

AMMONIA.

Inland country places—Scotland.

Rain obtained from	Elevation.	Wind.	Date.	Ammonia. Parts per Million.
	ft.			
Lanark	634	S. & W.	Dec. 1869	·36
Carnwath, Lanarkshire . .	702	—	Nov, 1869 to Jan. 1870	·42
Carlesgill, Dumfriesshire .	370	SW. & W.	Nov. and Dec. 1869	·26
,, ,, .	,,	NW., NE., & SE.	Spring, 1870	·56
Wanlockhead ,, .	1,330	SW.	Dec. 18, 1869, to Jan. 6, 1870	·16
Dalwhinnie, Inverness-shire .	—	—	Jan. 1870	·27
Aberfeldie	500	E. & W.	May 13 & 14, 1870	1·02
Snow from the hill behind Craigie, Perth	200(?)	After an E.	Jan. 4, 1870	·48
North Inch, Perth . . .	20	SW. & SE.	Jan. 1870	·68
Craigie ,, ...	—	—	Jan. and Feb. 1870	·90
Balgowan, Perthshire . .	—	—	Feb. 13 to 16, 1869	—
North Esk Reservoir, Edinburghshire	1,150	—	—	1·14
Crieff	245	—	Dec. 1869 to Jan. 1870	·14
Average . . .	—	—	—	·532

RAIN. TABLE XXXIII.

ALBUMINOID AMMONIA (MEASURE OF ORGANIC BODIES CONTAINING NITROGEN—INCLUDES PUTRESCIBLE SUBSTANCES).

Inland country places—Scotland.

Rain obtained from	Elevation.	Wind.	Date.	Albuminoid Ammonia. Parts per Million.
	ft.			
Lanark	634	S. & W.	Dec. 1869	·030
Carnwath, Lanarkshire .	702	—	Nov. 1869 to Jan. 1870	·012
Carlesgill, Dumfriesshire .	370	SW. & W.	Nov. and Dec. 1869	·020
„ „ .	„	NW., NE., & SE.	Spring, 1870	·080
Wanlockhead „ . .	1,330	SW.	Dec. 18, 1869, to Jan 6, 1870	·024
Dalwhinnie, Inverness-shire .	—	—	Jan. 1870	·026
Aberfeldie	500	E. & W.	May 13 & 14, 1870	·060
Snow from the hill behind Craigie, Perth	200 (?)	After an E.	Jan. 4, 1870	·052
North Inch, Perth . . .	20	SW. & SE.	Jan. 1870	·060
Craigie „	—	—	Jan. and Feb. 1870	·050
Balgowan, Perthshire . .	—	—	Feb. 13 to 16, 1869	—
North Esk Reservoir, Edinburghshire	1,150	—	—	·048
Crieff	245	—	Dec. 1869 to Jan. 1870	·010
Average . . .	—	—	—	·039

RAIN. TABLE XXXIV.

NITRIC ACID.

Inland country places—Scotland.

Rain obtained from	Elevation.	Wind.	Date.	Parts per Million.
	ft.			
Lanark	634	S. & W.	Dec. 1869	·185
Carnwath, Lanarkshire . .	702	Sly & W.	Nov. 1869 to Jan. 1870	·011
Carlesgill, Dumfriesshire .	370	SW. & W.	Nov. and Dec. 1869	·111
,, ,, .	,,	NW., NE., & SE.	Spring, 1870	·445
Wanlockhead ,, .	1,330	SW.	Dec. 18, 1869, to Jan. 6, 1870	·111
Dalwhinnie, Inverness-shire .	—	—	Jan. 1870	·185
Aberfeldie	500	E. & W.	May 13 and 14, 1870	1·086
Snow from the hill behind Craigie, Perth	200 (?)	After an E.	Jan. 4, 1870	·371
North Inch ,, . . .	20	SW. & SE.	Jan. 1870	·259
Craigie ,, . . .	—	—	Jan. and Feb. 1870	·371
Balgowan, Perthshire . .	—	—	Feb. 13 to 16, 1869	—
North Esk Reservoir, Edinburghshire	1,150	—	—	·456
Crieff	245	—	Dec. 1869 to Jan. 1870	·074
Average . . .	—	—	—	·305

RAIN. TABLE XXXV.

PERMANGANATE TEST.

Inland country places—Scotland.

Rain obtained from	Elevation.	Wind.	Date.	Parts by Weight. Oxygen required per Million Parts of Rain.	
				Instantly.	Total in a few Minutes with Acid.
	ft.				
Lanark	634	S. & W.	Dec. 1869	None	None
Carnwath	702	—	Nov. 1869 to Jan. 1870	,,	·049
Carlesgill	370	SW. & W.	Nov. and Dec. 1869	,,	None
,, ...	,,	NW. NE. & SE.	Spring, 1870	,,	·195
Wanlockhead . . .	1,330	SW.	Dec. 18, 1869, to Jan. 6, 1870	,,	None
Dalwhinnie . . .	—	—	Jan. 1870	·098	·292
Aberfeldie . . .	500	E. & W.	May 13 & 14, 1870	None	·325
Snow from the hill behind Craigie, Perth	200 (?)	After an E.	Jan. 4, 1870	·098	·341
North Inch, Perth . .	20	SW. & SE.	Jan. 1870	·146	·560
Craigie ,, . .	—	—	Jan. and Feb. 1870	None	·999
Balgowan ,, . .	—	—	Feb. 13 to 16, 1869	—	—
North Esk Reservoir, Edinburghshire	1,150	—	—	None	·282
Crieff	245	—	Dec. 1869 to Jan. 1870	,,	·128
Average . . .	—	—	—	·0285	·264

RAIN. TABLE XXXVI.

HYDROCHLORIC ACID (CHLORIDES).

Inland country places—England.

Rain obtained from	Date.	Hydrochloric Acid	
		Grains per Gallon.	Parts per Million.
Tring, Hertfordshire . . .	January 1870	·0929	1·33
Mattishall, Norfolk . . .	November „	·1022	1·46
Wilcot, near Baschurch, Salop .	August „	·1217	1·74
Ludlow (Stoke House) „ .	„ „	·1597	2·28
Banbury, Oxfordshire . .	— „	·1703	2·43
Aldershot	— „	·1793	2·56
Rugby, Berks	January „	·3407	4·87
Cirencester, Royal Agricultural College	Dec. 18, 19, 1869	·3650	5·21
Streatley, Berks . . .	Jan. 1 to Feb. 7, 1870	·3650	5·21
Bracondale, near Norwich .	— „	·3931	5·61
Geldeston, Beccles . . .	— „	·4258	6·08
Wilcot, near Baschurch, Salop .	January „	·6387	9·12
Average	. . . .	·2795	3·99

RAIN. TABLE XXXVII.

SULPHURIC ACID (SULPHATES).

Inland country places—England.

Rain obtained from	Date.	Sulphuric Acid.	
		Grains per Gallon.	Parts per Million.
Wilcot, near Baschurch, Salop .	August 1870	·1285	1·84
Tring, Herts	January „	·1470	2·10
Streatley, Berks	Jan. 1 to Feb. 7, 1870	·1764	2·52
Aldershot	— „	·2037	2·91
Cirencester, Royal Agricultural College	Dec. 18, 19, 1869	·2352	3·36
Rugby	January 1870	·2940	4·20
Ludlow (Stoke House), Salop .	August „	·2940	4·20
Geldeston, Beccles . . .	— „	·3234	4·62
Wilcot, near Baschurch, Salop .	January „	·3822	5·46
Banbury, Oxfordshire . .	— „	·5250	7·50
Mattishall, Norfolk . . .	November „	·7528	10·74
Bracondale, near Norwich . .	— „	1·1760	16·80
Average	. . . . .	·3865	5·52

RAIN. TABLE XXXVIII.

PROPORTION OF HYDROCHLORIC TO SULPHURIC ACID (CHLORIDES TO SULPHATES).

Inland country places—England.

	Proportion of Hydrochloric to Sulphuric Acid.
Streatley, Berks	1 to ·48
Wilcot, near Baschurch, Salop (January 1870)	1 „ ·60
Cirencester, Royal Agricultural College	1 „ ·64
Geldeston, Beccles	1 „ ·76
Rugby	1 „ ·86
Wilcot, near Baschurch, Salop (August 1870)	1 „ 1·06
Aldershot	1 „ 1·14
Tring, Herts	1 „ 1·58
Ludlow, Salop	1 „ 1·85
Bracondale, near Norwich	1 „ 2·99
Banbury, Oxfordshire	1 „ 3·08
Mattishall, Norfolk	1 „ 7·36
Ratio of Average Acids	1 „ 1·38

RAIN. TABLE XXXIX.

TOTAL ACID AND ACIDITY (ACID COMBINED AND FREE).

Inland country places—England.

Rain obtained from	Total Acid.		Acidity.
	Grains per Gallon.	Parts per Million.	
Tring, Herts	·2399	3·43	None
Wilcot, Baschurch, Salop (August 1870)	·2502	3·58	,,
Aldershot	·3830	5·47	,,
Ludlow, Salop	·4537	6·48	,,
Streatley, Berks	·5414	7·73	,,
Cirencester, Royal Agricultural College	·6002	8·57	,,
Rugby	·6347	9·07	,,
Banbury, Oxfordshire	·6953	9·93	Alkaline
Geldeston, Beccles	·7492	10·70	None
Mattishall, Norfolk	·8550	12·20	,,
Wilcot, Baschurch, Salop (January 1870)	1·0209	14·58	,,
Bracondale, near Norwich	1·5691	22·41	,,
Average	·6661	9·51	None

RAIN. TABLE XL.

AMMONIA.

Inland country places—England.

Rain obtained from	Date.	Ammonia. Parts per Million.
Wilcot, near Baschurch . . .	August 1870	·032
Mattishall, Norfolk	November „	·10
Streatley, Berks	Jan 1 to Feb. 7, 1870	·36
Tring, Herts	January „	·48
Ludlow (Stoke House), Salop .	August „	·52
Aldershot	— „	·76
Geldeston, Beccles	— „	·96
Rugby	January „	1·50
Cirencester, Ryl Agricultural College	Dec. 18, 19, 1869	1·80
Bracondale, near Norwich . .	— 1870	1·96
Wilcot, Baschurch	January „	2·08
Banbury, Oxfordshire . . .	— „	2·28
Average . . .	. . . .	1·07

RAIN. TABLE XLI.

ALBUMINOID AMMONIA.

Inland country places—England.

Rain obtained from	Date.	Albuminoid Ammonia. Parts per Million.
Ludlow, Salop	— 1870	·04
Streatley, Berks	Jan 1 to Feb. 7 „	·06
Tring, Herts	January „	·06
Mattishall, Norfolk	November „	·07
Wilcot, Baschurch	August „	·072
Banbury, Oxfordshire	— „	·08
Cirencester, Royal Agricultural College	Dec. 18, 19, 1869	·10
Aldershot	— 1870	·12
Geldeston, Beccles	— „	·16
Bracondale, Norwich	— „	·16
Wilcot, Baschurch	January „	·28
Rugby	„ „	3·30[1]
Average		·375
„ without Rugby		·109

[1] Found to be caused by using a wooden vessel instead of the glass.

RAIN. TABLE XLII.

NITRIC ACID.

Inland country places—England.

Rain obtained from	Date.	Nitric Acid. Parts per Million.
Ludlow, Salop	August 1870	·048
Cirencester, Royal Agricultural College	Dec. 18, 19, 1869	·111
Streatley, Berks	Jan. 1 to Feb. 7, 1870	·185
Wilcot, Baschurch	August ,,	·453
Tring, Herts	January ,,	·556
Wilcot, Baschurch	January ,,	·556
Geldeston, Beccles	— ,,	·715
Aldershot	— ,,	·887
Bracondale, Norwich	— ,,	1·087
Banbury, Oxfordshire	— ,,	1·087
Rugby	January ,,	1·482
Mattishall, Norfolk	November ,,	1·825
Average		·749

RAIN. TABLE XLIII.

PERMANGANATE TEST.

Inland country places—England.

Rain obtained from	Date.	Parts by Weight. Oxygen required per Million Pts of Rain.	
		Instantly.	Total in a few Minutes with Acid.
Mattishall, Norfolk . .	November 1870	None	None
Wilcot, Baschurch . . .	August „	„	„
Aldershot	— „	„	„
Streatley, Berks . . .	Jan. 1 to Feb. 7 „	„	·171
Ludlow, Salop . . .	August „	„	·325
Tring, Herts	January „	„	·414
Cirencester, Royal Agricultural College	Dec. 18, 19, 1869	„	·449
Wilcot, Baschurch . .	January 1870	„	·487
Rugby	„ „	„	·560
Geldeston, Beccles . . .	— „	„	.867
Banbury, Oxfordshire . .	— „	„	1·127
Bracondale, Norwich . .	— „	„	1·192
Average . .	. . . .		·466

RAIN. TABLE XLIV.

HYDROCHLORIC ACID (CHLORIDES).

From towns—Scotland.

Rain obtained from	Date.	Hydrochloric Acid.	
		Grains per Gallon.	Parts per Million.
Greenock, Old Prison Yard .	April & May 1870	·1703	2·43
Aberdeen	1870	·1926	2·74
Galashiels	May 11 to 20, 1870	·2000	2·86
Greenock, Bank Street . .	November „	·2129	3·04
Galashiels	September „	·2450	3·50
Edinburgh, in a garden, Morningside	Nov. 1869 to Feb. 1870	·2555	3·65
Perth, on a church tower .	January and February 1870	·3407	4·87
„ in a confined place, near lowest level		·4258	6·08
Edinburgh, Royal Observatory, Calton Hill	Nov. 1869 to Feb. 1870	·5110	7·30
Glasgow average . . .	1870	·6282	8·97
Dundee	—	1·5485	22·12
Average (without Glasgow) . . .		·4102	5·86
„ (with Glasgow) . . .		·4300	6·14
„ (without Dundee) . . .		·3182	4·54

RAIN. TABLE XLV.

SULPHURIC ACID (SULPHATES).

From towns—Scotland.

Rain obtained from	Date.	Sulphuric Acid.	
		Grains per Gallon.	Parts per Million.
Aberdeen	1870	·1176	1·68
Galashiels	May 11 to 20, 1870	·1470	2·10
„	September „	·5495	7·85
Edinburgh, in a garden, Morningside	Nov. 1869 to Feb. 1870	·5880	8·40
Greenock, Bank Street	November 1870	1·4280	20·40
Edinburgh, Royal Observatory, Calton Hill	Nov. 1869 to Feb. 1870	1·4700	21
Greenock, Old Prison Yard	April and May 1870	1·4700	21
Dundee	—	1·6333	23·33
Perth, in a confined place, near lowest level	January and February 1870	2·0496	29·28
Perth, on a church tower		2·1000	30
Glasgow, average	1870	4·9139	70·19
Average (without Glasgow)		1·1553	16·50
„ (with Glasgow)		1·4970	21·38

RAIN. TABLE XLVI.

PROPORTION OF HYDROCHLORIC TO SULPHURIC ACID (CHLORIDES TO SULPHATES).

From towns—Scotland.

	Proportion of Hydrochloric to Sulphuric Acid.	
Aberdeen	1 to ·61	
Dundee	1 „ 1·06	
Galashiels, May 1870	1 „ ·73	1·48
„ Sept. „	1 „ 2·24	
Edinburgh, Morningside	1 „ 2·30	2·58
„ Calton Hill	1 „ 2·87	
Perth, confined place, near lowest level	1 „ 4·81	5·48
„ on church tower	1 „ 6·16	
Greenock, Bank Street	1 „ 6·71	7·67
„ Old Prison Yard	1 „ 8·64	
Glasgow, average	1 „ 7·82	
Ratio of Average Acids (without Glasgow)	1 „ 2·82	
„ „ (with Glasgow)	1 „ 3·48	

RAIN. TABLE XLVII.

TOTAL ACID. (TOTAL ACID OF CHLORIDES AND SULPHATES).

From towns—Scotland.

Rain obtained from	Date.	Total Acid (Acids of Salts and free).	
		Grains per Gallon.	Parts per Million.
Aberdeen	1870	·3102	4·42
Galashiels	May 11 to 20, 1870	·3470	4·96
„	September „	·7945	11·35
Average . . .	. . . .	·5707	8·15
Edinburgh, Morningside . .	Nov. 1869 to Feb. 1870	·8435	12·05
„ Calton Hill .		1·9810	28·30
Average . . .	. . . .	1·4122	20·17
Greenock, Old Prison . .	April and May 1870	1·6403	23·43
„ Bank Street . .	November „	1·6409	23·44
Average . . .	. . . .	1·6406	23·43
Perth, church tower . .	January and February 1870	2.4407	34·87
„ confined place, near lowest level		2·4754	35·36
Average . . .	. . . .	2·4580	35·11
Dundee	—	3·1818	45·45
Glasgow, average . . .	1870	5·5421	79·17
Mean Average (without Glasgow) . . .		1·5956	22·79
„ (with Glasgow)		2·1595	30·85

RAIN. TABLE XLVIII.

ACIDITY (FREE ACIDS).

From towns—Scotland.

Rain obtained from	Date.	Acidity calculated as Sulphuric Anhydride.	
		Grains per Gallon.	Parts per Million.
Galashiels	May 11–20 1870	None	None
Greenock, Bank Street . .	—	,,	,,
Edinburgh, Morningside . .	—	,,	,,
Perth, N. Inch and church tower	—	,,	,,
Aberdeen	—	,,	,,
Dundee	—	·3758	5·37
Edinburgh, Calton Hill . .	—	·4782	6·83
Galashiels	September 1870	·9194	13·13
Glasgow, average . . .	—	1·0589	15·13
Average (without Glasgow) .	. . .	·2217	3·16
,, (with Glasgow) .	. . .	·3147	4·49

RAIN. TABLE XLIX.

AMMONIA.

From towns—Scotland.

Rain obtained from	Date.	Ammonia. Parts per Million.	
Aberdeen	1870		·18
Edinburgh, Morningside	Nov. 1869 to Feb. 1870	·14	
„ Calton Hill		2·48	
Average			1·31
Dundee	—		3·30
Galashiels	May 1870	3·2	
„	September 1870	5·2	
Average			4·20
Perth, confined place, lowest level	Jan. & Feb. 1870	3·7	
„ on a church tower	„ „	7·8	
Average			5·75
Greenock, Old Prison Yard	April & May „	3·30	
„ Bank Street	November „	13·10	
Average			8·20
Glasgow, average	1870		9·10
Mean Average (without Glasgow)			3·82
„ „ (with Glasgow)			4·58

RAIN. TABLE L.

ALBUMINOID AMMONIA.

From towns—Scotland.

Rain obtained from	Date.	Albuminoid Ammonia. Parts per Million.	
Edinburgh, Morningside . .	Nov. 1869 to Feb. 1870	·04	
„ Calton Hill . .		·08	
		—	
Average . . .			·06
Aberdeen	1870		·10
Perth, confined place . .	Jan. & Feb. 1870	·10	
„ a church tower . .	„ „	·10	
		—	
Average . . .			·10
Dundee	1870		·12
Galashiels	May 1870	·23	
„	September 1870	·45	
		—	
Average . . .			·34
Glasgow, average . . .	1870		·30
Greenock, Bank Street . .	November 1870	·40	
„ Old Prison Yard .	April & May „	·70	
		—	
Average . . .			·55
Mean Average (without Glasgow) . . .			·212
„ „ (with Glasgow) . . .			·224

RAIN. TABLE LI.

NITRIC ACID.

From towns—Scotland.

Rain obtained from	Date.	Nitric Acid. Parts per Million.	
Aberdeen	1870		·233
Greenock, Old Prison Yard . .	April & May 1870	·345	
„ Bank Street . . .	November „	·713	
		——	
Average . . .			·529
Dundee	1870		1·086
Galashiels	May 1870	·527	
„	September 1870	1·642	
		——	
Average . . .			1·084
Edinburgh, Calton Hill . .	Nov. 1869 to Feb. 1870	·530	
„ Morningside . .		3·495	
		——	
Average . . .			2·012
Perth, on church tower . . .	Jan. & Feb. 1870	·926	
„ from confined place, lowest level	„ „	3·150	
		——	
Average . . .			2·038
Glasgow, average	1870		2·436
Mean Average (Glasgow not included) . .			1·164
„ „ (Glasgow included) . . .			1·345

RAIN. TABLE LII.

PERMANGANATE TEST.

From towns—Scotland.

Rain obtained from	Date.	Parts by Weight. Oxygen required per Million Parts of Rain.	
		Instantly.	Total in a few Minutes with Acid.
Aberdeen	1870	None	·390
Galashiels	May 1870	None	·498
,, ...	Sept. ,,	·108	1·623
Average . .		·054	1·060
Edinburgh, Morningside . .	Nov. 1869 to Feb. 1870	None	·487
,, Calton Hill . .		·122	2·364
Average . .		·061	1·425
Dundee	1870	·065	1·885
Greenock, Bank Street . .	Nov. 1870	·216	2·598
,, Old Prison Yard .	Ap.&My ,,	·433	3·640
Average . .		·324	3·119
Perth, confined space, within one foot of lowest level	Jan. and Feb. 1870	·658	2·047
,, on a church tower .		·487	4·752
Average . .		·572	3·399
Glasgow, average . . .	1870	·671	10·040
Mean Average (Glasgow not included)		·179	1·879
,, ,, ,, included .		·249	3·045

RAIN. TABLE LIII.

Glasgow, 1870.

Rain obtained from	Hydrochloric Acid.		Sulphuric Acid.		Proportion of Hydrochloric to Sulphuric Acid.
	Grains per Gallon.	Parts per Million.	Grains per Gallon.	Parts per Million.	
St. Rollox . . .	·6387	9·12	2·9400	42·00	1 to 4·61
New City Road, Old Normal School.	·5110	7·30	3·4588	49·41	1 „ 6·77
Western Police Station.	·8517	12·17	4·5231	64·62	1 „ 5·31
Main Street, Gorbals, Old Police Office	·4867	6·95	4·9000	70·00	1 „ 10·07
Calton Police Station .	·3931	5·61	5·3453	76·36	1 „ 13·61
Bridge Gate Steeple, 82 ft. high	·7862	11·23	5·8800	84·00	1 „ 7·48
Boar's Head Close .	·7301	10·43	7·3500	105·00	1 „ 10·1[illegible]
Average . .	·6282	8·97	4·9139	70·19	1 to 7·8[illegible]

[1] Ratio of Average Acids.

RAIN. TABLE LIV.

ACIDITY (FREE ACIDS).

Glasgow, 1870.

Rain obtained from	Acidity calc. as Sulphuric Anhydride.	
	Grains per Gallon.	Parts per Million.
St. Rollox	None	None
New City Road, Old Normal School	·8540	12·20
Western Police Station	·8540	12·20
Calton Police Station	·9223	13·18
Boar's Head Close	1·0248	14·64
Main Street, Gorbals, Old Police Office	1·7080	24·40
Bridge Gate Steeple	2·0496	29·28
Average	1·0589	15·13

TOTAL ACIDS (ACIDS OF CHLORIDES, SULPHATES AND FREE).

St. Rollox[1]	3·5787	51·12
New City Road	3·9698	56·71
Western Police Station	5·3748	76·79
Main Street, Gorbals, Old Police Office	5·3867	76·95
Calton Police Station	5·7384	81·97
Bridge Gate Steeple, 82 ft. high	6·6662	95·23
Boar's Head Close	8·0801	115·43
Average	5·5421	79·17

[1] This does not appear to be probable, but, like the other specimens here, it was sent to me as collected by the instructions of John Carrick, Esq., Town Hall, Glasgow, and only many trials can prove it wrong.

RAIN. TABLE LV.

AMMONIA.

Glasgow, 1870.

	Ammonia. Parts per Million.
St. Rollox	1·80
New City Road, Old Normal School	7·33
Calton Police Station	7·40
Western „	9·20
Boar's Head Close	10·80
Bridge Gate Steeple, 82 ft. high	12·50
Main Street, Gorbals, Old Police Office	14·70
Average	9·10

ALBUMINOID AMMONIA.

St. Rollox	·07
New City Road, Old Normal School	·30
Western Police Station	·30
Boar's Head Close	·30
Calton Police Station	·35
Bridge Gate Steeple, 82 ft. high	·35
Main Street, Gorbals, Old Police Office	·40
Average	·30

NITRIC ACID.

St. Rollox	·345
Main Street, Gorbals, Old Police Office	1·271
Calton Police Station	1·456
Western „	1·456
New City Road, Old Normal School	1·827
Bridge Gate Steeple, 82 ft. high	5·162
Boar's Head Close	5·533
Average	2·436

RAIN. TABLE LVI.

PERMANGANATE TEST.

Glasgow, 1870.

Rain obtained from	Parts by Weight. Oxygen per Million Parts of Water.	
	Instantly without Acid	Total in a few Minutes with Acid.
Western Police Station	None	5·633
Calton ,, ...	1·040	6·933
New City Road, Old Normal School . .	·217	8·449
Bridge Gate Steeple, 82 ft. high . .	·260	8·666
Main Street, Gorbals, Old Police Office .	1·517	13·649
Boar's Head Close	·997	16·900
Average	·671	10·040

RAIN. TABLE LVII.

PROPORTION OF HYDROCHLORIC TO SULPHURIC ACID (CHLORIDES TO SULPHATES).

Towns—England.

	Proportion of Hydrochloric to Sulphuric Acid (Chlorides to Sulphates),
Runcorn, 1869	1 to ·92
St. Helen's ,, ...	1 ,, 3·48
Liverpool, 1870	1 ,, 3·90
Newcastle-on-Tyne, 1868 . . .	1 ,, 5·47
Manchester, 1869	1 ,, 7·08
Birkenhead, March 7, 1868 . . .	1 ,, 7·31
Manchester, 1870	1 ,, 8·29
London, 1869	1 ,, 16·45
Ratio of Average Acids . . .	1 to 3·94

RAIN. TABLE LVIII.

HYDROCHLORIC ACID.

Towns—England.

Rain obtained from	Hydrochloric Acid (Chlorides)	
	Grains per Gallon.	Parts per Million.
London, 1869	·0872	1·25
Birkenhead, March 7, 1868	·2217	3·17
Manchester, 1870	·4055	5·79
„ 1869	·4118	5·88
Newcastle-on-Tyne, 1868	·5678	8.11
St. Helen's, 1869	·6670	9·53
Liverpool, 1870	·7110	10·16
Runcorn, 1869	1·8022	25·74
Average	·6093	8·70

SULPHURIC ACID.

London, 1869	1·4345	20·49
Birkenhead, March 7, 1868	1·6210	23·16
Runcorn, 1869	1·6537	23·62
St. Helen's, 1869	2·3232	33·19
Liverpool, 1870	2·7714	39·59
Manchester, 1869	2·9163	41·66
Newcastle-on-Tyne, 1868	3·1111	44·44
Manchester, 1870	3·3593	47·99
Average	2·3988	34·27

RAIN. TABLE LIX.

TOTAL ACID.

Towns—England.

Rain obtained from	Total Acids.	
	Grains per Gallon.	Parts per Million.
London, 1869	1·5217	21·74
Birkenhead, near Liverpool, March 7, 1868	1·8427	26·32
St. Helen's, 1869	2·9902	42·72
Manchester, 1869	3·3281	47·54
Runcorn, 1869	3·4559	49·36
Liverpool, 1870	3·4824	49·75
Newcastle-on-Tyne, 1868	3·6789	52·55
Manchester, 1870	3·7648	53·78
Average	3·0081	42·97

ACIDITY.

London, 1869	·2713	3·87
St. Helen's, 1869	·2785	3·98
Manchester „	·5833	8·33
Runcorn „	·7993	11·42
Liverpool, 1870	·8096	11·56
Manchester „	·8416	12·02
Average	·5972	8·53

RAIN. TABLE LX.

NITRIC ACID.

Towns—England.

Rain obtained from	Nitric Acid. Parts per Million.
Runcorn, 1869	·278
Liverpool, 1870	·582
London, 1869	·840
Manchester, 1869	·886
St. Helen's „	1·413
Manchester, 1870	1·179
Average	·863

PERMANGANATE TEST.

Rain obtained from	Parts by Weight. Oxygen required per Million Parts of Rain.	
	Instantly.	Total in a few Minutes with Acid.
Runcorn, 1869	—	1·090
Manchester, 1869	·197	2·050
St. Helen's „	—	2·276
Liverpool, 1870	·662	3·896
Manchester „	·475	4·401
Average	·445	2·743

RAIN. TABLE LXI.

AMMONIA.

Towns—England.	Ammonia. Parts per Million.
London, 1869	3·45
St. Helen's ,,	4·56
Runcorn ,,	4·63
Liverpool 1870	5·38
Manchester, 1869	6·36
Manchester, 1870	6·57
Average	5·16

ALBUMINOID AMMONIA.

	Albuminoid Ammonia. Parts per Million.
Liverpool, 1870	·159
Runcorn, 1869	·190
London ,,	·205
Manchester ,,	·217
St. Helen's ,,	·230
Manchester, 1870	·285
Average	·214

RAIN. TABLE LXII.

HYDROCHLORIC ACID (CHLORIDES).

Manchester, 1870.

1870. Month.	Hydrochloric Acid (Chlorides).					
	A.		B.		C.	
	Grains per Gallon.	Parts per Million.	Grains per Gallon.	Parts per Million.	Grains per Gallon.	Parts per Million.
January . .	·5110	7·30	·4645	6·64	·4258	6·08
February . .	·6012	8·59	·7862	11·23	·4645	6·64
March . . .	·2555	3·65	·3194	4·56	·4645	6·64
April . . .	·3650	5·21	·3650	5·21	·2689	3·84
May	·3931	5·62	·3006	4·29	·4258	6·08
June . . .	·7296	10·42	·4444	6·35	·7301	10·43
July . . .	·3194	4·56	·2920	4·17	·3194	4·56
August . . .	·2044	2·92	·2044	2·92	·4088	5·84
September . .	·3430	4·90	·5110	7·30	·5110	7·30
October . .	·4645	6·64	·5110	7·30	·5874	8·39
November . .	·2324	3·32	·2842	4·06	·2436	3·48
December . .	·2352	3·36	·2044	2·92	—	—
Average . .	·3879	5·54	·3906	5·58	·4409	6·30

Average of twelve months, A, B, and C . . ·4055 5·79

A. All Saints . . 12 ft. above the ground.

B. „ . . 30 ft. „ „

C. Lit. and Phil. Society, George Street, 2 ft. above the ground.

RAIN. TABLE LXIII.

SULPHURIC ACID (SULPHATES).

Manchester, 1870.

1870. Month.	Sulphuric Acid (Sulphates).					
	A.		B.		C.	
	Grains per Gallon.	Parts per Million.	Grains per Gallon.	Parts per Million.	Grains per Gallon.	Parts per Million.
January . .	3·2666	46·67	5·8800	84	1·2783	18·26
February . .	7·3500	105	7·3500	105	3·2667	46·67
March . . .	2·9400	42	3·6750	52·50	1·4700	21
April . . .	4·7419	67·74	4·2000	60	2·9400	42
May . . .	7·3500	105	7·3500	105	2·9400	42
June . . .	2·2615	32·31	2·8560	40·80	1·5032	21·47
July . .	2·9443	42·06	4·0803	58·29	1·7850	25·50
August . .	3·5700	51	3·1733	45·33	1·9040	27·20
September .	2·8560	40·80	2·5970	37·10	3·5700	51
October . .	2·3800	34	2·3800	34	1·2985	18·55
November . .	3·0065	42·95	3·0065	42·95	1·6800	24
December . .	1·5522	22·17	3·1733	45·33	—	—
Average .	3·6849	52·64	4·1434	59·19	2·1487	30·70

Average of A, B, and C for 12 months . 3·3593 47·99

A. All Saints . . 12 ft. above the ground.

B. „ . . 30 ft. „ „

C. Lit. and Phil. Society, George Street, 2 ft. above the ground.

RAIN. TABLE LXIV.

ACIDITY.

Manchester, 1870.

1870. Month.	Acidity calc. as Sulphuric Anhydride.					
	A.		B.		C.	
	Grains per Gallon.	Parts per Million.	Grains per Gallon.	Parts per Million.	Grains per Gallon.	Parts per Million.
January . .	1·9129	27·33	1·1614	16·59	·3757	5·37
February . .	4·0992	58·56	2·2773	32·53	·6832	9·76
March . . .	1·4001	20·01	·6832	9·76	·0683	·98
April . . .	·8882	12·69	·4099	5·86	·3416	4·88
May	None	None	None	None	·2391	3·42
June . . .	1·0248	14·62	1·3664	19·52	·6832	9·76
July	1·5601	22·29	None	None	·2786	3·98
August . . .	·5572	7·96	·9472	13·53	·8358	11·94
September .	.5851	8·36	·7244	10·35	·2507	3·58
October . .	·4458	6·37	·2786	3·98	·3900	5·57
November . .	1·2816	18·31	1·4487	20·70	·8637	12·34
December . .	·4179	5·97	·9751	13·93	—	—
Average .	1·1811	16·87	·8560	12·23	·4554	6·51

Average of A, B, and C for 12 months . . ·8416 12·02

A. All Saints . . 12 ft. above the ground.

B. „ . . 30 ft. „ „

C. Lit. and Phil. Society, George Street, 2 ft. above the ground.

RAIN. TABLE LXV.

PROPORTION OF HYDROCHLORIC TO SULPHURIC ACID (CHLORIDES TO SULPHATES).

Manchester, 1870.

1870. Month.	Proportion of Hydrochloric to Sulphuric Acid (Chlorides to Sulphates).		
	A.	B.	C.
	All Saints, 12 ft. above ground.	All Saints, 30 ft. above ground.	George Street, 2 ft. above ground.
January	1 to 6·39	1 to 12·65	1 to 3
February	1 „ 12·22	1 „ 9·35	1 „ 7·03
March	1 „ 11·51	1 „ 11·51	1 „ 3·16
April	1 „ 12·99	1 „ 11·51	1 „ 10·93
May	1 „ 18·69	1 „ 24·48	1 „ 6·91
June	1 „ 3·10	1 „ 6·43	1 „ 2·06
July	1 „ 9·22	1 „ 13·98	1 „ 5·59
August	1 „ 17·46	1 „ 15·53	1 „ 4·66
September . . .	1 „ 8·33	1 „ 5·09	1 „ 7
October	1 „ 5·12	1 „ 4·66	1 „ 2·21
November . . .	1 „ 12·94	1 „ 10·58	1 „ 6·90
December . . .	1 „ 6·60	1 „ 15·53	—
Ratio of Average Acids	1 to 9·50	1 to 10·61	1 to 4·09

Ratio of Averages of A, B, and C for 12 months . 1 to 8·29

RAIN. TABLE LXVI.

TOTAL ACIDS.

Manchester, 1870.

1870. Month.	Total Acids (Acids of Chlorides, Sulphates and free).					
	A.		B.		C.	
	Grains per Gallon.	Parts per Million.	Grains per Gallon.	Parts per Million.	Grains per Gallon.	Parts per Million.
January . .	3·7776	53.97	6·3445	90·64	1·7041	24·34
February . .	7·9512	113·59	8·1362	116·23	3·7312	53·31
March . . .	3·1955	45·65	3·9944	56·76	1·9345	27·64
April . . .	5·1069	72·95	4·5650	65·21	3·2089	45·84
May	7·7431	110·62	7·6506	109·29	3·3658	48·08
June . . .	2·9911	42·73	3·3004	47·15	2·2333	31·90
July . . .	3·2637	46·62	4·3723	62·46	2·1044	30·06
August . . .	3·7744	53·92	3·3777	48·25	2·3128	33·04
September . .	3·1990	45·70	3·1080	44·40	4·0810	58·30
October . .	2·8445	40·64	2·8910	41·30	1·8859	26·94
November . .	3·2389	46·27	3·2907	47·01	1·9236	27·48
December . .	1·7874	25·53	3·3777	48·25	—	—
Average . .	4·0728	58·18	4·5340	64·77	2·5896	37

Average of A, B, and C for 12 months . . 3·7648 53·78

A. All Saints . . . 12 ft. above the ground.

B. ,, . . . 30 ft. ,, ,,

C. Lit. and Phil. Society, George Street, 2 ft. above the ground.

RAIN. TABLE LXVII.

AMMONIA.

Manchester, 1870.

1870. Month.	Ammonia. Parts per Million.		
	A.	B.	C.
January . . .	12	10·08	3·1
February . . .	22·9	—	6
March . . .	7·1	6·3	3·9
April	12·3	5·8	4·6
May	20·1	5	7·3
June	6·3	1·6	5·27
July	1·68	1·1	5·6
August . . .	8·5	·64	3·1
September . . .	5·8	·98	3·1
October . . .	4·2	4·8	1·8
November . . .	19·4	8·7	4
December . . .	3·8	6·8	—
Average . .	10·34	4·71	4·34

Average of the 34 specimens . . . . 6·578

A. All Saints . . . 12 ft. above the ground.

B. „ . . . 30 ft. „ „

C. Lit. and Phil. Society, George Street, 2 ft. above the ground.

RAIN. TABLE LXVIII.

ALBUMINOID AMMONIA.

Manchester, 1870.

1870. Month.	Albuminoid Ammonia. Parts per Million.		
	A.	B.	C.
January . . .	·17	·15	·10
February . . .	·40	—	·13
March . . .	·10	·15	·15
April	·30	·80	·10
May	·55	·65	·20
June	·25	·55	·20
July	·35	·25	·19
August . . .	·63	·50	·30
September . . .	·28	·25	·15
October . . .	·15	·25	·10
November . . .	·50	·15	·25
December . . .	·30	·15	—
Average . .	·33	·35	·17

Average of the 34 specimens . . . . ·285

A. All Saints . . 12 ft. above the ground.

B. „ . . 30 ft. „ „

C. Lit. and Phil. Society, George Street, 2 ft. above the ground.

RAIN. TABLE LXIX.

NITRIC ACID.

Manchester, 1870.

1870. Month.	Nitric Acid. Parts per Million.		
	A.	B.	C.
January	1·197	1·456	·856
February	2·568	—	1·456
March	1·642	·604	·901
April	1·456	1·456	·715
May	2·938	—	1·456
June	1·642	·530	1·271
July	·453	·713	—
August	·342	·342	2·751
September	·935	·926	—
October	·898	1·083	·527
November	1·454	1·297	·898
December	·898	·898	—
Average . . .	1·369	·931	1·203

Average of the 31 specimens . . . . 1·179

A. All Saints . . . 12 ft. above the ground.

B. „ . . . . 30 „ „

C. Lit. and Phil. Society . 2 „ „

RAIN. TABLE LXX.

PERMANGANATE TEST.

Manchester, 1870.

1870. Month.	Parts by Weight. Oxygen required per Million Parts of Rain.					
	A.		B.		C.	
	Instantly.	Total in a few Minutes with Acid.	Instantly.	Total in a few Minutes with Acid.	Instantly.	Total in a few Minutes with Acid.
January . . .	1·097	6·825	·731	5·606	·609	3·656
February . . .	—	—	—	—	1·097	5·361
March	1·218	7·311	·731	5·361	·975	5·118
April	·758	5·633	1·213	6·066	·498	3·580
May	—	—	—	—	None	3·683
June	·542	6·607	·488	6·283	·065	2·795
July	None	3·683	None	2·816	·108	2·166
August . . .	·346	5·412	·151	2·598	·183	1·948
September . .	·108	2·474	·108	4·113	·216	2·923
October . . .	None	2·165	·433	3·464	None	1·407
November . .	·433	7·144	·866	6·278	·065	2·598
December . . .	·649	4·113	·909	6·062	—	—
Average . .	·515	5·137	·563	4·865	·347	3·203

Average of the 31 specimens . . . . . ·470 4·365

A. All Saints . . . . . 12 ft. above the ground.

B. „ . . . . . 30 „ „

C. Lit. and Phil. Society, George Street . 2 „ „

RAIN. TABLE LXXI.

Liverpool—Spring, 1870.

Rain obtained from	Hydrochloric Acid.		Sulphuric Acid.		Proportion of Hydrochloric to Sulphuric Acid.
	Grains per Gallon.	Parts per Million.	Grains per Gallon.	Parts per Million.	
rescot St., Bridewell .	1·2775	18·25	·9800	14	1 to ·77
oburg Dock, Police Station	·2839	4·06	1·4700	21	1 „ 5·17
edford St., Bridewell .	·3931	5·61	1·8375	26·25	1 „ 4·67
ollingwood Dock, Police Station	1·2775	18·25	2·9400	42	1 „ 2·30
rgyle St., Bridewell . .	·3006	4·29	2·9400	42	1 „ 9·78
ord St. (Disinfecting Establishment)	·5110	7·30	2·9400	42	1 „ 5·75
heapside, Bridewell . .	1·0220	14·60	2·9400	42	1 „ 2·88
ontenoy St. (Glover's Brewery)	·5678	8·11	3·2667	46·67	1 „ 5·75
ose Hill, Police Station.	·3407	4·87	4·2000	60	1 „ 12·32
isenhale St., Bridewell	1·1356	16·22	4·2000	60	1 „ 3·70
Average	·7110	10·16	2·7714	39·59	[1] 1 to 3·90

[1] This figure is got by dividing the average in the third column by that in the first.

RAIN. TABLE LXXII.

ACIDITY (FREE ACIDS).

Liverpool—Spring, 1870.

Rain obtained from	Acidity calculated as Sulphuric Anhydride.	
	Grains per Gallon.	Parts per Million.
Coburg Dock, Police Station . . .	·3416	4·88
Collingwood Dock „ . . .	·3416	4·88
Bedford Street, Bridewell . . .	·5465	7·81
Prescot Street „ . . .	·5465	7·81
Argyle Street „ . . .	·6149	8·78
Ford Street (Disinfecting Establishment).	·6490	9·27
Cheapside, Bridewell	·6832	9·76
Rose Hill, Police Station . . .	1·1614	16·59
Fontenoy Street (Glover's Brewery) .	1·5714	22·45
Chisenhale Street, Bridewell . . .	1·6397	23·42
Average	·8096	11·56

RAIN. TABLE LXXIII.

AMMONIA.

Liverpool—Spring, 1870.

	Ammonia. Parts per Million.
Prescot Street, Bridewell	1·80
Coburg Dock, Police Office	2·10
Bedford Street, Bridewell	3·60
Collingwood Dock, Police Office	3·60
Cheapside, Bridewell	3·60
Argyle Street, Bridewell	4·05
Ford Street (Disinfecting Establishment) . .	5·58

	Ammonia. Parts per Million.
Fontenoy Street (Glover's Brewery)	7·90
Rose Hill, Police Station	9·20
Chisenhale Street, Bridewell	12·40
Average	5·38

ALBUMINOID AMMONIA.

Coburg Dock, Police Station	·07
Prescot Street, Bridewell	·10
Bedford Street „	·10
Collingwood Dock, Police Station	·10
Argyle Street, Bridewell	·10
Cheapside „	·15
Ford Street (Disinfecting Establishment)	·15
Chisenhale Street, Bridewell	·17
Rose Hill, Police Station	·25
Fontenoy Street (Glover's Brewery)	·40
Average	·159

RAIN. TABLE LXXIV.

NITRIC ACID.

Liverpool—Spring, 1870.

	Nitric Acid. Parts per Million.
Ford Street (Disinfecting Establishment)	·271
Prescot Street, Bridewell	·345
Collingwood Dock, Police Office	·345
Argyle Street, Bridewell	·456
Bedford Street „	·530
Coburg Dock, Police Office	·640
Cheapside, Bridewell	·715
Rose Hill, Police Office	·715
Fontenoy Street (Glover's Brewery)	·901
Chisenhale Street, Bridewell	·901
Average	·582

RAIN. TABLE LXXV.

PERMANGANATE TEST.

Liverpool—Spring, 1870.

Rain obtained from	Oxygen per Million Parts of Water by Weight.	
	Instantly without Acid.	Total in a few Minutes with Acid.
Collingwood Dock, Police Station . .	None	·731
Prescot Street, Bridewell . . .	„	1·145
Coburg Dock, Police Station . . .	„	1·462
Cheapside, Bridewell	„	2·315
Argyle Street „	·044	2 725
Bedford Street „	·244	3·046
Ford Street (Disinfecting Establishment).	·975	4·387
Rose Hill, Police Station . . .	1·462	6·580
Fontenoy Street (Glover's Brewery) .	1·706	7·067
Chisenhale Street, Bridewell . . .	2·193	9·504
Average	·662	3·896

RAIN. TABLE LXXVI.

SPECIMENS FROM LONDON, COLLECTED CAREFULLY AND WITH SLIGHT EXPOSURE ONLY.

ACIDS (FREE AND COMBINED).

London, 1869.

Rain obtained from	Date.	Hydrochloric Acid.		Sulphuric Acid (Anhydrous).		Proportion of Hydrochloric to Sulphuric Acid.	Acidity calculated as Sulphuric Acid (Anhydrous).	
		Grains per Gallon.	Parts per Million.	Grains per Gallon.	Parts per Million.		Grains per Gallon.	Parts per Million.
Guy's Hospital	February 1869	·1703	2·43	1·4700	21	1 to 8·63	·5387	7·70
Islington, a garden at Compton Terrace	December 1869	·1310	1·87	2·0276	28·96	1 ,, 15·48	·2733	3·90
Poplar	Nov. & Dec. 1869	·0568	·81	·7350	10·50	1 ,, 12·94	·2733	3·90
Caversham Road, NW., near Kentish Town Railway Station	Dec. 1869 and Jan. 1870	·0462	·66	1·4700	21	1 ,, 31·82	—	—
Edward Street, Hampstead Road, close to Cumberland Market, during a gale[1]	Nov. 27, 1869	·0319	·46	1·4700	21	1 ,, 46·08	None	None
Average		·0872	1·246	1·4345	20·492	[2] 1 to 16·45	·2713	3·875

[1] It is remarkable that during a gale the amount of acid should have been so small as in this case.
[2] Ratio of averages of the acids.

RAIN. TABLE LXXVII.

AMMONIA AND NITRIC ACID.

London, 1869.

Rain obtained from	Date.	Parts per Million.		
		Ammonia.	Albuminoid Ammonia.	Nitric Acid.
Guy's Hospital .	February 7, 1869	5·30	·5	—
Islington, a garden at Compton Terrace	December „	4·70	·13	1·482
Poplar . . .	Nov. and Dec. „	1·30	·09	·556
Edward Street, Hampstead Road, close to Cumberland Market, during a gale	November 27 „	2·50	·10	·482
Average		3·45	·205	·840

LONDON, 1870.

The following specimens from London were exposed longer than the others in most cases, and do not appear in the tables of averages already given. The following tables form a separate investigation, but in the averages at the end the two preceding are included.

RAIN. TABLE LXXVIII.

HYDROCHLORIC ACID (CHLORIDES).

London, 1870.

N. AND NW. DISTRICTS.

Rain obtained from	Date.	Remarks, Wind, &c.	Hydrochloric Acid.	
			Grains per Gallon.	Parts per Million
Edward St., Hampstead Rd., close to Cumberland Market, during a gale	Nov. 1869	WSW.; 5 ft. from the ground; 115 ft. above sea.	·0319	·46
Caversham Rd., near Kentish Town Railway Station	December to January 1870	W. and SW.	·0462	·66
Islington, Compton Terrace	March to September 1870	—	·1022	1·46
Ditto Ditto	December 1869	—	·1310	1·87
Stoke Newington Fire Station, 29	January to May 1870	Chiefly W.; 20 ft. from dwellings and 5 ft. from the ground	·2555	3·65
St. Pancras Fire Station	January to July 1870	—	·2555	3·65
Islington, Essex Rd. Fire Station, 16	—	—	·4866	6·95
Hampstead Fire Station, 11	February to May 1870	E. and W.; 10 ft. from ground; 450 ft. above the sea	·5685	8·12
St. John's Wood Fire Station, 10	—	—	·7301	10·43
Highgate Road Fire Station	January to May 1870	Westerly	·8661	12·37
Average			·3474	4·96

RAIN. TABLE LXXVIII.—*continued.*

SW. AND SE. DISTRICTS.

Rain obtained from	Date.	Remarks, Wind, &c.	Hydrochloric Acid.	
			Grains per Gallon.	Parts per Million
Guy's Hospital .	Feb. 1869 .		·1703	2·43
Westminster Fire Station	May 1870 .	SW.; 65 ft. from the ground and 30 ft. from dwellings	·5678	8·11
Clapham Fire Station, 51	—	—	·6387	9·12
Fulham „ „	March to May 1870	Westerly; 24 ft. from the ground	·6813	9·73
Fulham Road, near Consumption Hosptl	February to May 1870	Westerly; 43 ft. from the ground	·9291	13·27
Tooting Fire Station, 52	—	(Water alkaline)	·9310	13·30
Average			·6530	9·33

RAIN. TABLE LXXIX.

HYDROCHLORIC ACID (CHLORIDES).

London, 1870.

E. AND E.C. DISTRICTS.

Rain obtained from	Date.	Remarks, Wind, &c.	Hydrochloric Acid.	
			Grains per Gallon.	Parts per Million
Poplar . . .	Nov. and Dec. 1869	Chiefly westerly	·0568	·81
Watling Street Fire Station	January to February 1870	—	·2839	4·06
Bow Fire Station, 31	—	—	·3194	4·56
Hackney „ 27	—	—	·6813	9·73
Poplar, West India Road Fire Station, 33	January to May 1870	Chiefly westerly; 45 ft. from ground	·7301	10·43
Mile End Fire Station, 30	—	45 ft. above ground	·7862	11·23
South Hackney Fire Station, 28	January to May 1870	Chiefly westerly; over a closet; 12 ft. above ground and 40 ft. from nearest chimney	·7862	11·23
Fire Station, 36	—	—	·8517	12·17
St. Luke's Fire Station, 18	—	—	·9828	14·04
Wellclose Square Fire Station	January to May 1870	—	1·0220	14·60
Holborn Fire Station, 22	February to May 1870	—	1·0220	14·60
Bishopsgate Street Fire Station, 34	—	—	1·0220	14·60
Whitechapel Fire Station, 37	—	—	1·1355	16·22
Average			·7446	10·64

RAIN. TABLE LXXIX.—*continued.*

W. AND W.C. DISTRICTS.

Rain obtained from	Date.	Wind, Remarks, &c.	Hydrochloric Acid.	
			Grains per Gallon.	Parts per Million
Paddington Fire Station, 8	April and May 1870	Chiefly westerly	·6552	9·36
St. Giles Fire Station, 21	January to May 1870	„ „	·6813	9·73
Kensington Fire Station, 5	April and May 1870	„ „	·7301	10·43
Hammersmith Fire Station, 6	—	—	·9291	13·27
Regent Street Fire Station, 13	January to May 1870	„ „	1·7033	24·33
Average			·9398	13·42

RAIN. TABLE LXXX.

HYDROCHLORIC ACID (CHLORIDES).

London, 1870.

Five Stations—Names lost.	Hydrochloric Acid.	
	Grains per Gallon.	Parts per Million.
No. 1	1·6485	23·55
No. 2	1·2775	18·25
No. 3	1·0872	15·53
No. 4	·7301	10·43
No. 5	2·0440	29·20
Average . . .	1·3575	19·39
Average of all London . .	·7323	10·46
Woolwich Fire Station . .	·5678	8·11
Greenwich „ „ . .	·7862	11·23

RAIN. TABLE LXXXI.

SULPHURIC ACID (SULPHATES).

London, 1870.

N. AND NW. DISTRICTS.

Rain obtained from	Date.	Remarks, Wind, &c.	Sulphuric Anhydride.	
			Grains per Gallon.	Parts per Million
Islington, Compton Terrace,	March to Sept. 1870	—	·8162	11·66
Stoke Newington Fire Station, 29	Jan. to May 14 „	Chiefly W.	1·3601	19·43
Edward Street, Hampstead Road, close to Cumberland Market, during a gale	Nov. 27, 1869	WSW.	1·4700	21
Caversham Road, near Kentish Town Railway Station	Dec. to Jan. 1870	W. & SW.	1·4700	21
Islington, Compton Terrace .	December 1869	—	2·0276	28·96
Hampstead Fire Station, 11	Feb. to May 16, 1870	Ely & wly	3·1733	45·33
Highgate Rd. „ „ —	Jan. to May 15 „	Westerly	3·5700	51
St. John's Wd. „ „ 10	—	—	3·5700	51
Islington, Essex Rd. „ 16	—	—	3·8080	54·40
St. Pancras „ „ —	Jan. to July, 1870	—	4·0803	58·29
Average			2·5346	36·21

SW. AND SE. DISTRICTS.

Guy's Hospital . . .	February 1869	—	1·4700	21
Tooting Fire Station, 52 .	—	—	2·2848	32·64
Fulham „ „ — .	March to May 1870	Westerly	2·7200	38·86
Westminster,, „ — .	May 1870	SW.	3·3600	48
Fulham Road, near Consumption Hospital	Feb. to May 20, 1870	—	4·7600	68
Average			2·9190	41·70

RAIN. TABLE LXXXII.

SULPHURIC ACID (SULPHATES).

London, 1870.

E. AND E.C. DISTRICTS.

Rain obtained from	Date.	Remarks, Wind, &c.	Sulphuric Anhydride.	
			Grains per Gallon.	Parts per Million
Poplar	Nov. & Dec. 1869	Chiefly westly	·7350	10·50
Watling St. Fire Station	Jan. to Feb. 1870	—	1·1760	16·80
Bow „ „ 31	—	—	2·3800	34
Sth Hackney „ „ 28	Jan. to May 1870	Over a closet, 12 ft. above ground, and 40 ft. from nearst chmny; chiefly westly	2·5970	37·10
Poplar, West India Road Fire Station, 33	„ „ „	Chiefly westly	3·5700	51
Fire Station, 36	—	—	3·6187	51·70
Hackney „ „ 27	—	—	4·3939	62·77
Wellclose Sq. „ „ —	Jan. to Ap. 28, 1870	Chiefly westly	4·7600	68
St. Luke's „ „ 18	—	—	5·7120	81·60
Holborn „ „ 22	Feb. to My 20, 1870	Chiefly westly	5·9500	85
Mile End „ „ 30	—	45 ft. frm ground	7·1400	102
Whitechapel „ „ 37	—	—	8·1620	116·60
Bishopsgate St. „ 34	—	—	8·4000	120
Average			4·5073	64·39

RAIN. TABLE LXXXII.—*continued.*

W. AND W.C. DISTRICTS.

Specimens long exposed. Rain obtained from	Date.	Remarks, Wind, &c.	Sulphuric Anhydride.	
			Grains per Gallon.	Parts per Million
Hammersmith Fire Statn, 6	—	—	3·3600	48
Kensington „ „ 5	Ap. to My 23, 1870	—	3·5700	51
Paddington „ „ 8	„ „ „	Chiefly westrly	5·1940	74·20
Regent Street „ „ 13	Jan. to My 14 „	„ „	5·7120	81·60
St. Giles' „ „ 21	„ „ „	„ „	6·3467	90·67
Average			4·8365	69·09

RAIN. TABLE LXXXIII.

SULPHURIC ACID (SULPHATES).

London, 1870.

Five Stations—Names lost.	Sulphuric Acid.	
	Grains per Gallon.	Parts per Million.
No. 1	4·0800	58·28
No. 2	11·9000	170
No. 3	7·1400	102
No. 4	4·4044	62·92
No. 5	3·1733	45·33
Average . . .	6·1395	87·71
Average of all London . .	4·0372	57·67
Woolwich Fire Station . .	4·0810	58·30
Greenwich „ „ . .	5·7120	81·60

RAIN. TABLE LXXXIV.

PROPORTION OF HYDROCHLORIC ACID TO SULPHURIC ACID (CHLORIDES TO SULPHATES).

London, 1869 and 1870.

N. AND NW. DISTRICTS.

Specimens mostly long exposed. Rain obtained from	Proportion of Hydrochloric to Sulphuric Acid.
Highgate Road Fire Station	1 to 4·12
St. John's Wood „ „	1 „ 4·89
Stoke Newington „ „	1 „ 5·32
Hampstead „ „	1 „ 5·58
Islington, Essex Road Fire Station	1 „ 7·83
Islington, Compton Terrace, 1870	1 „ 8
„ „ 1869	1 „ 15·48
St. Pancras Fire Station	1 „ 15·97
Caversham Rd., near Kentish Town Rly Station	1 „ 31·82
Edward St., Hampstead Road, close to Cumberland Market, during a gale	1 „ 46·08
Ratio of Average Acids	1 to 7·30

SW. AND SE. DISTRICTS.

Tooting Fire Station	1 to 2·45
Fulham „ „	1 „ 3·99
Fulham Road, near Consumption Hospital	1 „ 5·12
Westminster Fire Station	1 „ 5·92
Guy's Hospital	1 „ 8·63
Ratio of Average Acids	1 to 4·47

RAIN. TABLE LXXXV.

PROPORTION OF HYDROCHLORIC TO SULPHURIC ACID (CHLORIDES TO SULPHATES).

London, 1869 and 1870.

E. AND E.C. DISTRICTS.

Specimens long exposed. Rain obtained from	Proportion of Hydrochloric to Sulphuric Acid.
South Hackney Fire Station, 28	1 to 3·30
Watling Street „ —	1 „ 4·14
Fire Station, 36	1 „ 4·25
Wellclose Square „	1 „ 4·66

Specimens long exposed. Rain obtained from	Proportion of Hydrochloric to Sulphuric Acid.
Poplar, West India Road Fire Station . .	1 to 4·88
St. Luke's " " . .	1 " 5·81
Holborn " " . .	1 " 5·82
Hackney " " . .	1 " 6·45
Whitechapel " " . .	1 " 7·19
Bow " " . .	1 " 7·46
Bishopsgate Street " " . .	1 " 8·22
Mile End " " . .	1 " 9·08
Poplar	1 " 12·94
Ratio of Average Acids . . .	1 " 6·05

W.C. AND W. DISTRICTS.

Regent Street Fire Station	1 to 3·35
Hammersmith " "	1 " 3·62
Kensington " "	1 " 4·89
Paddington " "	1 " 7·93
St. Giles' " "	1 " 9·32
Ratio of Average Acids . . .	1 " 5·15

RAIN. TABLE LXXXVI.

PROPORTION OF HYDROCHLORIC TO SULPHURIC ACID (CHLORIDES TO SULPHATES).

London, 1870.

Five Stations— Names lost.	Proportion of Hydrochloric to Sulphuric Acid.
No. 5	1 to 1·55
No. 1	1 " 2·47
No. 4	1 " 6·03
No. 3	1 " 6·57
No. 2	1 " 9·31
Ratio of Average Acids .	1 to 4·52
Average of all London	1 to 5·51
Woolwich Fire Station	1 to 7·19
Greenwich " "	1 " 7·27

RAIN. TABLE LXXXVII.

ACIDITY (FREE ACIDS).

London, 1869 and 1870.

N. AND NW. DISTRICTS.

Specimens mostly long exposed. Rain obtained from	Acidity calc. as Sulphuric Anhydride.	
	Grains per Gallon.	Parts per Million.
Edward Street, Hampstead Road, close to Cumberland Market, during a gale . .	None	None
Islington, Compton Terrace, December 1869	·2733	3·90
Highgate Road Fire Station . . .	·4736	6·76
Hampstead „ „	·6832	9·76
Stoke Newington „ „	·7801	11·14
Islington, Compton Terrace, March to September, 1870	·8358	11·94
St. John's Wood Fire Station . . .	·9194	13·13
St. Pancras „ „	·9472	13·53
Islington, Essex Road „ „	1·3930	19·90
Average	·7006	10·01

SW. AND SE. DISTRICTS.

Tooting Fire Station	Alkaline	—
Fulham „ „	·3985	5·69
Guy's Hospital	·5387	7·70
Westminster Fire Station	·8358	11·94
Fulham, near Consumption Hospital . .	1·3094	18·70
Average (of 5) . . .	·6165	8·81

RAIN. TABLE LXXXVIII.

ACIDITY (FREE ACIDS).

London, 1869 and 1870.

E. AND E.C. DISTRICTS.

Specimens long exposed. Rain obtained from	Acidity calc. as Sulphuric Anhydride.	
	Grains per Gallon.	Parts per Million.
Poplar	·2733	3·90
Poplar, West India Road Fire Station .	·4782	6·83
South Hackney „ „ . .	·6832	9·76
Bow „ „ . .	·7522	10·75
Whitechapel „ „ . .	·8358	11·94
Fire Station, 36	·9751	13·93
Wellclose Square Fire Station . . .	1·2537	17·91
Mile End „ „ . . .	1·8109	25·87
Holborn „ „ . . .	2·4238	34·62
Bishopsgate Street „ „ . . .	3·3432	47·76
Watling Street „ „ . . .	None	None
St. Luke's „ „ . . .	„	„
Average	1·0691	15·27

W.C. AND W. DISTRICTS.

Paddington Fire Station	Alkaline	—
Hammersmith „ „	·5293	7·56
Kensington „ „	1·2537	17·91
St. Giles' „ „	1·9502	27·86
Regent Street „ „	2·2288	31·84
Average (of 5) . . .	1·1924	17·03

RAIN. TABLE LXXXIX.

ACIDITY (FREE ACIDS).

London, 1870.

Specimens long exposed. Five Stations—Names lost.	Acidity calc. as Sulphuric Anhydride.	
	Grains per Gallon.	Parts per Million.
No. 3.	1·3930	19·90
No. 4.	1·5323	21·89
No. 1.	1·6716	23·88
No. 5.	2·2288	31·84
No. 2.	3·3432	47·76
Average	2·0338	29·05
Average of all London (including alkaline samples)	1·0652	15·22
Average of all London (leaving out the alkaline samples)	1·1279	16·11
Greenwich Fire Station	·8079	11·54
Woolwich „ „	1·6716	23·88

RAIN. TABLE XC.

AMMONIA.

London, 1869 and 1870.

N. AND NW. DISTRICTS.

Specimens mostly long exposed. Rain obtained from	Date.	Remarks.	Ammonia. Parts per Million.
Islington, Compton Terrace	March to September 1870	—	·60
Stoke Newington Fire Station, 29	January to May 1870	20 ft. from dwellings and 5 ft. from the ground, chiefly west	1·80
Hampstead Fire Station, 11	February to May 16, 1870	10 ft. from ground, 450 ft. above sea, easterly and westerly	2·10
Hampstead Rd., Edward St., Cumberland Market, a gale	November 27, 1869	5 ft. from ground, WSW., 115 ft. above sea	2·50
Islington, Compton Terrace	December 1869	—	4·70
Highgate Road Fire Station	January to May 15, 1870	Westerly W. . . .	6·50
Islington, Essex Road Fire Station, 16	—	—	6·70
St. Pancras . .	January to July 1870	—	7·61
St. John's Wood Fire Station, 10	—	—	8·60
Average . .			4·57

SW. AND SE. DISTRICTS.

Guy's Hospital . .	February 1869	—	5·30
Fulham Fire Station .	March 21 to May 12, 1870	Westerly, 24 ft. from the ground	6·40
Fulham Rd., near Consumption Hospital .	February to May 20, 1870	Westerly, 43 ft. from the ground	18·70
Average . .			10·13

RAIN. TABLE XCI.

AMMONIA.

London, 1869 and 1870.

E. AND E.C. DISTRICTS.

Specimens long exposed. Rain obtained from	Date.	Remarks.	Ammonia. Parts per Million.
Poplar . . .	November and December 1869	—	1·30
Whitechapel Fire Station, 37	—	—	2·10
Bow Fire Station, 31	–	—	5·90
South Hackney Fire Station, 28	January to May 14, 1870	Over a closet, 12 ft. above the ground, 40 ft. from the nearest chimney, chiefly W.	6·80
Fire Station, 36	—	—	8·10
Poplar, West India Road Fire Station, 33	January to May 2, 1870	45 ft. from the ground, chiefly W.	9·91
St. Luke's Fire Station, 18	—	—	10·30
Watling St. Fire Station	JanuarytoFebruary 1870	—	12·20
Mile End Fire Station, 30	—	—	13·20
Wellclose Square Fire Station	January to April 28, 1870	Chiefly westerly . .	18·60
Holborn Fire Station, 22	February to May 20, 1870	„ „	22·40
Bishopsgate Street Fire Station, 34	—	—	32·35
Average . .			11·93

RAIN. TABLE XCI.—*continued.*

W. DISTRICT.

Specimens long exposed. Rain obtained from	Date.	Remarks.	Ammonia. Parts per Million.
Paddington Fire Station, 8	April to May 23, 1870	Chiefly westerly	·60
Hammersmith Fire Station, 6	—	—	2·05
Kensington Fire Station, 5	April to May 23, 1870	Chiefly westerly	7·60
Average . .			3·42

W.C. DISTRICT.

Regent Street Fire Station, 13	January to May 14, 1870	Chiefly westerly . .	22·15
St. Giles' Fire Station, 21	January to May 14, 1870	„ „ . .	26·90

RAIN. TABLE XCII.

AMMONIA.

London, 1870.

Four Stations—Names lost.	Ammonia. Parts per Million.
No. 4	10·30
No. 1	11·90
No. 3	12·90
No. 2	39·80
Average	18·72
Average of all London	10·57
Woolwich Fire Station	6·50
Greenwich „ „	14·70

RAIN. TABLE XCIII.

ALBUMINOID AMMONIA.

London, 1869 and 1870.

N. AND NW. DISTRICTS.

Specimens mostly long exposed. Rain obtained from	Albuminoid Ammonia. Parts per Million.
Edward St., Hampstead Rd., Cumberland Market, during a gale	·10
Hampstead Fire Station, 11	·13
Islington, Compton Terrace, 1869	·13
Stoke Newington Fire Station, 29	·175
Islington, Compton Terrace, 1870	·20
St. John's Wood Fire Station, 10	·25
Highgate Road „ „ —	·27
Islington, Essex Road Fire Station, 16	·40
St. Pancras Fire Station	·75
Average	·267

SW. AND SE. DISTRICTS.

Fulham Fire Station	·10
Fulham Road, near Consumption Hospital	·47
Guy's Hospital	·50
Average	·357

RAIN. TABLE XCIV.

ALBUMINOID AMMONIA.

London, 1869 and 1870.

E. AND E.C. DISTRICTS.

Specimens long exposed. Rain obtained from	Albuminoid Ammonia. Parts per Million.
Poplar	·09
Wellclose Square Fire Station	·15
— „ „ 36	·20
South Hackney „ „ 28	·20
Bow „ „ 31	·20

Specimens long exposed. Rain obtained from	Albuminoid Ammonia. Parts per Million.
Poplar, West India Road Fire Station, 33	·20
Watling Street Fire Station	·20
St. Luke's „ „ 18	·45
Mile End „ „ 30	·46
Whitechapel „ „ 37	·55
Holborn „ „ 22	·65
Bishopsgate St. „ „ 34	1·08
Average	·37

W. DISTRICT.

Hammersmith Fire Station, 6	·20
Paddington „ „ 8	·30
Kensington „ „ 5	·35
Average	·283

W.C. DISTRICT.

St. Giles' Fire Station, 21	·30
Regent St. „ „ 13	·50

RAIN. TABLE XCV.

ALBUMINOID AMMONIA.

London, 1870.

Four Stations— Names lost.	Albuminoid Ammonia. Parts per Million.
No. 4	·25
No. 1	·30
No. 2	·30
No. 3	·35
Average	·30
Average of all London	·326
Greenwich Fire Station	·20
Woolwich „ „	·33

RAIN. TABLE XCVI.

NITRIC ACID.

London, 1869 and 1870.

N. AND NW. DISTRICTS.

Specimens mostly long exposed. Rain obtained from	Nitric Acid. Parts per Million.
Edward St., Hampstead Rd., close to Cumberland Market, during a gale	·482
St. Pancras Fire Station	·972
Islington, Compton Terrace, Dec. 1869	1·482
Stoke Newington Fire Station, 29	5·901
St. John's Wood Fire Station, 10	7·495
Islington, Essex Rd. „ 16	16·278
Hampstead „ 11	17·763
Highgate Road „ —	24·802
Average	9·397

SW. AND SE. DISTRICTS.

Fulham Road, near Consumption Hospital	3·680
Fulham Fire Station	5·533
Average	4·606

RAIN. TABLE XCVII.

NITRIC ACID.

London, 1869 and 1870.

E. AND E.C. DISTRICTS.

Specimens long exposed. Rain obtained from	Nitric Acid. Parts per Million.
Poplar	·556
South Hackney Fire Station, 28	1·454
Poplar, West India Road Fire Station, 33	2·198
Wellclose Square Fire Station	2·566
Holborn „ „ 22	3·335
Bishopsgate Street „ „ 34	4·048
Bow „ „ 31	7·013

Specimens long exposed. Rain obtained from	Nitric Acid. Parts per Million.
St. Luke's Fire Station 18	7·013
— „ 36	7·757
Mile End „ 30	16·659
Whitechapel „ 37	17·390
Average	6·363

W. DISTRICT.

Kensington Fire Station, 5	2·751
Paddington „ 8	21·096
Hammersmith „ 6	22·949
Average	15·598

W.C. DISTRICT.

St. Giles' Fire Station, 21	1·825
Regent St. „ „ 13	2·936

RAIN. TABLE XCVIII.

NITRIC ACID.

London, 1870.

	Nitric Acid. Parts per Million.
No. 1	2·195
No. 3	4·048
No. 4	12·572
No. 2	18·872
Average	9·422
Average of all London	8·121
Greenwich Fire Station	8·124
Woolwich „ „	16·278

RAIN. TABLE XCIX.

PERMANGANATE TEST.

London, 1869 and 1870.

N. AND NW. DISTRICTS.

Specimens mostly long exposed. Rain obtained from	Parts by Weight. Oxygen required per one Million Parts of Water.	
	Instantly.	Total in a few Minutes with Acid.
Stoke Newington Fire Station, 29	None	1·560
Hampstead Fire Station, 11	·130	2·425
Islington, Compton Terrace, 1870	·390	2·815
,, ,, ,, Dec. 1869	·942	4·643
St. Pancras	None	4·701
Islington Fire Station, 16	·325	6·066
St. John's Wd. ,, 10	None	6·066
Highgate Rd. ,, —	·065	11·374
Average	·231	4·956

SW. DISTRICT.

Fulham Fire Station	None	2·816
Fulham Rd., near Consumption Hospital	2·491	15·382
Average	1·245	9·099

E. AND E.C. DISTRICTS.

Poplar	·269	2·512
Bow Fire Station, 31	None	3·141
Poplar, West India Rd. Fire Station, 33	·260	3·358
South Hackney Fire Station, 28	·260	4·116
— ,, ,, 36	·195	5·741
St. Luke's ,, ,, 18	1·083	9·533
Wellclose Sq. ,, ,, —	·670	9·749
Mile End ,, ,, 30	·780	9·966
Whitechapel ,, ,, 37	·758	11·049
Holborn ,, ,, 22	4·550	19·500
Bishopsgate St. ,, ,, 34	5·850	23·400
Average	1·334	9·279

RAIN. TABLE C.

PERMANGANATE TEST.

London, 1870.

W. DISTRICT.

Specimens long exposed. Rain obtained from	Parts by Weight. Oxygen required per Million Parts of the Water.	
	Instantly.	Total in a few Minutes with Acid.
Hammersmith Fire Station, 6	·216	4·008
Paddington „ „ 8	None	5·630
Average	·108	4·819
St. Giles' Fire Station, 21	4·116	16·249
Regent St. „ „ 13	4·333	20·365
4 Stations—Names lost.		
No. 1	·217	4·333
No. 4	·542	5·091
No. 2	·520	5·741
No. 3	·585	7·366
Average	·466	5·632
Average of all London	1·019	7·886
Woolwich Fire Station	None	2·166
Greenwich „ „	·195	6·391

RAIN. TABLE CI.

MANCHESTER RAIN-WATER COLLECTED DURING 1869.

ACIDS (COMBINED AND FREE) AND ACIDITY.

Date.	Hydrochloric Acid.		Sulphuric Acid (Anhydrous).		Proportion—Hydrochloric to Sulphuric Acid.	Acidity calculated as Sulphuric Acid (Anhydrous).	
	Grains per Gallon.	Parts per Million.	Grains per Gallon.	Parts per Million.		Grains per Gallon.	Parts per Million.
January (*a*) .	·5110	7·30	5·6000	80	1 to 10·96	2·3087	32·98
February ,, .	·6387	9·12	2·9400	42	1 ,, 4·60	1·6930	24·20
March ,, .	·7300	10·43	7·3500	105	1 ,, 10·07	1·3664	19·52
April ,, .	·3006	4·29	3·6750	52·50	1 ,, 12·22	·5465	7·81
May ,, .	·3193	4·56	2·9400	42	1 ,, 9·20	1·4347	20·49
June ,, .	·5677	8·11	3·2666	46·67	1 ,, 5·75	·9565	13·66
July ,, .	·3931	5·62	3·2666	46·67	1 ,, 8·31	·4782	6·83
August ,, .	·3006	4·29	2·9400	42	1 ,, 9·78	·7857	11·22
,, (*b*) .	·1460	2·08	2·9400	42	1 ,, 20·14	None	None
,, (*c*) .	·2044	2·92	1·7294	24·71	1 ,, 8·46	—	—
September (*a*) .	·3407	4·87	3·2667	46·67	1 ,, 9·59	·4441	6·34
,, (*b*) .	·2555	3·65	1·9600	28	1 ,, 7·67	None	None
,, (*c*) .	·2044	2·92	·9800	14	1 ,, 4·79	·0683	·98
October (*a*) .	·4258	6·08	3·7800	54	1 ,, 8·88	1·1273	16·10
,, (*b*) .	·3931	5·61	1·5474	22·11	1 ,, 3·94	·1366	1·95
,, (*c*) .	·5678	8·11	1·5474	22·11	1 ,, 2·72	·1366	1·95
November (*a*) .	·7862	11·23	2·9400	42	1 ,, 3·74	·3074	4·39
,, (*b*) .	·7300	10·43	4·2000	60	1 ,, 5·73	·0342	·49
,, (*c*) .	·6012	8·60	1·4700	21	1 ,, 2·44	·1025	1·46
December (*a*) .	·1022	1·46	·9187	13·12	1 ,, 8·99	·1879	2·68
,, (*b*) .	·3194	4·56	2·9400	42	1 ,, 9·20	·3416	4·88
,, (*c*) .	·2222	3·17	1·9600	28	1 ,, 8·82	·3758	5·37
Mean . .	·4118	5·88	2·9163	41·66	1 to 7·08	·5833	8·33
Mean of (*a*) .	·4513	6·45	3·5736	51·05	1 ,, 7·91	·9697	13·85
Mean of (*a*) from August	·3911	5·59	2·7691	39·56	1 ,, 7·08	·5705	8·15
Mean of (*b*) .	·3688	5·27	2·7175	38·82	1 ,, 7·37	·1025	1·46
Mean of (*c*) .	·3600	5·14	1·5373	21·96	1 , 4·27	·1708	2·44

(*a*) Behind the laboratory, 12 ft. above the ground. (*b*) Same place, 30 ft. above the ground. (*c*) Behind the Literary and Philosophical Society, George Street, 2 ft. above the ground.

RAIN. TABLE CII.

MANCHESTER RAIN-WATER, 1869.

'AMMONIA' AND NITRIC ACID.

Month.	Parts per Million.								
	Ammonia.			Albuminoid Ammonia.			Nitric Acid.		
	A.	B.	C.	A.	B.	C.	A.	B.	C.
January . .	9·30	—	—	·20	—	—	·741	—	—
February . .	8	—	—	·30	—	—	·370	—	—
March . . .	13·30	—	—	·22	—	—	·926	—	—
April . . .	18	—	—	·25	—	—	1·112	—	—
May . . .	11	—	—	·25	—	—	·648	—	—
June . . .	6·90	—	—	·15	—	—	·926	—	—
July . . .	8·40	—	—	·30	—	—	1·668	—	—
August . .	4·20	1·50	9·09	·20	·15	·73	·741	1·482	—
September .	5·10	2·70	2·80	·30	·30	·25	·556	1·112	·741
October . .	9	4·50	2·40	·17	·19	·075	·741	0·926	·556
November .	5·20	4·60	2·10	·18	·15	·05	·741	1·853	·741
December .	1·50	7·50	2·90	·07	·15	·15	·556	0·926	·556
Mean . . .	8·32	4·16	3·86	·216	·19	·25	·810	1·260	·648
Mean from August	5	—	—	·18	—	—	·667	—	—

A. All Saints . . . . . 12 ft. above the ground.
B. ,, ... 30 ,, ,, ,,
C. Lit. and Philosophical Society, George Street . 2 ,, ,, ,,

RAIN. TABLE CIII.

MANCHESTER RAIN-WATER, 1869.

PERMANGANATE TEST.

Month.	Parts by Weight. Oxygen required per Million Parts of Rain-Water.					
	Instantly.			In a few Minutes with Acid.		
	A.	B.	C.	A.	B.	C.
September . .	—	—	—	·676	1·950	·910
October . . .	None	·026	None	1·846	2·336	1·301
November . .	·157	·202	·045	1·839	2·444	1·413
December . .	·179	·718	·449	2·061	4·464	3·364
Mean . . .	·112	·315	·165	1·605	2·798	1·747

A. All Saints . . . . . 12 ft. above the ground.
B. ,, ... 30 ,, ,, ,,
C. Lit. and Philosophical Society, George Street . 2 ,, ,, ,,

RAIN. TABLE CIV.

ST. HELEN'S AND RUNCORN RAIN-WATERS, 1869.

ACIDS (FREE AND COMBINED).

Rain obtained from	Date.	Hydrochloric Acid.		Sulphuric Acid (Anhydrous).		Proportion of Hydrochloric to Sulphuric Acid.	Acidity calculated as Sulphuric Acid (Anhydrous).	
		Grains per Gallon.	Parts per Million, or Milligrammes per Litre.	Grains per Gallon.	Parts per Million.		Grains per Gallon.	Parts per Million.
St. Helen's, W. wind	Feb. 18 to March 11	·9291	13·27	3·6750	52·50	1 to 3·95	·0683	·97
,, ,,	,, ,,	1·4599	20·86	1·9600	28	1 ,, 1·34	·4782	6·83
,, ,,	March 11 to April 22	·6387	9·12	2·9400	42	1 ,, 4·60	·4099	5·85
,, ,,	,, ,,	·8517	12·17	1·7294	24·71	1 ,, 2·03	·6833	9·76
,,	April 23	1·0220	14·60	1·8375	26·25	1 ,, 1·79	1·0248	14·64
,, High Street, much E. wind	April 22 to May 20	·4258	6·08	2·6727	38·18	1 ,, 6·27	·2733	3·90
,, ,, in a garden	,, ,,	·4258	6·08	1·8375	26·25	1 ,, 4·31	·2733	3·90
,, ,, ,,	May 20 to August 12	·3006	4·29	2·2615	32·31	1 ,, 7·52	·0683	·98
,, ,, ,,	,, ,,	·8517	12·17	3·6750	52·50	1 ,, 4·31	·1708	2·44
,, ,, ,,	October 2	·2044	2·92	1·4700	21	1 ,, 7·19	None	None
,, ,, ,,	August 12 to October 8	·5678	8·11	2·1000	30	1 ,, 3·70	·1708	2·44
,, ,, ,,	November 4	·5678	8·11	2·2615	32·30	1 ,, 3·98	None	None
,, ,,	,,	·4258	6·08	1·7818	25·45	1 ,, 4·18	,,	,,
,, ,,								
Average		·6670	9·53	2·3232	33·19	1 to 3·48	·2755	3·98
Eccleston, St. Ann's	October 11	·0852	1·22	·4900	7	1 to 5·75	—	—
Runcorn, W. wind	Feb. 25 to March 10	2·5550	36·50	1·9600	28	1 ,, ·76	·4987	7·12
,, near Alkali-Works	April 28 to Sept. 30	1·4600	20·86	1·9600	28	1 ,, 1·34	·8882	12·69
,, ,,	Sept. 25 to October 5	·6387	9·12	1·4700	21	1 ,, 2·30	·6832	9·76
,, ,,	November 9	2·5550	36·50	1·2250	17·50	1 ,, ·48	1·1273	16·10
,, ,,								
Average		1·8022	25·74	1 6537	23·62	1 to ·92	·7993	11·42

RAIN. TABLE CV.

ST. HELEN'S AND RUNCORN RAIN-WATERS, 1869.

'AMMONIA' AND NITRIC ACID.

Rain obtained from	Date.	Parts per Million.		
		Ammonia.	Albuminoid Ammonia.	Nitric Acid.
St. Helen's, W. wind . .	Feb. 18 to March 11.	6	·15	—
,, ,, . .	March 11 to April 22	6	·15	—
,, ,, . .	,, ,, .	4·55	·18	·926
,, ...	April 23 . . .	5·50	·20	—
,, High St., E. wind .	April 22 to May 20 .	5·40	·15	·926
,, ,, ,, .	,, ,, .	3·67	·17	·556
,, ,, ,, .	May 20 to Aug. 12 .	3·60	·70	—
,, ,, . . .	,, ,, .	4·25	·30	6·300
,, ,, . . .	October 2 . .	2·40	·30	·556
,, ,, . . .	Aug. 12 to Oct. 8 .	4	·08	·926
,, ,, . . .	November 4 . .	4·60	·20	·370
,, ,, . . .	,, . .	4·70	·20	·741
Average	. . . .	4·56	·23	1·413
Runcorn, near Alkali-Works .	April 28 to Sept. 30 .	1·50	·30	·371
,, ,, ,, .	Sept. 25 to Oct. 5 .	4·80	·10	—
,, ,, ,, .	November 9 . .	7·60	·17	·185
Average	. . . .	4·63	·19	·278

RAIN. TABLE CVI.

ST. HELEN'S AND RUNCORN RAIN-WATERS, 1869.

PERMANGANATE TEST.

Rain obtained from	Date.	Parts by Weight. Oxygen required per Million Parts Rain Water.	
		Instantly.	In a few Minutes with Acid.
St. Helen's (garden in High St.) .	May 20 to Aug. 12 .	—	4·992
,, High Street . .	,, ,, .	—	2·132
,, ,, . .	Aug. 12 to Oct. 8 .	None	1·233
,, (garden in High St.) .	October 2 . . .	—	·468
,, ,, ,, .	November 4 . .	None	2·557
Average	. . . . .	None	2·276
Runcorn, near Alkali-Works .	April 28 to Sept. 30 .	None	·520
,, ,, ,, .	November 9 . .	,,	1·660
Average	. . . . .	None	1·090

RAIN. TABLE CVII.

RAIN-WATER FROM GERMANY, 1869.

ACIDS.

Rain obtained from	Date.	Hydrochloric Acid.		Sulphuric Acid (Anhydrous).		Proportion of Hydrochloric to Sulphuric Acid.	Acidity calculated as Sulphuric Acid (Anhydrous).	
		Grains per Gallon.	Parts per Million.	Grains per Gallon.	Parts per Million.		Grains per Gallon.	Parts per Million.
Darmstadt (from a roof after rain had fallen some time)	February . . .	·0511	·73	2·9400	42	1 to 57·53	·0923	1·32
Darmstadt, during a thunderstorm .	May 26 . . .	·0511	·73	·7350	10·50	1 „ 14·38	None	None
Darmstadt	November, December .	·1022	1·46	2·4500	35	1 „ 23·97	·2733	3·90
Zwingenberg, near Darmstadt . .	July	·1460	2·08	·2261	3·23	1 „ 1·55	·0341	·48
Heidelberg, Schlossberg . . .	June 15 (collected in a few hours)	·1161	1·66	·1176	1·68	1 „ 1·01	·0341	·48
Giessen, top of Klinik building .	November, December .	·0852	1·22	·4200	6	1 „ 4·93	·1025	1·46
Average		·0919	1·313	1·1481	16·40	[1]1 to 12·49	·0894	1·273
Average of Darmstadt		·0681	·973	2·0417	29·17	[1]1 to 29·98	·1218	1·74

[1] The ratio of the average amounts of acids.

RAIN. TABLE CVIII.
RAIN-WATER FROM GERMANY, 1869.
AMMONIA AND NITRIC ACID.

Rain obtained from	Date.	Parts per Million.		
		Ammonia.	Albuminoid Ammonia.	Nitric Acid.
Darmstadt	February . .	7·75	·300	8·894
„ during a thunderstorm	May 26 . .	1	·075	1·112
„	Nov., Dec. . .	·80	·050	·482
Zwingenberg, near Darmstadt .	July . . .	·85	·150	·370
Heidelberg, Schlossberg . .	June 15 (collected in a few hours)	·40	·087	·370
Giessen, top of Klinik building .	Nov. to Dec. .	·70	·070	6·115
Average		1·91	·122	2·89

RAIN. TABLE CIX.
GERMAN RAIN-WATER, 1869.
PERMANGANATE TEST.

Rain obtained from	Date.	Parts by Weight. Oxygen required per Million Parts Rain.	
		Instantly.	In a few Minutes with Acid.
Darmstadt	November, December.	None	·157
Giessen, top of Klinik building .	„ „ .	„	·247
Average		None	·202

CISTERN-WATER AND OTHER SPECIMENS COMPARED, 1857.

Since specimens have been given of rain from London collected without long exposure, 1869, and after long exposure, 1870, the latter in some respects approaching cistern rain-water, although not reaching it, we may with propriety look here at rain from various parts of Manchester; it will show how much one part of a town may differ from another.

Acidity of Rain collected in Porcelain Vessels, 1857.

Rain obtained from	Acidity calculated as Sulphuric Acid (Anhydrous).	
	Grains per Gallon.	Grammes per Litre.
Pendleton, NW. District of Manchester:		
Wind blowing on the town	·3169	·0045
Not directly from the town	·8453	·0120
Wind touching outskirts of the town	·8981	·0128
From town	1·9018	·0271
From country	—	—
All Saints:		
At All Saints' Church	1·2679	·0181
" "	2·0075	·0286
For a short time	7·3960	·1056
Then fell to	2·1132	·0301
Next to	2·1132	·0301
And lastly	·8453	·0120
Ancoats:		
First Sample	2·3245	·0332
Second "	1·0566	·0150
Third "	3·0112	·0430
Average	2·0075	·0286
Maximum	7·3960	·1056
Minimum	·3169	·0045

Rain.—Cisterns in Pendleton, Manchester, 1857.

Rain obtained from	Acidity calculated as Sulphuric Acid (Anhydrous).	
	Grains per Gallon.	Grammes per Litre.
Cisterns in Pendleton:		
No. 1	·3169	·0045
„ 2	·2641	·0037
„ 3	·3169	·0045
„ 4	·5285	·0075
„ 5	2·0075	·0286
„ 6	3·3810	·0483
„ 7	2·4304	·0347
„ 8	2·1132	·0301
„ 9	·6335	·0090
„ 10	·7392	·0105
Average	1·2731	·0181
Maximum	3·3810	·0483
Minimum	·2641	·0037

Rain.—Lower Broughton, Manchester, 1857.

Rain obtained from	Acidity calculated as Sulphuric Acid (Anhydrous).	
	Grains per Gallon.	Grammes per Litre.
Cisterns in Lower Broughton:		
No. 1	·4228	·0060
„ 2	1·8130	·0259
„ 3	1·3734	·0196
„ 4	·6335	·0090
„ 5	1·1095	·0158
„ 6	1·2152	·0173
„ 7	1·5316	·0218
Average	1·1570	·0165
Maximum	1·8130	·0259
Minimum	·4228	·0060

Rain.—Hulme, Manchester, 1857.

Rain obtained from	Acidity calculated as Sulphuric Acid (Anhydrous).	
	Grains per Gallon.	Grammes per Litre.
Cisterns in Hulme:		
No. 1	1·6373	·0233
„ 2	1·3734	·0196
„ 3	1·6905	·0241
„ 4	1·7962	·0256
„ 5	2·5886	·0369
„ 6	1·4259	·0203
„ 7	1·8130	·0259
„ 8	1·5316	·0218
„ 9	1·9018	·0271
„ 10	1·4791	·0211
„ 11	2·4879	·0354
„ 12	1·7962	·0256
„ 13	2·1658	·0309
Average	1·8221	·0260
Maximum	2·5886	·0369
Minimum	1·3734	·0196

Rain.—Salford, Manchester, 1857.

Rain obtained from	Acidity calculated as Sulphuric Acid (Anhydrous).	
	Grains per Gallon.	Grammes per Litre.
Cisterns in Salford:		
No. 1	·5285	·0075
„ 2	·3169	·0045
„ 3	2·6418	·0377
„ 4	—	—
„ 5	2·6943	·0384
„ 6	·3169	·0045
„ 7	1·3734	·0196
„ 8	1·1095	·0158
„ 9	1·1620	·0166
„ 10	1·4791	·0211
„ 11	1·0566	·0150
„ 12	1·1620	·0166
„ 13	1·2152	·0173
„ 14	·5285	·0075
Average	1·1988	·0171
Maximum	2·6943	·0384
Minimum	·3169	·0045

Rain.—Ardwick, Manchester, 1857.

Rain obtained from	Acidity calculated as Sulphuric Acid (Anhydrous).	
	Grains per Gallon.	Grammes per Litre.
Cisterns in Ardwick:		
No. 1	1·2679	·0181
„ 2	2·1132	·0301
„ 3	1·2679	·0181
„ 4	2·3772	·0339
Average	1·7565	·0251
Maximum	2·3772	·0339
Minimum	1·2679	·0181

Rain.—Ancoats, Manchester, 1857.

Rain obtained from	Acidity calculated as Sulphuric Acid (Anhydrous).	
	Grains per Gallon.	Grammes per Litre.
Cisterns in Ancoats:		
No. 1	2·1658	·0309
„ 2	1·4791	·0211
„ 3	3·1696	·0452
„ 4	2·1132	·0301
„ 5	1·2679	·0181
„ 6	1·4791	·0211
„ 7	8·5924	·0513
Average	2·1810	·0311
Maximum	3·5924	·0513
Minimum	1·2679	·0181

Rain.—Variations according with the presence of Chimneys, 1857.

Rain obtained from	Acidity calculated as Sulphuric Acid (Anhydrous).	
	Grains per Gallon.	Grammes per Litre.
Cistern, No. 1	1·2679	·0181
„ „ 2	1·1095	·0158
„ „ 3	—	—
„ „ 4	1·4259	·0203
„ „ 5	—	—
„ „ 6	2·0601	·0294
„ „ 7	—	—
„ „ 8	·7924	·0113
„ „ 9	1·4791	·0211
Average	1·3558	·0194
Maximum	2·0601	·0294
Minimum	·7924	·0113
Average for the whole	1·5976	·0228

No. 1.

Rain obtained from	Hydrochloric Acid.		Sulphuric Acid (Anhydrous).		Total Acid.		Proportion of Hydrochloric to Sulphuric Acid.
	Grains per Gallon.	Grammes per Litre.	Grains per Gallon.	Grammes per Litre	Grains per Gallon.	Grammes per Litre.	
Cavendish Street, Manchester, June 8, 1851	·3976	·00568	1·0752	·01536	1·4728	·02104	1 to 2·70
Cavendish Street, Manchester, June 9, 1851	·5300	·00757	1·0752	·01536	1·6052	·02293	1 „ 2·03
Moss-side, just outside town, June 9, 1851	·8960	·01280	·8960	·01280	1·7920	·02560	1 „ 1

Rain.—Manchester, 1868.

Month.	Hydrochloric Acid.		Sulphuric Acid (Anhydrous).		Total Acid.		Proportion of Hydrochloric to Sulphuric Acid.
	Grains per Gallon.	Grammes per Litre.	Grains per Gallon.	Grammes per Litre.	Grains per Gallon.	Grammes per Litre.	
January	·9002	·01286	4·0110	·0573	4·9112	·07016	1 to 4·45
February	·9790	·01398	3·5224	·0503	4·5014	·06428	1 „ 3·59
March	1·2775	·01825	3·1108	·0444	4·3883	·06265	1 „ 2·43
April	·8516	·01216	5·6000	·0800	6·4516	·09216	1 „ 6·57
May	·8516	·01216	5·6000	·0800	6·4516	·09216	1 „ 6·57
June	—	—	—	—	—	—	—
July	·15096	·00216	1·4000	·0200	1·55096	·02216	1 to 9·27
August	·02783	·00039	1·8730	·0268	1·90083	·02719	1 „ 67·30
September	·73000	·01043	2·8000	·0400	3·53000	·05043	1 „ 3·83
October	·76650	·01095	2·2400	·0320	3·00650	·04295	1 „ 2·92
November	·51100	·00730	2·8000	·0400	3·31100	·04730	1 „ 5·48
December							

AIR.

SOLID BODIES IN THE AIR.

THAT solid bodies are found in the air has at all known times been a part of the commonest information. Their existence has even been extended to all space, and worlds themselves have been theoretically made out of them. That they are an important part of the economy of nature we need not doubt; they are partly inorganic, partly organic, and partly organised. These are found to a hurtful excess in some atmospheres, and in others they exist, we cannot doubt, in exactly the most useful amount. We have already seen that salts of soda and ammonia are found in the air. It is extremely probable that these are as solid particles; indeed, we may feel certain. The beautiful discovery of Professor Tyndall, that the solid bodies affect or cause the distribution of light, is one of the many uses to which these still mysterious agents have been applied. When these bodies are organic they plant their own peculiar vegetation or animal life over the earth, and affect men and animals sometimes for evil, producing, as we cannot doubt, various diseases. We shall probably learn that their usual effect is for good. As we rise high in the air on the mountain sides they diminish in quantity, showing that they gravitate like other bodies; this was proved by Pasteur of organisms, and a friend of mine tells me that the animals on the high Alps are peculiarly attracted by salt, as if the height prevented the usual supply in the rains. One of the uses of storms seems to be to supply the world with salt.

The history of this subject is a long one; it would take us into early Greece and drag us slowly to Rome, and through many dark portions of the Middle Ages. It

would even keep us wandering over all Asia and on towards the poles in the eastern and western worlds. It must not be written here, but I cannot forbear giving the picture drawn by Lucretius; it is wonderfully vivid. In another part will be given a quotation from Terentius Varro. Lucretius may be taken as a clear exponent of the existence of solid bodies in the air, and Varro of the belief in the organised character of some of them. For the present I shall be content with showing how old are the ideas.

From the invisible atoms of Leucippus to the motes in the sun we have abundant mention of bodies in the air.[1] Lucretius says :—

Observe whenever the light of the sun pours its rays into the dark parts of houses; you will see in the very light of the rays many minute bodies in the empty space, mingled together in many ways, and, as it were, in perpetual conflict; battles without pause, with many meetings and separatings, so that you may conjecture from this spectacle what is the quality of the tossing undergone by the primary particles of things in the great void. A small thing may serve as an example of great things, as the footsteps of knowledge. On this account it is more proper that you should give your attention to those bodies which seem to crowd confusedly in the sun's rays, because such confusions point to secret and unseen movements of matter. For you will see many struck with unseen forces, changing their course, driven backwards, now to this and now to that, and finally in all directions. Doubtless this erratic motion arises from the primordial *particles*, for the first Primordials are moved of themselves.—A little altered from the *Translation by Rev. John Tilly Watson.*[2] Book II., lines 113–131.

[1] See 'Organic Matter' and 'Marshes.'

[2] Contemplator enim cum solis lumina cunque
Insertim fundunt radios per opaca domorum;
Multa minuta modis multis per Inane videbis
Corpora misceri radiorum lumine in ipso;
Et velut eterno certamine prœlia, pugnasque
Edere turmatim certantia; nec dare pausam,

CRYSTALS FROM RAIN.

If the rain is found to contain salts with solid bases, we may expect to find them conveniently by boiling down. This plan was adopted in many cases, and a few examples are given. I do not say that we can tell all the crystals that are to be seen, but sulphates and chlorides are striking. In the rain of Uist, in the Hebrides, we see the chloride of sodium (common salt) almost alone; it stands out clear and distinct, and the crystals are larger than in the specimens from other places.

In the rain of Manchester there is a confusion of crystals, because of the great number; although sulphates are prominent, I do not see crystals of common salt. It would appear that a decomposition takes place; chloride of sodium is decomposed by sulphate of ammonium, and the result is sulphate of soda and chloride of ammonium. This we expect to take place when the salts are boiled down, but it also takes place on spontaneous evaporation, and perhaps also in the air; when sulphuric acid passes into the atmosphere in towns it probably seizes part of the sodium of the common salt in the same way, and so liberates hydrochloric acid—an alkali-work continually in action. We should then have that acid free in towns, arising simply from the combustion of coals, and not free sulphur acids only. Experience leads us to suppose

Conciliis, et discidiis exercita crebris;
Conjicere ut possis ex hoc, Primordia rerum,
Quale sit in magno jactari semper Inani;
Duntaxat rerum magnarum parva potest res
Exemplare dare, et vestigia notitiaï.
Hoc etiam magis hæc animum te advertere par est
Corpora quæ in solis radiis turbare videntur:
Quod tales turbæ motus quoque materiaï
Significant clandestinos, cæcosque subesse.
Multa videbis enim plagis ibi percita cæcis
Commutare viam, retroque repulsa reverti
Nunc huc, nunc illuc, in cunctas denique partes;
Scilicet hic a Principiis est omnibus error;
Prima moventur enim per se Primordia rerum.

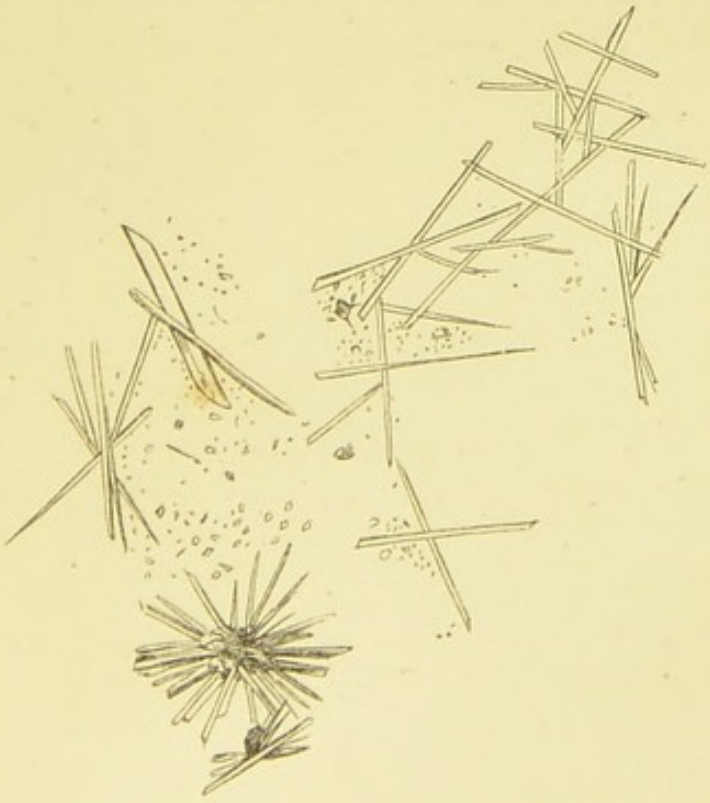
RAIN WATER CRYSTALS.

1



Manchester Rain.

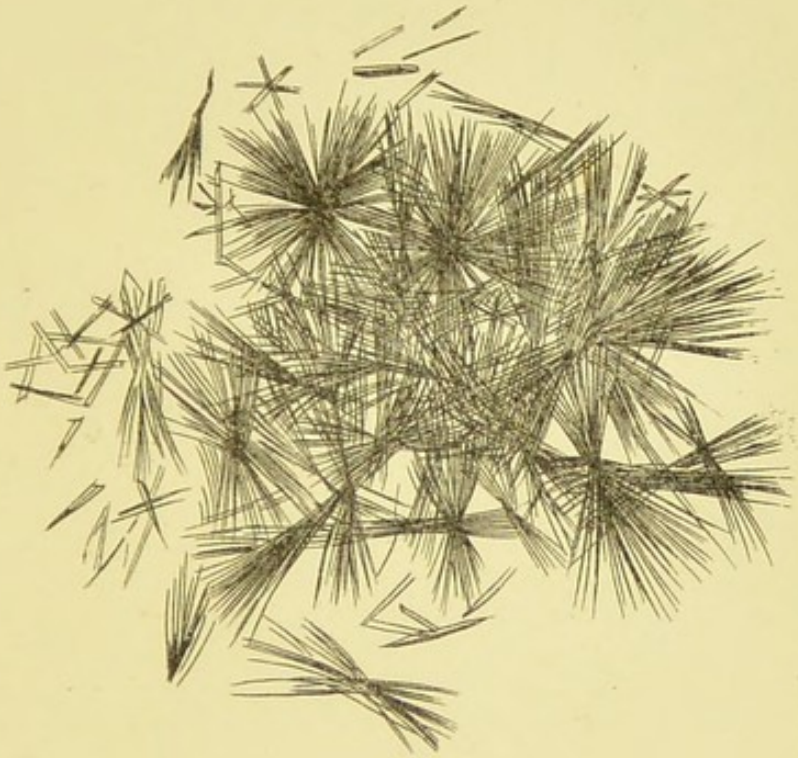
2



Manchester Rain. Fixed residue, oxide of iron removed.

Hanhart. lith.

3



London Rain – Fixed Residue.
London Hospital.

4



Sublimate from London Rain.

Hanhart lith.

5



Newcastle Rain.

6



Row Rain, Gareloch, Clyde.

Hanhart, lith.

this possible, although we know that, for complete decomposition of the common salt, considerable heat is required or excess of acid. Hydrochloric acid is more destructive to mineral substances—silicious stone, for example—than sulphuric, as I believe, and we have here a mode of forming it.

The Manchester rain has brown masses in it, consisting of oxide of iron in part and dust, consisting of coal-ashes, besides other substances which are organic (Fig. 1); when heated, so that the volatile matter is driven off, the remainder, soluble in water, deposits the crystals (Figs. 1 and 2).

I do not know why the London rain should have presented such a mass of crystals; they are more uniform in character than in the Manchester specimen, but less mixed with dust and rubbish; the large brown flakes of oxide of iron are absent. A sublimate from the London as well as the Manchester rain was found to contain muriate of ammonia. (See Figs. 3 and 4.)

The rain from Newcastle-on-Tyne has a character of its own, and consists of many small and beautifully defined crystals of sulphate of soda (Fig. 5). Although that town is so near the sea, I am not sure that I observe any common salt (Fig. 5).

The crystals from Row are mixed, being partly sulphate (Fig. 6). One of the proofs of a pure air seems, then, to be in the proportional amount of crystals of common salt. The nearer it is to the amount in ocean water, the less is the air tainted with decomposition. There must, of course, be no artificial source of common salt to mislead us.

Gernez has found sulphate of soda in the rain in Germany.

To obtain these specimens 200 cubic centimeters (about seven ounces) of rain-water were boiled down to the bulk of about one. One drop of this concentrated water was allowed to evaporate spontaneously on the

slide for the microscope. Although boiled down to the same amount of liquid, they would not be of the same amount of concentration. This fact would cause the crystallisation to be different and lead to another characteristic of the rains of towns. We can readily point out several classes, and it may happen that this will be a very convenient, and therefore favourite, way of showing the condition of a place. In cases of trials for nuisances, as well as general sanitary purposes, the crystals may be brought forward micrographically taken and magnified for printing. They could accompany a chemical examination as well as the general microscopic examination of the substances in the rain before boiling down. These, of course, may be considerably affected by the boiling when they are organic in origin, and by the concentration when they are purely inorganic.

ORGANIC MATTER IN THE AIR.

Organic matter has been long known to exist in the air, but it has been so little examined till of late that its history is not a very complex one. The part that has excited most attention is the existence of organised forms, and these again have been interesting, partly because their existence raised the question of spontaneous generation. On another account, however, they have not been without attention, because of the practical questions regarding the conveyance of disease and the commencement of putrefaction. It is only within a period equal to a human life that we have begun to picture to ourselves with any aid from science what is the structure of organic matter. Perhaps Anaxagoras, who thought that every substance was made up of particles of its own kind, believed that when wood

burned and disappeared into smoke it rose up into the air in the form of innumerable small particles, which still retained the character of wood; when a piece of zinc is raised into vapour each particle is still zinc, although when it takes fire a new substance is formed—oxide of zinc. When water rises up in vapour each atom, or rather molecule, is still water. But organised bodies are formed of much more complex material; they are destroyed by heat, and the complex molecule is broken up into others of a simpler kind, and no doctrine of homoiomereia can be carried out so far. Whole forests may burn, but not a particle of wood will of necessity be found in the air; we have learnt that nature has a strange power of combining a few elements in such variety of methods that thousands of substances are produced. It is perhaps the real secret half seen by alchemists showing abundant transmutations in the organic world, although we have failed to see analogous action with the metals themselves. It required little observation to find that organic substances pass into the air in some sense; we see them on the ground, and in a short time they disappear. How can we question their presence in the atmosphere? And yet men have come to it slowly, the more thoughtful excepted. Pestilential vapours in the air are expressions old and frequent, and the existence of fungi along with them, or signacula blood-spots and mould-spots, will not be a new subject to those who have read Bascombe's 'Epidemic Pestilences' and the careful work of Hecker, or even much more popular works. I have several times in books and papers quoted Bishop Berkeley's works as expressing in essence the most advanced opinions on the air.

The words in Siris are (par. 140):—

'Nothing ferments, vegetates, or putrefies without air, which operates with all the virtues of the bodies included in it—that is, of all nature. . . . The air, therefore,

is an active mass of numberless different principles, the general sources of corruption and generation; on the one hand dividing, abrading, and carrying off the particles of bodies—that is, corrupting or dissolving them—on the other producing new ones into being, destroying and bestowing forms without intermission.'

And in paragraph 141 he says:—

'The seeds of things seem to be latent in the air, ready to pair and produce their kind whenever they light on a proper matrix. The extremely small seeds of ferns, mosses, mushrooms, and some other plants are concealed and wafted about in the air, every part whereof seems replete with seeds of one kind or other. The whole atmosphere seems alive. There is everywhere acid to corrode and seed to engender. Iron will rust and mould will grow in all places.'

Having seen this idea expressed in its greatest breadth by one who thought carefully, but without experiment, it would be well to make a little addition by showing the results obtained by men of a very different habit, who preferred to limit their thoughts by their own tests.

The tendency of enquiry in modern times has been to establish a very ancient belief, that decomposing substances, animal and vegetable, produce disease, and are ultimately connected with infection and contagion. This result may be said to be the summary of all that has been done by the General Board of Health and Sanitary Commissioners of late years. It is true that some persons object to these conclusions, and there are even many who have attempted to prove that the gases from decomposing substances are beneficial to health. The fancies or superstitions of the population have somewhat assisted this latter retrograde idea, and we find some delighting in putrid canals and rivers, others admiring the gases from brick-kilns, and others again those from accumulations of farmyard manure. The pressure of

experience, however, is so strong that these ideas will probably soon vanish. It has often been asked, Will a sewer produce cholera, or plague, or cattle disease? We cannot say so, or that every kind of disease may be produced from such accumulations of organic matter. A few centuries back we, perhaps, had arrived at such progress in filthiness of habits that we attained the dignity of producing epidemics amongst ourselves. The great epidemics that have passed in modern times over Europe seem, however, to have come from some extraneous source, to act as if planted by seed, and not to have risen up spontaneously here. Without attempting to examine this matter carefully the result would seem to be, that whilst the decomposition of organised beings after death produces gases and vapours that are opposed to health, these gases or vapours are incapable of originating, although they may be capable of feeding, those diseases, such as cholera or plague, which have been observed at all times to come from a warmer climate. There must, however, be some first origin of these diseases, and we cannot prove that the first origin might not take place in our climate, although it seems probable that it requires a warmer sun and perhaps a long period of development besides. This, however, is sufficiently made out—that, when these diseases do come amongst us, they take root with most effect in those places where decomposing matter is found. If we were to suppose one of the seeds planted in a rich, fertile soil of decomposing matter, we should have a pretty fair description of the fostering effect of impurity on disease. It would, in fact, appear as if the putrid matter itself took the disease and transferred it to the living. There seems to be nothing entirely opposed to this view of the case. The question, however, is, and has always been, What is the nature of that substance which may be said to form the beginning of the disease? Chemists have been inclined to consider

it a substance in process of decay, as the quotation from Liebig elsewhere given shows. Physiologists and microscopists have been more inclined to consider it as an organised substance. When Gay-Lussac passed a bubble of air into the juice of grapes, and found that fermentation began at once, it was believed that the oxygen was the prime mover, and that, when once begun, the action did not cease. When, however, Dusch and Schroeder found that flesh did not decompose if the air were previously passed through a good filter of cotton wool, some difficulty was thrown on the subject. It would appear as if oxygen were not the only agent in the atmosphere causing decomposition. The investigations of M. Pasteur, who found the subject in this uncertain condition, have advanced it so far that we may now with certainty reason in the belief that not only are organised substances really found in great abundance in the atmosphere, but that they are the cause of some hitherto entirely mysterious phenomena, putrefaction included. His object was first to enquire into the possibility of spontaneous generation, and he found that carefully filtered air allowed no organisms to appear in vegetable solutions. He found that near the usual surface of the ground these organisms were so numerous that, whenever a vessel containing vegetable matter fit for their growth was opened for a very short time, they were found to enter, that in cellars and damp and quiet places, where there was no air or dust floating about, these organisms were fewer, and that, as he ascended the sides of the Alps and the Jura, they diminished in number. A commission of the French Academy confirmed his results. If we examine previous enquiries into the compounds resulting from the decomposition of organic substances, we shall find nothing which is at all calculated to bring out such an intelligible rational view of the origin of many diseases, and also of some phases of putrefaction. Chemists, when they have

examined products of the latter action, have found sulphuretted hydrogen, carburetted hydrogen, hydrogen, carbonic acid, nitrogen, ammonia, acetic acid, lactic acid, butyric acid, and numerous uncertain bodies having no activity, and, to all appearance, utterly incapable of producing those prodigious results that are found when that force begins to work which produces plague, small-pox, or black death.

Sulphuretted hydrogen, itself one of the most deadly of gases, has not been known to produce instantly any disease. It cannot, in fact, be said in any way to be similar to an infection. It may lower the tone of health, and it does so, and it may gradually diminish vitality to such an extent that disease ensues; or, if it is in very great quantity, it may kill as suddenly as a cannon shot; but it never can for a moment be compared with the infectious matter of smallpox or of scarlet fever. Chemists much in a laboratory are exposed to it daily; sometimes in considerable quantities, and the result has been as stated. The same may be said more or less of all substances found by chemists as the result of putrefying matter. It is not here asserted that these gases are not unwholesome, for their unwholesomeness is more or less, beginning with sulphuretted hydrogen, which is already said to be so deadly; but it is extremely important to distinguish them from those hitherto and still mysterious substances which convey infection. The question is quite open. Is the cause of disease an organic substance in the process of decomposition conveying that decomposition to another body, or is it an organised germ? [Of the substances found to be the products of putrefaction and passing into the air, only a very small part seems to be active—that is, in a state of change. It may be collected by alkalies, and also by porous bodies. Gases and acids are not of themselves bodies in active chemical change, neither are they organised.]

The two great theories may be called Liebig's and Pasteur's; the first, Liebig's, dealing with organic decomposing matter ready to communicate its action by its own activity. That this idea has a sound scientific basis will to a great extent be admitted. I am disposed to think it quite undeniable at present, but not as an explanation of putrefaction or fermentation. The second, that of Pasteur's, leads to organised bodies or germs, and, although he is also in the position of not having originated the idea, the clearest proof and expression is due to him. He does seem to have retained firm hold of a part of the battle-ground gained from chemistry. There is probably a point where the organic and organised touch so nearly as to be difficult to distinguish; but here the distinction between the two is very real, and the point of contact is still to be sought.[1]

Having observed that the breath contained organic matter, and that the emanations from crowds of persons were found, when condensed on the walls, to contain large quantities, I published the somewhat rude experiments in the 'Memoirs of the Chemical Society' for 1846. The experiments were considered new, but I think that, if the previous works of chemists were examined, it would be found that the spirit at any rate was not new. 'The[2] air has often been called a general receptacle for all impurity; nature has made it a universal purifier by giving it so large an amount of free oxygen. It is oxygen which purifies, and bodies which are impure have a tendency to oxidise, after which they become pure. No doubt the air of a town contains a portion of all exhalations which arise in a town. These are such as come from living bodies in the first instance—exhalations which can never be got rid of, but which it is probable are not at all dangerous unless accumulated.

[1] 'Report to the Royal Commission on the Cattle Plague.'

[2] From the 'Reports of the British Association,' 1848, p. 17.

. . . 'That animals constantly give out a quantity of solid organic matter from the lungs may readily be proved by breathing through a tube into a bottle, when the liquid, or condensed breath, will be collected at the bottom of the bottle; or by breathing through a tube into water, when a solution of the same substance will be found in the water. This would scarcely require proof if we considered that breath so frequently has an organic smell; perhaps, rather, it always has an organic smell, and when it is bad the smell is often offensive, containing decomposing organic matter. If this condensed breath be put on a piece of platinum, or on a piece of white porcelain, and burnt, the charcoal which remains and the smell of organic matter will be conclusive. If it be allowed to stand for a few days (about a week is enough), it will then show itself more decidedly by becoming the abode of small animals. These are rather to be styled animalcules, and very small ones certainly, unless a considerable quantity of liquid be obtained; they may be seen with a good microscope. Animalcules are now generally believed to come from the atmosphere, and to deposit themselves on convenient feeding-places—that is, they only appear where there is food or materials for their growth—and they prove, of course, the existence of that continuation of elements necessary for organic life. At the same time their presence is a proof of decomposing matter, as their production is one of the various ways in which organised structure may be broken up. Such a liquid must, of course, be an injurious substance, giving out constantly vapours of an unwholesome kind.

'I mentioned some time ago that I had got a quantity of organic matter from the windows of a crowded room, and I have since frequently repeated the experiment. This matter condenses on the glass and walls in cold weather, and may be taken up by means of a pipette,

If allowed to stand some time, it forms a thick, apparently glutinous mass; but, when this is examined by a microscope, it is seen to be a closely-matted confervoid growth, or, in other words, the organic matter is converted into confervæ, as it probably would have been converted into any kind of vegetation that happened to take root. Between the stalks of these confervæ are to be seen a number of greenish globules constantly moving about, various species of Volvox accompanied also by monads many times smaller. When this happens the scene is certainly lively and the sight beautiful; but, before this occurs, the odour of perspiration may be distinctly perceived, especially if the vessel containing the liquid be placed in boiling water.

'If air be passed through water, a certain amount of this material is obtained, but I have found it difficult to pass a sufficient quantity through. If it is made to pass rapidly, absorption does not take place, and evaporation of the water is the consequence; if it passes slowly, it requires many weeks to pass a hundred cubic feet through a small quantity of water. I continued the experiment for three months, but although I obtained sulphuric acid, chlorine, and a substance resembling impure albumin, I did not get enough to make a complete examination; and, indeed, this could not be expected, as I found that in that time less than a thousand gallons of air had passed through.

'When this exhalation from animals is condensed on a cold body, it in course of time dries up, and leaves a somewhat glutinous organic plaster; we often see a substance of this nature on the furniture of dirty houses, and in this case there is always a disagreeable smell perceptible. I have no doubt that this is a great cause of the necessity for constant cleaning, which experience has found and made to be a very general practice in England and elsewhere. In other words, it is a reason

why that which is not cleaned becomes dirty, a question which I have often felt great difficulty in answering.

'Water is necessary to the spontaneous decomposition of animal matter, and it is probable that in a warm climate this coating of walls and furniture would not be so dangerous as with us, where everything is exposed to moisture a considerable part of the year. In a warmer climate it will probably be diffused more into the atmosphere, and not be so much retained as it is by the moisture which dissolves it or to which it attaches itself. It will probably be found that this substance is not poisonous if taken into the stomach, but it is known to be poison breathed into the lungs, as we know the air of crowded rooms to be. The quantity is small that we do breathe; but, at the same time, we must remember that it is diffused in air, and has therefore a surface as extended as the volume of the air in all probability; and we know that a cubic inch of sulphuretted hydrogen will scent at least some hundred cubic feet of air.

'As this substance of which we speak is organic and contains carbon, hydrogen, and nitrogen, with other elements, it is capable of oxidation, and it no doubt is continually undergoing oxidation in the air, probably forming carbonic acid, water, and ammonia It is also not unlikely that this is a greater source of the ammonia of the atmosphere than the mere fœtid decomposition of animal matters, which does not occur to a large extent in nature, provision being made for its removal by animals, and by vegetation especially.

'Organic matter in contact with water constantly gives off an odour of some kind, and especially if heated, so that it would appear as if steam or vapour were capable of taking up much more than that which we call volatile matter.

'If organic matter be allowed to decompose in the air, it gives out carbonic acid, ammonia, sulphuretted hydrogen, and probably other gases. Priestley has

shown that, if it decomposes in water, it gives out an inflammable gas. If, however, it be exposed to the action of soil, other circumstances being favourable, it is converted partly into nitric acid.

'None of these cases occur purely in our towns, but all of them occur to some extent. Carbonic acid and ammonia occur in all reservoirs of refuse, and sulphuretted hydrogen occurs also in abundance. It was once very perceptible in London, as Sir Kenelm Digby complains much of the state of the streets, when silver could not be kept clean in his day. This may be observed now in many towns, and is, in fact, not uncommon. This is a disagreeable smelling gas, and, wherever it is abundant, will be easily detected by the nose. It may be detected readily in many courts and alleys, also at the mouths of sewers, and in some parts of the Irwell and Medlock at Manchester, where they are filled with organic matter and alkaline and earthy salts. Ammonia generally accompanies it, so as to diminish its bad effects.

'Ammonia itself is probably of no injury unless in excessive quantities, and may be considered as one of the most wholesome forms in which nitrogen and hydrogen, as gases, pass into the air. A decomposition such as this occurs ordinarily in towns, as there is a certain exposure to air always.

'In cases where there is no exposure, or at least when the substance is in water, inflammable gases are produced, as Priestley has shown and Liebig (I might say fully) explained.

'It would seem as if, when decomposition commenced, oxidation of one portion necessarily took place, leaving the other portions without oxygen, unless in cases where an abundance could be obtained. Dalton found the gas from the floating island at Derwentwater to contain carburetted hydrogen and nitrogen. The carbon and the hydrogen are deprived of oxygen entirely, whilst more

oxidised bodies, as carbonic acid and humus, are left, the latter body to be in time entirely oxidised, as Liebig has shown. Whether the nitrogen comes off alone or as ammonia, the same division of a substance into oxidised and deoxidised occurs, as we see in the fermentation of sugar, where carbonic acid, a body oxidised, and alcohol, a body to a great extent deoxidised, occur. We have only to suppose compounds of carbon, hydrogen, and nitrogen coming from decomposing matter, to show us the great danger. It is not to be trusted that these bodies always appear in the mode of combination mentioned here; their modes of combining are various, and these elements form the most active poisons known to us.

'A certain amount of moisture is almost essential to the escape of odour from many bodies. It probably arises from two causes. The vapour of water is a vehicle for organic matter, and water favours decomposition in bodies, so that as they decompose the vapour is given out. From whatever cause, it will be found that moisture rapidly facilitates the escape of odour. Mineralogists avail themselves of this when they breathe on a mineral and then ascertain the smell. The moisture of an evening, or even artificial moisture, causes the flowers to give their scents, and the moist state of the atmosphere before or after a shower causes also a great fragrance in a flower garden. But, whilst this is caused, the same laws are operating for injurious effects wherever there is a reservoir of putrid matter; for then the exhalations are also abundant, and bubbles may be seen to rise from filthy water. It is not improbable that the state of the atmospheric pressure may cause this, as Mr. E. W. Binney has shown that the gases in coal-pits are caused to escape rapidly during a lowering of the barometer. Bodies that are moist will therefore give out more organic vapours; if there be abundance of water, as in a lake, the vapours would to a great extent be dis-

solved, even if the same kind of decomposition were to proceed as in merely moist or marshy ground. We might expect then that soil, if moist, will give out, not pure vapour of water, but water with organic matter in it. Wet soil is a little acid generally, and if very acid is bad land—sour, as it is called—but if made alkaline either by the direct adding of ammonia or by decomposition producing ammonia, it becomes fertile. If any alkali be added, which gives out ammonia by decomposing the humate of ammonia in the ground, the same state of fertility is attained. This end is generally attained by adding lime. This state of almost neutrality of the soil is also regulated by nature, and a fertile alkalinity is obtained by the rapid decomposition of organic matter through moisture and heat. In this alkaline and warm state more vapours will of course be given off, and the ammonia will assist in the removing of organic matter into the air. How far this occurs on sowed land has not yet been seen by me satisfactorily; but on peat land the ammonia formed is abundant in hot weather, so much so as to be perceptible directly by the senses, (?) and to take with it in solution a large quantity of humus and salts of humus, containing food for plants, as I showed in a paper to the Philosophical Society of Manchester. I mention this to show how organic matter may be lifted into the air, and why hot weather promotes it; also I wish to show how various this matter must be in its properties, as all vegetable solutions give out a certain amount of matter from them. To ascertain if organic matter were really to be got from such vapours from land, I collected some dew by condensing it on a glass cylinder, and allowing it to drop into a glass below. The fewness of the evenings favourable for the purpose this year has of course retarded me. I saw plainly, however, that the substance thus obtained from the dew was very different from that obtained by condensation in

a warm room; whereas that from a crowded room was thick, oily, and smelling of perspiration, capable of decomposition and productive of animalcules and confervæ; the dew was beautifully clear and limpid. When boiled down the odour was not disagreeable, and I may say not remarkable; but, when the small portion of solid matter which remained dissolved in it was exposed to heat, the smell was that of vegetable matter with very little trace of any nitrogenised substance. It was also rather agreeable than otherwise. The dew was collected in a flower-garden, and I have no doubt, in favourable weather, of being able, in dissimilar situations, to get it of different characters. It is not improbable that the matter in the dew may be a measure of the amount in the atmosphere; if so, the decided difference between that of the country and that of crowded rooms is to be remarked, and may probably form a good guide towards a knowledge of comparative purity of atmosphere.

'In walking along the fields on an evening, when there is much dew, it may be observed how much effect a dry soil has; indeed, I might almost say the climate of a field will be found to vary almost every yard. Every cause of cold, the formation of a drain, the lowness of any spot, its being higher or more level or more sheltered, is indicated by this delicate thermometer, the rise of vapour and the perception of cold. If we ascend higher, the same is seen on a larger scale, on miles instead of yards. A house may be in a clear atmosphere and the lawn before it in an impenetrable fog. One foot in height makes a difference, and one foot also of level distance, if the ground should differ in quality. The damper places give us a feeling of freshness, and cause also a slight irritation of the nose. Every wall causes a certain amount of dampness, and even in a windy day a leafless hedge will protect one side from evaporation. In these respects, therefore, we may say truly, that every

field or house in the country, as well as, I believe, every house in the town, has its own peculiar climate.

. . . . . .

'But the most curious point is the fact that organic matter is never absent, although the rain be continued for whole days. This matter is capable of promoting animalcular life to some extent, and small specimens may be seen moving solitary in it. If allowed to stand in a bottle, this may be more clearly detected. On this matter I must say more at a future time.

'My chief wish is to show that the general notions entertained by persons as to the air of towns are not without the support of what is called scientific observation, although at the same time the effects on life are greater than chemists by any observations could have made out.

'Vogel and others have found organic matter in the atmosphere; and Dr. Southwood Smith, in looking for matter which might produce fever, found an organic substance, I believe, in some of the streets of London. I give in detail only what I have myself observed.

'If this matter should, from any cause, be exposed to a decomposition more rapid than usual, we have before us a state of things worse, because more general, than a bad sewer, and which can account for many diseases. I am therefore disposed to think of it as Lord Bacon thought of the cause of jail-fevers. "Out of question such smells consist of man's flesh or sweat putrefied. There may be great danger of such compositions in great meetings of people within houses, for poisoning of air is no less dangerous than poisoning of water. And these empoisonments of air are more dangerous in meetings of people, because the much breath of people doth further the reception of the infection."—Bacon's *Sylva Silvarum*.'

EXPERIMENTS TO FIND THE AMOUNT OF ORGANIC MATTER IN THE AIR.

I shall begin with my first method of determining the amount of oxidisable matter in the air, not concealing the fact that it has not been found sufficiently practical, and not at all forgetting that Mr. Condy also used permanganate. That gentleman has done so much with the salt, that I do not doubt that he acted quite independently. I can take little credit at best, as my work was merely an application, and I am quite willing to do without any, preferring to give it to Forchammer, who first made use of the deoxidising power of permanganate. Now this property has become one of the most useful in the daily work of the laboratory for various purposes. The method for the air turned out to be correct, but its mode of application never pleased me much. The difficulty was mechanical; it is not easy to cause a measured quantity of air to come in full contact with a certain amount of liquid. I gave up using it, but now have the pleasure of returning to it with more favour, by making a few improvements. Some day it will be further improved. The following description is taken from the 'Report on the Air of Mines,' 1864.

Permanganate Test.

The organic matter has been a subject of great search, and it has escaped the observation generally. Vogel and Boussingault chiefly have laboured on the subject, and proved the existence of dissolved matter in crowded rooms and in unwholesome swamps. Some years ago I published some enquiries which excited great interest. It was desired that the process recommended at the same time should become so easy that everyone could use it

as easily as a thermometer. I have not been able to reduce it to that simplicity for all cases, but shall here endeavour to show exactly how far the plan is capable of popular use.

Forchammer first used permanganate of potash to test the organic matter in water. I had occasion to use this salt, or mineral chameleon, in the laboratory; and it struck me as very remarkable that, whilst the solution remained unchanged for a long time in a bottle, a thin stratum exposed to the air became brown in a few minutes. I breathed upon it, and it became brown more rapidly. This seemed to give the clue to the cause of the decomposition which the permanganate underwent in the atmosphere. The result was exactly that which occurs when we attempt to filter the salt through paper. The paper decomposes it instantly.

The next attempt was to obtain results from pure and impure air, and these were so decidedly different that a means of measurement seemed to be attained.

The apparatus used was very simple; it was merely a bottle of the capacity of 100 cubic inches. A very weak solution of permanganate was made; but even a very weak solution gives a very strong colour. A small burette holding 100 grains was filled with the solution of permanganate. The air was pumped out of the bottle, leaving the mouth open, so that the air of the place streamed in, and the stopper was put on. This stopper was of caoutchouc, perforated with two holes; one to allow a tube to enter, through which the permanganate was poured into the bottle; another to allow, at the same time, a corresponding amount of air to pass out.

The liquid was poured into the bottle, and allowed to roll round the sides. It seemed to me that no improvement was obtained by allowing more than ten minutes for this. A small quantity was used at first; the colour was instantly removed; a small quantity more

was added, and so on until the colour remained permanent, showing that the air contained no more of the material fit for decomposing permanganate.

Before making this public I had made above 300 experiments. Of these, however, few only were published. They were given in a lecture at the Royal Institution soon after 1858. At that time I used oxalic acid to try the strength of the solutions; but oxalic acid is not a very sharp test, and I doubt if all the results are so definite as might be wished; but as a scientific fact, and a mode of showing the existence of organic matter in the air, the plan was sufficiently explained.

In trying the experiments in town and country, however, there was a difference given which could not easily be dealt with. The town air always required more permanganate: and no wonder; this was in reality due, to some extent, to sulphurous acid from the burning of coals. This might be mistaken for organic matter; and could this mode of testing show the difference? It could not readily show it, but, when it is carefully observed, it is seen that the sulphurous acid of towns acts instantaneously. The sulphuretted hydrogen of putrid gases acts also instantaneously, but the organic matter acts slowly. Still the organic matter does act.

The following table will give a fair specimen of the figures obtained; I need not here record them all. I certainly was not satisfied with the apparatus used, and endeavoured to obtain better results by means of an aspirator; that is, by driving the air through a solution through which it might bubble. The experiments made in this way were satisfactory in a scientific point of view, as may be seen below, proving the same points; but an aspirator is not generally applicable.

It was for the reasons found here that, in spite of many requests, I have never seen it proper to let it go forward as a popular apparatus. So far as putrefying matter or

sewer matter is concerned, as well as the sulphurous acid of coal smoke, the action is so rapid that anyone may use the plan; but, when organic matters purely are concerned, it frequently requires much patience, and some of that peculiar form of discretion which can only come with some knowledge of chemistry.

The following were made with the burette in a bottle of the capacity of 100 cubic inches. The number of grains of the solution of permanganate decolorised by 100 cubic inches of air is given in the last column, and the comparative amount of oxidisable matter is thereby seen. The bellows-pump used to remove the air from the bottle is figured with Pettenkofer's apparatus.

Grains of Solution of Permanganate of Potash, Decomposed by equal Quantities of Air. The last Column gives the Comparative Amount of Oxidisable Matter in the Air.[1]

Place.	Date, 1858.	Temperature.	Grains of Solution of Permanganate used.
		Fahr.	
London:			
Wellington Street	July 13	70°	45
" " higher up	"	"	25
Waterloo Steps	"	"	42
Blackfriars, in the street	"	"	25
Southwark Bridge (at river)	"	"	55
Street near the same bridge	"	"	22
A little distance from river	"	"	22
Street near Fetter Lane	"	"	30
Fenchurch Street Station	"	77°	18
Loughton (in Epping Forest, on the hill)	"	"	16
" inside a house	"	69°	18
Camden Square	"	72°	26
" "	"	"	26
Sixty miles from Yarmouth, at sea	Aug. 1	69·5°	3·5
Hospice St. Bernard in a mist	—	—	2·8
Lake Lucerne in middle-evening	—	—	1·4

[1] This comparative amount having been mistaken for an actual amount, I must call attention to the fact that no actual quantity is attempted to be given in the table.

Grains of Solution of Permanganate of Potash, Decomposed by equal Quantities of Air—continued.

Place.	Date, 1858.	Grains of Solution of Permanganate used.
All Saints, Manchester	June 22	40
" "	"	40
" " inside laboratory .	"	48
" " outside " .	"	40
" " " " .	"	37
Air drawn from a pig-stye . . .	"	70
All Saints, Manchester	June 21, evening	28
" "	June 22, morning	36
" " inside laboratory .	June 23, morning	34
" " outside " .	"	30
" " " " .	"	26
" " " " .	Evening	15
Pig-stye	"	63

It is needless to quote more from this early paper, as the method used was imperfect, and the results therefore not to be considered quite right, however correct the principle.

A very much larger vessel than here used would make the test sharper, but there is a great objection to bulky apparatus.

A better mode of conveying the test into the bottle was devised—a tube passing through the stopper was furnished with an elastic ball. The stopper and ball are taken out, the ball is pressed, and the point dipped into the permanganate. The ball is then allowed to expand, and the liquid rises. The taps are then closed, and the stopper put on. When the liquid is to be added to the bottle, the taps are opened and the ball pressed; by this means any amount may be passed into the bottle without contact of air. If there is a graduated tube connected with the ball, the amount may be read off there.

In this way we may work with very small quantities of liquid and of air.

The method of preparing the liquid and testing it need not be given in detail, as I doubt if any but chemists could manage it.

From the late experiments one cubic centimeter of a solution of permanganate is made equal to ·00225 grammes of metallic iron. A strong solution is prepared and tested, then diluted to the proper amount. The dilution must take place with very pure water. The best water is got by distilling deep well-water, or else by allowing some to stand with a considerable amount of permanganate of potash for a night and then distilling it. (To this ought to be added caustic soda, and the ebullition continued for a while, as in Wanklyn's process.) The solution must remain clear with some purple in it, and must not become red.

I have made some solution very pure by weighing out a crystal of permanganate and adding water to it, but, on the whole, I prefer to begin with manganate, the green salt. By adding water to this, it becomes reddish or purple.

The solution of a crystal was used in the following experiments :—

Organic and other Oxidisable Matter in the Air.

5 cub. cent. (77·20 grains) of the permanganate were used, and air passed through it.

1 cub. cent., tested with protosalt of iron = ·00225 iron, or ·00032 oxygen.

	Place.	Air passed through Solution.	
		Litres.	Cubic Feet.
	Air on the hills near Buxton . . .	Above 90	Above 3½
1	At the laboratory	64·750	2·286
2	,, ,, ...	61·680	2·178
3	,, ,, ...	61·680	2·178
4	,, ,, ...	67·848	2·396
5	,, ,, ...	66·306	2·341
6	,, ,, ...	64·750	2·286
7	,, ,, ...	64·750	2·286
8	,, ,, ...	66·306	2·341
9	,, ,, ...	67·848	2·396
10	,, ,, ...	65·521	2·313
	Mean at Manchester	65·143	2·300
1st	In the lead chamber after three persons had remained about two hours	46·260	1·633
2nd	Do.	49·344	1·742
3rd	Do.	43·176	1·524
	Mean	46·260	1·633
	Human breath	44·718	1·579
	,, ,, ...	47·802	1·688
	Mean	46·260	1·633
	Air from a midden	52·428	1·851
	,, ,, ...	57·054	2·014
	,, ,, ...	47·402	1·674
	Mean	52·294	1·846
	In lead chamber with vapour of gunpowder	43·176	1·524

The amounts here are not exactly the same as in my former experiments, made six years ago (writing in 1863). The earlier were made with an alkaline salt, the latter with a pure permanganate. It seems to me that the former is the most sensitive, and for the following reason :—

When acid, whether carbonic or any other acid is added to the manganate, it becomes red, and the air which is passed through it, giving up its carbonic acid, causes this redness. When the change once begins it more readily goes forward. The solution is still more easily decomposed when sulphuric acid is added to it, although another cause seems then to act; that is, the sulphuric acid assists in retaining and decomposing the organic substances.

There may also be some other causes which affect the decomposition of the manganate. Sometimes it is very easily reduced; sometimes otherwise; so that I fear we have to deal either with a variable substance or with very variable conditions. The latter is the more probable, and, at any rate, the causes of a change are very numerous. As a rule, however, the amount of organic or other oxidisable matter in the air ought to be so small that, with 5 cubic centimeters (77 grains) of the solution, it will require nearly four cubic feet to remove the colour when the air is caused to bubble through, and if less removes it the air is not the purest. Sometimes, as in the case of putrefactive matter, or of sulphuretted hydrogen, a few score cubic inches will be enough, in very bad air still less. In no case, however, will these experiments be complete with such a small quantity of air as 100 cubic inches, unless the liquid is shaken in the bottle, or made to roll round its sides. In that case, as it will be seen, much less liquid was used, as well as much less air.

It is fatiguing to roll the bottle about in the hand, and I confess my patience was far too much tried to continue further, and my assistants became tired also. I proposed, therefore, the use of a flexible aspirator—an instrument since found of the greatest value for many experiments. It is composed of caoutchouc cloth. It will draw air into itself through a solution, and with such regularity that,

even if left alone for two hours, it will be found not to have varied its pace. With water aspirators, as generally made, a variation is continually taking place. The amount passed by the flexible aspirator is measured by the fall of the lower and loose end. My earlier experiments were more accurately measured; viz., by passing the air first through a Croll's or Glover's dry gas meter, in which it was easy to measure to a few cubic inches. Those made in a bottle of definite dimensions were not subject to any error of measurement. This flexible aspirator is also an apparatus which might be more employed in laboratories. If inverted, it may be used for pressing air through a solution. This too it performs with great regularity. The air or gas may be collected from any spot, and carried till it is required; but extreme purity is not obtained in this way, on account of the sulphur from the vulcanised rubber cloth.

If people have much work to do, even the trouble of filling and emptying such a vessel may be too fatiguing; and to avoid this evil Mr. J. B. Dancer, of Manchester, has made a very beautiful apparatus, adapted at first for this process only, but capable of being used for numberless purposes in a laboratory. When the connections have been made as there shown, and the stoppers opened, the flow of water will commence from the upper bottle, drawing the air through the solution. When this is done, the bottle is reversed by a touch of the hand. It is moved with the greatest ease. The operation commences again. The bottles are measured in parts of a cubic foot, or in cubic centimeters, as required. This is a great improvement on that of Dr. Reid's. (The reversing aspirator was, I believe, a French invention.)

The 4-bulb glass is intended to allow of very rapid absorption. The first bulb is only half filled, and, when the gas passes rapidly, the liquid is thrown up in spray and thus better exposed.

When any caoutchouc is used it must be black; that is, either the common natural or vulcanised rubber boiled with caustic soda. If ordinary vulcanised rubber is used in contact with permanganate, the solution is instantly decomposed. If the air from the inside of a bag of this rubber be used, it instantly destroys the permanganate. In fact, that which we smell of vulcanised articles decomposes the salt.

It must be remarked that, when putrid gases are passed through the solution of chameleon, the amount may be tested sharply, but when the exhalations of the skin or the lungs are passed through, this sharpness is not observed. For example, I have not observed a difference when two persons were in the lead chamber, and when there were three. The cause may be that only a limited quantity is kept in suspension in the air, the rest being deposited. The senses confirm this. If, however, actual putrefaction occurs, the amount is less limited. This leads us to think that the test may be of special advantage in tracing out the existence of putrid gases, either in houses, hospitals, or the human breath itself; but this subject requires labour, and that labour has not been bestowed. The amount of labour necessary to obtain such results as are here given has been more than can be imagined by those who have not been devoted to the search of such hidden and subtle existences and phenomena. I make this remark as I know that many persons seem to think that I have neglected the subject too much of late; but if I have, it has not been from idleness. Another energetic campaign might bring it forward an important step.

Since writing the above I am able to state that an atmosphere containing ·19 of pure carbonic acid feels very agreeable, and cannot, at least in half-an-hour, be distinguished from pure air. To find such a condition Mr. P. H. Holland suggested a manufactory of soda-

water. This was found at Messrs. Jewsbury and Brown's, in Manchester. The windows were wide open, and fresh air was coming in, but the amount of gas escaping was enough to keep the lowest amount of carbonic acid at ·1 whilst the bottlers breathed at least ·19, but generally much more. The place was fresh and agreeable. This fact I consider as sufficient to enable us to throw on the organic matter all the first blame of the discomfort in badly ventilated rooms. So pleasant was the air in the manufactory that very much more carbonic acid could have been borne. It was not apparent to the senses; indeed, it will be seen that 4 per cent. was not very evident to the senses, although it affected the vitality; whilst we thus throw on the organic matter the sensible discomfort of the time, we are not yet able to say that ·19 of carbonic acid caused no physiological effect. That point is for the present reserved. The absence of an effect which the sense of smell could detect did not hinder a very remarkable lowering of the pulse when air containing nearly 4 per cent. of carbonic acid was breathed.

Here, however, a step is distinctly gained. We owe our discomfort to organic matter; although this has been said for some years, it is not anywhere so fully proved as by the several facts now gained.

THE ORGANIC MATTER IN METALLIFEROUS MINES.[1]

It is not difficult to prove that organic matter exists in the air of mines. The assurance that human beings live there would of itself be sufficient to convince us of its existence. Organic matter flows into the air as a necessary result of the phenomena of life. If we did not know that human beings existed there, the proof would still be easy; as we find along the sides of the

[1] This also from 'Report on Air of Mines.'

mine, in places where the amount of water is neither too great nor small, and where other conditions do not prevent it, that a fine, soft, white covering is given to the rock like the nap of the finest white velvet, but much finer. When the water and light are favourable, it shines like the morning dew on spiders' webs, but I know no appearance above ground giving an idea of so much delicacy. It looks pure as it is white; and it is pure—it purifies the air.[1] When examined more nearly, it is found to resemble rather the mould which comes on preserves, but blanched thoroughly by its absence from light. It may readily be collected, but so wonderfully light is it that you may gather the crop from many square feet, and almost doubt at last that you have any harvest. It dries up at last into a very tough substance, which Professor W. C. Williamson, F.R.S., says is a mass of confervæ. I did not give him any in a fresh state, so that he had no opportunity of making any full examination.

When viewed under the microscope, it presents a mass of transparent fibres matted together and interwoven in a most complete manner. Its analysis is, perhaps, scarcely to be depended on as far as nitrogen is concerned, as the quantity used for combustion with oxide of copper was small, and the amount in possession insufficient for a second trial. Nearly two per cent. of nitrogen was found.

I sent for more of the substance, but my request was not clearly understood. There is an efflorescence of sulphate of lime, and also of other salts, which very much resembles this fungous growth; the latter is frequently taken for the inorganic salt. The specimen examined was obtained from the Botallack Mine, but the same kind was seen in other places.

[1] It may, however, be itself a dangerous substance; we may think, in connection with it, of the blood-spots of the time of the sweating sickness.

In a beer-cellar the fungous matters that grow around are proof of exhalations arising from the beer; and no cellar can show an analogous phenomenon better than mines. At the same time these substances are not found in all parts of the mine, because the confervoids seek a suitable soil and climate, and these differ every few yards underground. Even in a cellar there is no fungous covering over the whole wall; it is confined to peculiar spots, which receive it sometimes in great excess. In a country there are little nooks of land which are so favourably placed as to become natural hospitals, and in a garden there are little corners which have all the effects of hothouses. So one vineyard has the best wine in a country, and one plant in that vineyard is more remarkable than any other. For similar reasons the confervoid spoken of does not grow on every spot in the mine, but it is common, and must feed only on the organic matter supplied by the miners. That which is found on decayed wood is much more abundant.

Decayed wood is much blamed for giving out bad air. In this country the amount of wood used is often so small that we may leave this source of evil out. When 4,000 fir-trees are supplied yearly to a mine, as to the Dorothea Mine, at Clausthal, and the Dolcoath Mine, in Cornwall, this source of carbonic acid must not be overlooked.

As I had worked a good deal on the subject of organic matter in the air, it was natural that I should attend to it in the mines; and, indeed, my first visit was accompanied with an apparatus for testing it with permanganate, as explained when speaking of the organic matter of the air.

A solution of permanganate was used, 20 grains of which were decolorised by 100 cubic inches of air. In the mines 100 cubic inches removed the colour from 28, 30, 39, and 50 grains, and farther on in the same level from 70 grains. In another mine 50, 48, 52.

It was clear that deoxidising agents were abundant in the mine at certain places. But it must not be supposed that this test fixes on organic matter only. It is affected by sulphides and sulphites instantaneously, and is therefore a test of the amount of these salts besides other substances, as already explained.

The air from gunpowder contains sulphuretted hydrogen and sulphide of potassium. If a large jug, or even a basin or tumbler, be moistened on the sides with acetate of lead, whilst a few grains of gunpowder are laid on a dry spot below, and fired off by touching with a hot wire, the sugar of lead or acetate will become black instantly. A deep vessel is better for this purpose, in order to confine the smoke. No such large amount comes from cesspools on ordinary occasions. We see at once that such a gas must act so as to reduce the oxide of iron in the blood, and render much of the air which is taken into the lungs valueless.

For this reason it was found quite useless to apply in the mines the test which has been used for other places. It tells of substances in a group, a sufficient value at times, but not sufficient where a minute enquiry is wanted. The following experiments, made for the purpose of discovering organic matter, if few, were remarkably satisfactory.

Seventy-six tubes in which gas had been collected were washed out with water. This solution contained chiefly sulphate of potash. I was scarcely sure of any soda without the spectrum test. It also contained a little chlorine, as we might expect, the gunpowder not being free from chlorides; there were also nitrates from undecomposed gunpowder, and partly, perhaps, from the explosions made when analysing.

The ammonia was equal to one grain in 250 cubic feet. This result is given as only approximate; still it looks probable. We do not know to what this ammonia

is united; it would probably be to carbonic acid; and it may have come partly from the combustion of gunpowder. If it had come from organic matter only, we should still have been in a difficulty, as we do not know how much nitrogen would be found with a given amount of animal exhalations. If the nitrogen came in a compound with the same percentage of nitrogen as fibrin, we should then have one grain of organic matter in 36 cubic feet.

Part of the material from the tubes was burnt, and the unmistakeable odour of nitrogenous substances was perceived; burnt feathers give it in its ideal purity. As a final proof that organic matter was operated on, carbon, black and combustible, remained behind.

True the total amount obtained was small, and the actual estimation of quantity in such cases has a strong appearance of a guess; but, in order to obtain proof of the existence of the organic matter, without regarding the quantity, a far smaller amount might be operated on with confidence. The inorganic salts are found as might be expected—the acids and the bases also.

This experiment is also a proof of the observations by the microscope being correct. The salts observed are proved and tested in a way that is more convincing to a chemist than the mere fact of seeing the undisturbed substances. Still the microscope was a valuable aid, and showed the salts clearly to the most inexperienced. Indeed, the drawings were all made by one who knew nothing of chemistry.

Perhaps some persons may think that, in washing out seventy-six tubes, some impurity may have entered, vitiating the results. The same water was used to each, so as to avoid any accumulation of matter from the water. They were not thoroughly washed out, as this would have caused too much liquid to be used, and the water was not driven on them from a laboratory water-

bottle, but by pouring. Besides, experience in looking for organic matter has shown me that such noticeable quantities are not easily obtained; the difficulty is to obtain them at all from air.

However, for self-satisfaction, several fresh tubes were opened and treated with permanganate. That in tube marked 'Dufton' decolorised 50 grains of the permanganate solution, acting at once; this tube must have contained sulphides, I suppose. It is the best proof of their existence which I obtained, but it is not complete. This is an amount which would not be decolorised by less than 250 cubic inches of common air; and here in the mine we find about 2 cubic inches enough. This specimen is one which had a very large amount of the crystals in it. The air in Z Z 2 acted similarly, to a less extent, and more slowly, which is still a better proof of organic matter, and to me a complete one. Sulphuretted hydrogen acts instantaneously. The slow action of the organic matter is very convincing when one becomes familiar with it. Of course the tubes that had no enclosed mine air were tried, and no such results were obtained from them. This mode of trial promised well, but it was not carried out far enough.

These proofs of organic matter were considered more than sufficient, and it was not deemed wise to destroy many specimens, as I was exceedingly anxious to obtain as much evidence as possible on the two gases oxygen and carbonic acid. However, a few were opened for experiments on—

The Sense of Smell and Organic Matter.

Some substances are best known by the smell, and in reality only by the smell. Yet the sense of smell is not generally taken by chemists as a guide of the highest class. It is not easy to see why; a part of the reason

may be that this sense leads us so accurately to such a great distance that we are apt to presume upon its power, and to judge of substances in amounts really too small even for it. We lead it to extremes more readily than we lead other faculties. In the mines the sense of smell is not capable of exercising itself well. In order to give it a fair field, the organ must work in pure air, and the substance to be tested must be put to it for a short time only. Pure air must frequently be resorted to. If we act in this way, we find that in the mine air there is the smell of tallow, perspiration, and tobacco. These smells were found, not in all specimens, but in some. The tubes containing the air were simply broken and brought near the nostrils. I have some tubes made in a form that will enable us to blow out a small quantity at a time rather than squander it away as hitherto obliged. Still, even by this squandering method, there was enough in two cubic inches of gas to prove it several times to the satisfaction of all the inmates of my laboratory. It required no more proof; it was clear to the observation. There was the close smell, simply like a school-room or church, and the excessively disagreeable smell, such as I have met in the mine itself, of human perspiration.

But the most curious point is, that on several occasions I found the smell to be aromatic, and one gentleman who smelled it said at once that the smell was that of apples. I recognised apples in it also, but still was conscious of something besides, and simply called it aromatic. I was extremely surprised to find in several German books that the apple smell is an ancient attribute of badly ventilated places. The organic substances decompose, and compounds are formed after a time less disagreeable than at first. One tube had a smell of sulphurous ashes, another of burning oils, another of putrefactive matter—according to a friend, Mr. Edward

Hunt, B.A., to whose capacity of recognising by smell I had often found advantage in appealing.

A bottle full of the air blown into the nostrils would be a very delicate test, and easily made. When I was shut up in the lead chamber, not imagining that the air was bad, those outside, who breathed from the chamber through a tube, found the atmosphere very unpleasing. Such an experiment is as good as the sense of sight can furnish. It is as good even as the sense of touch; nay, more, you may prove by the sense of sight and touch that a certain atmosphere is hurtful, but you will not prevent men from entering it, except after long and laborious processes of reasoning. But give that atmosphere a bad smell, and the argument is finished at once. This sense must not be despised by the intellect. If the intellect should venture to show such pride, it will be soon obliged to submit and confess itself beaten by the instincts. This unexpected incident, caused by the sense of smell, leads us to a new and very simple mode of testing air. We take it from its place, we stand in pure air, and blow the air to be tested into our nostrils.

NEW METHOD OF USING THE PERMANGANATE TEST OF AIR.

This method may be called new, slight as the change appears. The novelty, however, consists merely in the larger amount of solution used to wash the air; so that the work is much better done, and the larger amount of air washed ensures a more visible result. About 30 cubic centimètres of pure water are taken, and a small amount of a known solution of permanganate added. This is shaken with the air of the bottle. The air is then pumped out as described under 'Air-Washings,' and the bottle shaken again until the whole colour is re-

moved, or a sufficient amount to enable one to test the remainder, so as to be able to estimate the difference. The actual amount of oxygen taken is then calculated. The objection to this process is still its delicacy, but, in treating the organic matter or other oxidisable substances in the air, this fault will always remain more or less.

Rain has been examined in the same way with very interesting results, as seen in the tables, and rain must be considered as natural air-washings.

Air.—Permanganate Test, or Air-Washings tested with Permanganate.

Place.	Date and Weather, 1870.	Oxygen required Instantly.		Total Oxygen required in a few Minutes with Acid.	
		Grains per Million Cubic Feet of Air.	Grammes per Million Cubic Mètres of Air.	Grains per Million Cubic Feet of Air.	Grammes per Million Cubic Mètres of Air.
York Place . .	Jan. 21 . .	70·23	160·73	135·62	310·37
Front of laboratory .	„ 24 . .	277·51	635·07	479·31	1096·91
„ „ .	„ 27, misty .	227·05	519·60	484·37	1108·48
Yard behind „ .	„ 20 . .	244·36	559·21	527·58	1207·36
„ „ .	„ 27, misty, soot falling .	655·92	1501·07	908·20	2078·40
A water-closet . .	Jan. 26[1] . .	70·63	161·65	226·82	519·07
A privy . . .	„ 23 . .	492·95	1128·11	870·36	1991·80

The following experiments furnished the data for the table given above :—

[1] At York Place, and apparently well ventilated.

Air-Washings of 37·5 *Litres of Air* (*the Amount used for an Experiment*) *tested with Weak Permanganate.*

Place.	Date, 1870.	Milligrammes. Oxygen required by Washings of 37·5 Litres of Air.	
		Instantly.	Total in a few Minutes.
York Place . . .	Jan. 21	·0060	·0116
Front of laboratory .	„ 24	·0238	·0411
„ „ .	„ 27, misty	·0195	·0416
Yard behind „ .	„ 20 „	·0210	·0453
„ „ .	„ 27 „	·0563	·0779
A water-closet . .	„ 26 „	·0061	·0195
A privy . . .	„ 23	·0423	·0747

Tests for Ammonia and Nitric Acid.

To this point there is some progress made in finding organic matter, but the most definite experiments are those which give the ammonia and the nitric acid. Several chemists have attended to the ammonia since Liebig showed its importance for agriculture, and estimated the amount in the rain-water at Giessen. Some of the most complete trials have been made in France, and I think it interesting to look at the summary already given.

The difficulty of the experiments, or rather the long time they occupied, prevented the completion of that great number that were actually required for full information, until Nessler's test for ammonia became better known, and until Schulze's test for nitric acid was established as a safe one. Several have laboured hard to obtain sure and delicate tests for ammonia and nitric acid, and, trifling as the matter may at first seem, we cannot doubt that the knowledge gained by simplifying these tests will be great. I believe that to no one is the

subject so much indebted as to Mr. Wanklyn, who, along with Messrs. Chapman and Smith, has worked out carefully those processes, and made them good laboratory machines, so to speak; but, whilst he has done this, he has done more, as he has decomposed many nitrogen compounds, and obtained from them ammonia, which can be tested by the Nessler process, thus putting into our hands a mode of separating the ammonia which has resulted from decomposition, and which may be innocent, from that potential ammonia ready to come from albuminoid substances, and often allied with compounds dangerous to health.

As soon as these tests became to my mind satisfactory for obtaining comparative results, I set to work to apply them in such a way as to improve our knowledge of the atmosphere. The absolute separation of the ammonia of organic origin may be a little different.

I have been able by these means to give the amount of ammonia, nitric acid, and albuminoid matter in rain and in air with, I believe, great accuracy; and the result is, as we might expect, a gradation from the pure to the impure. We may have large quantities of ammonia in air not infected, but albuminoid ammonia will be greatest in infected air, if we are at all correct in believing either the existence of matter in a state of decay as the cause of disease, or in the existence of germs which may be the products and cause of disease in animals or in plants. These organisms may be themselves active and healthy, however unwholesome their activity may be to us. The comparative amount of this ammonia has been estimated in numerous cases in the air, and the results may be now taken as practical and easily followed. Although the two classes of ammonia have been given in the tables of air and rain, the following results, obtained when investigating the method, will probably stand as a better explanation of the distinction.

Experiments to find whether the Soluble or Insoluble Portion of the Air-Washings gives most Ammonia.

Air-Washings from		Free Ammonia.		Albuminoid Ammonia.	
		Grains in Equal Quantities of Air.	Grammes in Equal Quantities of Air.	Grains in Equal Quantities of Air.	Grammes in Equal Quantities of Air.
Woburn Square, W.C., and Street off Regent Street	Soluble	16·614	38·022	15·573	35·647
	Insoluble	None	None	42·575	97·433
Large Yard near St. Katherine's Docks	Soluble	21·442	49·073	36·758	84·125
	Insoluble	1·858	4·253	31·916	73·043
New Kent Road, Pleasant Place, and Kennington Park.	Soluble	None	None	37·705	86·282
	Insoluble	,,	,,	25·981	59·453

Taking the first four experiments, which are the most complete, we have—

	Proportionate Amount.
Ammonia in solution	19·028
Organic ammonia in solution	26·165
Ammonia in solid	1·858
Organic ammonia in solid	37·245

This proves the insoluble matter to contain the greater part of the organic nitrogen.

By these means we can tell exactly the comparative amount of albuminoid matter represented or actually present in a given amount of air, as well as the ammonia, and we may add nitric acid, from previous decomposition. Taking the two ammonias together, we have in one instance 32 grains in a million cubic feet in good air. Of this $15\frac{1}{2}$ are albuminoid, which will represent about seven times its weight, or 108 grains of albuminoid matter, which may be the matter forming germs, seeds, and, perhaps, full-grown plants, at that particular time and place. It is not absolutely certain to me that the whole of the material is decomposed, and on this point I am quite prepared to find that some failure has been

made. On the comparative quantity I have no doubt, as the experiments are very numerous and consistent, extending over a long time, as well as to a great variety of cases, some at least giving absolute quantities.

The figures given in 1868 and 1869 were called comparative. I was afraid to say that they actually represented the amount in a million cubic feet, although mentioning that it was intended to do so at first. The fear rose from the method of making the experiments; it was not clear how many strokes of the pump were necessary to ensure a new supply of air to the bottle. It was necessary not to give too little, and it was also of importance not to draw in too much, because, although it would soon escape, it would pass over the liquid and be partly washed.

The following will explain the method:—

Latest Method of making the Experiments.

A bottle of the capacity of about 2,000 cubic centimètres, or nearly two quarts, was filled with the air to be washed. This was done by means of a pump of the capacity of about a tenth of a cubic foot of a flexible material. (This pump taken from the 'Report on the Air of Mines,' see Estimation of Carbonic Acid).

The tube from the flexible pump in a closed state is put into the bottle, the mouth being left open so far as the tube permits it. The pump is then drawn open, and the fresh air rushes into the bottle. The bottle contains 30–50 cub. cent. of the purest water. This is shaken with the air. Of course the tube is previously drawn out and the glass stopper put on. It is very important that the water should be pure. The test for purity is exactly that which is used for the ammonia. If none can be found in the water used until it has washed the air, then the source is the air. The same

test must be made for the albuminoid ammonia. The water must therefore be boiled with soda or potash and permanganate before distillation. After this it must be tested. There is no use in testing it before, as it will certainly contain impurities, and even after being distilled alone. Patience is not lost in excessively careful preparations for experiments of this kind.

The testing afterwards is exactly the process for testing waters as given by Messrs. Wanklyn, Chapman, and Smith.

Measure of the Air used.

The amount of air contained in the bottle must be carefully ascertained. The temperature and pressure have not been taken for this class of experiment. At a future time it may be a question of weight of air; at present it is of bulk. One bottle is far too small for the estimation of the organic matter, or even of the inorganic salts, except on rare occasions. It was necessary to fill it many times—from ten in bad cases to a hundred or more in good—nearly 1,000 have been used in some. The question then rose, When one stroke of the pump drawing one-tenth of a cubic foot of air was used, being more than the contents of the bottle, did a whole bottlefull of fresh air enter? It was not certain for some time, and a long series of experiments was required to prove it. The trial was first made by using a common air-pump, and making a nearly complete vacuum in the bottle, then allowing the air to enter. By this means an exact measure was obtained; the value of the vacuum was measured by a column of mercury. Strange to say, excessive amounts were obtained so frequently that the experiments were not considered of any value by this method. Still, being theoretically exact, it was destined to be the test of all other modes of measuring, and the cause of the excess must be discovered.

In order to find how many strokes of the bellows-pump were equal to the amount taken out by the air-pump to make room for a fresh supply, it was determined to try the hydrochloric acid of the air, because there was reason to fear that there were circumstances in connection with the apparatus and mode of working which would affect the amount of ammonia.

Air-Pump.			Four Strokes of Bellows-Pump.		
Litres of Air.	Grains per Million Cubic Feet of Air.	Grammes per Million Cubic Mètres of Air.	Litres of Air supposed to have been used.	Grains per Million Cubic Feet of Air.	Grammes per Million Cubic Mètres of Air.
83·49	133·19	304·81			
83·49	105·07	240·45	93·75	144·60	330·93[1]
66·88	190·78	436·60	75	255·19	584[1]
67·78	164·72	376·95	75	170·13	389·33
67·93	152·62	349·26	75	180·76	413·67
Average .	153·29	350·82	Average .	187·67	429·48

It is clear that four strokes of the bellows-pump are too many.

One stroke of the pump was then tried, and it was found that, although it contained scarcely a half more air than the bottle itself, the amount of air brought in was sufficient to refill it. The amounts obtained by the two methods are as nearly the same as two experiments made on one method. We cannot expect identity of result to one or two units in cases where the air is constantly changing, and still less when the amount is calcu-

[1] In making these two experiments a short tube was connected with the bellows-pump, and, in consequence, the hand was close to the mouth of the bottle when the air was being changed. For all the other experiments a glass tube bent at right angles was used, so that the hand was nearly a foot away from the bottle.

lated on such a large quantity as a million cubic feet. If we calculate it for the actual amounts used, the differences are wonderfully small.

Air-Pump.			One Stroke of Bellows-Pump.		
Litres of Air.	Grains per Million Cubic Feet of Air.	Grammes per Million Cubic Mètres of Air.	Litres of Air supposed to have been used.	Grains per Million Cubic Feet of Air.	Grammes per Million Cubic Mètres of Air.
68·93	108·75	248·88	75	106·33	243·33
69·78	114·29	261·54	75	116·96	267·67

A perfect accordance to a per cent. is not to be expected in ever varying air, still less to one or two in a million. The general similarity is sufficient for the purpose.

The sulphates in the air were tried in the same way, and the results were—

Air-Pump.			One Stroke of Bellows-Pump.		
Litres of Air.	Grains per Million Cubic Feet of Air.	Grammes per Million Cubic Mètres of Air.	Litres of Air supposed to have been used.	Grains per Million Cubic Feet of Air.	Grammes per Million Cubic Mètres of Air.
68·93	3362·38	7694·76	75·	3565·67	8160
17·66	6561·97	15016·99	19·25	6946·12	15896·10
Average .	4962·17	11355·87	Average .	5255·9	12028·1

It was always observed that the air-washings, when the air-pump was used, were darker and contained more solid floating matter than when the loose flexible pump was employed on the open bottle. It was found that whilst by one action of the flexible pump the same amount was

got as by the more exact measurement of the rigid air-pump when acids were estimated, the same nearness of result could not so easily be obtained for ammonia, unless so much care was used that it was impossible in distant places and in the open air. The flexible pump was therefore taken. In experiments where good air and bad differ more than 100 a very close approximation is not required, and in the present state of the question cannot be used. If, for example, pure air gives less than twenty of ammonia, and impure above 200, it is the same to us in meaning as if it were 250, or even more.

The cause of the increased amount of ammonia when the fixed air-pump was employed was found to be in the organic substances used for closing the bottle, or rendering it tight, or employed in manipulating. By keeping it open, except when the clean glass stopper was put on, the difficulty vanished.

AIR-WASHING.

Having finished many analyses of rain, it was next determined to imitate it by art, and to wash the air so as to obtain from it the extraneous matter for examination. It is of the highest importance that we should learn the character of the air that we are called on to breathe without waiting to observe its effect on our health. I certainly am pleased to have been so far successful, and to see the promise of still more success. It is surprising to find the figures of the comparative results stand out in such beautiful order. Something like this I did expect, but to find so few cases out of the rule, and of the 'means' none whatever, was beyond my expectation. The numbers follow the same order as those for rain; we see, therefore, that some conditions of the air, so

far as regards the substances mentioned, can be settled by experiment on a dry day and under cover. The air was washed in a bottle, as already explained under 'Experiments to find the Amount of Organic Matter in the Air.' A better mechanical way will be found, no doubt, but this is sufficient to show its practicability. I have made an apparatus to be driven by the wind and pump air through a liquid at the same time, registering the number of strokes, and consequently the bulk of the air; but, although it may be of use, it cannot approach the truth so nearly by a great distance as by the more laborious method adopted. Still it will do for rough results, which can be compared among themselves. Although air-washings enable us to examine air where rain cannot come, and to a great extent must supersede the examination of rain, the latter must not be neglected, since we obtain from it a knowledge of the contents of the atmosphere at a time when we cannot watch it, and we obtain also the mean of a long period. The rain, as well as the air-washing, will still be used for the examination of the air of the country and of towns, whilst the air will be washed in houses and close places.

Very little requires to be said in explanation of these tables, all of which have been already published in the 'Reports under the Alkali Act' from 1868 onwards.

HYDROCHLORIC AND SULPHURIC ACID IN THE AIR.

From 'Alk. Act. Rep.,' 1868.

No.	Place.	Date, 1868.	Weather and Time of Day.	Remarks.	Proportion of Hydrochloric Acid to Sulphuric Acid.	Hydrochloric Acid.		Sulphuric Acid (Anhydrous).		Total Acid.	
						Grains in a Million Cubic Feet.	Grammes in a Million Cubic Mètres.	Grains in a Million Cubic Feet.	Grammes in a Million Cubic Mètres.	Grains in a Million Cubic Feet.	Grammes in a Million Cubic Mètres.
1	Grosvenor Square, Manchester	March	. . .	. . . . .	1 to 13·99	49·43	113·1	682·80	1562·9	732·23	1676·0
2	„ „	April 2	Cloudy during the morning and afternoon	. . . . .	—	42·65	97·6	—	—	—	—
3	„ „	Oct. 5	Bright day, 12 to 6 P.M.	E. wind . . .	1 to 1·95	180·90	414·1	353·70	809·9	534·60	1224·1
4	„ „	„ 7	Fine day, 3.30 to 5.30 P.M.	W. wind . . .	1 „ ·46	155·50	356	75	171·7	230·50	527·8
5	„ „	„ 20	Mist, 10.30 to 12 noon	W. wind; rain night before	1 „ 17·34	88·10	201·7	1526	3493	1614·10	3694·7
6	„ „	Nov. 20	Fine, 10.15 A.M. to 12.30 P.M.	. . . . .	1 „ 67·02	59·80	136·9	4008·30	9173·0	4068·10	9309·9
7	„ „	„	. . .	. . . . .	1 „ 3·04	185·20	423·9	562·5	1287·5	747·70	1711·4
8	„ „	Nov. 23	Dull, 2.45 to 4 P.M.	. . . . .	1 „ 6·66	59	135·1	393·4	900·8	452·40	1035·9
9	„ „	„ 26	. . .	. . . . .	1 „ 11·11	135·50	310·2	1505·5	3446·1	1641	3756·3
10	York Place . .	Oct. 12	Fine, 2.30 to 4.30 P.M.	SW. wind . . .	—	—	—	55·7	127·4	—	—

HYDROCHLORIC AND SULPHURIC ACID IN THE AIR—*continued.*

No.	Place.	Date, 1868.	Weather and Time of Day.	Remarks.	Proportion of Hydrochloric Acid to Sulphuric Acid.	Hydrochloric Acid.		Sulphuric Acid (Anhydrous).		Total Acid.	
						Grains in a Million Cubic Feet.	Grammes in a Million Cubic Mètres.	Grains in a Million Cubic Feet.	Grammes in a Million Cubic Mètres.	Grains in a Million Cubic Feet.	Grammes in a Million Cubic Mètres.
11	„ . .	Nov. 12	Fine, 3 to 4.15 P.M.	N. wind . . .	1 to 13·44	77·80	178·1	1045·5	2393·1	1123·30	2571·3
12	„ . .	„ 13	„ „	„ . . .	1 „ 3·63	185·2	423·9	562·5	1287·5	747·70	1711·4
13	Greenheys Fields, near Manchester, on S. side	Oct. 12	Fine, 2.30 to 4.30 P.M.	SW. wind . . .	1 „ 12·30	79·5	182	978·2	2239·1	1057·70	2421·1
14	In a field between Stretford and Didsbury	„ 1	Cloudy . .	NE. wind; cold .	1 „ 3·19	112·6	257·8	359·5	823·2	472·10	1081·1
15	„ „ „ .	„ 5	Dull damp morning, but bright afternoon, 1 to 3 P.M.	NE. wind; rain during the previous night	1 „ 3·97	98·5	225·5	391·7	896·9	490·20	1122·5
16	„ „ „ .	„ 22	Showery, 12.30 to 3 P.M.	High W. wind; very wet all the previous day. Straws got into jar	1 „ 13·91	51	116·7	709·4	1623·8	760·40	1740·5
17	„ „ „ .	Nov. 26	Fair, 3.30 to 4.45 P.M.	Mist fell during last half - hour, and brought an odour of decaying vegetable matter	1 „ 15·38	57·27	131·1	881·1	2016·8	938·37	2147·9

18	St. Helen's, open space behind schools	Oct. 15	Raining heavily, 10.30 A.M. to 1 P.M.	W. wind . . .	1 „ 7·26	176·3	383·3	1289·3	2321·4		
19	Field about one mile E. of station	„	Fair, 2.30 to 4 P.M.	High W. wind . .	1 „ 14·10	66·8	152·9	942	2156·2	1008·80	2309·1
20	Behind St. Helen's Schools	Dec. 7	Fine, 10.30 to 12 noon	Light SW. wind. Smoke from works came once or twice	1 „ 11·49	199·48	456·6	2292·91	5247·2	2492·39	5704
21	E. of railway	„	Fine, 3 to 5.15 P.M.	W. wind. A little smoke from works during the time	1 „ 5·32	129·66	296·9	689·93	1579·2	819·59	1876·1
22	Behind St. Helen's Schools	Dec. 8	Rain, 10 to 11 A.M.	Raining all time. W. wind	1 „ 2·34	285·04	652·4	666·77	1526·2	951·81	2178·6
23	Mile N. of St. Helen's	„	Rain, 12 to 1.30 P.M.	Raining all time. Wind changed to E.	1 „ 4·76	39·95	91·4	190	435·1	229·95	526·5
24	Hill near Poole's Cavern, S. of Buxton	Oct. 29	Showers of hail and rain, 11 A.M. to 2 P.M.	High W. wind . .	1 „ 30·55	31·78	72·7	971	2222·6	1002·78	2295·3
25	Field below Buxton Lime Co.'s quarry, E. of Buxton	„	2 P.M to 5 P.M.	Very high W. wind, and heavy hailstorm	1 „ 29·63	54·57	124·9	1617·1	3701·5	1671·67	3826·4
26	On top of a hill S. of Buxton, and a little W. of a quarry	Nov. 23	Dull and damp, 11 to 12 A.M.	Strong NW. by W. wind. Not so much air washed as usual on account of smoke coming over the hill	1 „ ·66	312·5	715·3	207·3	474·7	519·80	1190·
27	On the side of a hill N. of Buxton	„	Clear, 4 to 5.15 P.M.	A most beautiful sunset. No wind	1 „ 1·18	65·6	150·2	77·5	177·4	143·10	327·6
28	On the top of a hill at Burbage, W. of Buxton, and N. of lime quarries	„	Dull, 1 to 2.15 P.M.	Day got brighter about 2 P.M. Wind in light gusts from the NW.	1 „ 2·96	132·7	303·8	393·3	900·6	526	1204·5
29	On the cliffs near Uncle Tom's Cabin, N. of Blackpool	Dec. 1	Raining till 11 A.M., 11 to 1 P.M.	This is about 30 yards from the sea. S. wind (not coming directly from the sea)	1 „ 6·94	37·36	85·5	259·45	594·1	296·81	679·6
30	In a field N. of Blackpool	„	Fine, 3 to 4.30 P.M.	Field about 600 yards from above. Tide out. Light S. wind	1 „ 7·66	20·27	46·4	155·30	355·6	175·57	402

Hydrochloric and Sulphuric Acid in London Air, November 1869.

Place.	Date and Weather.	Hydrochloric Acid.		Sulphuric Acid (Anhydrous).		Proportion of Hydrochloric to Sulphuric Acid.
		Grains per ? Cubic Feet.	Grammes per ? Cubic Mètres.	Grains per ? Cubic Feet.	Grammes per ? Cubic Mètres.	
Oxford Square, Lisson Grove, and near Belsize Park	November 5, raining	116·129	265·762	835·193	1911·340	1 to 7·19
King's Cross (two squares)	8, dull	52·267	119·614	—	—	—
Space near Holborn Viaduct, Woburn Square, W.C., and off Regent Street	11, frosty	—	—	986·276	2257·093	
Islington, Hoxton, Dalston, and Hackney	8, showery	—	—	386·292	884·030	—
Bethnal Green Road and Stepney	9, dull	—	—	899·964	2059·569	—
Cavendish Place, Wandsworth Road	6, fine	108·459	248·208	540·809	1237·642	1 to 4·98
Average		92·285	211·194	729·707	1669·935	1 to 7·91[1]

Air of the Underground Railway (Metropolitan), November 1869.—*The Air was washed in the Train passing up and down between South Kensington and Moorgate Street. Hydrochloric and Sulphuric Acids.*

Date and Time of Day.	Hydrochloric Acid.		Sulphuric Acid (Anhydrous).		Proportion of Hydrochloric to Sulphuric Acid.
	Grains per ? Cubic Feet.	Grammes per ? Cubic Mètres.	Grains per ? Cubic Feet.	Grammes per ? Cubic Mètres.	
Nov. 11 and 12, morning	358·571	820·586	2865·279	6557·192	1 to 7·99
Nov. 15, 9.30 to 11 A.M.	392·850	899·029	4419·526	10114·085	1 ,, 11·25
Nov. 15, 1 to 3 P.M. .	268·091	613·523	2644·172	6051·190	1 ,, 9·86
,, 10.30 to 11.30 P.M.	102·945	235·589	2961·423	6777·216	1 ,, 28·76
Average . .	280·614	642·182	3222·600	7374·921	1 to 11·48[1]

[1] Proportion of average acids.

Ammonia.

This alkali exists in the air as carbonate, chloride, sulphate, or sulphide. It rises from the ground as a product of decomposition of organic substances, and then chiefly with carbonic acid or sulphur. The oxidation soon converts part, and the excess of sulphuric acid converts the rest, into sulphate of ammonia, which then comes in contact with chloride of sodium, when decomposition takes place, as mentioned when speaking of sulphate of soda. This ammonia is not organic matter, but it shows that decomposition is going on somewhere so near as to leave its products perceptible. It is like the dry bones of animals, showing that life has been there. It is interesting to remark how the amount varies; it is greatest near the ground, and also at the backs of houses when refuse matter is deposited there. This is exactly as one might expect.

Ammonia and its salts, although to be distinguished from substances producing distinct forms of disease, must not be considered as indifferent bodies when in large quantities. We do not breathe so well in their presence; the senses have the power of feeling very small amounts of solid matter, and the breathing is easily inconvenienced by it.

Ammonia (Free or with Acids).

Air obtained from	Conditions.	Grains per Million Cubic Feet.	Grammes per Million Cubic Mètres.
Laboratory office, 10 A.M. .	. . . .	46·610	106·667
„ „ 4 P.M. .	. . . .	58·262	133·333
Gas-room, 10 A.M. . .	. . . .	57·113	130·703
„ 4 P.M. . .	. . . .	83·074	190·114
Yard behind laboratory .	Fine, Oct. 28, 1869	41·538	95·060
„ „ .	Freezing, snow on the ground, Dec. 28	46·729	106·939
„ „ .	Damp, Jan. 13, 1870	25·961	59·411
„ „ .	Fine „ 25 .	83·074	190·114
„ „ .	Foggy „ 26 .	62·305	142·586
Street, open . . .	Raining and strong wind, Dec.	31·152	71·292
		53·582	122·622
Bedroom, 9 P.M. . .	Feb. 15, 1870 .	29·076	66·540
„ 7 A.M. . .	„ 16 „ .	41·537	95·057
„ „ . .	Nov. 1, 1869 . .	62·301	142·585
		44·305	101·394
Midden	Fine, cold, Oct. 27 .	233·050	533·333
„	„ „ 28 .	103·841	237·640
„	„ „ 29 .	103·841	237·640
		146·911	336·204

This ammonia will measure the amount of sewage matter thrown into the atmosphere, and ought to be greater where there is a greater exposure of manures to air, and especially to air and water. We cannot tell the amount of injury which it does in minute quantities, but we object to it chiefly because it is the result of decomposition, and is an ingredient in the most impure air. It has therefore very bad relations and keeps bad company, whilst if it increases so as to be perceptible to the senses,

it becomes unpleasant and of course unwholesome. This was said under rain, but is repeated here to prevent mistakes.

Air-Washings of London.—Ammonia (Inorganic).

Date and Weather.	Air obtained from	Ammonia.	
		Grains per Mill. Cub. Feet.	Grammes per Mill. Cub. Mèts.
Nov. 4, windy . .	Chelsea (three places) . .	19·936	45·630
„ „ and a shower of rain	Brompton (three places) .	20·769	47·530
Nov. 5, morning; fine	Hyde Park (two places) .	12·655	28·910
Nov. 8, dull and windy	King's Cross (two squares) .	20·769	47·530
Nov. 11, frosty . .	Woburn Square and off Regent Street	16·614	38·022
„ „ „ . .	Space near Holborn Viaduct	12·460	28·517
„ 8, showery .	Islington, Hoxton, Dalston, and Hackney	26·701	61·108
„ 9, dull . .	Bethnal Green and Stepney .	41·534	95·057
„ „ „ . .	Large yard near St. Katharine's Docks	23·300	53·326
„ 10, morning; cold and damp	London Bridge . . .	27·419	62·752
Nov. 9, dull . .	The Bank of England . .	29·074	66·540
„ 6, fine . .	Westminster Abbey Yard .	20·767	47·530
„ 13, windy	Embankment of Parliament Houses	21·119	48·334
„ 10, fine . .	Back street near Lambeth Workhouse	88·700	203·003
„ „ „ . .	New Kent Road, Pleasant Place, Kennington Park	24·921	57·027
„ 6 „ . .	Near Vauxhall Bridge . .	16·614	38·022
„ „ „ . .	Cavendish Place, Wandsworth Road	29·074	66·540
„ 13, very strong wind	A field two miles past Clapham Junction	29·613	67·772
	Average . . .	26·780	61·286

Air of the Underground Railway (Metropolitan), November 1869.—*The Air was washed in the Train passing up and down between South Kensington and Moorgate Street.*

Date and Time of Day.	Ammonia.	
	Grains per Mill. Cubic Feet.	Grammes per Mill. Cub. Mèts.
November 11 and 12, morning . . .	47·927	109·681
November 15, 1 to 3 P.M.	15·196	34·777
Average	31·561	72·229

Glasgow Air-Washing.

Air obtained from	Weather.	Date, 1870.	Ammonia.	
			Grains per Mill. Cubic Feet.	Grammes per Mill. Cub. Mèts.
A green in Elmbank Street, near St. Vincent Street	S., thawing, snow on ground	Feb. 26 .	34·614	79·214
Union Street, near Argyle Street	WSW., fine	March 1 .	41·537	95·057
Charlotte Street, Gallowgate	Fine .	„ 2 .	44·504	101·847
Finnieston Quay .	WSW. .	„ 1 .	16·021	36·665
Average . .	. .	. .	34·169	78·196
Shore, Innellan, Firth of Clyde, wind from NE.	NNE. wind.	March 3 .	22·845	52·281

Everything here is not as expected, but I cannot doubt that strict enquiry would show that some of the experiments were incorrect, or made on unfair specimens of air. The air at Innellan appears too high. At the time the wind was not from the sea or from the hills, but blowing along the shore, which will not give the best. It suits

very well to compare with Glasgow, but the error seems to be in the specimen from Finnieston, which we must suppose to be too low. It is quite in a busy part of the town. We must not, however, feel certain of any error; the district, although full of workshops, is not densely inhabited. I expected emanations from the river even in March, but it appears that they were not found, and the experiments tend to show rather a rush of fresh air up the river into the town.

Ammonia, Albuminoid or Potential.

It has already been explained that this is the ammonia which comes from organic substances, either alive, as animals and vegetables, or dead and in decay. This substance is therefore the most important of the chemical constituents. We must not, however, draw too many conclusions from its presence. The organic matter is sometimes quite sound, and so far from producing disease it may help to drive it away. All that we can say is, that organic nitrogenous matter shows that some organisms of a hurtful kind may be present. If there is no excess of albuminoid ammonia, the hurtful organisms or other matter may be presumed to be absent. If there is an excess, it is well to avoid such air until we know that it is from a source which produces not only that which is wholesome but which continues to be so. The results obtained regarding albuminoid ammonia are as beautiful as those in the inorganic ammonia column. The quantity is such as we might expect, and is increased by population. Living beings raise the numbers, but still more their abrasions and offscourings. By this enquiry we can test the amount of ventilation either of a close or open situation.

Albuminoid Ammonia.

Air obtained from	Time and Weather.	Albuminoid Ammonia.	
		Grains per Million Cubic Feet.	Grammes per Million Cubic Mètres.
Laboratory office, 10 A.M. .	. . . .	116·525	266·667
„ „ 4 P.M. .	. . . .	128·177	293·333
Gas-room, 10 A.M. . .	. . . .	93·458	213·878
„ 4 P.M. . .	. . . .	186·916	427·756
Yard behind laboratory .	Fine, Oct. 28, 1869 .	41·538	95·060
„ „ .	Freezing, snow on the ground, Dec. 28	155·764	356·464
„ „ .	Damp, Jan. 13, 1870	93·458	213·878
„ „ .	Fine „ 25 „	138·457	316·857
„ „ .	Foggy „ 26 „	96·920	221·800
Street, open . . .	Raining and strong wind, Dec.	114·227	261·407
		116·544	266·710
Bedroom, 9 P.M. . .	Feb. 15, 1870. .	83·074	190·114
„ 7 A.M. . .	„ 16 „ .	146·210	334·601
„ „ . .	Nov. 1, 1869 .	83·070	190·114
		104·118	238·276
Midden	Fine, cold, Oct. 27 .	233·050	533·333
„	„ „ „ 28 .	207·683	475·280
„	„ „ „ 29 .	103·841	237·640
		181·524	415·417

Air of the Underground Railway (Metropolitan), November 1869.—*The Air was washed in the Train passing up and down between South Kensington and Moorgate Street.*

Date and Time of Day.	Albuminoid Ammonia.	
	Grains per Million Cubic Feet.	Grammes per Million Cubic Mètres.
November 11 and 12, morning . . .	199·697	457·005
November 15, 1 to 3 P.M.	126·637	289·808
Average	163·167	373·406

Air-Washings of London.—For Ammonia (Organic or Albuminoid).

Date and Weather.	Air obtained from	Albuminoid Ammonia.	
		Grains per Million Cub. Feet.	Grammes per Mill. Cub. Mèts.
Nov. 4, windy . .	Chelsea (three places) . .	48·180	110·300
„ „ „ and a shower of rain	Brompton (three places) .	56·125	128·320
Nov. 5, morning; fine	Hyde Park (two places) .	37·965	86·890
Nov. 8, dull and windy	King's Cross (two squares)	58·148	133·080
Nov. 11, frosty . .	Woburn Square and off Regent Street	58·148	133·080
„ „ „ . .	Space near Holborn Viaduct	66·455	152·091
„ 8, showery .	Islington, Hoxton, Dalston, and Hackney	65·286	149·371
„ 9, dull .	Bethnal Green and Stepney .	83·086	190·114
„ „ „ . .	Large yard near St. Katharine's Docks	68·674	157·168
Nov. 10, morning; cold and damp	London Bridge . . .	61·166	139·987
Nov. 9, dull . .	The Bank of England . .	62·301	142·585
„ 6, fine . .	Westminster Abbey yard .	37·381	85·551
„ 13, windy .	Embankment of Parliament Houses	71·805	164·336
„ 10, fine . .	Back Street near Lambeth Workhouse	105·595	241·670
„ „ „ . .	New Kent Road, Pleasant Place, Kennington Park .	63·686	145·735
„ 6, „ . .	Near Vauxhall Bridge . .	66·455	152·091
„ „ „ . .	Cavendish Place, Wandsworth Road	58·148	133·080
„ 13, very strong wind	A field two miles past Clapham Junction	118·451	271·088
	Average . . .	65·947	150·919

Glasgow Air-Washing.—Albuminoid Ammonia.

Air obtained from	Weather.	Date, 1870.	Albuminoid Ammonia.	
			Grains per Mill. Cubic Feet.	Grammes per Mill. Cubic Mètres.
A green in Elmbank Street, near St. Vincent Street	S., thawing, snow on ground.	Feb. 23 .	117·688	269·328
Union Street, near Argyle Street	WSW., fine	March 1 .	178·016	407·387
Charlotte Street, Gallowgate	Fine .	„ 2 .	112·743	258·012
Finnieston Quay .	WSW. .	„ 1 .	124·611	285·171
Average . .	. .	. .	133·264	304·974
Shore, Innellan, Firth of Clyde.	NNE. wind	March 3 .	60·228	137·832

Air-Washing.—Summary of Results.—Ammonia (Average Free and Albuminoid).

Air obtained from	Number of Experiments.	Free Ammonia.		Albuminoid Ammonia.	
		Grains per Million Cubic Feet.	Grammes per Million Cubic Mètres.	Grains per Million Cubic Feet.	Grammes per Million Cubic Mètres.
Innellan . .	1	22·845	52·281	60·228	137·832
London . . .	18	26·780	61·286	65·947	150·919
Glasgow . .	4	34·169	78·196	133·264	304·974
A bedroom . .	3	44·305	101·394	104·118	238·276
Inside and outside office at Manchester	10	53·582	122·622	116·544	266·710
Underground Railway (Metropolitan)	2	31·561	72·229	163·167	373·406
A midden . .	3	146·911	336·204	181·524	415·417

Air-Washing.—Average Total Ammonia.

Air obtained from	Total Ammonia.	
	Grains per Million Cubic Feet.	Grammes per Million Cubic Mètres.
Innellan	83·073	190·113
London	92·727	212·205
A bedroom	148·423	339·670
Glasgow	167·433	383·170
Inside and outside office	170·126	389·332
Underground railway (Metropolitan)	194·728	445·635
Midden	328·435	751·621

Summary of Comparative Results.

The actual meaning of the analyses will be seen from the following tables, better probably than by anything that has yet been said.

Air—Total Acid.

Relation to Blackpool taken as 100.

Blackpool[1]	100
Didsbury[2]	282
Buxton	327
London	348
St. Helen's	488
Manchester	498
Underground Railway (Metropolitan)	1483

Air—Hydrochloric Acid.

Relation to Blackpool taken as 100.

Blackpool	100
Didsbury	277
London	320
Manchester	396
Buxton	415
St. Helen's	516
Underground Railway (Metropolitan)	974

[1] Lancashire coast.

[2] Four miles S. of Manchester.

Air—Sulphuric Acid (Anhydrous).

Blackpool	100
Didsbury	282
Buxton	315
London	352
St. Helen's	484
Manchester	513
Underground Railway (Metropolitan)	1554

Air—Total Ammonia.

Relation to Innellan taken as 100.

Innellan[1]	100
London	112
A bedroom	179
Glasgow	202
Inside and outside of office	205
Underground Railway (Metropolitan)	234
A midden	395

Air—Ammonia.

Relation to Innellan taken as 100.

Innellan	100
London	117
Underground Railway (Metropolitan)	138
Glasgow	150
A bedroom	194
Inside and outside of office	235
A midden	643

Air—Albuminoid Ammonia.

Relation to Innellan taken as 100.

Innellan	100
London	109
A bedroom	173
Inside and outside of office	193
Glasgow	221
Underground Railway (Metropolitan)	271
A midden	301

[1] Firth of Clyde.

The results in this part being the most striking, and at the same time remarkably consistent with theory, I cannot doubt that the method will be used daily in examination of air in all cases in which the unaided senses feel it difficult to decide.

Results showing the extent of Accuracy attained by using Messrs. Wanklyn, Chapman, and Smith's Modification of Schulze's Process for the Determination of Nitrites and Nitrates.

Milligrammes. HNO_3 taken as KNO_3.	Milligrammes. HNO_3 found and corrected.	Error.
·622	·630	+ ·008
·622	·630	+ ·008
·311	·296	− ·015
·311	·296	− ·015
·187	·196	+ ·009
·187	·185	− ·002
·062	·074	+ ·012
·062	·055	− ·007
·062	·074	+ ·012

The nitric acid found was corrected by subtracting from each result ·037 mgm. HNO_3, the amount usually obtained when a piece (about ·5 grm.) of aluminium was dissolved in pure soda solution, as the following result shows. The mean of the results was not used, because some of the experiments were made after ·037 had been taken as the error in the determinations of nitric acid in rain-waters.

The reason for subtracting ·037 was found by several trials. A solution of caustic soda, from which all ammonia was boiled off that might happen to be present, was allowed to act on aluminium. After the action the solution was again boiled, and ammonia was found corresponding to ·037 nitric acid. In other words, whether by absorption of ammonia from the air or by the action of the nitrogen, or some unobserved peculiarity in working, an

unavoidable quantity remained. It would probably be ·039, as some later trials indicated, but the ·037 was adopted from earlier trials, and there seemed no reason to change it because of the small difference shown in this series.

The experiments were made as nearly as possible like others for the determination of nitrites and nitrates.

Milligrammes.	HNO_3.	
·0370	·0559	·0278
·0370	·0370	·0445
Average . . .	·0399	

'CARBON IN THE AIR.[1]

'By examination of several chimneys, I came to the conclusion that about 1 per cent. weight of the coal used is sent off in the condition of carbon or tarry matter. This will be equal to nearly 60 tons a day for Manchester. Supposing it to be 60 tons, and a space 600 feet high filled equally with the vapour and carbon, allowing a thorough change twenty times a day, then only 3 tons would exist in suspension at a time in the atmosphere. This would be only 1 grain in 5,689 cubic feet, or 1 grain in a cube of rather less than 18 feet (17·852).[2] This is sometimes more, sometimes less, than the truth, in all probability. It is about the truth for a very clear day. The half of this will be more frequently true. With 1 in 5,689, or in the best seasons, we should breathe a grain of soot in twenty-one days: a small quantity, but it is given in irregular doses, and, as it has a very large surface, a grain appears very large. The carbon separates from the rest of the smoke much sooner than the gases, and is often seen floating at a long distance from its source, when all the gaseous matter must

[1] From paper 'On the Air of Towns,' pp. 35, 36, Chem. Soc. 1858.

[2] This is assuming a space of sixteen square miles of surface.

have been removed, as if it were the dark skeleton of the smoke alone, without the vitality of diffusion to decide upon any course.'

'TARRY MATTERS IN THE AIR.[1]

'Besides the carbonic acid, as a result of the combustion of coal, the products of distillation must also be considered. In one case I found exactly one-half of the carbonaceous matter of the smoke to be volatile. 100 cubic feet gave 8 grains of soot, 4 grains being volatile. If so, the amount which I have given for the carbon and tar, &c., may be stated thus:—

½ per cent. of the weight of coal used given off as carbon.
½ " " " tar, and other volatile products of coal, all of which may be included.
Or 30 tons of tar per day, and 30 of soot,
The numbers gave from ·83 to 1½ tons of tar per day, and ·83 to 1½ of soot in the atmosphere at a time.

'Now, I have ascertained by experiment that some of these products are capable of decomposing manganates, and, when judging of the unwholesomeness of a district by the amount of organic matter, care must be taken not to be misled by this. The error cannot be high, because if there be one-half of tarry matter, capable of decomposing a manganate, to 100 of coals, then the 99 of coals taken as carbon, which is near enough for the case, will give 363 of carbonic acid, or there will be only 1 of tarry matter to 726 of carbonic acid. The acid must therefore increase enormously before its accompaniment, the tar, can produce any effect. Besides, only a portion of this tarry or distilled matter is decomposable during the time of an ordinary experiment. It must not, however, be forgotten.'

[1] From 'On the Air of Towns,' pp. 36, 37.

'EFFECT OF THE ATMOSPHERE ON STONES, BRICKS, MORTAR, &c.[1]

'It has often been observed that the stones and bricks of buildings, especially under projecting parts, crumble more readily in large towns, where much coal is burnt, than elsewhere. Although this is not sufficient to prove an evil of the highest magnitude, it is still worthy of observation, first as a fact, and next as affecting the value of property. I was led to attribute this effect to the slow, but constant, action of the acid rain. If it affects substances with so great an excess of silica, it is not to be expected that calcareous substances will resist it long, and one of the greatest evils in old buildings in Manchester is the deterioration of the mortar. It generally swells out, becomes very porous, and falls to pieces on the slightest touch. Some mortar in this condition, from a building behind the house of the Literary and Philosophical Society of Manchester, was examined.

'9·18 grs. of this gave 7·57 of $BaOSO_3$, or 28·33 per cent. of sulphuric acid = 48·16 per cent. of sulphate of lime.

'It is not to be wondered at that iron oxidises readily, and that galvanised iron is valueless in a district where the acid rain converts it at once into a battery. It will be observed that this style of roofing is preserved in exact proportion to its distance from manufacturing districts.

'Iron by itself also becomes readily oxidised in this acid atmosphere. Bronze, too, is rapidly blackened, and articles of brass become affected to a great depth, losing their strength. I suppose the sulphurous acid forms on the surface a coating of sulphide of copper, whilst a sulphate is washed away if exposed to rain.'

[1] From 'On the Air of Towns,' p. 37, 1858.

'CARBON ON THE SURFACE.[1]

'The smoke of large towns is guilty of an offence to the eye, and through the eye it offends us both intellectually and æsthetically; in other words, the darkness and gloominess react on the character, especially of those not accustomed to the place, in such a manner as to make them distinctly conscious of a change. Those accustomed to it are not conscious of the effect; nevertheless it acts upon them in such a way as to destroy some of the finer instincts of perception of natural beauty. But this is a long subject and a difficult one, leaving room for much difference of opinion. The actual amount of carbon on Manchester buildings, although apparent, is very small by weight. It collects on the rough surfaces principally, and rough mortar soon becomes perfectly black, even when the red of the brick remains moderately clear. Rough bricks also take it up in greater quantity than smooth, and become black in proportion to their roughness.

4·4 × 4, or 17·6 square in., gave of carbon ·17 grains.
3 × 2·7, or 8·1 „ „ ·02 „

The last is equal to 320 grains—about three-quarters of an ounce or 21 grammes—on a wall 30 feet by 30. This is, I believe, a great deal above the mark; at least, I believe, a house will appear dingy with a minute portion of this. The experiment might be performed on a larger scale.

'The fact that the rough portions retain the carbon suggests a cure—viz., smooth bricks. Polished or glazed bricks and similar mortar would render the rain capable of washing the carbon off, but certainly it will be much better not to allow it ever to arrive there. The importance of preserving the beauty of the original materials is daily increasing.'

'A cure for some of these evils ought certainly to be

[1] From 'On the Air of Towns.'

found. Already one cure for the evils connected with organic matter has been made known to us, and that is the removal of the impure matter by means of water. If disinfection were added to this, it might be made complete. The cure of the black carbon, or smoke-burning, is in the hands of every man. The smoke is always blacker as coal is cheaper. Our towns are not in earnest on the subject.

'Mr. Holme, of Manchester, claimed the use of lime and of common salt mixed with the coals for removing smoke, but, finding lime of no value in giving the peculiar white colour to the smoke, he gave it up, and used salt only. On examining the effect of the salt upon the vapour, I found that it diminished the quantity of the sulphurous acid given off, and on examining the effect on the ashes, I found a greater amount of sulphur in them than when otherwise treated. I give here the result of a few experiments made to ascertain the action of salt and coals.

'Amount of sulphur driven off from a specimen of coal by distillation :—

Coal alone.

1.	At a low red heat . . .	·4692	per cent. of sulphur.
2.	At a higher heat . . .	·5655	„ „
3.	At nearly white heat . .	·6755	„ „

Coal with Common Salt.

4.	With 5 per cent. salt	·4526	per cent. of sulphur.
5.	„ „ at higher heat . .	·4843	„ „
6.	„ „ nearly white . .	·5557	„ „

'This makes a difference of 18 per cent. in the amount of sulphur sent off, the least being sent off by the mixture of common salt. It has, in fact, been long known that sulphur decomposes common salt, at least when the elements of water can be readily supplied, but still more may we expect it when carbon, &c., assist.

'The remainder, or cinder, contained—

From No. 3	·4332 per cent. of sulphur.
From No. 6	·5448 „ „

'Finding this result, it occurred to me that lime would be a much more efficient substance for retaining sulphur, the salt, on account of the white fumes given out, being entirely inadmissible. I distilled some coal with the following result:—

	Sulphur.
Coal distilled alone . . .	·4338 per cent. in the distillate.
„ with 5 per cent. lime . .	·1754 „ „
„ 10 „ „ . .	·0511 „ „
„ „ „ „ . .	·0616 „ „

I fear, however, there is nothing practical here.

'DUST IN RAILWAY CARRIAGES.

'When sitting in a railway carriage with a friend (Mr. James Young, of Bathgate), that gentleman observed that the particles of dust which floated in the air seemed to shine with a metallic lustre. I immediately collected some, and found that the larger class were in reality rolled plates of iron, which seemed to have been heavily pressed and torn up from the surface. Another and smaller class were less brilliant, and when looked at with a considerable power showed many inequalities of surface, which would be interesting to study. Probably these were the particles which were not torn up but rubbed off. The dust enters the mouth and lungs, and has to be taken as one of the evils of railway travelling, although we do not know that these small particles are worse than those of sand. At any rate, it is clear that some kind of iron will wear down more readily than others, and we ought to have that which will wear down least. By observing what takes place in the carriages on a dusty day,

every man may to some extent compare the iron of different railways. Those which give off the largest pieces in greatest quantities, are to that extent the worst, as regards health. The shareholders may calculate the endurance.'[1]

MODE OF ESTIMATING THE CARBONIC ACID OF THE AIR.

The method now used for very exact determination is called Pettenkofer's in its mature form, but, as with most other inventions, its early and later years have not been spent in the same place.

Dalton, in 1802, used a bottle filled with 102,400 grains of rain-water, and says that if it 'be emptied in the open air, and 125 grains of strong lime-water poured in, and the mouth then closed, by sufficient time and agitation the whole of the lime-water is just saturated by the acid gas it finds in that volume of air. But 125 grains of the lime-water used require 70 grain measures of carbonic acid to saturate it.'[2] The carbonic acid he found equal to ·1 per cent. by weight. Dalton at that early period wrote correcting the estimate by Humboldt, which was 1 per cent.

In 1830 Mr. William Hadfield made experiments, which were published in the sixth vol. of the same Society's memoirs. He says, 'Mr. Dalton found in his enquiry into the properties of the several gases constituting the atmosphere, that the quantity was only one 1460th in volume, or 1,000th in weight, and since that time M. Saussure, of Geneva, has made abundance of experiments on the same subject, the result of which seems to show that the quantity is somewhat less than that just assigned.'

[1] 'Proceedings of the Lit. and Phil. Soc. of Manchester,' Nov. 1865.

[2] 'Mem. of the Lit. and Phil. Soc. of Manchester,' vol. 1, second series. Paper read 1802.

'M. Saussure's method is to procure a large glass globe of known capacity (one cubic foot or more), into which he passes a quantity of barytic water, more than sufficient to neutralise the carbonic acid in that volume of air. By agitation the carbonic acid is united to the barytes, forming an insoluble carbonate of the earth. The quantity of carbonate so formed, being carefully ascertained, affords data for determining the quantity of carbonic acid.

'Mr. Dalton considers a globe of one-fifth of the size sufficiently ample, and uses lime-water of a well-known strength instead of barytic, taking care to have more than enough to engage the acid gas; after the agitation and absorption, the residue of lime-water is poured out, and its reduced value is then ascertained, as it was before, by means of some test acid of a known strength. Thus data are gained for the calculation of the carbonic acid engaged to the lime.

'In my investigations of this subject I have adopted Mr. Dalton's mode, and from December 1828 to 1830 the experiments have been made in a glass bottle of a balloon shape, of the capacity of 471 cubic inches, fitted with a brass cap and stop-cock for the purpose.

'The experiments of the present year (1830) have been made in a larger bottle, of the capacity of 498 cubic inches. The method of filling the latter bottle with air was a little different from that of the former, for, instead of filling the bottle with rain-water, as was the case with the first bottle, to get the air in, the end of a bellows-pipe was introduced into the latter and the air blown in.'

Mr. Hadfield was a pupil of Dalton's, and learned the practice that Dalton had begun, using lime-water to shake with the air, and afterwards sulphuric acid for ascertaining the amount combined. He obtained ·80

volume carbonic acid per 1,000 of air. This is high for air generally, and, although he lived at Cornbrook, on the banks of a putrid canal, it must be much too high.

Pettenkofer, of Munich, lately took up the subject, at first using lime, afterwards baryta, to remove the carbonic acid from the air, and oxalic acid to test the solution. The bottles in which the experiments were made were dried with great care, and the solutions of oxalic acid made very delicate. One cubic centimètre of this solution was made equal to a milligramme of lime, but it may also be made equal to a cubic centimètre of carbonic acid. The strength, however, must vary with the air. If the air is very bad, it is more convenient to use a stronger solution, as the amount required of a very weak one would be inconveniently large. This change of solution according to circumstances is rather troublesome. An excess of baryta must be used, and well shaken with the air; the solution remaining is then tested with oxalic acid, to see how much is left uncombined. The point of neutralisation is found by putting a drop of the liquid on a piece of turmeric paper. The use of the oxalic acid renders the test more exact, as it is not apt to decompose the carbonate, and it makes this method Pettenkofer's.

The bottle I use is somewhat different in shape from Pettenkofer's; it has a very wide mouth—such a jar, in fact, as is used by confectioners for sweetmeats. The use of the wide mouth is to allow the hand to enter, so as to clean and dry the jar; this is done with a pure linen cloth, which has been well washed, first in acid and then in distilled water, till neutral. The use of the hand and cloth saves a great deal of trouble, and repeated analyses have proved that from neither does any hindrance to accuracy arise. Instead of blowing air into the vessel, I draw it out with a flexible bellows-pump. In this I think I gain an advantage. When the bottle is cleaned



For Carbonic Acid, by Pettenkofer's process

and dried, the baryta is added, and the elastic cover is then put on. (See drawing of the bottle and bellows.)

All the experiments given in this volume, and all made for purposes requiring great exactness, are by this method. The convenient and easy methods given for daily life do not aspire to scientific accuracy.

COAL-SMOKE.

FROM 'REPORT UNDER THE ALKALI ACT,' FOR 1868.

CIRCUMSTANCES led me to examine the condition of the common smoke. It may be said not to lie in my province, but the escape of muriatic acid into it occasionally was the cause of my attention. It was also interesting to know if, by analysing the escaping gases or smoke, the actual amount of air passing up could be found. It will, I think, be useful to many if I give an account of the results in full, and first I shall speak of the ordinary coal-smoke only. The experiments were made at the sugar-refinery of Messrs. Fryer, Benson, and Forster, who afforded every facility, and allowed me to make the blackest smoke occasionally for a few minutes; the same permission being given by the city authorities.

The height of the chimney is 200 feet. The earlier specimens of smoke were taken from the lower part, the later from a height of about 30 feet, where a hole was pierced. The speed of flow was 12 feet per second; the diameter of the chimney at this part was 10 feet internally; the amount of coal burnt was 35 tons per day. There were six boilers, and many other fires. When taken from the lower part the following results were obtained in 1867 :—

Carbonic Acid.	Carbonic Oxide.	Oxygen.	Nitrogen.
6·03	—	13·89	80·08
6·46	—	11·23	82·31
6·09	—	12·14	81·77
7·93	—	11	81·07
5·69	—	14·04	80·27
7	·72	12·34	79·94
8·52	4·41	9·31	77·76
4 42	—	18·20	77·38

These amounts were viewed as representing the smoke from the boiler-ranges, and they have been published in a former report. It was found, however, that, as various flues entered the chimney, the gases had not become properly mixed, and the speed could not be ascertained. If carbon alone were burnt, the amount of oxygen and carbonic acid would amount to 21, or within a small fraction of it; but, as hydrogen is burnt also, there is an equivalent of oxygen removed, and the two gases form the watery vapour which is found in coal-smoke, independently of that which is obtained from the air which supplies the fire.

In November 1868 other specimens were taken, with results considerably different. It would seem that much more air was then passing up the chimney. Before taking these specimens of black smoke, coals were thrown on rapidly, and the doors shut, so as to consume as little of the smoke as possible.

The amounts on November 21, 1868, were as follows:—

BLACK SMOKE.

Sugar-Works, Large Chimney.—From the Lower Opening, November 21, 1868.

Gases.	1.	2.	3.	4.
Carbonic acid, CO_2 . .	7·67	7·47	7·31	7·13
Carbonic oxide, CO . .	None	None	None	·52
Oxygen, O	12·61	8·11	10·59	12·93
Olefiant gas, &c. C_nH_{2n} . .	None	None	None	None
Nitrogen, N	79·62	84·42	82·10	79·42
	100	100	100	100

Here only one of the four specimens contained carbonic oxide. The blackest smoke was not formed on the first trial. The specimens were taken in Bunsen's tubes;

the tubes were narrow at the extremities where they were connected, and there was a great difficulty in drawing out the gas. A cubic foot was passed through in order to clear the tubes. This was done by connecting a caoutchouc aspirator with the external one and drawing the aspirator open; a process which required considerable strength, both on account of the small size of entrance to the tubes, the pressure of the atmosphere, and the chimney current.

Another specimen was taken on November 5.

Black Smoke.

Sugar-Works, Large Chimney.—Samples taken at the Bottom of Chimney. November 5, 1868.

Gases.	1.	2.	3.
CO_2	7·25	7·09	7·07
CO	3·80	4·46	6
O^1	7·41	7·57	7·92
C_nH_{2n}	None	None	None
N	81·54	80·88	79·01
	100	100	100

Here we have carbonic oxide in every specimen, and in none of them a compound of hydrogen.

Another specimen was taken on November 26.

This time the opening to the chimney was made about 30 feet above the ground, so as to obtain a better mixture of gases.

[1] O found by absorption with pyrogallic acid in this instance.

Black Smoke.

Sugar-Works, Large Chimney.—Samples taken from the Opening, 30 feet above the ground, November 26, 1868.

Gases.	1.	2.	3.	4.
CO_2	6·17	6·75	5·62	6·08
CO	1·55	·48	·02	·57
O	12·22	12·36	13·24	12·24
Hydrogen, H	None	None	None	None
Marsh-gas, CH_4	,,	,,	·77	,,
$C_n H_{2n}$	·13	,,	None	,,
N	79·93	80·41	80·35	81·11
	100	100	100	100

In every one of these carbonic oxide is found, and in two a carburetted hydrogen also.

A specimen was taken on December 2.

Black Smoke.

Sugar-Works, Large Chimney.—Samples taken 30 *feet above the ground, December* 2, 1868.

Gases.	1.	2.	3.	4.
CO_2	6·64	6·32	6·38	6
CO	·40	·98	·20	·34
O[1]	11·17	11·86	12·17	12·24
H	None	None	·46	None
CH_4	·02	·78	·52	·54
$C_n H_{2n}$	None	None	None	None
N	81·77	80·06	80·27	80·88
	100	100	100	100

Here, in the blackest specimens, carbonic oxide is found along with a hydrogen compound in all.

It seemed remarkable that so much air should go away unburnt, and, thinking that this might be connected with exceptional conditions, specimens were taken

[1] O found by absorption with pyrogallic acid.

from a smaller chimney, which served for very few fires. The results are :—

BLACK SMOKE.

Sugar-Works, Small Chimney.

Gases.	Samples collected Dec. 21, 1868.		Samples collected Dec. 24, 1868.	
	1.	2.	1.	2.
CO_2	3·51	3·89	2·84	3·77
CO	·68	·59	None	·55
CH_4	·45	·41	,,	None
O	13·54	14·08	18·46	16·52
N	81·82	81·03	78·70	79·16
	100	100	100	100

It was next needful to find again if any carbonic oxide escaped with common moderately brown smoke. None had been found on any previous occasion, and specimens were taken from the upper hole in the large chimney, giving —

COMMON BROWN SMOKE.

Sugar-Works, Large Chimney.—Opening 30 feet high.

Gases.	Samples collected Dec. 9, 1868.				Samples collected Dec. 15, 1868.			
	1.	2.	3.	4.	1.	2.	3.	4.
CO_2	4·26	4·14	4·25	4·38	5·05	5·06	5·19	5·34
CO	None.	None.	None.	None.	None.	None.	None.	None.
H	,,	,,	,,	,,	,,	,,	,,	,,
CH_4	,,	,,	,,	,,	,,	,,	,,	,,
N	79·11	80·02	80·92	82·11	80·54	80·35	80·18	79·95
O	16·63	15·84	14·83	13·51	14·41	14·59	14·63	14·71
	100	100	100	100	100	100	100	100

The smaller chimney was next examined for the same purpose, and it gave, December 21—

Common Smoke.

Sugar-Works, Small Chimney.

Gases.	Collected December 21, 1868.	
	1.	2.
CO_2	2·53	2·08
CO	None	None
CH_4	„	„
O	18·61	17·7
N	78·86	80·22
	100	100

It seems perfectly clear that the black smoke contains not only unburnt carbon, but unburnt gases. The actual amount of free carbon is small; the amount of gas is large.

Allowing one half per cent. of carbonic oxide, and as much marsh-gas to exist along with seven per cent. of carbonic acid, the amount actually obtained may be calculated thus:—

				Grammes.
7	litres carbonic acid	in 100 of	black smoke =	13·76648
·5	„ carbonic oxide	„	„	·62575
·5	„ marsh-gas	„	„	·35779

The heat may be calculated thus:—

	Units of Heat.
3·754494 carbon burning to carbonic acid × 8,080 =	30,336
·2681 „ „ oxide × 2,473 =	663
Total heat produced	30,999
The carbonic oxide ·62575 × 2,403 would give .	1,503
The ·35779 marsh-gas × 13,063 would give .	4,673
Total heat if the gases were all burnt .	37,176

When the carbonic oxide only is left unburnt, there is a loss of 4 per cent. on these amounts. The hydrogen of the coal was not estimated. During the time that the marsh-gas and other hydrocarbons escape there is a loss of 12·6 per cent. This, however, can last for only a short time.

Of course the black smoke cannot form after the more volatile parts of the coal are driven off, so that the percentage is not on the whole coal, most fortunately.

Carbonic oxide exists only in connection with the black smoke not permitted by Parliament, and it is fortunate that it should be put down, as it is one of the most poisonous of all gases.

Another point comes out clearly. It is not from want of air or oxygen that the black smoke is not burnt, the amount of air here is found far in excess of that required. Now patentees have too much confined themselves to the supply of air, a commodity easily given but not so easily used; the want is in reality heat. The air is present, but it is carried away so rapidly along with the gases that it has no time to combine. It seems to be a matter of time. The reason for this opinion lies in the following experiments, published two years ago (in 1866), but better placed here:—

From a Common House-Fire.

Source of Gas.	Carbonic Acid.	Carbonic Oxide.	Oxygen.	Nitrogen.
Gas from chimney 4 ft. above the fire-place	·35	—	19·63	80·02
	1·65	·38	19·29	78·68
Gas from the middle of a good fire	19·46	·09	—	80·45
	20·90	·10	—	79
A great mass of coal over the fire, the gas taken from below the glowing mass	17·50	—	2·46	80·04
	17·44	—	·39	82·17
A heap of glowing coal, gas taken close to the spot where carbonic oxide was burning	15·43	3·49	·96	80·12
	18·17	2·48	—	79·35
Gas from the clear fire below .	16·10	—	4·95	78·95
Gas out of the same fire at the upper part, one inch below the surface	17·21	—	4·25	78·54
	20·80	·99	—	78·21

Carbonic oxide sometimes escapes even here.

We find that the slow combustion of a common fire

allows less air to pass, nearly the whole of the oxygen being taken up. This is quite in agreement with the known desire to increase boiler room, as it is technically termed, in order to increase combustion. The fires are slow, but the gases escaping are better burnt.

This seems in contradiction to the desire expressed to increase the draught of chimneys in order to produce combustion. The wish may be explained by the fact that heat is increased, and combustion may be attained, in two methods—one by increased heat for a short time, and the other by moderate heat for a longer time.

There is another mode of explaining the non-combustion of the gases. It may be that the air does not mix with them until it has been tossed about for some time. Indeed, we know for certain that flues may contain muriatic acid on one side and not on the other. In this case the necessity for heat, mixture and time is obvious.

It must not be supposed that it is meant that in all cases there is an excess of air when there is black smoke; we might expect quite the contrary, but black smoke may and does exist with an excess of air. In flues, from puddling furnaces especially, it seems to me that time and air only are wanting, but especially air well mixed, in order to obtain combustion.

I do not intend to propose any plan for smoke-burning, although the experiments point clearly in the direction of keeping the gases longer heated, and it seems to me that I could point out instances in which this is successful.

There are several furnaces which burn smoke perfectly, although, taking all respects, I do not know any furnace which has no disadvantage. It is seen that slow combustion with abundance of air produces no carbonic oxide; but carbonic oxide is produced with rapid combustion, even if the air be in excess, we must add, unless heat be continued. But it may also be formed by decom-

posing the carbonic acid by excess of carbon. The carbonic oxide found in black smoke was not formed by this method, because there was oxygen enough present. By the present methods of burning coal under boilers air is abundant in all cases examined, but heat is not continued so long as to cause perfect combustion in most cases.

Blast furnaces and similar fires are not included here. These furnaces give out carbonic oxide in great abundance, and the oxygen is altogether removed, as the analyses by Bunsen and Playfair show. The gas from these furnaces is taken down and burned with excess of air in many cases now, thereby removing a most poisonous gas and gaining fuel of great value.

Other constituents of the smoke are water, soot, and sulphurous acid.

Black Smoke.

Water, Soot, and Sulphurous Acid.

100 cubic feet of Black Smoke contained —

Date.	Water.	Soot.	Sulphurous Acid.
	Grains.	Grains.	Grains.
November 18, 1868 . . .	—	—	33·48
„ 20 „ . . .	797·41	—	—
„ 21 „ . . .	1047·44	18·52	—

The water depends on the amount of hydrogen in the coal; the sulphurous acid on the amount of sulphur in the coal. The soot contained a tarry substance of a brown colour; it was dissolved out with ether. I suppose that carbolic acid, and other products of the distillation of coal, exist there; indeed it is almost certain, although, perhaps, it would be useful to make some more enquiry into this substance.

We learn from these experiments why people prefer to make black smoke when they want a violent and sudden

heat. The amount of carbonic acid in the black smoke is greater than in common smoke, and so more coal is actually burned in a given time, and therefore more heat is given. At the same time the amount of waste is greater, and the gases are more hurtful. This has long been known, although not in this way proved.

It seems to be the desire of all coal-burners to obtain a violent draught. Some persons have attempted to make the draught regulate itself by dampers, moved as the heat rises or falls. As we see that gases of the most combustible kind are formed by the coal, it is clear that, if the draught is obstructed, the fireplace and flues will fill up with these gases, which will burn with a rapidity rising up to an explosion when fresh air is allowed to enter. I have seen the result of two or three cases in which an explosion from this cause, or from a similar obstruction, had taken place, and have been led to speculate on the effect of one, even a small one, if it happened at a time when the boiler was in a state of considerable tension. It is exceedingly probable that the shock from below would cause an answer from within, which would burst the boiler. Whilst fully believing that too much air is taken up the chimneys, and an enormous loss of heat caused thereby, I am no less persuaded that there is an excess of gas occasionally in the fireplace, especially at the front, and momentary obstruction may extend it along the flues. A moment is enough when the speed is above 12 feet in a second.

Whenever there is black smoke there is water in the smoke, because the black carbon is deposited from hydrogen compounds, which burn readily. Pure hydrogen uncombined was not found. Sulphuretted hydrogen also seems to be always absent. Sulphur does certainly come from coals in combination with hydrogen, but it is so easily separated that it never reaches the flues. There

are several attacks on the hydrogen and sulphur compound, fortunately for us, as otherwise our towns would really be uninhabitable when fires were smoking. Sulphuretted hydrogen is decomposed by heat. When this occurs in the absence of oxygen the solid sulphur is deposited; but, as we see, there is always air enough in the smoke for this event, and so the sulphur burns; this is the chief source, if not the only source, of the sulphurous gases arising from smoke. If any of the sulphuretted hydrogen remained unburnt, the sulphurous acid would itself decompose it, forming a deposit of readily combustible sulphur, and increasing the amount of the sulphurous acid. Then the gas itself is readily combustible, and forms sulphurous acid and water by burning. For these reasons that dangerous gas, sulphuretted hydrogen, is not allowed to pass up our chimneys when burning coal.

It may be asked why the vapour of water does not decompose in the presence of carbon, red hot as the carbon is, we suppose, before it issues in a black condition. There is a mode of consuming smoke which consists in simply blowing into the furnace a jet of steam. The effect is instantaneous and remarkable. A clear flame is produced in a few seconds. This circumstance has probably induced many persons to attempt to burn steam; it certainly has the appearance of burning, but these people do not consider that water is already burnt; it is a product of combustion; chemically speaking it is an oxide, and can burn as little as ashes can burn. The cause of the appearance is easily imagined. The water is decomposed by the heated carbon, whilst hydrogen and carbonic oxide, both combustible gases, are formed. These again come in contact with air, and then burn readily and clearly. The consequence is that no carbon appears in the smoke; it is burnt by halves, by two processes instead of one; and the flame extends

itself forward, as a little time is required for these operations.

By one method of employing steam there is a large amount of air supplied at the same time. The steam, however, without the excess of air brought by it, produces, as it appears to me, good results, but that must be in cases where there is already abundant air and heat.

Still the question arises, Why does not the steam formed by the coal prove sufficient? I confess this is not clear to me, and a little more information is required. It is not, however, an absolute mystery, as we can readily imagine an insufficient quantity to be one cause and the want of good mixture another. We do know that air and smoke may rush together through a flue quite unmixed for some time.

It may now be asked, What is the result of this?

It is seen that the manufacturer's interest is not to make black smoke, because it is expensive.

The public are interested in its removal, because it adds an unwholesome gas to those gases and solids already contained in ordinary smoke.

The excessive amount of air used entails an enormous waste of heat. This has not been calculated.

As to the mode of burning black smoke, the attempts of many have failed, because, although they supplied air, they did not at the same time consider heat, time, and due mixture. I have seen some very efficient methods, but it is difficult to say, in the majority of cases, whether they owed much of their result to the skill of the individuals. This much is clear, that so long as we use the primitive method of merely lighting a heap of coals under a boiler full of water in order to get steam, our results will be also primitive. What else can we expect?

No practical experiments were made by attempting to construct or to alter furnaces, as my object in these

experiments was not to burn smoke, but simply to understand a subject so frequently coming before my notice, and indirectly, if not directly, connected with alkali-works.

A FEW INSTANCES OF VARIOUS QUALITIES OF SMOKE (SO CALLED).

It is well known that when there is abundance of air given to fuel at the proper time and place, the result of the combustion of carbon is carbonic acid. If, however, there is more carbon than air, the result is carbonic oxide. These are the general rules, and yet we find, on examining the smoke in chimneys, that there is an excess of oxygen at the same time that there is some carbonic oxide formed. The explanation does not seem difficult. The air rushes into the chimney over a great surface unequally covered with coal; some of this air is very little changed; some of it, passing through the most active part of the grate, is deprived of its oxygen, and even the carbonic acid is deprived of some, so urgent has been the demand. There is no time to average matters; the whole must be done in the time that the air passes through, let us say, eight inches of coal, or, at the very most, the length of the fire. The hottest part, generally, is the first eight inches. That space is passed over by the air very rapidly; a common amount will be the twentieth part of a second, sometimes more, sometimes less. Now we know that even combustion, or chemical action, requires time; if that were not the case, it would be impossible to blow out a flame, unless by a current continued long enough to cool the substance so low as to produce no gas; whereas we may blow out a candle by a jerk, so to speak, of the air, whilst the gas goes on evolving for some time after. Even a gas-flame may be put out by the gentlest puff, if it is sudden and exactly at the

aperture. The reason seems to be that the flame is blown away before it has time to light the gas behind it. It will be blown out even if the air is warmer than the gas issuing.

'When coal is burnt every pound consumes the oxygen of 150 cubic feet of air; when there is one per cent. of sulphur in the coal this will be equal to ·46 grains in a cubic foot of the smoke, or ·92 of sulphurous acid, nearly one grain of sulphurous acid in a cubic foot of smoke.' But the air is not supplied to the coal in this stinted way; twice the amount is usually supplied, and, even four times the amount, with frequent cases where still more is sent up.

Burning coal with one per cent. of volatile sulphur:—

	A Cubic Foot of Smoke at the Ordinary Temperature contains Grains of Sulphurous Acid.
Using 150 cubic feet of air to one pound of coal	1
„ 300 „ „	·5
„ 600 „ „	·25

When burning coal with two per cent. of volatile sulphur double the amount is given out.

When 300 cubic feet of air are used, a common quantity, and when there are two per cent. of sulphur in the coal, the amount of sulphurous acid in the smoke is 2·2885 grammes in a cubic mètre.

When muriatic acid escapes at alkali-works from the pan where it is formed, there is always some extraneous matter mixed with it. The deposit from it may be judged of from the following five analyses.

			No. 1.	No. 2.	No. 3.
Iron peroxide	19·866	26·946	6·400	19·483	24·407
Alumina	3·171	4·614	3·545	3 905	4·689
Lime	·371	·486	2·110	1·383	·482
Magnesia	·414	·298	·751	·708	·281
Soda	11·311	2·808	24·093	12·653	6·003
Silica	6·049	3·309	13·020	3 292	4·166
Sulphuric acid	53·212	48·007	48·695	54·286	26·220
Chlorine	2·141	2·034	·092	2·278	20·409

From these it appears that there is some solid matter, and amongst it sulphates; when time is given to deposit, the sulphates take the opportunity better than the chloride and muriatic acid, which are more volatile; of these a little may escape through chinks. 'First Report under the Alkali Act, for 1864.'

There must be some other constituents not mentioned. Arsenic, for example, is found, as it is also found in the following.

The deposit taken from the top of a blast furnace may be said to be the condensed smoke. A specimen from a furnace not now at work — Walker, Newcastle—was analysed in my laboratory, and was found to consist of—

Arsenic	·08	
Oxide of lead	·24	
Peroxide of iron	23·35	16·03 soluble; 7·32 nearly insoluble.
Alumina	14·37	
Silica	26·63	
Sulphate of lime	12·76	
Carbonate „	2·50	
Lime (probably with silica)	2·12	
Sulphate of potash	6·66	
Chloride of sodium	4·89	
Sulphate of soda	1·72	
Magnesia	1·63	
Carbon	1·95	
Lithium	Trace	
Vanadium	„	
	98·90	

Another blast furnace, near Newcastle, gave a deposit of the following composition:—[2]

Arsenious acid	·08
Oxide of lead	·07
„ cobalt	Trace

[1] By a young friend in my laboratory, Mr. Wm. Browning.

[2] Analysis made in my laboratory by my young friend, the late John Dalziel, of Leith.

Oxide of manganese	·72
„ zinc	·14
Sesquioxide of iron	19·71
„ aluminium	11·09
Lime	10·47
Magnesia	4·27
Silicic acid	21·74
Phosphoric acid	2·79
Carbonic acid	·16
Sulphate of lime	4·35
Hydrate „	·37
Sulphate of potash	9
Chloride of potassium	2·33
„ sodium	3·06
Lithia	Trace
Carbon	3·62
Sand	4·40
Moisture	·80
	99·17

There are many works sending out other gases into the air. There are sulphuric acid works in great abundance. Some are innocent, or nearly so, but very few. They are improving, but a little pressure would hasten their improvement. They send out both sulphuric acid and nitrous acid, and, we may add, sulphurous acid, with its general accompaniment of arsenic when pyrites is used.

There are copper-works which burn pyrites, and send out enormous quantities of sulphurous acid—not, of course, quite pure, but mixed with metals. Of these arsenic is the most common, and a little copper there must be. Of these works there are two which condense a large portion of their acid. Messrs. Newton, Keats, and Co., St. Helen's, condense above a quarter of all that comes from roastings of fresh ore.

There are also manure-works making superphosphate of lime, and sending out in many cases compounds of fluorine, besides sulphuric acid, when it is manufactured at the spot.

Glass-works and works for earthenware are also not quite innocent, sending out sulphur acids when sulphate

of soda is used, and hydrochloric acid when common salt is used.

In the 'Fourth Report under the Alkali Act, 1867,' a list of works capable of being nuisances was given according to the French arrangement. I may refer to that as very fairly stating what may occur. See Appendix.

Alkali-works, notwithstanding their supervision, give out muriatic acid more or less, although in some cases it is very little. They all manufacture sulphuric acid, and therefore send out sulphur vapours at times, and nitrous gases. The muriatic acid is not perfectly pure, but contains arsenic, as does the sulphurous. Mr. Henry Arthur Smith has lately found in the acid, leading to the condenser from the pans where the salt is decomposed, ·158 grains in 1,000 cubic feet, as a mean of 12 analyses, making the total in a day 115 grains from a pan.

This may be considered small.

In the chimney of the same works he found per 1,000 cubic feet ·086 grains of arsenic. 500 cubic feet of air were used in each experiment. The coke from the tower of the same works was found to contain 2·886 per cent. of arsenic, and a great deal in some of the deposits from the pan gases.

Arsenic and Copper.

Mr. Dugald Campbell some years ago found arsenic in iron pyrites in so many places—coal, I believe, included—that one was inclined to believe it a constant accompaniment. I looked for it in coal pyrites, and then found it in thirteen out of fifteen specimens; so that we must now add arsenic to the number of impurities in the atmosphere of our towns. True it has not been obtained directly from the atmosphere, but we must believe it to pass into the air with the sulphur. One or two coal brasses (as the pyrites in coal is called) contained copper, a metal that is also to some extent volatilised, as may be readily

observed wherever copper soldering takes place. Although an extremely small amount of copper is carried up from furnaces, it is not well entirely to ignore it. The amount of arsenic, however, is very likely not without considerable influence; and we may probably learn the reason why some towns seem less affected than others by the burning of coal, by examining the amount of arsenic as well as sulphur.[1] I do not doubt that the amount could be estimated in the air of places where there are many copper works. Other metals might be mentioned.

'ACTION OF THE AIR ON THE BLOOD.'[2]

'No conclusion seems to have been arrived at respecting the exact nature of the effect on the lungs of the atmosphere of large towns and that supplied pure by nature. Many persons believe that no difference really exists, and that the different effects which are supposed to be experienced arise, in reality, from the different conditions and occupations of life. Not later than this week (in 1859) evidence has been given in a committee of the House of Commons ignoring any such differences. The phenomena which I have just explained will, I trust, put all question aside, although I feel assured that to those who have already studied the subject no proof of actual difference was needed.

'The question might now be asked, What is the method by which the air of towns affects human life? The answer always has been that it is principally through the medium of the lungs, and that the blood must in time become somewhat altered. That delicate and mysterious liquid has not, so far as I know, been made to explain the reaction.

[1] 'Proceedings of the Manchester Lit. and Phil. Soc., Oct. 16, 1860.'

[2] From paper 'On the Air of Towns,' 'Quarterly Journal of the Chem. Soc.' 1859, pp. 29–33.

'Finding that an artificial test was capable of indicating the state of the air, it seemed to me probable that the blood, being in reality more delicate, at least with the assistance of time, might also undergo some peculiar change which might be made sensible to the eye.

'I passed some ozonised air through blood, and found instantly a remarkable reddening. I then passed the common air of Manchester through another portion of blood, and obtained, after a few minutes, a very red colour. The effect of a very small amount of ozone, even a bubble of the ozonised air, was sufficient to give a maximum of brightness. The phosphorous vapours were not removed from the air, and I am aware that many substances in small quantities brighten blood. Of these phosphoric acid is, I believe, one, but no such marked result was got by the acid alone.

'Having familiarised myself with the appearance produced by shaking a measured portion of blood with a given amount of air, with repeated variations for several days, using both new and old blood, I proceeded to try the same at the sea-side. It was found that blood diluted with an equal volume of water was most convenient for many of the experiments; for, although the colour and all the changes are somewhat different, the comparative results are exactly the same. It also very much assists the observation to have the results confirmed by both conditions. The blood also keeps longer when diluted. Of course for many observations to dilute the blood is to destroy it. I had found that the experiments were not altered in character by using blood two or three days old, or even much older; but I so contrived as to begin the experiments at the sea within three hours after leaving them in Manchester, so that both the eye and the material might come fresh to their work.

'I expected that the large amount of ozone in sea-

air would rapidly redden the blood, and that the reddening would be much greater than in Manchester. The effect was otherwise; it was decidedly less, and much less. The trials were repeated at various periods of the first and the next day, and with uniform results. It was not easily explained, but it was at least satisfactory to know that there was a difference.

'Finding that phosphoric acid in small quantities gave a lighter colour to the blood, I tried also minute quantities of sulphurous acid. The blood by this means is made less clear, as it seems to me, of a lighter but not such a rich red; after a while it becomes darker.

'In order to obtain air perfectly free from the acid impurities of the town, it was passed through caustic soda into a bottle of the same size always used in the experiments, and air from the town was introduced into another. An equal amount of blood was poured into each without removing the stopper, by the means already described. When shaken it was plainly seen again that the Manchester air caused a greater transparency or a lighter red.

'By these experiments, in which I got no contradictory results, I conceive it is shown that the atmosphere of a town has a peculiar effect on the state of the blood, an indication of which is capable of being rendered distinctly perceptible to the ordinary eye. This will, in course of time, act for good or evil on the constitution. I say for good or evil, because, although I do not for a moment doubt the superiority of that condition of the atmosphere which nature has given us to breathe over all other conditions induced by us, I can imagine that circumstances might arise where such a change as this alluded to might be favourable, or, in other words, where the atmosphere of such a town would have a favourable curative effect.

'That is, we have an abnormal reddening caused by

acid vapour, but, although greater, not productive of an identical effect, because not effected by the oxygen only, which is the agent for the natural decomposition required.

'If acids assist oxidation of the blood in the same manner as they do the oxidation of many other bodies, then they cause the action of the lungs to go on more rapidly and hasten the current of animal life, producing that greater restlessness of the system which is the peculiarity of great towns. I am inclined to believe that, by following up this enquiry, such questions will receive a distinct answer. As the blood is such a delicate test, it is highly probable that the true action of various climates will best be known by studying in this manner the direct action of the atmosphere; it is true that an inorganic test capable of similar changes would be more convenient, but many will be needed to supply the manifold character of blood itself, and all the substances that can be used may still produce united effects, explaining less than one experiment with blood.

'If the true explanation be found in the increased oxidising effect of the air of towns, the carbonic acid will not be so hurtful in the air as the sulphuric, although the latter exists in such small quantities. Mineral acid fumes, I know by too much experience, are exceedingly irritating to the nervous system. At the same time I am not aware of any experiments with carbonic acid and the blood beginning with a natural, wholesome amount and rising up by ·01 per cent. at a time. I tried only a few hastily, with minute amounts, but got no such results as by sulphurous acid (see others made since).

'If, then, the eye can see those changes in the blood, it is not to be wondered at that those minute portions amongst which chemical changes act should, by their accumulated agency, influence the whole phenomena of life.

The plan of estimating the carbonic acid will give also every other acid equally, but, when it is desired to know of any effect arising from acids stronger than carbonic acid, the blood itself may be used as a test. This, however, in the hands of anyone who does not accustom himself to it, may give fallacious results, as the effect is best seen after many comparative observations.

'The value of these tests will be known only when it has become a common experiment, and an easy one, to ascertain the purity of an atmosphere and the efficiency of systems of ventilation, disinfection, and general purification.

'I can readily imagine cases in which a fallacious result will be given—when for example, the air is richly laden with the perfume of flowers. Probably the materials producing the odour will be decomposed like putrid matter; but this must be left to further enquiry. Even in such cases a great preponderance of odour is found prejudicial to the health, and the luxurious perfumes of autumn border closely on and readily pass into unwholesome emanations.

'The breath is very variously affected, as we may suppose, by the state of health. I did not, however, find that it was capable, in the few cases tried, of decomposing as much permanganate as the worst cases mentioned of air out of doors in unclean or crowded places. I found, however, remarkable differences in the amount of organic matter in the breath when sweet and when disagreeable.'

It appears that in this paper I tried to account for the more exciting life of towns by the very agents which I considered unwholesome—namely, the acid gases—supposing them to assist the oxidation of the blood, and supposing also that the reddening proved oxidation. We know they do aid this action in many cases; the blood

thus artificially oxidised might, of course, both be productive of excitement and less invigorating. It is a speculation, but the facts are worthy of observation.

USE OF ASPIRATORS.

It has been said that the use of aspirators for drawing air through solutions was not found a safe method. This is more especially true when the substances are very minute. The amount of solution required is sometimes inconveniently great. The substances that pass through solutions are minute solid bodies in parts; this is especially the case with such as are slightly touched by oil, such as the carbon of smoke. This body is not moistened even by considerable shaking with a watery liquid. The difficulty is not sufficiently removed by increasing the surface by the use of moistened solid bodies. There is, however, another and greater difficulty. When aspirators are used it is necessary that the air should be drawn through tubes, bottles, or passages of some kind, generally of these three kinds, and the substances sought may be deposited in any one of these places. The sides of the tubes and bottles seem rapidly to take up the solids. We see this in the case of the muriatic acid of towns, where it exists we may suppose as common salt, if part is not in chloride of ammonium. A large portion of the sulphuric acid also adheres probably in chief part that which is combined with ammonia. When passed through solution the amount of hydrochloric acid obtained per million cubic feet is 9·9, 12, 14, 18, 34, and 56 grains, and by shaking a solution with the air, above 100; but if tubes and empty bottles are used, sometimes the whole is taken, as in the two cases given, where it was very low—12 to 15, if these experiments were of any value beyond showing how much was lost. The fact of any whatever being

taken up is inconvenient; the salts spoken of may be washed off, but all solids cannot be so removed. I have, therefore, given up the method for highly refined work, such as the examination of the air is, when we deal with substances which we cannot weigh.

Air—Aspiration through Solutions.

Place; Date, 1868; Weather.	Hydrochloric Acid.		Sulphuric Acid.		Proportion of Hydrochloric to Sulphuric Acid.
	Grains in 1,000,000 Cubic Feet.	Grammes in 1,000,000 Cubic Mètres.	Grains in 1,000,000 Cubic Feet.	Grammes in 1,000,000 Cubic Mètres.	
Manchester.					
Grosvenor Square (*a*) .	34	77·86	Not determined	Not determined	—
Back of Laboratory (*b*) .	56·34	129·018	—	—	—
Back of Laboratory, from June 29 to July 8; fine, warm (*c*)	9·90	22·671	420·50	962·945	1 to 42·47
Back of Laboratory, from July 14 to August 1; fine, and very warm (*d*)	12·66	28·991	298	682·420	1 „ 23·53
Back of Laboratory, from October 21 to October 30; wet most of the time (*e*)	18·51	42·388	Not determined	Not determined	—
Back of Laboratory, November 18 and 19; fine, cold (*f*)	14·55	33·319	—	—	—

In the first two experiments no long tube branching out from the washing bottle was attached, but in the case of the other four such a tube was used. It has been found that such a tube retains the HCl, hence the difference in the quantities. If the average of the first two amounts of hydrochloric acid be taken and compared with the average of the two sulphuric acid determinations, the proportion of HCl to SO_3, is as 1 to 7·95.

(*a*) Solution of nitrate of silver used. Solution became dark. (*b*) Very dilute solution of soda used. (*c*) E. wind during four days, W. wind during five days. (*d*) Solution of soda used. W. wind. (*e*) Pure water used. W. and NW. winds. (*f*) Pure water used. N. and NE. winds.

Air—Aspirator. Experiments with Tube and Empty Bottle.

Hydrochloric Acid retained by Empty Bottle and Tube		Hydrochloric Acid retained by Pure Water.		Sulphuric Acid retained by Bottle and Tube.		Sulphuric Acid retained by Pure Water.	
Grains in one Million Cubic Feet.	Grammes in one Million Cubic Mètres.	Grains in one Million Cubic Feet.	Grammes in one Million Cubic Mètres.	Grains in one Million Cubic Feet.	Grammes in one Million Cubic Mètres.	Grains in one Million Cubic Feet.	Grammes in one Million Cubic Mètres.
12·60	28·854	None		25	57·25	116·90	267·701
15·17	34·739	,,		42·43	97·165	60·34	138·178
Trace	Trace	,,		68·58	157·048	203·22	465·374

ORGANIC AND ORGANISED MATTER.

It is very important to distinguish between dead organic matter which may be floating in the atmosphere in a soluble or insoluble state, and living organic matter which can only be in the latter condition. That organic matter, the débris of living bodies, exists in the air we can show by incontestable proofs ; but, even if it could not by any means be taken out of the atmosphere, so as to bear witness for itself, we might be assured by a very simple mode of reasoning that it did exist there. We see before us great masses of living matter pass yearly into the atmosphere ; a yearly crop of animals and plants are disposed of, and evidence powerful to the most careless senses shows that it does not pass off as pure air. It seems quite marvellous that men generally should so long have neglected the simple facts of the case ; but it seems still more marvellous that they should have so long hesitated and disputed after abundant proofs had been set before their eyes. These proofs were of two classes. The first were the instinctive observations, if we may so call them, of the multitude, and these science has always been ready to despise ; whereas, if it had a proper

appreciation of the greatness of even untaught human nature, it would gladly follow and learn to explain its utterings, no matter how incoherent may be the sentences and strange the language. The second class of proofs were given by the few gifted men who had carefully thought on the subject and made it abundantly clear. But modern thought required everything translated into its own peculiar dialect, and perhaps our conceit at the discovery of methods, which we believe to be more exact than of old, has driven us moderns also to find new proofs for ourselves. We must add also that the result has been greatly to extend the details of our knowledge, although the principles may be still little changed.

It is now known that, by the most complete action of the air, the organic matter of living things passes into the condition of the few gases and vapours of which it has been originally composed; but there is a less complete action, seconded by an internal commotion of the particles themselves, which breaks organic bodies into numberless forms. By the assistance of water great volumes of animal and vegetable matter of a solid character split up and pass off as gases and vapours; and so rapid is the action that we can see the bubbling caused by their escape, which takes place with a speed such as might be likened to boiling by aid of fire. So general are these chemical actions that we may consider them as the recognised method of dealing with living things. It is nature's mode of burial. If these gases are carefully examined, they will be found to contain bodies of organic character; fatty matters can be readily recognised, among others, and these require a greater time and more play of the air for their complete conversion into inorganic substances. These constitute some of the floating bodies which we so fear in the atmosphere, but not them all.

Having already, in another volume,[1] given a decided opinion regarding the theory of the action of germs in organic liquids, I may give here only the general view taken, and then discuss one or two points, as far at least as my observations and a non-medical education fit.

I held it proved that rapid decomposition, such as fermentation and putrefaction, does not go on without the presence of organisms, and that Pasteur had made this clear, although he had not begun the idea. I could not see that the other great theory, that of Liebig—namely, that organic decomposing matter communicated its action by its own activity—had been yet applied successfully to fermentation and putrefaction.

The history of this one part of the subject, the action of organic germs, is long. (See the beginning under 'Marshes.') Only a few points are to be given here. It was found by Dusch and Schroeder that when cotton wool filled up the entrance to a vessel containing flesh, putrefaction might be delayed for months, as if the active bodies were filtered out by the wool. When I tried a similar experiment, and passed air through cotton fibre, the microscope showed that solid bodies had deposited upon it. In this, as in some other cases, I sought the aid of Mr. Dancer, who confirmed my observation. When the cotton was put into water the salts and acids of the atmosphere, all the soluble substances no doubt, were washed out and could be detected and estimated in the liquid. I cannot, however, say that I ever washed the fibre so as to bring it to its original condition.

Pasteur had found that if the air was allowed to pass through long tubes, it lost the power to excite decomposition, the active agents being deposited on the sides or walls of the tube. In a similar way, but after him, when examining the air, I found that even salts and acids

[1] 'Disinfectants and Disinfection.'

were deposited in the tube; and if a long tube were used or a large bottle made to form part of the passage of the air, the sides took up such a large portion of the solid matter that no intelligible result could be obtained. The matter seemed to be lost. It took me long to believe that the sides of the tube could be so efficient.[1]

Many experiments have lately been published on the effect of heat in destroying the smaller forms of life and in preventing their formation. There are two questions—Are they killed by boiling water, or by a temperature not far on either side of it? and can they reappear without the organic substances being brought to the atmosphere? I have tried many scores of experiments on a point allied to this, but it was not attempted to trust to the microscope; I looked rather to decomposition or putrefaction as a test; and, taking this, I know of no instance where it failed to show that heat, little above boiling water, was destructive of those agencies which cause putrefaction. Now, as the belief in the agency of organised forms causing putrefaction was firm, the presumption was natural that organised forms were destroyed, and did not again appear. There is, however, another step; the air contains many organised forms, and we do not know to which this activity is owing. If bacteriæ, &c., appear when there is no decomposition, it cannot be owing to them that decomposition begins. The flesh will not, within two years, putrefy without the assistance of substances in the air which have not been treated like that air which has been heated along with the meat. My belief is that it will never decompose after any number of years, but one may keep these beliefs separate from experiment.

Still further, if the flesh be highly heated, it will not putrefy even in the air, or it will do so very slowly,

[1] 'Fifth Report under the Alkali Act, for 1868.'

according to the amount of heat applied. This is a subject for future enquiry, like many others, I fear. Heated to from 230° F. to 275° F. the solution may be kept in the air—that is, in a bottle half-filled with air—even for years without becoming putrid.

I drew these conclusions:—1. That boiling water, and even less than boiling water, if time were given, destroyed those substances, whether organisms or not, which produced putrefaction. 2. That these did not form again. 3. That one must be careful in dealing with animal fibres, &c., to prove that they really have not undergone such a change as to be actually different substances after long exposure to heat. We may be led far wrong. 4. That the living forms which cause putrefaction are not known to us—that is, if any living forms whatever appear after exposure to high heats, they are not those which cause putrefaction. That they do appear is not my assertion. It may be said that this leaves a loophole for those who say that the organisms are not the causes, but are only accidentally present. Allowing this to be true, we must take the evidence as it stands, and it would not injure us to have more evidence, although I take the opinion of the other side until that evidence is produced against it.

The questions regarding organic matter in the air and organised matter are somewhat mixed together in their origin, and are only now beginning to be treated separately. The real question in late days is not the presence of organic or even organised bodies; we require to discover now the quantity, character, and functions.

It is sufficiently known that the bodies cannot be gaseous, and if organised, they must be solids. We have many speculations as to their origin, but some bodies, such as those forming the red covering of snow, not unfrequently seen, and others believed to have come from volcanoes, have enabled men to prove both organic and

inorganic constituents in the air. Some of these have been believed to be meteoric and not volcanic. Only lately, whilst this passes through the press, Ehrenberg has furnished us with numerous forms and a long and laborious history.

Indeed, for some time attempts have gone in the direction of measuring the quantity of matter in a comparative way, and there was more an inclination to look at the amount and the state of decomposition than the form, which latter must now be important. Although the idea of life existing in the air was made out sufficiently clear as an occasional or even common thing before Pasteur wrote, he first gave the substances duties to perform, giving, in fact, a finish to the enquiry up to this stage, right or wrong ; namely, that putrefaction and fermentation are begun only when there are present in the air bodies which are not gases or vapours, but solids, and have an organic structure or are living.

Dr. Dundas Thomson did some excellent work, published in 1852 by the Medical Council of the General Board of Health. He has given drawings of the substances found in the wards of a cholera hospital, in the atmosphere of a sewer, and in the external atmosphere. It is better to give his own conclusions than any description :—

'1. That in the atmosphere of a cholera ward mechanical matters were diffused throughout the air derived from the inmates ; that sporules of fungi and germs of vibriones, or vibriones themselves, were obtained by filtration from the atmosphere ; all of these bodies being adulterations, so to speak, of the pure oxygen and nitrogen, which alone constitute the wholesome predominating constituents of the elastic fluids destined for respiration.

'2. That from a ward only partially filled with patients affected with cholera, substances were separated which were mechanically dispersed to the very summit of the apartment, mixed with fungi or their sporules, while no

vibriones, unless in the form of faint traces, could be detected.

'3. That in the atmosphere of an empty ward, communicating, however, with a ward containing cholera patients, mechanical matters were obtained, and traces of fungi and perhaps of vibriones.

'4. That in the external air adjacent to an hospital substances mechanically distributed were likewise found, and sporules with fungi were also detected to a considerable extent, but no vibriones.

'5. That in the atmosphere of a sewer, bodies were also found in mechanical diffusion, associated with sporules, fungi, and vibriones.

'6. That the air obtained under the three first conditions from wards possessed an acid reaction, that the external air likewise indicated a similar chemical condition, and that the sewer atmosphere was alone alkaline.

'7. That although animal and vegetable life seems unequivocally to be diffused through cholera atmospheres, it would be premature to infer a "connection between the disease and those organisms until comparative trials have been extensively made on other conditions of air; and that the present researches must only be considered as a single stone placed as a contribution towards the foundations of a large structure."'

The work of Dr. Thomson was well done. It is a pity that he could not give years to continue it. The air was drawn through cooling apparatus and liquids, such as water and acid, by means of an aspirator consisting of an air-tight cistern of wood lined with zinc, and of the capacity of 16 cubic feet.

This work was done with a view mainly to the theory of the propagation of disease rather than with any relation to putrefaction, but the two have been put together by medical men from time immemorial.

As already mentioned, it was when working at disin-

fection for the Royal Commission on the Cattle Plague, that I began the system of collecting the solid matters by simply washing the air by shaking pure water in a bottle. The solid matters are removed by the water. The idea was taken from the previous permanganate process, which I had worked in a similar manner. By this method it is found in a very few minutes that solid bodies are taken out of the atmosphere. This operation shows to the naked eye the difference between good and very bad air, so far as solid matters are concerned. The liquid may be at once transferred to the microscope and examined. Forms will be seen, but usually no motion for some time.

It invariably happens that the air of the country takes many more bottles to produce an effect on the water perceptible to the eye. The air of a cow-house will give an appearance to the water which will not be caused by good air, unless from fifty to one hundred times the amount is used. The air at the front street is decidedly better by this test than the air behind the houses, in such towns at least as have open middens. This is a remarkable fact, and it can be shown by the rude apparatus indicated to any moderately good eye, but still better by the aid of a good microscope. Even a good pocket-magnifier will assist much in showing differences. The first appearance is that of a slight milkiness, but, when examined, many points are seen floating, and after a while some are found to be elongated ; the latter are, in fact, minute hairs or filaments of various kinds. If allowed to stand, more varieties are perceived, and more motion of a decided kind. The molecular must not be mistaken for the animal motion. Although for many years previous quite familiar with motion in substances from the air and with organic matter, I had never obtained such a variety as on the occasion of examining the cow-houses during the cattle plague, and I looked a good deal to my friend

Mr. Dancer to describe them. He has done so very fully so far as known bodies are concerned, and I shall add his paper, or most of it, beginning with mine read in 1868, in Manchester.[1]

A SEARCH FOR SOLID BODIES IN THE ATMOSPHERE.

I have so frequently for many years attempted to find, and have found, organic substances which have passed from the air into the liquids in which they were collected, that perhaps the Society will scarcely attend to another attempt, although it indicates, I think, some progress.

. . . . . .

Whilst examining some matters relating to the cattle-plague I found one or two remarkable points. I had before that time used aspirators to pass the air through liquids, except in the oxidation experiments. At that time I used simply a bottle which contained a little water. The bottle was filled with the air of the place, and the water shaken in it. The difference of air was remarkable. A very few repetitions would cause the liquid to be muddy, and the particles found in many places were distinctly organic.

Before speaking of my last experiment, it may interest the Society first to hear of a few of these previous attempts, the latest made till recently. I shall therefore quote from a report to be found in the appendix to that on the cattle plague.

'Mr. Crookes also brought me some cotton through which air from an infected place had passed. It was examined at the same time. Taking the cotton in the mass, nothing decided was seen; but when it was washed, some of the separate films were coated over with small nearly round bodies presenting no structure, or at least only

[1] See Publications of Lit. and Phil. Soc.

feeble traces of it, and perhaps to be called cells. I had not sent gun-cotton, as I intended, to Mr. Crookes, fearing the rules of the post; otherwise there would have been more certainty that the bodies spoken of did not exist previously on the cotton. However, Mr. Dancer, who has examined cotton with the microscope oftener than most persons, even of those experienced in the subject, had never observed a similar appearance.

'The liquid had also a number of similar bodies floating in it.

'It was then that Mr. Crookes sent a liquid which he had condensed from the air of an infected cowshed at a space a little above the head of a diseased cow. This was also examined, and it presented similar indications of very numerous small bodies. Not being a professed microscopist, I shall not attempt a description, but add that they clearly belonged to the organic world, and were not in all cases mere débris. We found also one body a good deal larger than the rest; it resembled somewhat a Paramecium, although clearly not one.

'We found no motion whatever; and only this latter substance could be adduced as an absolute proof of any living organised form being present. Next day I examined the same liquid; and, whether from the fact of time being given for development or from other causes, there was a very abundant motion. There were at least six specimens in the field at a time, of a body resembling the Euglena, although smaller than I have seen it. When these minute bodies occur, it is clear that more may exist; and germs in this early stage are too indefinite to be described. The existence of the vital spark in the organic substances in the air alluded to is all I wish to assert, confirming by a different method the observations of others. It might, of course, be said that since the bottle was opened at Mr. Dancer's the air at that place may have communicated them. I answer that, before it was opened, a good glass

could detect floating matter; some of it, however, as in the microscope, proved indefinite enough.

'Finding this, and fearing that the long time needful to collect liquid from the atmosphere might expose it also to much dust, I used a bottle of about 100 cubic inches dimensions, and putting into it a very little water, not above five cubic centimètres, I pumped out the air of the bottle, allowing the air of the place to enter. This was done six times for each sample, the water shaken each time, and the result examined. This was done with the same bottle that was used in my early experiments with permanganate, and by the same method, except that water instead of that salt was used. At first considerable numbers of moving particles were found; but it was needful to examine the water used, and here occurred a difficulty. It was not until we had carefully treated with chemicals, and then distilled the water again and again, that we could trust it. Particles seemed to rise with the vapour; and, if so, why not with the evaporating water of impure places?

'Having kept an assistant at the work for a week, and having myself examined the air of three cow-houses, I came to the conclusion that the air of cow-houses and stables is to be recognised as containing more particles than the air of the street in which my laboratory is, and of the room in which I sit, and that it contains minute bodies, which sometimes move, if not at first, yet after a time, even if the bottle has not been opened in the interval. There is found, in reality, a considerable mass of débris, with hairs or fine fibres, which even the eye, or at least a good pocket-lens, can detect. After making about two dozen trials, we have not been able to obtain it otherwise. Even in the quiet office at the laboratory there seemed some indications.

'I found similar indications in a cow-house with healthy cows; so I do not pretend to have distinguished the poison of cattle plague in these forms; but it is clear

that where these exist there may be room for any ferment or fomites of disease; and I do not doubt that one class is the poison itself in its earliest stage. It would be interesting to develope it further.

'I have recorded elsewhere that I condensed the liquid from the air of a flower-garden, and found in it, or imagined I found, the smell of flowers. I do not remember that I looked much to the solid or floating particles, thinking them to be blown from the ground; but it does not affect the result, whether they be found constantly in the air or are raised by the action of currents.'

Lately I tried the same plan on a larger scale. A bottle was filled with air and shaken with water. The bottle was again filled and shaken with the same water; and this was repeated 500 times, nearly equal to $2\frac{1}{2}$ million cub. c., or 2,495 litres.[1] As this could not be done in a short time, there was considerable variety of weather —but chiefly dry, with a westerly wind. The operation was conducted behind my laboratory, in the neighbourhood of places not very clean, it is true, but from which the wind was blowing to other parts of the town. I did not observe any dust blowing; but if there were dust, it was such as we may be called on to breathe. The liquid was clouded, and the unaided eye could perceive that particles, very light, were floating. When examined by a microscope, the scene was varied in a very high degree; there was evidently organic life. I thought it better to carry the whole to Mr. Dancer and to leave him to do the rest, as my knowledge of microscopic forms is so trifling compared with his.

Having requested Mr. Dancer to undertake the examination by the microscope, he wrote the following, and sent it to the Philosophical Society of Manchester:[2]—

'The air had been washed in distilled water, and the

[1] The total quantity is not absolutely correct; but it is unimportant.

[2] 'Microscopical Examination of the Solid Particles from the Air of Manchester,' by J. B. Dancer, F.R.A.S.

solid matter which subsided was collected in a small stoppered bottle, and on the 13th of this month Dr. Smith requested me to examine the matter contained in this water. An illness prevented me from giving it so much attention as I could have wished.

'The water containing this air-washing was first examined with a power of 50 diameters only, for the purpose of getting a general knowledge of its contents; afterwards magnifying powers varying from 120 to 1,600 diameters were employed.

'During the first observations few living organisms were noticed; but, as it afterwards proved, the germs of plant and animal life (probably in a dormant condition) were present.

'I will now endeavour to describe the objects found in this matter, and begin in the order in which they appeared most abundant.

'1st. *Fungoid Matter.*—Spores or sporidiæ appeared in numbers, and, to ascertain as nearly as possible the numerical proportion of these minute bodies in a single drop of the fluid, the contents of the bottle were well shaken, and then one drop was taken up with a pipette; this was spread out by compression to a circle half an inch in diameter. A magnifying power was then employed, which gave a field of view of an area exactly 100th of an inch in diameter, and it was found that more than 100 spores were contained in this space; consequently the average number of spores in a single drop would be 250,000. These spores varied from 10,000th to 50,000th of an inch in diameter. The peculiar molecular motion in the spores was observable for a short time, until they settled on to the bottom of the glass plate; they then became motionless.

'The mycelia of these minute fungi were similar to that of rust or mildew (as it is commonly named), such as is found on straw or decaying vegetation.

'When the bottle had remained for 36 hours in a room at a temperature of 60° the quantity of fungi had visibly increased, and the delicate mycelial, thread-like roots had completely entangled the fibrous objects contained in the bottle and formed them into a mass.

'On the third day a number of ciliated zoospores were observed moving freely amongst the sporidiæ. I could not detect any great variety of fungi in the contents of the bottle, but I cannot presume to say that all the visible spores belonged to one species; and, as there are more than 2,000 different kinds of fungi, it is possible that spores of other species might be present, but not under conditions favourable for their development. Some very pretty chain-like threads of conidia were visible in some of the examinations.

'The next in quantity is vegetable tissue. Some of this formed a very interesting object, with a high power, and the greater portion exhibited what is called pitted structure. The larger particles of this had evidently been partially burnt, and were quite brown in colour, and were from coniferous plants, showing with great distinctness the broad marginal bands surrounding the pits; others had reticulations small in diameter. They reminded me of perforated particles so abundant in some kinds of coal.

'The brown or charred objects were probably particles of partially burnt wood used in lighting fires.

'Along with these reticulated objects were fragments of vegetation, resembling in structure hay and straw and hay-seeds, and some extremely thin and transparent tissue showing no structure. These were doubtless some portions of weather-worn vegetation. A few hairs of leaves of plants and fibres, similar in appearance to flax, were seen, and, as might have been expected in this city, cotton filaments, some white, others coloured, were numerous; red and blue being the predominant colours. A few granules of starch, seen by the aid of the polari-

scope, and several long elliptical bodies, similar to the pollen of the lily, were noticed. After this dust from the atmosphere had been kept quiet for three or four days, animalculæ made their appearance in considerable numbers, the monads being the most numerous. Amongst these we noticed some comparatively large specimens of paramecium aurelia, in company with some very active rotiferæ; but after a few days the animal life rapidly decreased, and in twelve days no animalcula could be detected.

'*Hairs of Animals.*—Very few of these were noticed, with the exception of wool; of this both white and coloured specimens were mixed up along with the filaments of cotton.

'After each examination as much of the drop of water as could be collected by the pipette was returned to the bottle, in order to ascertain if any new development of animal or vegetable life would take place, and the stopper of the bottle was replaced as quickly as possible, to prevent the admission of the particles from the air in the room; and I am tolerably certain that the objects named in this paper are those which the bottle contained when Dr. Smith brought it to me.

'The particles floating in the atmosphere will differ in character according to the season of the year, the direction of the wind, and the locality in which they are collected, and, as might be expected, are much less in quantity after rain.

'The small amount of fluid now remaining in the bottle emits the peculiar odour of mildew, and at present the fungoid matter appears inactive.

'For the purpose of obtaining a rough approximation of the number of spores, or germs of organic matter, contained in the fluid received from Dr. Smith, I measured a quantity by the pipette, and found it contained 150 drops of the size used in each examination. Now, I

have previously stated that in each drop there were about 250,000 of these spores, and, as there were 150 drops, the sum total reaches the startling number of 37½ millions, and these, exclusive of other substances, were collected from 2,495 litres of the air of this city—a quantity which would be respired in about ten hours by a man of ordinary size when actively employed. I have to add that there was a marked absence of particles of carbon amongst the collected matter.'

Mr. Dancer describes carefully. The actual germs of disease are unknown. My wish was to let them develope until they showed distinctive characters. This will probably be done some day, and we may see one turn out to be the beginning of cholera, another a germ of scarlet fever, and so on, whilst a third may be an important agent in promoting health. But there bodies are, and have been shown to be. Their weight or quantity, as illustrated by the production of albuminoid ammonia from the air, has already been very fully illustrated in this volume. They are, to a certain extent, not merely found, but measured, weighed, counted, and analysed. When I speak of a paramecium, euglena, &c., I suppose these only accidental bodies feeding on the minuter and less known.

The mode of existence of these living things is a speculation to us. Why do they float, and why do they not fall? They seem to rise as readily as vapour, and they are not merely dry dust driven up by wind. They are found, as Dr. Thomson showed, in sewer and hospital air; the sewers are always wet. They are found less when the rain falls, so that the rain washes them down into the earth. The advantage of rain is well seen, and it is remarkable how much it shows itself. When we look at the death rate in the various counties, those which have a good deal of rain are certainly lowest in most cases, if not in all. Warmth accompanied with

dryness seems to have an equally good result, by shrivelling up the organic matter. The germs are then less elevated into the air, as there is seldom much wind with heat, and if any should be so elevated, they would be dried up as butterflies are in a burning sun, according to Sir Emerson Tennent.

When these spores, germs, or living forms rise into the air, do they incline to fall again? The evidence seems to be that they do so, and deposit themselves in quiet places, even when not carried down in dew. They even fall in water if allowed to rest quietly, and a pipette may bring them up from the bottom of a glass in greater numbers than from the liquid above. If so, why do they rise? They are so small that the mere rise of vapour may be a current sufficient to take them up; if so, then currents must be continually removing them. They may feed on the organic substances, which rise in considerable quantity, and they may grow in abundance and size on the food thus supplied. For several years the corner of a street I daily passed presented nothing worthy of observation, but at last it was found that almost constantly a number of flies were at that place in summer. This gradually connected itself with the smell of worts; a brewery had started up, and these flies fed and lived on the substances that affected the sense of smell. We see birds flying about catching prey which to us is invisible, until it is collected in distinct pieces in their mouths, and we see whole nests full of young fed with prey caught in this way. We see, on the other hand, the hawk looking about for his victim, and seizing it with much noise in living pieces, made distinct enough to us by their struggles. And we see the great eagle calmly sailing aloft, looking for an opportunity to lift up its prey with dignity. In the impure air we have a lower scale of life, feeding in a way analogous, close to us, but out of our sight.

During wind these minute objects must be driven

onwards, and away from the spot of feeding; indeed, it is scarcely possible to find a day when the air is so still as to allow them to rest over the place from which they escaped. They must be scattered over a great space. And we must now ask if they can do mischief when so carried, but first we must be clear that they can do mischief at any time? What harm do we receive from breathing them? It has been abundantly proved that air from middens and such impure places is less wholesome than air from free and open places. We must not return to that question; its answer has been the basis of the whole sanitary movement of late years, and it has been sufficiently discussed. If anyone brings doubts, then I can only request him to read the evidence adduced by Mr. Edwin Chadwick, and the numerous body that gathered round him, either to agree with him or to oppose him, or who have been attached to or stirred up by the General Board of Health, the Metropolitan Sanitary Commission, and the Medical Council, over which is Mr. Simon. It has been shown here that the air to which allusion has been made contains less oxygen than pure air, but it has not been shown that such is the cause of the evil consequences of living in it; or rather we know that this minute diminution of oxygen is not the sole cause, although it may not be entirely harmless. We know, also, that this impure air contains more carbonic acid than pure air, but it has been made clear that this carbonic acid is not the cause of infection. We may give up the ammonia-salts and nitrates, because we know their action to be such as not to produce infectious diseases, fevers, or putrefaction, or even special diseases, although they may in some respect be injurious after a long time. None of the gases or vapours known to us can be imagined to be guilty from any property of theirs hitherto found. It is true that they may lie low, or be washed down, or brought down, by rain or vapour, so as to be

found in the evening fogs, and that this occurs is certain. Some of these may be injurious to health, but none of them have the character of albumen capable of putrefaction, and they can be included neither in the Liebig nor the Pasteur theory, whilst their characters known to us do not throw light on the beginnings or progress of marsh-fevers or epidemics. We may speak with some certainty as to the latter, but on the former—namely, the marsh-fevers—there is more ignorance to be acknowledged, although the statement as put is correct. That some evil will result is, however, likely enough from the great mixture of substances in the evening dew of a rich clime, leaving results independently of the albuminous decompositions and organisms, but the exact knowledge is not with us. Gases, vapours, albuminoid substances, plants, and animals must all produce their peculiar effect in the atmosphere. We must learn to subdivide and not to throw them recklessly together.

After viewing the known and the probable action of all these bodies, we are driven to seek for the conveyance of infection in the solid bodies of the atmosphere, not forgetting that some may, like albumen, be dissolved in water, or be carried in little globules like pus. But it cannot be merely because of the solidity of the particles that they do the evil complained of. This we know, because the amount of solid matter which is breathed before affecting the health seems considerable, and certainly far beyond that which is contained in the air of which we are speaking. This is learnt from regions of chalk and sand during hot weather, as well as from coal-pits and Sheffield workshops, where inorganic matter is so much breathed, producing its peculiar results. We are therefore driven to the organic matter, and the question arises, Is it the chemically changing and decomposing or the living matter that does harm? Do we explain the occurrences by the theory of Liebig or the theory of Pas-

teur? And can we reconcile the two, or is one true and the other false?

It has already been admitted that there are important decompositions of organic substances which do not proceed without the presence or intervention of some vibrio, or at least let us say organism. That, of course, is no proof that Liebig is wrong when he says that a state of decomposition is transferable from one body to another by chemical action. Even mechanical action begins chemical decomposition, and that chemical change does incline to continue itself must be held true. This may possibly be the case with all chemical action; it is, however, recognised only in some cases hitherto. This is, however, not the most powerful method of destroying organic substances at ordinary temperatures. It seems that the decay of organic substances without the presence of organisms is much slower than with it. Flesh, freed from certain organisms, will keep long even if moist, but it will eventually be oxidised; and meantime the change taking place in the arrangement of its molecules would require study. But the change caused when the usual occupants of the atmosphere are present is one so rapid, that it can only be compared to the chemical action of inorganic bodies, the action, for example, of strong acids on carbonates. The formation of oil of bitter almonds, caused without organisms, shows that we must be careful not to speak too generally. We may confine ourselves for the present to albuminoid substances.

We may, perhaps, learn that the organism is not the real agent, whilst the decomposed parts, either in or out of solution, are. Since Graham taught us the peculiarities of colloid substances, we know that a substance may be in solution, and still refuse to pass along with other substances through a filter of membrane, and that even without membrane decomposition may take place

by diffusion. This knowledge renders the proof of the action of plants, for example, in fermentation, as distinct from the liquid, more difficult. It is good to recognise the difficulties in the two views of the question whilst quite clearly believing in the necessity for organisms in putrefaction, &c., until the action shall be proved to go on without them.

This does not prevent one from thinking that decomposition is transferred from one body to another; it is extremely probable that this will show itself more and more valuable as an explanation of very obscure phenomena. I call it Liebig's idea, because, although mentioned by Willis in the seventeenth century (see Kopp's 'Geschichte der Chemie,' vol. iv. p. 293), and by Stahl (p. 294), and again by Place at the beginning of the eighteenth, it was first developed in all its fulness by Liebig, and he first taught us to understand it. In the time of Willis the chemical constitution of molecules was very crudely conceived, and the language of Willis himself shares in the crudity.[1]

It must be remembered that the parts of which we are composed are continually undergoing change; the blood and all the fluids are especially active. Let us picture to ourselves the amount of this activity. If we hold our breath a few seconds, we are uncomfortable, a very little more and we are unconscious; if we inhale a little undiluted sulphuretted hydrogen, we fall down unconscious, as if struck by lightning; if we inhale even carbonic oxide, which we cannot smell, we fall down at once; by carbonic oxide obtained by combustion from an iron furnace men have been suddenly thrown down without warning. The action of many poisons taken into the mouth is equally rapid, a question at most of seconds. The action of restoratives also shows the great rapidity of the chemical movements in the body.

[1] See 'On Fermentation,' cap. 8 & 9.

When we are exhausted by bad air, one whiff of fresh and pure wind brings comfort in a time so short that we cannot measure it. If exhausted by want of food, a certain amount of strength is gained by one spoonful of soup as soon as swallowed, and if thirsty, even before liquid has reached the stomach, we are refreshed if it has touched the mouth. In cholera the decompositions that take place go on with fearful rapidity; the solids of the body are transformed with a speed which has always been compared to that of putrefaction, but which may be called more rapid. We know of no action of neutral organic bodies at low temperatures so rapid as this without the presence of organised structure, great or small. Although we may believe, with Liebig, that there is the tendency to transfer action purely chemical from one set of compounds to another; it is, so far as we know, so slow in nitrogenous or albuminoid substances, that the activity of animal life appears to overpower it in the living animal, and its tenacity in the dead. If, however, the higher animal life itself is to be overpowered, it must be by an activity greater than itself, and it is this activity which we find when organisms are present.

This is an easy way of overcoming the difficulties of the case, and for a time I was willing to remain there, but this question arose—Why do organisms produce the result? As a general answer I replied to myself, The operations of life are, to a great extent, chemical, perhaps entirely chemical; chemical laws regulate the formation of the solids and liquids within us, and the gases coming off are constituted with as exact attention to the laws of molecular combination and the atomic theory as if they had been mineral substances formed during ages in caverns, and possessed of a crystalline activity only, instead of taking an active part in life. Whatever vitality may be, it certainly allows matter to act in conformity with chemical laws in nature, and these are

the greater and the more general; they are in reality universal, whilst vitality at best makes use of the great stock of matter, and of force, which is so abundant, for all purposes; or we may say universal chemical force keeps certain movements in organic and inorganic bodies equally in action. In this direction we do not know how far it is right to go. The chemical theory becomes, however, the widest, and it has a greater future for itself, although the organic department of the theory has been more fully developed of late.

Again we may say the decomposition is really not chemical, but it is rather dependent on the physical structure of membranes, by means of which the portions of the fluids are separated, and, if so, why not also the portions of molecules, so that decomposition takes place? If this were the case, we must ask for the fundamental cause, and we come to the primary activity of atoms and molecules. If, for example, sugar is not a very strongly built compound, but has in itself the capacity of forming two of great activity—carbonic acid ready to expand to a gas, and alcohol of ready volatility, although still liquid—the two primary activities are opposed, and seek an opportunity to show themselves. This would be given when an obstruction took place. We may have only a separation of the parts of a compound by diffusion. The difficulty then occurs to suppose the action to take place so frequently as to account for the influence of very few and microscopic existences, such as the organisms present. But the germ theory has the advantage in the fact of the germs being present, and over this difficulty it is not easy to pass. We may, then, remain in belief. But germs are not ultimate facts in nature, and if they act by their physical structure only it may be by the osmotic action of a peculiar membrane promoting diffusion. In this case we are obliged to look to the size of the bodies producing the action. According

to Graham the membrane becomes constantly worn, but again the same wearing takes place with earthenware as an osmotic agent, and membrane can grow. The process, too, is slow, and we seem to demand an amount of membrane not to be found in the vibrios or bacteriæ of putrefaction. Of course we know that they, or their membranes, must have some power, and we could explain a beginning by their aid, but we have a difficulty in imagining them keeping up the constant action. To suppose them to cause the beginning only would be to use the two theories. If a beginning is made, why does it not go on without the organisms? Does it require the continued little impulses, from the organisms?

If we give up the chemical action of the membrane, and look to diffusion only, although facilitated by membranes, we are led to the fundamental characteristics of matter, the movements 'per se' of the 'Primordia rerum' of Lucretius, who watched dust in the sunbeams, and the 'primordial impulse' assumed by Graham, or Clausius, from the observation of movements which cannot be seen by the eye, but only by complex experiment and intellectual processes. If, then, these movements, purely physical, are the actual cause, we may perhaps imitate them without the aid of organisms, which, in any case, we might consider as coming in only as intermediate agents, the wider law acting through them. Some such thing may possibly turn out to be the case; but at present we must remember the organisms are there.

I can easily suppose some one to add, 'This is merely begging the question,' and, 'If it is applied to minute why not to all organisms?' This reasoning is not quite correct. The larger animals do not cause commotions in bodies around them so as to effect decomposition. They decompose substances within themselves truly, but the amount decomposed is small in proportion to their bulk. The activity of the bodies seen, if they are the active

agents, is out of all known proportion. The distinction is clear enough; if we say that the tendency of chemical decomposition to continue itself is proved, then we must add that, in certain cases, this is not known to be done without the intervention of organisms of a low kind. This, like many great struggles, will end in separation, but we can suppose the chemical to include the organised without either being distorted.

The theory of germs, so far as disease is concerned, has an independent position, and if we give up the notion of decomposition and diminish the analogy to fermentation and putrefaction, or throw it aside altogether, we can still keep the idea of a deposit of plants or animals, and this theory may readily be retained by those who retain firmly that held by Liebig.

I did not intend to speak of these theories, but it seemed to me that two so well supported by nature must have some point of agreement, and tried to imagine it. We have not hitherto, with sufficient clearness, distinguished between the theories of fermentation or putrefaction taken together and the propagation of disease. The first has the microscope to support it; the second has not so far advanced; the first may, perhaps, by the reasoning used, be reconciled with the chemical theory; but the second, the germ theory of disease, may, in all probability, be quite irreconcileable. If, for example, a disease were caused by a plant or animal forming a settlement, and by its life obstructing the life of others, it would be a distortion of reason to refer it all to chemical action. The same distortion does not take place when we reason on minute organisms beginning chemical decompositions.

To prevent confusion we may then go over the points. First, we have the chemical decomposition tending to continue itself, as in mechanical action. The resemblances of physical and chemical action at the points of meeting,

are much too numerous to allow this theory of Liebig's to be forgotten.

Next we have the beginning of fermentations and putrefactions in the presence of organisms, and the prevention of these movements by keeping out organisms. This class of decomposition is peculiar; it seems to begin as organic and end as purely chemical. This is independent of the explanation of the ultimate cause being physical, or chemical, or being capable of being produced without vitality.

Thirdly, we may have the analogues to fermentation and putrefaction in the living animal producing disease by chemical decomposition strictly speaking, however begun. I could not venture to say which diseases have this similarity; various forms of blood-poisoning will probably be included.

Fourthly, we may have the action of the germs not producing chemical decomposition analogous to that in fermentation and putrefaction; but by self-multiplication obstructing the healthy development of the solids or the normal changes in the blood.

It seems important to keep these ideas distinct. As to the last, although there seems little good reason to doubt it, we shall never be said to have done enough towards its elucidation until we find the exact germ that produces one known disease or more; that must be waited for as the *experimentum crucis*.

We may look at the consequences for a little. By this subdivision, which is merely a collection of non-original ideas, we get rid of the necessity of looking to fermentation as an analogy by which we must learn in all cases of infection, and we have the life of the plant and the animal to follow as containing the analogy. We extend the activity of chemical action through organisms, although we cannot include all the phenomena of composition and decomposition within and without animal

life. The intermediate phenomena of putrefaction and fermentation are kept from being mixed up with the others. We have the probability of diseases corresponding to all these different modes of decomposition.

In other words, whilst it seems to be important to separate the action of specific germs in disease from mere fermentation and putrefaction, these may still have their analogues in animal fluids. We may even go further, and whilst we admit the chemical action to be very great in these first two cases, we may also admit that the action of vibrios and other organisms may be a cause of continued rapidity of diseased action. In healthy animals we have also rapid chemical processes going on, the continuation not explainable on purely chemical principles, and there we may suppose it to be promoted by the action of organisms of a minute class. In other words we may have diseases analogous to fermentation or putrefaction begun by organisms causing decomposition, and others not analogous to these, but caused equally by organisms doing evil by their increase and not by chemical decompositions. Some persons may object to the connection of such great effects with such a small cause. After all, the greatness of the cause must be measured by its effects. We are told of the millions of cells that may grow in a fungus in a minute; we cannot tell how many are formed, destroyed, and again formed during putrefaction. It is not at all necessary that the one small set which begin should continue to do all the work. On the other other hand there is, in all probability, a decided measure of cause and effect. If only a very small amount of infectious matter be present, the action ceases. If this were not so, nothing could prevent the contamination of a whole city instead of only certain individuals. It is impossible to imagine that any persons in an infected city could escape entirely all contact with infectious

particles, although they might escape all infection. The dilution of the air in fever wards is said to destroy infection; it merely dilutes, and so it is in all cases of danger from putrid matter. It is a struggle between the healthy and the unhealthy action, and the latter may be reduced so low as to be dangerous to no one. This is the natural and inevitable conclusion, and it is a great consolation. It is one on which the hopes of sanitary reform must rest. It seems clear that a very small amount of these germs do mischief, but not an infinitely small; when too large we dilute the air and are free.

But, even allowing that the living things did not multiply to an enormous extent, as for a moment imagined, the explanation is not left hopeless. Let us suppose nothing but chemical relations to be affected by the intervention of organisms. These may act many thousand times in a second without being themselves destroyed to give place to others. They may be agents for forming or holding the membrane or mechanism which does so.

A curious instance takes place in the decomposition of muriatic acid. It is decomposed by salts of copper in the presence of air and vapour of water, and this decomposition is so rapid, that we can only suppose the copper to act in this wonderfully rapid manner—thousands if not millions of times in a second. This has been found by Mr. Henry Deacon, and made the basis of a great manufacture. If this is not the explanation, a more mysterious agency still must be called into aid, and it is easy to multiply mysteries instead of diminishing them.

The method is purely speculative, and although the work the living germs do is to some extent so also, their presence in many cases leads us to conclusions which, by a simple process of reasoning, extend themselves over a great field of thought and enquiry. The theory of their activity enables us to picture to ourselves phenomena which were previously beyond all

comprehension; and it opens to us a new region of thought and action, a new method of cure for many evils. Were it even false, we must welcome it for a while, because it has led men to efficient means of avoiding disease and has given salutary results.

I have produced only the arguments that seemed most striking or newest and freshest, leaving the history of the subject to be written.

WEIGHT AND SIZE OF ORGANISED PARTICLES IN THE AIR.

Let us take the average germ to be one 5,000th of an inch in diameter; some persons would require the size to be much smaller, but this will be quite enough for us to comprehend at present. This would give a cubic volume or bulk of one 238,700,000,000th of an inch. Then, in measuring the amount of that which appears to be ammonia from organic matter in the air, we obtain in some cases about 70 grains in a million cubic feet, equal to about eight times that amount of the commonest animal matter, supposing it to have the same specific gravity as water, although it is rather heavier. We should then have 529,560,000,000 germs in a million cubic feet of air—equal to about two cubic inches of them. This is really a large amount, and may be too high; but it is the measurement for a busy city. In one cubic foot we should, of course, have 529,560 germs, altogether ·000560 of a grain, or above one 2,000th. This is so high that it seems probable that this result obtained of the organic matter in some air is, as said, true only of particular places. I have sometimes thought that we caused an error by being present to make the experiment, and this is probably the case. If so, we must find a method of overcoming this difficulty. If we take the size of some infusoriæ described by Ehrenberg and others, we

shall increase the actual number in a volume of air very much, and probably do right in this, but this change would not affect the weight.

The number of organisms, if we may vary the word, obtained from 2,495 litres, or 88 cubic feet—the amount of air washed by the liquid I sent to Mr. Dancer—was calculated by him to be 37,500,000. The total weight did not appear to me to be so great as that arrived at by my calculation—namely, ·000560 of a grain for a cubic foot—but, disregarding the weight, this would give for a million cubic feet 426,000,000,000, or in one cubic foot 426,000 germs. This method of calculation brings us to nearly the same number as before. In this matter we must consider 529,560 to be very near 426,000; one method starts from the number, the other from the weight and size. Taking again 70 grains of albuminoid ammonia to be in a million cubic feet of some air, and 426,000 organisms in a cubic foot, 760,000,000 would make up one grain, and occupy 1,786 cubic feet of air.

The numbers will startle no one accustomed to such calculations; had one 40,000th part of an inch been used as the diameter of a germ, how much longer the line of figures might have been made!

On the other hand pure country air is not found to contain any such amount of ammonia. I have not found the probable lowest point as yet. In pure air the number of organisms will also be fewer in an equal proportion, or, if not the number, the actual weight. In impure places we can imagine them growing larger without actually growing more numerous.

Besides organic substances there are many other particles. We have seen that there are some of common salt and of sulphate of soda and ammonia as well as other earths and alkalies. There are even molecules of iron in the organic substances, and phosphorus of course. In

the town there is carbon, and there are minerals such as the country affords in all places. If there be some in every foot of air, the subdivision must be very minute if uniform. To produce uniform distribution we must have a greater number of particles when the total weight is small than when it is large, unless we increase the distances; but by increasing the distances we destroy the very uniformity we seek, unless we take larger spaces into account.

EFFECTS OF THE SURFACE ON AIR.

The observations of all nations point to the quality of the soil on the surface of the ground, and of course including a small depth, as a condition greatly influencing the health of a country. That they are true must be believed. Peacock, in one of his admirable tales—'Crotchet Castle'—has a character 'Firedamp,' who says, 'The great object of a wise man should be to live on a gravelly hill.' This is not falsehood, but only caricature. We cannot deny the effect of thick clay, impenetrable by water, which rarely dries up, or is always drying up, but never succeeding, and that of gravel, sand, or other porous strata. The influence of drainage and evaporation are worthy of long discussion; but, on the other hand, the air-washings lead to the belief that the air contains a little of the material from the soil beneath it, even on very improbable occasions. Where vegetation exists, its remains, or the débris of plants, appear. Where there is water we have at least some of the salts of the water; and where there is a chalk soil we have chalk, even on a wet day. I cannot, from observation, go on to say that on a clay soil the air contains clay, but I should not be surprised even at that. Over such a soil there is more prolonged evaporation; much time is

given to attempts at drying. We have, therefore, a greater continuation of moisture in the air, and, let us remember, not moisture from the clouds, but vapour which has risen from the soil and then condensed. The effect of such moisture is not simply that of pure water, otherwise it would little matter whether we were surrounded by it or by the moist air from the sea. We cannot tell all the bodies that may arise under these conditions, but there is enough known to lead us to believe that we must attend to minute differences of soil, such as men generally called fanciful are disposed to observe. I find myself compelled to look to the effects of surface in a way that a few years ago I should have considered ridiculous. We must learn to have more respect than we show for the results arrived at by the unaided senses of man working for ages; they have been able to penetrate secrets which individuals in a lifetime can scarcely understand, and which we cannot prove except by the aid of scientific apparatus and careful thought on the result of experiment. This has been very well illustrated in the ancient and modern history of air and water.

If soil affects the atmosphere, how does it happen that it is such an excellent disinfectant? Substances are not purified in the air to the same extent in the same time as in the soil. The great amount of surface in the porous soil is the cause of the rapid action mainly, but the acid humates act, and probably other substances, chemically. There are modes of oxidising in the air beyond the power of ordinary oxygen; one of these is by means of nitrogen oxides, and another by ozone; but even these require great time and space. If we throw sulphuretted hydrogen water or its compound with ammonia on a few inches of soil, it passes through oxidised in a few minutes; we obtain such an action in the air only in great volumes, although in the end the work done may be better for its purpose. It is a common opinion that the soil of a town

is continually growing worse by constant habitation, unless the evil be continually drained off. Now we must modify this. In our towns, closely inhabited as they are, the evil is not necessarily growing, even although all the impurity is not drained off. We drain off the excess, such as cannot be purified by the porous strata. The soil itself is a great purifying agent—the very soil of towns which we complain of as constantly increasing in impurity.

The refuse heaps of the suburbs of towns become as pure as new soil in a few years, and in very porous places they can do harm for a very short period only. We require only not to overload the soil, or give it too much to do. To use the words of a previous paper—[1]

'This complete purification and thorough removal of the organic matter occurs chiefly in town wells, the complete change being much less frequent in the country. The cause of this lies, no doubt, in the slower and more thorough filtration, and therefore the more elaborate cleansing when passed through a hard soil, and in the removal of the rain-water by surface drainage, or by sewers, before it is allowed to weaken the solutions by being absorbed into the soil. The amount of organic matter which is removed or altered in this way is surprising, and the power of effecting this transformation is a most important and valuable part of the functions of a soil. Of course the same change takes place in country places—in gardens, for example, well ornamented and not well drained, where the water has much organic matter in solution, and stands long on the soil. The change takes place very close to the cesspool or to the flow of organic matter, and at a very short distance water may be found containing very little besides inorganic salts. Very near a sewer, in one of the worst

[1] 'On Air and Water of Towns—Action of Porous Strata.'—*Mem. Brit. Association*, 1851.

streets in Manchester, I found sand which was almost free from organic matter, although the drain had given way, and was allowing the sand to absorb whatever it could. The same observation was made on the sand of a churchyard in which burials were very frequent, showing the great advantage of a porous soil in removing offensive materials by an agency within itself, preventing the corruption of the atmosphere to an extent greater or less, according as the powers of the soil are under or overtaxed.

'As an agent for purifying towns this oxidation of organic matter is one of the most marvellous; we might almost say, and necessary. If the impure organic matter were taken underground (unaltered) by the natural flow of water, the state of the subsoil would become pestilential in the extreme, and towns could not be inhabited without such careful drainage as we have never yet seen; in fact, the soil would require to be impenetrable. To inhabit a place for a few months would be to make it unhealthy. Instead of such a result we have the soil of towns which have been inhabited for centuries, or for a time longer than history can tell us of, in a better state than the soil round many a country-house. St. Paul's Churchyard may be looked upon as one of the oldest parts of London, I suppose; the water there is remarkably free from organic matter, and the drainage of the soil is such that there is very little if any salt of nitric acid in it (written in 1851). A well at Tower Hill had but little organic matter, but only partly nitrated. Of course there are parts of a town where the matter becomes too great to be managed well, and, being combined with bad drainage, even the active state of the subsoil, which seems to do its utmost to destroy all elements of disease which enter it, is not sufficient to remove the amount continually supplied to it from some hundreds of acres of closely built ground. Even in these

cases, however, we are more surprised at the comparative purity of the subsoil than the impurity. As it is, the impurity of the soil in certain parts of towns requires proof; and, although proof can be got, and is got, without this action of the soil, the proof would stare us in the face and hunt us out of its vicinity. It has been necessary to prove the great amount of evil resulting from burials in towns, whilst the enormous amount of organic matter has disappeared rapidly from our view, and the evil seems only to become distinct to us when the mould of the churchyard has become, in a great measure, the remains of organised beings.'

Notwithstanding this, we have abundant proof of the ready sympathy which the air has with the surface of the ground, although the wind sweeping over it would lead us to suppose that all was equalised. The passage of some influence must be rapid and constant. Indeed, we cannot fail to observe that even in a room every foot of ground has its own climate, and every cat makes its choice with wonderful care and skill.

Farmers seem not to attend to this sufficiently; they break down fences in modern times, and hedges and trees unmercifully. The appearance of the land suffers, but so do the cattle. If anyone doubts the varieties of climate on one field, he has only to look at the careful choice made by animals. No one can fail to see how even sheep avoid the damp, and if there be one bare and gravelly part, it is preferred for a bed even to the greenest and softest grass. It is true that hedges and walls, as well as trees, shelter from warm as well as cold winds. When a warm wind blows, then the snow behind the shelter lies longer unaltered than the exposed. Animals then avoid these parts very readily, but, when there is no shelter from cold winds, they have no choice, and they evidently suffer severely. In this country they often require shelter from cold, and sometimes they feel

equally the demand for shelter against heat. Even very slight elevations sensibly affect the flow of air, and therefore the ventilation and climate of a place. On a plain, such as the sea, the wind blows strongly, although when inland it is little felt. Streets also are in a calm, when outside the town there is much wind; this is according to the aspect of the street. Narrow courts are still more defended from currents, and to such an extent as to escape for ever full purification; the evil, up to a point, grows faster than its removal, and it is not enough that they should be kept as clean as other more open places, but to be on equal terms their surfaces require to be kept cleaner. On a high spot the wind blows more constantly than even on the smooth surface of the sea.

Most of the close places in this country are made by art, although some are made by nature. The worst of the natural class are partly in the power of man; they are narrow glens with close woods. Devonshire has many such examples. The houses seem to nestle comfortably in them, but, as we may believe, with a loss to the inhabitants of some vigour. Even in some very narrow glens in the highlands of Scotland, where one would expect abundant rain and wind to cause sufficient mixture, I am informed that it is common for those who stay at the house to lose their health, whilst those who go out among the sheep do not suffer. The state of the houses does not appear to be the cause, as these are no worse there than in better ventilated glens. But we may leave out the question of valleys made by nature until we purify those made unwholesome carefully by artificial means, and called streets, lanes, courts, alleys, and rooms, which are bad enough by their closeness as well as by their wonderful want of cleanness of surface in so many towns. It would appear, then, that the surface affects the air in more than one way by obstruction of its movements which

[1] See also 'Effects of Acids on Vegetation.'

causes floating particles to be exchanged, and by sending out organic substances into the atmosphere, hurtful, perhaps, of themselves, and also when decaying. The direct action of the animal and vegetable matter decomposing on the surface, evaporating there, or being carried mechanically there, is entirely separate. Climate is affected by that which is on the ground, by the action of the porous ground, by the shape of the ground, and by the quality of the ground. Our climate is not all brought from distant places by the winds.

It appears, in fact, that organic matter is incapable of passing deep into a close soil; by conversion into soluble salts it becomes soluble, and is easily washed away in an inorganic state. When this occurs in the country, the use of it is not so apparent as when it occurs in a town. When there is a great excess of organic matter on the surface of the ground, and decomposition is rapid, the results are of course often bad, and, if ammonia is formed too rapidly, bad also for the soil, which loses its food for plants; but it is generally retained in manures by the humic acid, as Voelcker has shown. The formation of nitrates prevents the passage of nitrogen into the atmosphere; it subjects the soil, however, to a loss by drainage. The use of nitrates as a manure has been long known to be valuable; our old English philosophers have been well aware of it; namely, Boyle and Bacon. Before the nitre can be formed in the soil the ammonia must be oxidised, and before it can be used as food for plants the process must be reversed, and it must be deoxidised. The oxygen stored up may be applied for rapid oxidation, and in this way it becomes a kind of concentrated atmosphere, a source of air by which to form carbonic acid from the mould of the soil. We can readily view it as a great stimulator of vegetable life, besides being a source of nitrogen to feed the plants. In dry climates, perhaps, it might be viewed as a supply of

water, which we might readily consider formed by the union of the oxygen with the hydrogen of organic substances, of which bodies there are in some surfaces a considerable amount. The organic matter in a soil may be supposed to decompose in two methods—by the formation of ammonia and by that of nitric acid. If the soil is very alkaline and moist, the conversion of the organic matter into ammoniacal compounds is very rapid. I put some soil not very rich in organic matter into this condition by the aid of a little ammonia, so as to make it alkaline, and the consequence was a very intense putrefactive decomposition, not in any way differing, so far as could be perceived, from that of ordinary putrefaction. These nauseous and unwholesome odours are therefore possible from the soil of our fields; but any occurrence such as this on a large scale would be disastrous; and the ground is protected from it by an almost constant acidity, which sometimes increases so as to be injurious, forming what is called sour land. This very acid state generally occurs in wet land, where it is probable that alkalinity would be more injurious, but the soil may be found alkaline in a well manured garden, if the ground is dry, without apparent injury. The other mode in which the organic matter may decompose is by the formation of nitric acid, the nitrogen obtaining oxygen directly from the air, and so providing against the excess of ammonia which might readily occur in certain soils, producing results which might be fatal to animal life, if not provided for by an enormous vegetation. At the same time it does seem reasonable to suppose that the cause of a diseased climate may frequently be found in such a decomposition as that mentioned, when too much ammonia has been formed, or too little nitrogen has been removed by the formation of nitric acid. All the circumstances may be present for the purpose—abundance of nitrogen compounds and moisture; even a very warm climate is not essential. And

it also seems reasonable to suppose that the formation of nitric acid is one of the means by which such evils are avoided, whether in town drainage, where it becomes evident from the great amount found, or in the imperfectly drained and rich, although not swampy, lands of tropical climates, where the large amount of nitrogen, if converted into ammonia, would, no doubt, produce the worst effects.

Excellent, however, as a porous soil is and a gravelly hill, it is possible to overload them; and, in that case, their porosity tends to evil, because the passage from one place to another becomes so easy. This is one of the evils that does not take place on a clay soil, where sewage matter remains unabsorbed and traceable, however noxious.

MARSHES.

This subject is too important to be treated in one chapter, and I have not much of my own to say, but it seemed right to give a few words from the latest writings in Italy at a time when it is possible that, by driving onward our plans of irrigation by sewage, we may be changing considerably the atmosphere of certain districts. The plans which are given in this volume for examining air will show, I believe, clearly whether the irrigated land causes the air above to be inferior or otherwise. The subject of miasms from marshes has been taken up in Italy with renewed vigour, and a few extracts may find place here from a little volume, called 'Il Miasma Palustre: Lezioni di Chimica Igienica, date nell' Istituto provinciale di Mantova, a Antonio Selmi.' Padova, 1870.

'Let a ray of light enter a dark room, and you will see along the line myriads of bodies, to which, not long ago, was given the name of *limo*, equal to atmospheric dust.' (P. 8.) A beginning like that by Lucretius.

'One of the most hurtful maladies is the intermittent fever, which, if not cared for in time, may grow from an intermittent to a deadly one. The symptoms have been studied by medical men, and the cause of the fever has received the name of miasma. Of these miasmas there are various, according to their origin. Some proceed from animal bodies in putrefaction, and also from sound bodies, and these may cause a bad habit of body and other illnesses, more or less acute, but they never produce intermittent fevers with special character; these are produced by vegetable substances, which are subjected to putrefaction, particularly if alternately immersed in and taken out of water.' (Pp. 9, 10.)

He says afterwards that such fevers were never found near the large manufacture of manures at Montfaucon, or on the canal of Vettabia, which receives the sewage of Milan.

He quotes Siminot as saying that, 'wherever the marsh miasm exists, man is put into the dilemma either to remove the cause or to be annihilated.'—'In such places the deaths are more numerous than the births.' (P. 12.)

'The existence of a substance in the air capable of disarranging greatly the organism in certain circumstances was admitted by the ancients as by the moderns, and Varro, in his book 'De Re Rustica,' attributes the cause to insects, which are produced spontaneously where the waters become marshes, and penetrate into the organs. Such for a long time was the general opinion.' (P. 14.)

But Varro had not a microscope.

'I have said that the smell of the marshes was considered by the ancients as the cause of fever, and one reason is, that those who are attacked are always those who have been exposed at night to the influence of the air, but more particularly at the approach of night, or the first twilight of morning—properly speaking, at the

time of the formation of the dew; and this is the time when the smell is most intense.' (P. 17.)

'Humboldt said that in Mexico the fever did not trouble those who were 900 mètres above the marshes.' (P. 18.)

This is very high.

The author then reasons that the cause cannot be a gas.

He collects the moisture of the air by freezing—

'Having in one night, at a temperature of 10°, collected 670 cubic centimètres of liquid, I concluded that, as every cubic centimètre had a specific weight of 1·004, the liquid had been taken from 137 cubic mètres of air.' ('A cubic mètre of air, according to Pouillet, containing 4·91 grammes of vapour of water.') 'The water was examined. After allowing it to stand it was found by the microscope to contain a deposit. This was formed of a multitude of seeds of algæ and myriads of microscopic infusoriæ swimming. This, my first observation, was confirmed at the same time by Dr. Pietro Balestra at Rome .... He uses these words: "In the microscope the condensed dew exhibits only a surprising quantity of spores and sporanges."' (Pp. 34, 35.)

Dr. Balestra has given his observations in the 'Memoirs of the Academy of Florence,' I believe. I have not yet been able to see them.

The reference to Terentius Varro led me to look up that work which, however common in name, very few people seem to possess and still fewer to read. The chapter referred to is the 12th of 'De Re Rustica,' lib. i., using the text of the Amsterdam edition of 1623. A portion runs thus:—

'Danda opera ut potissimum sub radicibus montis silvestris villam ponas, ubi pastiones sunt laxæ, ita ut contra ventos qui saluberrimi in agro flabunt. . . . Advertendum etiam si qua erunt loca palustria, et propter easdem causas, et quod arescunt, et *quod in iis crescunt*

animalia quædam minuta, quæ non possunt oculi consequi, et per æra intus in corpus per os et nares perveniunt, atque efficiunt difficiles morbos. Fundanius: Quid potero, inquit, facere, si istiusmodi mi fundus hæreditate obvenerit, quo minus pestilentia noceat? Istuc vel ego possum respondere, inquit *Agrius*, vendas quot assibus possis; aut, si nequeas, relinquas. At *Scrofa*, vitandum, inquit, ne in eas partes spectat villa, ex quibus ventus gravior afflare soleat; neve in convalli cava; et ut potius in sublimi loco ædifices, qui quod perflatur, si quod est quod adversarium inferatur, facilius discutitur. Præterea, quod ab sole toto die illustratur, salubrior est, quod et bestiolæ, si quæ prope nascuntur et inseruntur, aut efflantur, aut aritudine, citò pereunt.'

Having no translation by me, I shall give one, not such as a critical Latin scholar would give, but such as may be excused in one who has only occasionally read Latin since his youth.

'The greatest care ought to be taken to have a villa built at the foot of a woody hill, where the pastures are open and exposed to the most wholesome winds.

. . . . . .

'Attention must also be given to see if there are any marshy places, both for the reasons given and because they dry up, and because in them certain minute animals grow, which cannot be followed by the eye, but which enter into the body by the mouth and nose, and cause troublesome diseases.

'Fundanius: "How then shall I prevent the pestilence, if my farm comes to me by inheritance?" "That I can so far tell. Sell it for as much as you can get; and if you cannot sell it, you must leave it," says Agrius. Scrofa added, "You must take care not to build the villa where the wind is violent, or in a deep valley; it is better to have it on a high situation, where anything that blows against you is easily driven away. Besides, that place is healthier which

is lighted all day by the sun, because the little animals (animalcules) if they are produced and brought in are either blown away again or die on account of the dryness."'

How could this idea come into the minds of men without microscopes? It seems to me that it must have come from observing the midges or smaller gnats that rise from water so frequently in the summer evenings. The mind can readily picture to itself some so small as not to be visible. Indeed, the midges that do torment us in the evening in moist places are not actually seen. If this were the origin it would really be only an accident that prompted the reasoning. We have seen that the latest Italian idea is rather that the cause of the evil from marshes is not animal.

What is a marsh? This is not very easily defined. There must, however, be present water and plants. We are not quite sure that the plants must be of a particular class, although it would seem that plants of a certain class are among the guilty. We may with great certainty look to at least two sources of malaria from marshes, the living and the dead plant, without saying whether the poison is itself animal or not. When water contains more vegetable matter than it can oxidise, by the amount of absorbed air, some is given out as we know, and the results are bad. There may be apparent exceptions, when the soil is so porous that the water passes rapidly through, and carries with it the dissolved matter before it has time to evaporate. If the soil is not porous to a great extent, the first may happen—that is, some will pass into the air; if very porous, the second—that is, it will be carried below until oxidised: when the first happens, the plants may be better fed, or there may be a loss by surface drainage according to the condition of the soil: when the second happens, there is the action in the under drainage with its consequences.

Now how much sewage of a given strength will any given soil endure? We must know the exact strength, also the rapidity of drainage, next the amount of rainfall, as some of the elements in our calculation. That which may be pleasant in one place may cause a marsh fever in another. After deciding that there shall in any given place be no excess of matter coming off, the next question is, How much of the manure is actually used? We are often told of the great amount of crops obtained, but the true economical point is, How much is saved? The sanitary point—How much escapes into the air?—is the chief question, but that passing into the water is not to be neglected. (See 'Conversion into Nitrates,' p. 508.)

This, like ventilation and many other problems, refuses a simple solution.

To give examples. It seems probable that the soil of the south of England, being more porous than in most places in the north, will be very well suited for irrigation by sewage so far as the sanitary question is concerned. Besides this, the rainfall in the middle, east and south-east, being smaller than in the west and north, there is a reason why these first parts should take up sewage more easily without hurting the atmosphere. The example was set in the north, it is true, but in a rough way. The amount of sewage which soil can bear must be according to its porosity and the rainfall, when ice does not interfere. But these are not the only points; the amount is complicated by fertility which again is modified by temperature, and the question for any untried soil or climate is so difficult as to be at present out of our power of satisfactory calculation. In such a case, we are driven to ask the question directly of nature, and this must be done in several places and with great care. How much sewage will affect the atmosphere at various seasons of the year and in various parts of Britain? I believe the

methods used in this volume will decide whether the air is more loaded with material, and we can reason on the probability of the good or evil of that material.

I give no decided opinion further than this: that to treat all places in a similar manner is to seek failure, and to apply experiments in Middlesex to prove what can be done in Lancashire or in Lanarkshire is to rush on disappointment. The labour required is enormous: it is to be hoped that we shall now have experiments better regulated, hitherto thirty years have scarcely produced new results.

The economic question requires the same diversity of treatment. I suppose the British Association Committee will give it the attention so much required. Their work is very promising. I felt it right to say, when speaking on this subject, that we must not make new marshes; and I speak of no chimera.

The death-rate in Scotland even for counties follows remarkably near the density of population, although the effect of personal influence seems too small to be felt, except in the interior of houses, and leads us to look to the soil as one of the causes. When towns come in, all other causes retire, and meteorology as well as soil, with the earth and the air, are comparatively feeble in forming the annual average, although the former at least never fails to affect the monthly and seasonal average to a very large extent.

There seems to be a confusion in many minds between peat bogs and marshes, but the difference is very great. No peat-bog gives out marsh fevers and agues, although the cold and wet may induce rheumatism. The acid peat prevents decomposition, and so removes all the results of putrefaction, which some people suppose to be the origin of the evil in marshes. The living plants which may be the cause are not in the peat-bogs.

VENTILATION.[1]

The demands of ventilation would best be explained it we could reply to these questions :—What is the smallest amount of carbonic acid which may be called injurious? and what is the smallest amount of organic matter?

The amount of carbonic acid in the air is under ·04 per cent. in places that are healthy, but not above ·032 in the most open and healthy places. About five times that amount affects a candle sensibly, a photometer being used, and it is extremely probable that less affects it also; are human beings affected as readily? I rather avoid this question at present; we have not facts enough. But we do not require to speak of the gas except in conjunction with organic matter, and 1 per mille, Pettenkofer's point, seems a very natural point to fix on as the first stage of very bad ventilation when the exhalations from the body are present with it. Let us take the two together, and we then find that much depends on the temperature also. If the day be warm we may pass from a room having ·06 in a hundred of carbonic acid to the air with ·03 and feel refreshed. If the day is not warm, we do not feel the difference; at least, such persons as I have examined do not. *The conclusion is that in the early stages of the want of ventilation the organic exhalations are the most injurious.* Now these increase with the temperature whilst the acid does not. For this reason we ventilate in warm weather for the organic matter far more than for the sake of the acid. As the former has not hitherto been estimated by weight, we may view the subject only in relation to the carbonic acid. I think it probable that we shall be able to view it also in relation to other substances. For example, so much temperature

[1] Partly from the 'Report on the Air of Mines,' 1863.

will represent so much organic exhalation, and the volume of air will differ accordingly. This is not attempted yet, but it is now in our power.

It is often asked, How often must we renew the air of a place in order to keep up the purity at a given point? The first column of this table gives the amount per cent. of carbonic acid which we may suppose to be maintained for any purpose in the air, the other columns the amount of air required in order that the amount of carbonic acid should not be exceeded.

Amount of Pure Air required per Hour, in Cubic Feet.

Carbonic Acid per cent.	For a Man.	For a Candle.	For a Man and Candle together.
·4	100	50	150
·3	133	66	200
·25	160	80	240
·2	200	100	300
·1	400	200	600
·09	444	222	666
·08	500	250	750
·07	571	286	857
·06	665	333	1,000
·05	800	400	1,200
·04	1,000	500	1,500
·03	1,333	666	1,999
·02	2,000	1,000	3,000

If a man brings 100 cubic feet of air to contain ·4 per cent. of carbonic acid in an hour from zero, he will bring 1,000 cubic feet up to ·04 in the same time, and he will, therefore, at the end of the hour be ready for another 1,000 cubic feet of air to prevent the amount exceeding the proposed limit. But if the air which is supplied contains already ·04 per cent. of carbonic acid, he will not be able to keep the air of the space so pure. Let us suppose that

he chooses for his limit ·06—that is ·02 higher than that which he receives as fresh—he will bring 2,000 cubic feet up to that in an hour, and this will be the constant supply wanted. The table will, therefore, do for that condition equally well; we only require to look at ·02 above the air of the place, and we see 2,000 feet stand opposite it.

If this is correct, the amount of air required for ventilation will depend on the quality of air supplied. Let us take a manufacturing town with ·04 of carbonic acid and ventilate 1,000 cubic feet. A man will bring the air up to ·08 in an hour, he will then require another 1,000 if he does not allow it to go higher, or if he prefers it to stop at ·06, he will require 2,000.

Amount of Air containing ·04 per cent. Carbonic Acid—say town air—required per person per hour.

	Cubic Feet.
To keep air at ·05	4,000
„ „ ·06	2,000
„ „ ·08	1,000
„ „ ·10	666
„ „ ·12	500
„ „ ·14	400
„ „ ·16	333
„ „ ·18	286
„ „ ·20	250

But let us suppose the air to contain only ·032 per cent. of carbonic acid, we do not require so much.

To keep air at ·05, subtract ·032, and we have ·018, which is used to divide ·40, giving us 22·22, which multiplied by 100 = 2,222 cubic feet.

As some persons put down 0·6 as the amount of carbonic acid produced in 100 cubic feet of air in an hour, column II. is added for that mode of calculation. If this amount be general, all the quantities would require to be increased in the same ratio, but I go by my own experiments.

Cubic feet of Air containing ·032 *per cent. Carbonic Acid, required per person per hour when* ·4 *is produced in* 100 *cubic feet per hour, and when* ·6 *is produced.*

	I. When ·4 is produced.	II. When ·6 is produced.
To keep air at ·05	2,222	3,333
" " ·06	1,428	2,142
" " ·08	833	1,250
" " ·10	588	882
" " ·12	454	681
" " ·14	370	555
" " ·16	312	468
" " ·18	270	405
" " ·20	238	357

When the ventilation is desired to be very good, the amount required when very pure air is supplied is much less than with imperfect air. As the demands become less, the difference diminishes. If ozone were taken into consideration, the difference would probably be much greater; but I do not know what allowance to make for that body, and have left it out of consideration. In the smoky towns there is *none* at all.

The space 100 cubic feet, of the first table, has been supposed for convenience; but if it were 1,000 cubic feet, the actual amount of air consumed by a man would, of course, not be less or more, and the supply would require to be the same for his lungs; but the time required to produce the effect on the total space would be different. This point having been very strongly held by some scientific men, the conclusion has been drawn that in all spaces, and under every circumstance, the same amount of air must be supplied. Now, it is true that the same amount is to be actually breathed, and if this breath is thoroughly mixed with all the air of the room, the same amount must be supplied for ventilation, whatever the size of the room. But let us suppose—the most common case—that the thorough mixing does not take place, and we have at once a different amount of air required. The calculations become, therefore, incorrect on this supposition,

and the above tables have this error. The other condition —namely, imperfect mixing—is so various and characteristic, that we cannot reduce it to rules; we may hold a smoking substance in the midst of an apartment, and find the smoke go directly to an opening without mixing with the air of the room. It goes, too, with a velocity greater than that of the air of the room, otherwise an opening must be supposed sufficient to change the whole air of the room in a few seconds, that being the time required for the smoke to reach the opening. If we could drive the impure air in a similar way in a narrow current towards its exit, we might manage ventilation with a very small amount of air. To do this is a matter of great importance, because the expense of building is becoming so great, that very few men can afford to pay for a large enough house, and whilst rents are rising, the rooms of the middle classes have actually diminished in some places, and within these few years. Yet the evil of small rooms is great, because rapid currents are required for ventilation; cold currents are hurtful, and the warm difficult to obtain. If, however, we could obtain warm air-currents, it would not be important for us to have the rooms so large. It is a question of price. I believe the warmth must be obtained as the first demand of nature, and without it civilisation will go back. When men are cold, they give themselves to physical exercise, and if that is impossible, to discomfort, in which the mind refuses to do more than to complain, if it cannot forget. Which is cheapest for us? Is it to build large rooms and to have less warmth with slow currents, or to build small rooms and to have more warmth with rapid currents? It is to be wished, that the former should be the rule for private houses, more comfort and convenience are promised, and mechanism is not required; if it were, it could not be obtained. For hospitals, the use of mechanism is more within possibility. The expense of large rooms, when the architec-

tecture is of a kind intended to ornament a capital city, is very great; should we not gain by a judicious system of warming? Our methods of warming are very cumbrous, and we seem to be behind ancient Rome and modern Russia. We warm the air which changes in a moment when a door or window is opened, and we do not warm the house itself. Builders make the walls thinner in these days, and we sit at a fire very much as savages do over a blaze in the open air.

This is less the case with large rooms, where we require slower currents. We may next ask, Is there any advantage in rapid currents at any time? There is; in the case of infectious diseases, it would seem in the abstract to be of the greatest importance that the patient should be in a current, speaking as a chemist, and not a physician. The first reason is for his own sake. Even in health we poison ourselves, and in disease we tend more rapidly in the same direction. Infectious emanations may be collecting round a patient, and if so, the still air will keep them more carefully near him. I speak only generally, and do not enter on the hospital controversy.

Perhaps we cannot have rapid currents in large rooms very easily, so much air is required; but we can have frequent changes of air. It is clear, however, that the rapid removal of the air collecting around patients with infectious diseases, and probably, also, non-infectious, is most likely to promote health, both in the patients and in the attendants. Few people can stand the rapid motion of cold air, and if we must have rapid currents, they must be heated.

The source of the air with which we ventilate ought probably to be high in all cases, but even here we must move slowly. We are not quite sure that any infectious disease ever sends its emanations high into the air. Disease seems to creep along the ground; the causes may be at a considerable height, but we are compelled to suppose

them very thinly disseminated there; and the action seems to be according to quantity as well as intensity; towards the surface they congregate and are active. This we see from the evening air especially in marshy places; it is only after a certain repetition of the attack of the more thinly diffused wandering substances falling down from the atmosphere and accumulating, that men yield to the influence. As a rule, it would be unnecessary to purify the air of the daytime, if in an open place, even in average towns; and in most places it would be unnecessary to purify the air of the night in this country. It would, however, be better to warm it in northern and damp climates, and even in temperate climates, in order to produce a difference of temperature between the air entering the room and that within it, even if the necessity arising from the cold of rapid ventilation did not occur. In inhabited rooms the moisture increases as much as the organic matter, and the condition of the air is similar to that of the evenings of summer: whenever the temperature goes down a little, there is a deposit of dew; but when the warmth increases, the air is laden with moisture, and the condition resembles that near a warm close vegetation. In both cases ventilation is wanted. Our walls become saturated with moisture if they are porous, if not porous they are covered with streams of water. The moisture has organic matter in it which is not removed by mere drying, and the effects are very long in leaving. We may know this from breathing the air of any bed-room in a London hotel, or in most private houses in towns. People are afraid to keep their windows open because of the smoke without, and so they retain the organic matter. We can readily smell this, even if arising from healthy persons, and it has ceased to be a matter of surprise. If unhealthy persons are present, unhealthy matter may be expected to diffuse.

If, then, any disease is propagated by organic germs

living on the organic matter of the atmosphere, or associated with it, it is not at all wonderful that the disease should lurk in corners of houses, in clothes, or other porous matters, simply because we can trace floating matter to its lodgment in such places.

It is remarkable how readily porous bodies absorb the moisture of the air, and substances with it. I find that the leather on the bookcases in my study, where gas has been used, is made rotten, and in exact proportion to the height, the highest being so frail that it can scarcely be handled, whilst the lowest is still pretty firm, although much less so than at first. The amount of sulphuric acid in the pieces is also in proportion. The intermediate are affected in an intermediate way. No better proof can be had of the absorptive action of these porous substances, and of the unequal state of the atmosphere in various parts of a room.

When rooms which have absorbed organic matter have been shut up, the original peculiar smell ceases, and a musty one takes its place ; we recognise something which instantaneously brings that of mould to our minds. We cannot doubt that the air in such cases is full of the spores of such plants; the plants themselves grow in abundance, and we know well that when they grow they readily send out colonies. The leather of the bookcase was said to show the inorganic bodies; the books themselves are covered with the organisms when care is not taken, so that one small room gives an epitome of the whole subject. We have here, therefore, no mysterious agent, but one that is perfectly plain. Why should the agent be mysterious in the case of the infectious disease ? It is only so far a mystery—we do not know the different plants or organisms, and so cannot tell whether we have health or disease in them by merely examining them through a microscope.

If porous substances have the characteristics alluded

to, why use them? There are some difficulties here. If a wall is to be cleaned frequently, and rubbed when wet, it is better that it should not be porous. That seems quite clear; but when these processes cannot be undertaken, it really seems as if it were better to have it porous. Such substances absorb moisture in some seasons, and give it out slowly at others. Our clothes are of this kind. It is not possible to have warm coverings not porous. Porous bodies hold also a good deal of air, and they cause oxidation more readily. Nature has employed them for disinfection more than any others. The whole soil of the earth is a great disinfectant, kept in constant activity, being constantly required, holding in itself the most nauseous and unwholesome things, and still having healthy people living over it. However, the soil may be too full, and at times it becomes so, and therefore we run to places which cannot contain much impurity, such as bare rocks; and in such places we obtain pure air. If in houses we have too much organic matter for the porous substances to oxidise, we must resort to non-porous surfaces; but then they must, like the rocks, be often washed, or excessively exposed to the air or the warm sun.

To purify rooms the air must blow long into them, or every part must have the organic matter rubbed off by the hand. This is a sufficient rule for both hospitals and private houses. Good rubbing will purify furniture, and this our housewives know; long-continued currents of air are also known to be good, but better as a supplement to rubbing. The rules are very easy chemically, but mechanically they are difficult. This is merely a repetition of that which has been said elsewhere, and long ago, although it is here stated in other words. The world must be told everything in ten thousand different ways before it learns, and it is wearisome to repeat the lesson. I am only saying, also, what every clean house-

keeper carries out; and yet there is an apparent novelty in it when we compare it with the sayings and doings of many persons, intelligent and observing although they be.

Reasons for Ventilating.

It is often asked how much fresh air we must allow to come into a room in order to keep it wholesome; so far as carbonic acid is concerned, some information has been given; but the amounts vary so much, that we shall never be able to answer the question as it arises in all its changes unless we consider our reasons for ventilating.

The first is certainly the dislike to organic smelling substances evaporated from living beings. As some of them are very volatile, a very little rise of temperature increases their amount; and in warm weather we require a change of air so frequently, that we cannot make any use of the carbonic acid test. The amount of change is infinite; we require it for every breath, and we do not consider whether a door is sufficiently open; we open all doors and windows, or leave the house entirely.

Let us take the other extreme—a very cold room—an Eskimo ice hut. The amount of air wanted is wonderfully small; we do not know how much the carbonic acid may rise, but it must be very high. The organic matter is frozen, and is probably condensed on the ice; it may be inhaled as a solid, and in a form not to affect the smell. For a similar reason we require less ventilation in cold weather: it is not foolish, as some will endeavour to persuade us, to take less, but it is a natural instinct. We object to the cold, and we learn that heat is a more pressing want than even pure air, whether the organic matter affects our senses or not.

The next reason for ventilating is allied to the first; we say it is to produce freshness. This means that, although all the air of the room be quite new, it has

received a something from the surfaces in the room which must be cleared out. This is the reason that housewives like to keep the doors and windows open, and allow the air for a time to blow through the house. This process removes the last particles from the furniture, and is that finish which polishing cannot give. If the undefined impurity exists in large quantities, only rubbing can remove it rapidly, and this is done when we clean thoroughly walls and furniture. If very bad, and time presses, whilst elegance is not a demand, we cover the whole, and find whitewashing to be a ready mode.

The other reasons for ventilating have arisen from scientific inquiries. We wish to remove the carbonic acid; we might be able to find this gas by the senses if it were very abundant; but it is not so as a rule, until after our senses have informed us of the organic accompaniments. It furnishes, however, one of the most important, and probably the most important of the reasons in every-day life, because carbonic acid is the most rapidly hurtful to animal life of all the emanations from the person. It lowers the vitality rapidly, and kills with indefinite warning. The best warning is the organic matter, which acts for both. The warning by carbonic acid is called indefinite, because people lose vitality, but do not observe that the cause is in the atmosphere, there being no smell connected with that gas. At night, when lights are burning, the carbonic acid warns better than the other impurities, by simply putting out the candles. This seldom occurs in private houses, but dim burning is common enough. Mr. James Napier, F.C.S., tells me that he has seen the candles beginning to go out in a small meeting-room in the country which had a low ceiling and was crowded. It was needful to keep the door open. How blunt, then, is our perception of carbonic acid!

Another reason for ventilating is to remove solid

floating bodies, including infectious matter, fungi, and peculiar emanations arising from disease or other anomalous conditions. These floating bodies can sometimes be distinguished by the sense of smell—in the case of mould, for example. If it were possible to describe a smell completely, this class would be largely subdivided, since experience has made many persons very learned on this subject. It is, however, a knowledge which we have not yet been able to receive from or communicate to others. This whole question requires careful examination. The knowledge of the smell cannot be taught, but it may guide us to much that can be taught. Ventilation for this class of bodies will be probably much more attended to in future; care must be taken to drive them to the nearest opening, and not to allow them to diffuse themselves through the room in which they may be produced. In some cases resort must probably be had to rapid artificial and heated currents.

We ventilate to remove smoke and ordinary dust; these are easily seen; and we use ventilation to procure dryness. Moisture rises constantly from the skin, and if an inhabited apartment is not ventilated, that moisture accumulates. By opening the windows we cause floors to dry sooner, and we remove moisture from all the apartments, and that moisture has generally organic matter with it. If we ventilate with very moist air, we shall dry nothing; if we have very dry air, we may dry too thoroughly. It is, however, better for us to have what is called practically dry air, that is, air capable of containing much more moisture than it has. The amount of drying work done is according to the dryness, warmth, and speed of the air; with little of these qualities, a great bulk of air is required, where otherwise a small bulk might have sufficed. By warmth and dryness we have an advantage in requiring less rapid currents. I have said elsewhere that a climate has a

certain advantage from being very rainy. To call dryness an advantage is not a contradiction. When rain falls down and washes the air, we can feel the benefit; when the substances floating in the atmosphere are dried up, we can imagine the advantage; but when the air is kept loaded with moisture which does not fall as rain, and is not carried off by wind, we can easily understand why the results should be hurtful. That it is not the watery vapour itself that injures may be learnt from dye-houses, where men spend their lives in all conditions of dampness, sometimes in steam dense enough to make it difficult to see to the distance of a yard. There they have not the heavily-laden moisture of hot-damp climates with rich vegetation, and they have abundant warmth, so that the moisture is not used for absorbing heat and producing colds.

In a very temperately, and I may say beautifully-written pamphlet by Dr. De Chaumont on 'Ventilation and Cubic Space,' he discusses the amount of air required for 1,000 cubic feet for an individual, and refers to a discussion on the subject which had about the time (1867) taken place.

He says, p. 9:—

'Now to attain this minimum, that is, to ensure the carbonic acid ratio never rising above ·06, we must supply 3,000 cubic feet per head per hour; for, taking Professor Donkin's formula—

$$x = p + \frac{P}{A}\ ;\ \text{when } x = \cdot0006,\ A = 3{,}000.^{1}$$

Here p = carbonic acid per cubic foot.
P = ·6 carbonic acid given off by one adult in an hour.
A = delivery of fresh air in one hour.
x = ratio of carbonic acid desired (in this case ·0006 per foot.)'

[1] Leaving out a term of the formula, which becomes rapidly inappreciable. The complete would be $x = p + \frac{P}{A} - \frac{P}{A}\epsilon^{\frac{-At}{c}}$.

The conclusion to which this leads—namely, 3,000 cubic feet per hour—it may be right to adopt when there is uniform diffusion; but I may say that I did not obtain in the lead-chamber so much. The amount was ·4 cubic feet of carbonic acid per hour, which would lead to 2,000 cubic feet per hour with uniform diffusion. My trials were not made on large men, although on healthy.

At the end Dr. De Chaumont says:—

'1. We cannot safely accept a lower standard than ·06 per cent. of carbonic acid.

'2. We cannot safely legislate for anything short of uniform diffusion in an air-space.

'3. Uniform diffusion being supposed, we cannot preserve our minimum standard of purity with a less delivery of fresh air than 3,000 cubic feet per head per hour.

'4. We cannot safely change the air, on an average, oftener than six times in an hour without producing draughts.

'5. With ordinary means of ventilation, we can seldom hope to succeed in changing the air even six times in an hour.

'6. We must provide an air space which will admit of the delivery of 3,000 cubic feet per head, per hour, and at the same time preclude the necessity of changing the whole air so often as six times per hour.

'7. To fulfil all the above conditions, a minimum of 1,000 cubic feet per head is absolutely necessary.

'8. To provide the supply of 3,000 cubic feet per head per hour, so that the velocity of the current at the point of entry should not exceed 5 feet per second, 48 square inches of total inlet and outlet area ought to be provided, and this independent of the chimney, if there be an open fireplace.'

Men compelled to come to a conclusion are obliged to postpone many questions, and I should think, that under

the judicious management of Dr. De Chaumont, such an arrangement would be very good. If, however, I were asked to view the matter in the abstract, not for the sudden use of a builder, I should say, regarding No. 1, the amount of ·06, recommended also by Pettenkofer, is very good at a temperature not to be considered cold. When cold, we can stand a great deal more ; how much I do not know. Nature prefers the increased carbonic acid to the increased cold. As before said, the cause of this is the organic matter being condensed by the cold. On the other hand, organic matter rises with warmth, and oppresses us before the carbonic acid increases much. This division of the subject has not been attended to.

No. 2 cannot be carried out fully, except when the air is rather warm ; but the nearer we approach to uniform diffusion, the more hurtful must it be, and especially in cases of disease. Professor Donkin is quoted as saying, p. 8 : 'It seems hardly conceivable, though it is mathematically possible, that the whole quantity of poison remaining permanently in a room could be reduced by any contrivance below that of uniform diffusion.' If I understand this right, this uniform diffusion for removal of impure air is never resorted to in private life, and, perhaps, never occurs at all when open fires are used. If a room is full of smoke, we may put up a window and see the smoke driven forwards almost unmixed with the air ; and if a door is opposite the window, and clear passage, the whole may be cleared with extremely little diffusion. The act of filling the room equally with pure air is not uniform diffusion ; but if air, pure or impure, mixed itself equally with that of all parts of the room, that would be uniform diffusion. This, practically, does not occur in our rooms. Suppose the room to be full of carbonic acid for an extreme case, let us pour in air above and make an opening below, and there would be scarcely any diffusion. Let us, again, suppose it to be filled with coal-gas—we might make an opening above

and drive air in below; and this actually is done at gas works, the gas coming out free from the air till very near the junction. The diffusion of gases is in reality a slower operation than we have imagined, and this is the reason that our courts, alleys, and houses become so easily unwholesome. One part may be cold and another warm in the same room; one man may breathe ·08, and another ·15 of carbonic acid; at one part he may be comfortable, at another he may faint; a current of pure air may run along the floor to the fire, when those above it may be poisoned. These figures are from actual analyses.

The other proposition by Professor Donkin is, that 'the same supply of air will equally ventilate any space,' or, what is the same thing, as much air is required for a small space as for a large space. Uniform diffusion being allowed, this follows as a matter of course.

Cotton as Absorbent or Filter.

As Dusch and Schroeder found cotton to keep out the germs in the air, I tried it on the chlorides and sulphates with great success; the experiment was made with a water aspirator, and must therefore be compared with those done in a similar way. When the cotton is washed, the salts are found in the water.[1]

Air.—Water Aspirator.—Air drawn through Cotton Wool free from Acids.

DATE.		Hydrochloric Acid retained by Cotton Wool.		Hydrochloric Acid retained by Pure Water.		Sulphuric Acid retained by Cotton Wool.		Sulphuric Acid retained by Pure Water.	
		a.	*b.*	*a.*	*b.*	*a.*	*b.*	*a.*	*b.*
Nov. 19 to 25.	Fine. NE. and SE. wind.	None.	None.	None.	None.	15·317	35·053	153·17	350·530
Nov. 26 to Dec. 17.	Fine most of the time. NW. and SW. wind.	17·525	40·106	1·168	2·673	60·851	139·257	162·27	371·355

a. Grains per mill. cubic feet. *b.* Grammes per mill. cubic metres.

[1] See experiments with cotton 'Fifth Report of Proceedings under the Alkali Act, 1868,' p. 59.

Organic particles seem to be retained even more forcibly, and, as elsewhere mentioned, the cotton was found by the above-mentioned chemists to prevent putrefaction.

Dr. Tyndall has been still more successful in removing particles from air by the use of cotton, as I understand.

Dr. Stenhouse proposed that for many purposes the air should be filtered through charcoal, and Professor Tyndall has proposed filtering through cotton wool. The advantage of charcoal as a filter is peculiar; charcoal not merely absorbs gases and vapours, but it decomposes thoroughly some of them, and, by a process of oxidation, makes the most noxious innocent. It may be said to go on for ever, at least I am not aware that Dr. Stenhouse has found any limits in time to its work, so long as it is supplied with air. No mere mechanical absorbent has this advantage. A greater activity was found when the charcoal was platinised, and this substance is peculiarly Dr. Stenhouse's. It is not, however, used in a platinised state in large quantities, simply because it is too expensive.

The ventilation of sewers is in many cases very important, i.e., when people use no disinfectants; but the ventilation is of course not a pleasant idea, because the gases are thrown into the air, and when there, we scarcely can tell what they will do. They become diluted certainly, and their power of mischief diminished, and for some this may be as good as destruction. This advantage we gain by ventilating directly into the open air. It is the least of two evils. To remove this objection, the charcoal filter was invented by Dr. Stenhouse; at least, this was one of its applications, and it is certainly a great step. The question may be asked, Is it a permanent step? It seems to be true that we cannot lay hold of the gases of a sewer except in part; they make their way by passages innumerable. I do not doubt that we could build houses that would exclude them, but we do

not build such. If ventilation is to be the cure, we may also add ventilation inwards, so that the air may find its way into the sewer, and not out of it, except at prescribed spots from which we can defend ourselves. It is proposed by Peter Spence, Esq., to feed furnace fires with it, and so to burn out the evil as well as to send off its products of combustion.

This seems very complete in principle. A ready mode of using the draught of an ordinary fire for the purpose would be good. Mr. Spence applied the heat of his kitchen fire to this purpose with good effect, and perhaps this solves the whole problem of the ventilation process; but care must be taken to have the chimneys tight, because the smoke from one chimney frequently passes into other chimneys, and thence into other rooms, making, in the case of sewer-gas, the end worse than the beginning. Here the structure of our houses, with their hollow walls and interminable secret and to us unknowable passages for air, come in to prescribe great caution lest we carefully drive the poison into the place where it can do most harm. Mr. Spence's other plan, to use chimneys outside of the house, is free from this danger. However, this danger might be removed by proper building, if it is not too expensive to do right. Houses built on a hill have a ready mode of sending off their gases by ventilating their sewers, but the houses generally become themselves the ventilators, and in towns the highest often suffer most from this reason, one apparently easy to avoid. We have in use no perfect way of ventilating sewers, and for this reason I have a good deal advocated their disinfection, by which I mean the use of substances which prevent the formation of the noxious gases, instead of allowing them to form first so as to be sent away. The new plan of sewers, which may be called that of the late Board of Health, is to wash everything out so rapidly that it cannot find time to putrefy. This idea is good, but

is not easily attained by the greatest part of humanity; a few favoured places only seem to have it. At present, the disinfection of sewers is equal to any mode of ventilation actually in practice; but unless a better result can be obtained, we shall be obliged to change our minds regarding the use of water-closets, and allow the luxury to be foregone. The water increases the rapidity of decomposition, and the gases come off in a more concentrated state whenever the temperature rises above 54° F. than when there is little water. The hope of success by water alone is in a great excess of it. Mr. Thom, of Chorley, who tried the rapid removal by abundance of water in a country house, found the pipes to fill up by aggregations of fatty matter; and judging it impossible to wash this away, he opened the sewer-end, which hitherto had been closed to keep out the rats, and, in order to prevent these animals being washed away, made sidings or landings for them as the only perfect scavengers he could find. Whether disinfection would annoy these workers, who can say without full trial? The two extremes are the safest, want of and excess of water. The first cannot be had in a sewer, and whenever there is stagnation a dangerous medium state is attained. In very dry climates, the evil may be more readily avoided than in temperate ones, and in very cold climates still more so.

I have been desirous of putting to use the modes introduced of examining air in order to settle the important question, whether we suffer from the natural mode of treating manures on the midden system more than from the watercloset system. Having been educated into a thorough belief of the great value of the water system, I have supposed that the greater amount of impurity always found in the air behind houses with middens, and in the neighbourhood of these places, was a sufficient answer to the question; the examination of the air of Manchester given in this volume having led to it, but unfortunately the air

of Glasgow gave a no better result; on the other hand, London seemed to give the balance for the water system. The difficulty may be stated thus:

The midden covers more ground than the closet, and there is greater exposure of surface to the air.

The midden is a great accumulation also, and the amount of manure lying about a town where this system is employed is enormous—many acres for a large city.

The water-closet system offers little surface exposed to view.

There is very little accumulation.

The midden system allows free exposure to the air; ventilation is effected naturally, and there is no machinery for concentration; whereas, when not much rain is allowed, the want of water prevents decomposition.

The water system facilitates decomposition. Water does this to an enormous extent; and although the amount of manure present in the town is by this plan comparatively small, the decomposition being faster, we have not proportionate freedom from the gases.

We are not sure, however, that the gases and other products of decomposition are the same from both. The channels in the water system prevent great admixture of air with these gases, which for that reason they attack us in a more concentrated form than those from the middens when they do reach us.

But the chief objection is, that the channels point so much into the houses, and the houses are so built that the whole surface of floors and walls may be permeated, and the bad air may blow out of any hole made in the plaster of the wall or in the floor, unless the house is properly built.

As I said in 'Disinfection and Disinfectants,' 'water is the most powerful agent of infection known to us, as well as of disinfection. Substances which preserve for ever when dry become putrid at once when moist. All

organic bodies decompose most rapidly in water, and if sent out of our towns laden with riches, water dissipates them all, and sends them into the air.' This must be taken with limitations. It does not send all into the air, but it helps to disperse all rapidly.

I have come long ago to the conclusion that the water-closet is one of the greatest of luxuries invented in modern times; but also think that 'the midden is better than the bad sewer.' Also that all mankind cannot have the water-closet. It may be found quite unsuitable from excess of cold or excess of dryness, and it may be too wasteful and costly in very wet climates, and on soil not suitable for irrigation. The question is, therefore, not a simple one for a *yes* or *no*, but an extremely complicated one, where many conditions must be balanced.

This experiment, however, is desirable—the examination of the air outside of the houses of a sewered town and a midden town. I feel almost confident of the answer—indeed, the analyses in this volume may be said to give it, because the backs and fronts of the houses of a midden town give different air, the backs giving worst; whereas the backs and fronts in a water-system town cannot be different one would suppose. Glasgow and London not being similarly built are not suited for comparison.

The next point is the examination of the air inside the houses of these two classes of towns. I think it probable that, as matters are now conducted, the water system will be the worst in all houses not large enough to have sufficient separation. This is found in the smaller houses of Glasgow, to my mind at least very distinctly; also in houses where the best mode of placing the sewers, &c., is not employed. We come to this, that the danger is outside the house on the midden system, and inside on the water system, when the danger does exist. By good building and ventilation the gases

will be thrown outside in both cases, and the sewer will then, in suitable climates, have the best of it. But we must remember a system is not a cure; we must have details correct.

On the other hand, in small courts the air of the outside necessarily enters the houses, taking much of the midden air, and the danger is found both inside and outside. There is certainly much to say in favour of ventilation of sewers, even without leading the gases through fire, from the fact that everything we think of in connection with these matters leads us to admire a free passage of air, the absence of close places, and the absence of all methods of concentration, gathering or keeping in a place any foul air when found. This has a very wide bearing. One point must not be forgotten in carrying out this principle, that all accumulation in towns ought to be free from water as much as possible.

Another is, that no panacea is found for the evils of decomposition any more than for the diseases of humanity. In other words, no one plan will do everywhere.

A rather wild clamour has taken place about sewers, since writing this, and bad as their gases are, the danger arising from them has been much exaggerated. Let us look at most of our manufacturing towns. They use the midden system. The want of the water system has been said to be the cause of their unhealthiness; now, some have laid the evil on the sewers. We know enough to show that neither system by itself will make our bills of mortality satisfactory.

On the mechanism of ventilation of houses there is much to learn, and he will make a great mistake who takes that also for a simple problem; merely the supply of so much air without regard to speed, warmth, and shape of apartment, and one may say situation of house, will not bring the desired end. I wish I could give the speed of air tolerable without injury at various tem-

peratures; but I cannot, and it will take long to make the experiments, as they must be made on many persons. Perhaps the records of the ventilation of the House of Commons would furnish this best.

SPECULATION.

I have almost entirely avoided speculation, and dealt with very dry facts, unillumined by any act of the imagination, or at least nearly so. We have had enough of fancy in sanitary matters for many years, yet a speculation is of the utmost advantage to an inquirer; it is an appearance of a something in the mist before us, and we go to prove it. Were all the mist devoid of appearances, and we could see no symptom of a footing, we could only grope, and all our discoveries would be mere accidents. It is not well to give all our imaginings to the world, but I am disposed to repeat one with more emphasis than before, so as, if possible, to moderate the fear that some persons have conceived of the bodies found in the air, and called organic germs.

We have many people so afraid of this organic matter of the air, and of all its floating particles, that they would like to filter it all out and breathe the gases pure. We must not allow our fear to go too far. We have no reason to be sure that air free from floating particles would be wholesome; we have only the proof that, if there is an excess of some kind, it is unwholesome. Apparently everyone can breathe air tainted with any disease without being hurt, if the taint is small enough. Inconceivably small particles injure; but we must learn to divide even the inconceivably small. We can bear a larger amount of taint if it is diluted enough. Dilute sufficiently the air of a hospital, and infection ceases. One short time of the infected air produces disease; a long period of the diluted air produces none, although

the number of particles that must pass over a certain spot must be much greater in the long time than when the stronger mixture passes in the short time. We learn from this that the amount that does injury is not infinitesimal; there must be a certain quantity. I do not doubt that we shall measure that quantity readily; we can readily measure the amount of ammonia and organic matter in the infectious and non-infectious atmospheres. The really practical work must begin after this. It was my desire to have done so in one of the hospitals, but having begun I was interrupted, and it must not be done in a hurry. I do not feel hopeless of being able to say that in a scarlet-fever atmosphere there must only be so much nitrogenous organic matter and so many germs, otherwise infection will be certain. Still the best work will be the microscopic, when we gain the power of distinguishing one germ indicating one disease from another germ indicating another disease, either at once or in some stage of their development.

But not to lose the thread of the argument, let us say again, a certain quantity is necessary for infection. We must not be frightened at living in places where the innocent quantity may possibly be. We cannot find a place where we are free of all. If we only allow our own breath to accumulate we have evils enough, and if others are near us we have still more; we can escape from our own atmosphere and that of our friends only by diluting, unless we live entirely out of doors, on the tops of hills, or on the decks of ships, where the dilution of the organic matter may be so far carried out as to be practically equal to its annihilation, or where so much air and time have been given, that the infectious particles really are destroyed.

If it is a speculation that germs of animal or vegetable matter bring disease in the air, we must bring a counter speculation that germs also bring life and vigour. It has

been said that putrefaction is begun by certain organisms, although the result is a chemical breaking up of the substance putrefying, and so of fermentation. If carbonic acid is not formed in sugar and albumen during the acts of fermentation and putrefaction without the aid of microscopic organisms, how is it that it is formed in the living being without the aid of such organisms? Or do the organisms not increase its formation to say the least. We begin to be cold at the same time that putrefaction almost ceases; we begin to feel very warm when it begins to move most rapidly; and we are unable to endure the higher heats which diminish its activity. Men have long supposed that the decomposition of albuminous and saccharine bodies had an analogy with the operations of life.

We must next ask whether the operations of life could go on without these organisms. It is probable that we could never completely try the experiment. Even if we obtained the purest air we could not absolutely, so far as we know, ensure the entire absence of the minuter organisms from ourselves. We can reason, therefore, only from imperfect cases. We may then ask the next question: Does life go on more rapidly where there are few or where there are many of the atmospheric organisms? It seems clear that life, at least of a kind, goes on more rapidly where they are most numerous. If we take the length of life, it is to be feared that the answer is clear enough; life comes sooner to an end. The end, however, may be caused either by diminished or increased action of the vital functions. I have shown that carbonic acid increases the rapidity of action of the lungs, and diminishes that of the pulse. This shows diminished vital power and increased desire for power; and it may be sufficient to explain the peculiar condition of the classes who live in crowded places. But if we take the effect of the organic matter also, and consider its action, it is

extremely probable that irritability, feverishness, and incapacity for steady, calm work may be the physical consequence, whether we consider this matter as acting in a purely chemical way, or as organisms, or in a double manner. An irritability may be caused by the blood having a bad influence on the brain, which will become supplied with a liquid which has already a tendency to act too strongly to enable it to give up the requisite demands to the brain. Of course this is put in very general terms. This may be a physical cause of drunkenness and many vices of unregulated minds.

We may speculate a little further. 'As iron sharpeneth iron, so doth the countenance of a man his friend.' Cities are always more active minded than rural districts. May there not be something physical in the excitement, the air around us being filled with particles exciting activity in our blood, which activity may be wholesome up to a certain point, but beyond it being less and less under the control of reason.

The other side may also be seen. When men accustomed to towns take a sea voyage, it is generally said that the activity of the mind is diminished; they become calm, and are capable of spending whole days in idleness. Of course we might speculate a good deal further, and imagine that we could tell the variations in the germs, and so on; but it is not well to go very far at a time in these directions, and I think it better to remain at the general belief in the probable more or less excitement or tendency to chemical decomposition in the living blood caused by those same substances that cause decomposition in a putrefying substance. If this is correct, there must be a quantity which will do good, and another which will do evil.

Speaking of this subject, a friend told me a year or two ago that he had been speculating also on these bodies, and had imagined them influencing very strongly and

strangely the germination of all animals, including the human being.[1] This is an interesting subject. One is inclined at first to think it not correct, since the mixture by wind might cause the influences over all places to be alike. This, he says, would not hinder his theory being correct; but I should like him to speak in his own words of the reasons on which his opinion on this and the action of the uvula are based. Another speculation—the first writer of which I do not remember the name of—was given out at the British Association[2]; it was to the effect that the same germs are found through all space, and so organisms may come from other worlds. This is a fine link for all nature, but it does not put farther from us except in miles the origin of living beings, and is therefore not of the highest importance. There is no *à priori* objection to the opinion, if we could only imagine organisms standing out against cold for that wonderfully long time which would be needed for them to pass through the great regions of space. This suggests subjects for experiment on the action of cold, an old inquiry well worth resuming.

We have not found such organisms in meteorites, so far as I know; but there are some of these bodies which contain organic matter. There is even a supposed one which contains the forms of organised matter; it is a piece of wood charcoal. However difficult to account for the amount of oxide of iron it contains, I think it more probable that it was produced on the earth. I analysed it about the year 1849. Others containing hydrocarbons have been found. I do not know if the microscope has been sufficiently applied to them.

There seems nothing absurd in supposing important aids to the growth of animals to come from the air, as well as from the earth; and if we can prove a community of property in air and its contents to exist among other

[1] Mr. John Thom.

[2] By Sir William Thomson.

worlds and ours, as well as among other continents and Europe, we go to the beginning of some very important inquiries.

With us, however, it is of immediate consequence that we should keep to the facts that immediately affect us. We have not yet any reason to believe that the health of the world has been affected by meteoric matter from space, although this idea is by no means new, being before our time. We do, however, know that health is affected by matter projected into the air by influences at work around us, and these influences must be fully studied as sanitary agents. This leads us to think for a few minutes on crowding in large towns.

Crowding.

No one can read over the analyses of the air and rain of the towns and country places without coming to a very old conclusion, namely, that to crowd a population is one of the very worst faults. It was asked, when I was speaking to the Medical Officers of Health of London on this subject, what was the standard I held out for air in a town. This may be answered so: A place in which people are healthy may be said to have good air—let us take it as such. The analyses show that in Glasgow, for example, there is inferior air; and that which approaches nearest to Valencia for rain and corresponding air approaches nearest or surpasses the best hitherto found, so far as chemical analysis goes. We must not set up any too high standard; it is best to begin by destroying the worst. Let those courts, alleys, and streets which show the greatest mortality and the worst air be destroyed or improved without foolish mercy. There is a want of willingness to pull down dangerous property, but a readiness to rush forward to save the life of the greatest criminals. Reason is out of the question in the

matter ; we are misled by an uneducated feeling. We like to save property, forgetting that deadly weapons and poisons are subject to peculiar laws, and their indiscriminate use is forbidden to the nation. Houses that produce death are not property : as well might a man claim his debts as such. If a man sells unwholesome meat, the law interferes ; if he sells the use of a room with fever in it, the nation seems not to complain. Officers of health point out such places, but the public still refuse to destroy them, and great numbers are slain annually by legal methods, whilst strict measures are taken to prevent a few annually being killed by arsenic—a death more agreeable than the lingering misery in the lower parts of our crowded towns. I know that the lowest classes living in poisoned houses die from other causes than bad air ; but I am speaking of air at present, and that is one of the causes. The time must come—and the sooner the better—when it shall be enacted that no land shall contain more people per acre than we know, by experience in several places, can live healthily thereon. The same thing must be said regarding houses, although these are more difficult for Governments to deal with, because of the degradation of some of the population. Still the limitation must be attained, and for that we must strive. As to the number on an acre, that may be settled at once, and enacted at once ; indeed, we may say so also of houses, provisionally.

The tables of analysis given show the condition of air and rain in places influenced by manufactories ; and it is important to take for them a starting-point also. If we cannot cure all the evils, we can at least cure some ; and if a place has already attained a certain degree of impurity as tried by these tests, it seems quite fair to interdict further extension of manufactories in it, unless, of course, these are carried on in an improved manner. By constantly urging the best and attacking the worst, we may

attain great results. At present there are no towns carrying out the sanitary principles which we do understand. Faith in the principles of sanitary economy is wanted, and I confess I am not surprised at this in the minds of many of the public who cannot give much time to the study. The question is not simple, and the rules for judging of the condition of a place have not been clear. I hope this will soon cease to be the case.

EFFECT OF ACID GASES ON VEGETATION.

This action of acid gases on vegetation causes more complaints against chemical works and other factories than any other purely external circumstance connected with them. Although we know too little on the subject, I hope that one of the advantages of the Reports under the Alkali Act, or in this volume, will be to enable us to know when the air is so much acidified, as to make vegetation hopeless. We require only to examine the rain or air of a place and compare it with those of places which have and which have not been injured. We must, however, compare places as much as possible in a similar climate, the driest allowing more acid without injury. We do still better, if we can obtain experiments made in the place itself which is the object of complaint, injury, or study.

[1] Having examined many plants in order to see if it were possible chemically to show that acid gas has been the cause of injury to them, I have come to the conclusion that it is in our power in many cases. The Belgian Commission came to a similar conclusion. I do not, however, feel able to speak so decidedly as the members of that body in all cases. If, for example, the leaves are broken, from whatever cause, the juice of the

[1] Proceedings under the Alkali Act, 4th Report, for 1867.

plant giving out hydrochloric and sulphuric acids, as chlorides and sulphates, presents a great difficulty. Care must be taken on this point. Many plants contain chlorides even on the surface of the leaf, at a great distance from alkali works, and comparisons must be made with caution before drawing conclusions.

The larger spots made in gross cases can often be pronounced at once to be caused by acids.

There is, however, a deterioration which cannot be chemically traced to acid gases, and which the Belgian Commission decidedly pronounces to be the result of other causes, being accompanied with minute fungi. Before going so far in this other direction as these gentlemen, it seems better to wait until we can answer these questions: Is it not possible to cause, by means of gases, a deterioration in the atmosphere sufficient to effect an early decay of plants without injuring the life of fungi? And is it not possible that the weakness of the plant may rather subject it to the attacks of fungi? (see p. 557)

The Belgian Report mentions 2,000 mètres, as the greatest distance from chemical works at which damage was observed. This must depend on the size of the works, and the number which are together. In this country, 2,000 mètres, or 2,187 yards, is a distance sometimes found quite insufficient for protection.

It is mentioned that during rain the damage does not extend so far, although it is more severe, because the rain washes the gas down to the ground near to the place of exit. It may be also mentioned that in moist weather, with a low barometer or light and wet air, the rise of gas cannot be so high as with a high barometer; when the dew point is low the gas is more readily condensed. This happens in the damp atmosphere during the day, but it is more observable during the night, leading many persons to suppose that the gas is given out at night, even in cases where no such plan is ever attempted.

It is on these wet days that the roots are injured. The Belgian Commissioners say little of roots, still it is clear that both in Belgium, France, and Germany roots are destroyed and the ground laid bare, whilst actions for damages are not wanting.

The remarks made in the Belgian Report regarding the protection given to crops by the undulations of the soil will be remarked with interest. Many have observed that a hedge, or a tree, or a wall, have remarkable influence; the side from which the wind blows may have the vegetation below it burnt up, whilst the hedge itself will be greatly injured, whereas on the other side both the hedge and the crops below it may be safe. Gaps allow the wind to pass through unaltered, and to do injury. Indeed, I have observed a slight wall, and also a board not many inches higher than the plant protected, to be in some bad cases quite efficient. How is it possible? has been asked. Does the gas go in straight lines unmixed? We know that dry gas does not do so in dry air; it mixes with the air, and is every moment more widely spread. Then why does it not mix with the air behind the protection? The best reason assigned is, that in the case of the acids which are generally found guilty of doing injury, there is no gas mixing uniformly with the air. The gas is absorbed in small globules of water, and these are thrown forward by the currents of the wind, and are driven over any wall or interruption to a distance in a curve from the top. If this is a true explanation, it is an extremely interesting one; the peculiar effect is not confined to visible vapour so far as I can ascertain. Shall we proceed farther, and argue from it that the vapour of the air does consist of small particles of liquid? Shall we not rather argue that the acid present attracts water, and the completely gaseous form is therefore not retained in a moist climate?

It might be asked whether the cause is not the same

in character as the protection of snow from being melted by a warm wind when that wind blows behind a wall, and can roll over it on to the snow beneath? (See Action of Surface.) Here the rapid changing of surface in the case of direct blowing is productive of much more rapid action. To vary the words, Is it analogous to the case of cooling, freezing or hardening of the same snow when a cold wind blows directly upon it whilst the snow on the other side is protected and remains soft? There, again, the influence of rapid change of surface comes forward. In the case of the acid there is also a rapid change of air. Nevertheless the fact of the dry atmosphere removing the evil seems to decide that rapidity of flow and frequent change of the air is not the cause, but that the real cause is in the many and minute globules of water which are formed with or without the aid of the acids and which become saturated with the gases in the air; these retaining some of the solids, are tossed as solids are tossed or driven by violent winds in direct lines according as they are in the full currents or in side currents, or are caught by eddies and whirlpools.

In 1867, I requested Mr. Rothwell, of Croft, near Warrington, to give his opinion regarding the action of acids on plants. He had been very often employed to estimate the amount of damage done at St. Helen's, and I had, and have, much confidence in his judgment. He sent the following:—

Capacity of Plants to resist Acid Fumes.

Written by Mr. Rothwell at my request.

Violent winds ruffle and break off the leaves of trees and shrubs and damage herbaceous plants, such as corn, &c., by breaking the stem, and thus stopping the flow of sap, and in the blooming season of any plant or tree lessen or injure the crop for that year. But generally there is a clear distinction between damage done by a storm and that by a bad vapour.

The latter *shrivels* and curls up the leaves, does not break them off or make them ragged. A storm never discolours the bark of a shrub, tree, or makes it fast to the stem. Bad vapour does both.

With a low barometer and a gentle air travelling at the rate of a mile in the hour, the vapour sweeps gently along near the surface of the earth, and does great damage to plants. A storm and bad vapour together do great damage in summer.

As respects fruit trees, I am also at a loss to say which are soonest affected; but my idea is that cherries, greengages, and other finer sort of plums, are sooner injured in the *fruiting*, but not in the *foliage*. I think damsons are soonest affected in this. Fruit trees, I think, should be in Class No. 1.

I think I have told you that in all my thirty-three years' practice of valuing damage by bad vapours, in the year 1867 I have had the greatest difficulty in satisfying myself. Even now, when the valuations are sent in, I do not feel satisfied. I have known many years colder and wetter, but in the last thirty years I do not recollect seeing such rapid changes of temperature, which are injurious to plants as well as animals.

I send a list of trees and plants soonest affected by noxious vapours, and in the order they are put in this list according to my views, but will not vouch for its correctness, as I have not paid much attention to the order in which they are affected:—

Forest Trees.

1. Larch.
2. Spruce Fir.
3. Scotch „
4. Black Italian Poplar.
5. Lombardy „
6. Ash.
7. Oak.
8. Elm.
9. Birch.
10. Alder.
11. Sycamore.

Fruit Trees.

[1]1. Damson.
[1]2. Greengage.
[1]3. Halewood Plum.
4. Jacob „
5. Pears.
6. Apples.
[2]7. Cherries.

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[1] St. Helen's was a good country for fruit—now all gone.
[2] Not killed soon, but stopped bearing.

Shrubs, Evergreen, and Wild Plants.

1. British Laurels.
2. Portugal „
3. Aucuba Japonica.
4. Barberry Evergreen.
5. Hazel.
6. Guelder Rose.
7. Sloe Thorn.
8. Hawthorn.
9. Raspberries.
10. Gooseberries.
11. Blackberries.
12. Gorse.
13. Hollies.

Farm Crops.

1. Potatoes.
2. Mangel.
3. White Clover and Rhubarb.
4. Red Clover.
5. Trefoil.
6. Rye Grass.
7. Wheat.
8. Oats.
9. Barley.
10. Common Turnips.
11. Swedes.

Nothing is affected sooner than the common fern; the larch fern especially; marestail and giant fern also.

I have not studied much as to the order plants are affected, I therefore will not say I am correct. I have compared my opinion with the ideas of a person who is a farmer, and has lived many years in the neighbourhood of St. Helen's. We do not differ much in our opinions on this subject; in some others, in connection with bad vapours, we do.

All the plants under one number in the list we consider to be affected at the same time.

From noon of October 26th, 1867, to noon of the 28th the weather was very stormy, the wind varying from S.W. to W. and N.W. To the east of St. Helen's and Widnes common turnips were *very much* damaged in the leaves; mangel *much* damaged, and swedes a little. Thorn fences and fern much damaged. The bulbs of the turnips, &c., would not be much, if any, less in weight, as the season was too far advanced. I therefore put no damage on those crops. Young clovers in the stubbles were much damaged, but I only put a money value on this in one instance, when the young clover was much advanced in growth. In all other cases I considered there would be no injury of any importance.

The manufacturers considered the damage was by the salt water from the sea. As it was possible this might be the case, I made up my mind to prove it. I therefore proceeded by rail to Rufford, and then walked across the country, passing between Prescot and St. Helen's to Cronton, and then from

there by Widnes to Warrington. At Rufford there is a clear sweep from the sea, without any town or works intervening. Here there was not the least damage to be seen, and this was the case till I got in the line of Liverpool and Prescot; then damage was distinguishable to fences and root crops (there was no corn out), but nothing like to the extent as to the east of St. Helen's and Widnes. All right about Cronton. From Widnes to near Warrington very bad. This showed that the smoke of Liverpool and Prescot had a bad effect, but not equal to that of St. Helen's and Widnes.

The sea in that storm did no harm so far inland.

Second List of Plants affected by Noxious Vapours, mixing the Classes according to the Effects produced on each.

I.

Fern, only in the summer.
Scotch firs, spruce, and larches, a little in winter.
Clover, white and red, receives damage in winter to the roots.
Trefoil „ „ „ „
Rye grass „ „ „ „
Poplars.
Hawthorns.
Potatoes.

II.

Wheat receives some damage in winter.
Oats, in May; when in the grass state soon receives damage.
Barley.
Mangels.
Common turnips.
Rhubarb.

III.

Laurels, British and Portugal
Aucubas „ „
Yews „ „
Holly „ „
Gorse „ „

} These plants receive damage in winter, but more in summer.

Old grass meadow and pasture receives much damage in the winter.

IV.

Ashes, oaks, hazels.
Horse chestnuts.
Walnuts.
Spanish chestnuts.
Sloe thorn.

Second List of Plants affected by Noxious Vapours—continued.

V.

Swedish turnip and cabbages.
Damson.
Other fruit trees.
Beech.
Elm.
Birch.
Alder.
Sycamores.

Trees exposed to noxious vapours get bark-bound, and then cannot thrive, and thus they take damage in the winter. The sap cannot flow when spring comes.

Grass-land takes as much damage in winter, I think, as in summer, as it is more exposed in winter, and the bad vapour floats more on the surface of the land. On fields much exposed to the vapours handfuls of dead grass can be pulled up in the spring, smelling strongly of the vapour.

W. R.

This last observation, made by Mr. Rothwell, has not been verified by my experience. It may also be said, as I believe, that a storm may curl up the leaves of trees and render them brown without causing them to be ragged. A remarkable result is observed in wheat exposed to acid gases. The crop may be to appearance full and ripe when scarcely a trace of grain is to be found. This dies at an early stage and withers up, whilst the rest of the plant takes its apparently usual course.

Mosses may be seen to grow in the acid-rain of towns when trees, shrubs, and grasses disappear.

By a few experiments, not yet published, I find that one of acid in 50,000 of water destroyed all appearance of chlorophyll in some water plants in less than a fortnight. Sulphuric acid acted much more rapidly than muriatic. This was quite against expectation and general belief. Another experiment showed that sulphurous acid

had very much less effect than either the sulphuric or muriatic. This was also quite unexpected. Why is muriatic acid most feared? The reason seems to be that it is driven off from the common salt with great violence and in great quantities at a time, so that it is more overwhelming in its attacks than sulphuric acid, whilst on the other hand it unites with water and does not disperse so readily as sulphurous.

The Appendix is a natural sequence to this part of the volume. It will, to some extent, show the character of work done relating to this subject under the Governments of two foreign States, and may be useful as well as interesting.

APPENDIX.

FROM FRENCH AND BELGIAN SOURCES.

Here it will be interesting to see portions of the Belgian report, entitled: 'Fabriques de Produits chimiques. Rapport à M. le Ministre de l'Intérieur, par la Commission d'Enquête, instituée par arrêtés royaux des 30 août 1854, 25 mai et 6 septembre 1855. Deuxième partie.' 'Recherches et Observations relatives à l'Influence des Emanations acides des Fabriques des Produits chimiques sur la Végétation.'

The commission consisted of Comte L. de Baillet, president; G. E. Guillery, vice-president; J. T. P. Chandelon, secretaire; C. Davreux, J. B. Depaire, M. Dugniolle, M. Everaerts, E. Gauthy. M. Chandelon is professor of chemistry at the university of Liège and inspector of works for the government.

II.—*Influence of the Hygrometric and Barometric Conditions, and the Temperature of the Air.*

We have shown that it is not sufficient that the currents should pass over the plants, but it is necessary that they should pass at the same level, and so touch them. It may be seen, then, that all the circumstances which occur directly or indirectly to maintain at a high level or to press towards the ground the columns of smoke which escape from the chimneys, and in which the acid gases are expanded, contribute to prevent or to favour the production of the damage which these last exercise upon divers plants. Now the hygrometric condition, the barometric condition, and the temperature of the air, are evidently in the number of those circumstances which either directly or indirectly modify the nature of the winds and the atmospheric currents.

It is thus that the moist fogs wet the surfaces of plants, rendering them more fit to condense the hydrochloric acid contained in the currents which strike them. The noxious action is then exercised on those plants which are sensible to it.

On the other hand, by the passage of the currents of acid gases, a certain number of little aqueous drops charged with hydrochloric acid, fall from the fogs upon the leaves and other organs of the plants; this last effect may take place even when there is no fog, for besides the acid gases and the several products of the combustion of coal, there is a very great proportion of watery vapour in the smoke which escapes from the chimneys of chemical works. It is easy to understand, then, that there must sometimes fall from this smoke drops of water containing a slight quantity of hydrochloric acid, and sometimes also a little soot. It is in this way that we have explained the origin of certain spots which are manifestly produced by drops of water charged with acid, and of which we have proved the existence on the leaves of plants near to works.

III.—*Influence of Rain.*

The rain also has its part to play in influencing the production of the phenomena of deterioration which are manifested in the action of acid emanations; it brings the hydrochloric acid gas into the region where the smoke currents cross it, and the result evidently is that a certain number of drops of water charged with a little acid fall upon the soil or upon the plants.

This is only within a certain radius around the works and confined to the direction followed by the currents of smoke, otherwise very little. It is in fact impossible that it should be otherwise, because the column formed by the smoke cannot pass along a zone of rain without giving up to it all the chlorhydric acid which it contains. We believe, then, that during rain at a distance of a thousand mètres, for example, from the works, and very probably much less distant, the column in question will scarcely contain any chlorhydric acid. It is, therefore, a manifest error to suppose, as many persons do, that when it rains the drops of water can inclose a certain

proportion of chlorhydric acid, even at a great distance from the works. The rain, on the contrary, becomes in this case a most efficacious process for the condensation of muriatic acid, which prevents it reaching any place distant from the works, even when it is in the line of the wind.

We may affirm, therefore, that when rain falls more or less continuously for one or more days, places at a distance from chemical works are absolutely defended from the contact of chlorhydric acid, whether gaseous or dissolved in water.

Many persons with whom we have found ourselves in communication during our observations in the province of Namur, have shown themselves strongly imbued with a contrary idea. They believed that the rain waters constantly retained a certain quantity of this acid at every distance from the works. As experiments conduce to conclusions more sure than theoretical reasoning, we have made a great many practical researches relative to this question. In working with blue litmus paper we have never in any case found the smallest trace of acidity in the droplets of water collected on the surface of plants when we operated at a great distance from the works, even in the direction of the wind.

This result did not vary, whatever was the origin of the water upon the surfaces of the plants, for it was always perfectly neutral, whether it came from the rain or was produced by dew, or by the condensation of moist fogs.

On the contrary, at a short distance from the works, we have constantly found an acid reaction in the water collected on the plants which were in the direction of the gaseous currents, and were enveloped in the currents, whatever was the origin of this water.

Frequently in the neighbourhood of works we have been convinced that the water came from the dew or the humidity of the fogs, and became spread over the surface of the plants, and still it did not contain a trace of acid, even in the direction of the wind. This is what takes place when the smoke is not driven towards the ground so as to form by contact with the earth horizontal currents, but passes horizontally at a great height, or rises obliquely into the atmosphere. The experiments which we have recorded give us to understand that under certain circumstances the rain assists the noxious

action of acid gases in the neighbourhood of works. This is especially the case with chlorhydric acid, for in softening the plants it gives them the power of condensing, but not of retaining, this substance upon their surface.

If this is so, we can understand, all things being equal in other respects, that the noxious influence which certain gases exercise on vegetation in the neighbourhood of chemical works ought to do greater damage during periods of rain than during a dry season.

It is, therefore, necessary to consider, as an element of assistance in appreciating researches of the kind which occupy us, what are the rainy winds. As in all parts of the continent of Europe, north of the Alps and the Pyrenees, the same general causes appear to regulate the distribution of rain, we may cite the observations which M. de Buche has made at Berlin, how much rain is brought by every wind.

. . . . . .

IV.—*Influence of Topographic Situation, Distance, and Configuration and Undulations of the Soil.*

A circumstance to which we have always attended as being of great importance in the examination of the state of vegetation in the neighbourhood of chemical works is the topographic situation of the places in which we have made our researches; when, for example, we have made investigations in a wood we determined what were the parts which were exposed by their position to be directly touched by the currents of gas and vapour coming from the works, and at what distance they were situate in relation to it.

Experience has demonstrated that the greater part of the deterioration which we have observed in the neighbourhood of works, and which ought to be attributed to their influence, has been caused by the acid gases striking the plants directly in the form of currents. The damage produced in any other way is not of much importance. It is the topographic situation and the distance of one point in relation to the centre from which the acid emanations are disengaged which causes them to be directly struck by them, or which places a protection against them.

A host of different cases may present themselves, but it is

always easy to know the best way of proceeding, as one of two things happens most frequently: either the columns of smoke which escape from the chimneys of the chemical works begin to fall at a short distance from their point of departure, and form then horizontal currents at the surface, or they move at a great height in the atmosphere, and extend themselves often very far, being disseminated and dissipated by degrees. We have often remarked even a mixed case, that is to say, a division of the column of smoke into two portions, one of which was raised obliquely into the atmosphere, whilst the other, after being driven against the soil, formed a horizontal current a little oblique to the direction of the wind.

When the smoke did not fall to the soil it was observed generally that it followed a direction more or less horizontal, or rose obliquely into the air. It is very rarely seen to rise nearly vertical.

After determining the position of the point fixed on in relation to the chimney, it is easy to see whether or not it will be struck by the smoke, and what part is most exposed to the smoke. . . . . But the smoke which escapes from the latter does not contain absolutely all the gas or noxious vapours which are sent into the atmosphere, in consequence of the operations at chemical works. There are others, for example, sulphurous acid and sulphuretted hydrogen, which rise from the heaps of soda waste. The emanations of this kind are driven forward in the direction of the wind, and finish by disseminating and mixing with a great quantity of air. . . . (Several drawings showing the direction which the smoke takes, especially striking the higher grounds, are left out.)

We might give many cases, but sufficient is said to show the great influence of the rising and falling of the soil. As to the relation of the distance from the centre, from which the gases escape, and the effects produced by them, it is clear that since the currents they form are rarified, they mix with the air and are dispersed in proportion as they recede from the point of departure. The consequence is that the noxious influence is only seen in a limited radius round the works.

After what we have said relative to the circumstances which have attracted our attention in investigating the state of vegetation, we shall explain shortly the principal facts which

prove most precisely that different kinds of plants suffer considerably in the neighbourhood of chemical works.

I.—*Woody Plants.*

Woody plants having no very defined duration, the damage which is done to them at a particular moment may still be perceived in the following year when the branches are grown. The result is that from year to year it increases in such a way that one can judge better of the consequences of the change. In other respects, woody vegetables, such as hedges, bushes, or isolated trees, more or less elevated, whatever be their position in the neighbourhood of a chemical work, have different aspects under different conditions of exposure in relation to the centre from which the gas comes; consequently the comparative examination of plants in different aspects furnishes a collection of facts of the highest importance for the solution of the problem which occupies us here.

Some examples will show this truth.

Let us suppose trees, bushes, or hedges in a position where they are sometimes touched by the fumes which proceed from a chemical work, what will be the result if the acid gases contained in the currents have the power of hurting plants? They will necessarily produce this effect wherever they come in contact with them. The side, therefore, which is struck by the gases will be injured, and those sheltered from the attack will remain untouched.

The evidence is clear and simple. What ought we then to conclude? It is this:—

1. For all trees, hedges, and bushes sufficiently dense in foliage the damage which is due to the direct action of the currents of acid gases ought not to be seen except on the surfaces turned towards the works from which the gases escape, and by no means on the other surfaces. The first in reality form a kind of screen, which protects the latter against the attack of the flowing gases.

2. On the other hand, for trees, hedges, and bushes which are not close in foliage, either by nature or in consequence of some loss of a great many of their branches, the damage ought to show itself as far as the side opposite to the works.

3. In a hedge very thick and tufted every opening or loss

of continuity, on the side towards the works, will permit currents of gas to pass through and give rise to damage even on the side opposite to the works.

4. In a wood which is composed of coppice and low skirting plants exposed to the direct action of fumes which are loaded with acids, the damage produced is not observed except in a straight line, the breadth of which is in proportion to the facility with which the current of smoke can penetrate it, whilst the size increases according as the coppice wood is opened up by the increased damage produced.

In the latter case in reality the bushes situated at the border of the wood become incapable, by the loss of the foliage and branches, of opposing a sufficient barrier to the progress of the currents.

5. Every break in the continuity of the brushwood exposed to the direct action of the currents, produces necessarily a deterioration, which is observed at a greater distance from the border at first than it would be were the brushwood not broken.

6. Every shelter, every obstacle, whatever it may be, which prevents the currents of vapour from striking certain woody plants, sensitive to the action of acid gases, must prevent these plants, placed as it were behind a screen, from suffering from the fumes. This kind of screen may in some places consist of a bank of land, of a wood, of plantations of poplars and other trees, a hedge, a line of houses, a wall, &c., forming as it were a curtain.

An important consequence is to be deduced from these considerations. If it is in such a protection that the woody fibres of certain kinds ought to be sound and free from the damage which plants of the same nature present in positions where they are not preserved from the contact of acid currents, it follows, of course, that they will cease to be exempt from the damage when the protection is wanting. *Now this is precisely what takes place according to numerous observations.*

Let us give in a general way some of these observations, chosen amongst those which have shown us what conclusions we ought to draw.

Sometimes we have seen woody plants of the same kind and of the same age, growing on the same soil, and sensibly at the

same distance from a chemical work, and under the same direction of the wind, presenting on one hand very great deterioration on the side towards the chemical works, and, on the other hand, manifesting none when they were found concealed behind a house interposed between them and the works. Sometimes it was a row of houses or other buildings, placed one beside the other in a manner more or less continuous, and behind which the woody plants presented no sign of deterioration which could be attributed to acid gases; but an opening, a breakage in the line of buildings, even if not more than two mètres in breadth, produced upon trees of certain kinds, placed directly in the way or a little to the side, more or less damage on their leaves or branches.

Often also we have remarked in gardens exposed to the fumes of chemical works that wall plants or other trees protected by a wall had their leaves or branches in very good condition as high as this wall, but above this height the leaves appeared to be burnt, whilst the young buds died. The wall served as a screen to the trees; their branches were deprived of shade as soon as they passed it.

When woody plants, very sensitive to the action of acid gases, grow behind a hedge or a curtain of bushes, we have also remarked several circumstances of great value for showing the nature and the cause of the damage presented. For example, when a hedge or a bush which acted as a screen was higher than the acid gases, and very thick and tufted, all the leaves and branches remained in a normal state; but, on the contrary, when the heads or bushes were lower than the gases, the leaves and branches above were damaged, leaving those below in good position. When any interruption presented itself in the hedge or the curtain of bushes, the woody plants in question were damaged from base to summit when no other protection was afforded.

After having indicated several of the principal particulars which have guided us in our researches into the damage done by acid gases we must show exactly how they are characterised.

Damage to the Leaves and Stipules.

The damage to the leaves and stipules is shown principally by a change of colour making spots in different positions and of different magnitude, according to circumstances. Sometimes the parts which correspond to these spots on the leaves become torn or perforated; but it is remarkable that these organs, although very much spotted, do not often fall immediately, although we have remarked that defoliation takes place at an earlier period than that which is usual for the kind of tree, in the case of the hazel for example. We have proved by a multitude of observations that on all the parts of woody plants exposed to the currents of vapour in general from chemical works the principal stains were marginal. We have always observed a striking contrast between the state of the foliage of trees more or less high, of hedges and of bushes, according to their position with regard to the acid fumes. In view of this a greater or less number of the leaves showed marks, more or less large and continuous, upon their borders, and very frequently shrivelled, jagged, and shrunk up. These marginal stains encircling the leaves more or less completely, and occupying more or less a part of the stem, were never found on the part opposite the works when the trees, hedges, or bushes were sufficiently close.

It was different with trees, hedges, or bushes which were extremely open on account of the loss of their branches, or too young to have acquired numerous shoots.

The facts to our eyes can receive only one interpretation, which is that the marginal stains of the leaves are produced by the action of the currents of acid gases when they strike the organs directly.

Besides the marginal stains which we have spoken of, we have often observed that there were others which existed on all the surfaces of the plant indifferently, and occupied a part, either on the surface, on the middle of the limb, or on the edges; but in this latter case they were not more or less continuous, they did not encircle the leaf to a great extent, and were not essentially elongated in form; at least the spots of this second kind were of a rounded form, sometimes irregular and a little elongated. We have considered them generally

due to the action of droplets of water charged with a very little acid. As to the origin of this acid water it is easy to conceive it, as it may usually come either from the rain or from mists, or from the condensation of a little acid, on the side turned towards the works, caused by the drops of rain or dew when the leaves are struck by the currents of fumes.

The manner in which this is done is evident, for it is clear that the water of the rain or fogs, which forms into drops after having condensed a little acid, may come in contact with all the surfaces of the trees, hedges, or bushes.

In general we have proved that the marks of the second kind had sensibly the same colour as those of the first. In some cases, however, we have seen the contrary, as towards the central parts of the spot was seen a little deposit of blackish soot, which indicated that in falling the drop charged with acid had brought this soot with it, at the same time as the vapour of water, the acid gases and the other bodies which enter into the composition of the smoke.

As the vapour of water enters largely into the composition of the latter, we understand that even in a dry season drops of water charged with acid may fall upon the leaves of vegetables situate below the column of smoke, but in this case they arise from the condensation of a portion of the vapour of water which comes from the chimney along with the acid gases. We have, in reality, found on the leaves two kinds of spots or stains due to the action of the acid: the one marginal, encircling the leaf, or a considerable part of it extending a certain distance towards the rib of the leaf, and sometimes, but rarely, touching it in such a way as entirely to surround the limb. These are produced when the acids strike the foliage either as currents acting immediately, or after being in part condensed by the moisture on the leaves, due to the rain, dew, or fogs.

The others are of the same colour as the preceding, but are much less important, as they occupy much less of the surface of the leaf, and are generally much less numerous. They are sometimes marginal, but more frequently central, and are more or less rounded, although they may have an irregular or elongated form. Like the former, they sometimes cause the tearing of the leaf or perforation of the limb.

Whatever the number, colour, extent, or position of these different spots, we have never seen the least trace of cryptogamic parasites, or any indication that any parasitic animal had been an agent in producing the effect. They arise from no organic malady or special defect in the organisation. We have found them on plants growing on the most diverse soil and exposed to all quarters; to south and to north, to east and to west; but only in the neighbourhood of chemical works, except in a few cases, which we could easily explain from the existence of influences prejudicial to the plants. We shall mention one of these observed by a member of the Commission on a farm in Brabant. Three large and vigorous vines appeared in perfect health, only there was observed on some of the leaves of each a reddening and shrivelling of the border. The damage to the leaves appeared to resemble that mentioned above as due to the acid gases; it was, therefore, important to find the cause. It was then found that below each of the damaged leaves there was an opening from which the air from the stables and cattle sheds escaped, and from which, for want of cleanliness, a great deal of ammoniacal vapour proceeded. It was, therefore, clear that the noxious vapours were carbonate of ammonia and sulphuretted hydrogen acting on some of the leaves of the vines: the stains were not found on any place except above the openings from which the ammoniacal fumes came.

The proofs already given that the two classes of stains spoken of, as observed in the neighbourhood of chemical works, are caused by acid vapours may be supplemented by the following:—

1st. That the stains of the first kind exist only at the parts exposed to the currents of gas, and that they disappear, even when near a chemical work, from the most sensitive ligneous plants, if a shade is interposed between them and the works.

2nd. They are very unequally spread in different directions around the works, so that in some they are little, if at all, observable. For example, in places against which the west, south-south-west, south-east, and south-south-east winds blow, we have observed the greatest damage.

We can only interpret this by saying that plants suffer most

which receive the fumes most frequently. This is the case with those to which the smoke is driven by the most dominant winds.

3rd. They showed the same character close to the works where the operation of the gases was evident as at a distance where the influence was less clear.

4th. Going in any given direction, we found constantly that they diminished as the works were left, and gradually disappeared.

5th. Having acted on several plants with hydrochloric acid, we have obtained at pleasure stains analogous to those which constituted the damage in question. This, for example, is the effect produced on the leaves of the hazel, apple tree, cherry tree, gooseberry bush, &c., to which we have especially directed our attention.

Altogether the examples we have given are sufficient to leave no doubt as to the cause of the damage done to the leaves. We have undertaken certain chemical researches to be afterwards detailed, whilst we give here some of the results. These researches have furnished new proof of the possibility and reality of the action of the acid gases on the leaves.

They have shown us, 1st, That in certain directions round chemical works the water found on the surface of the leaves, under certain circumstances, contains a mineral acid coming from the works, viz., a little hydrochloric acid.

2nd. That in operating on the leaves of the oak and the wych-elm in convenient situations, those which are effected may sometimes be proved to have a little chlorhydric acid present, whilst nothing of the kind is found on sound leaves of the same species collected far from chemical works.

3rd. In submitting to chemical analysis the leaves upon which numerous and large spots have been produced artificially by chlorhydric acid, only a very small quantity of that acid is found seven days after its action.

Let us enumerate the woody plants on the foliage of which we have established the existence of extensive damage, more or less frequently, of a kind altogether or in part the same as that we have above indicated. We give here the names of the greater part of the plants according to the degree of sensibility to the noxious influences of the acid gases :—

1. Wych-elm (Carpinus betulus).
2. „ (Carpinus incisa).
3. Hazel (Corylus avellana).
4. Oak (Quercus robur).
5. Beech (Fagus sylvatica).
6. Birch (Betula alba).
7. Sycamore (Acer pseudoplatanus).
8. Maple (Acer campestre).
9. Willow (Salix cinerea).
10. Hawthorn (Cratægus oxyacantha).
11. Spindle tree (Euonymus Europæus).
12. Elm (Ulmus campestris).
13. Lime tree (Tilia platyphyllos).
14. Sloe (Prunus spinosa).
15. Larch tree (Larix Europea).
16. Bramble (Rubus fruticosus).
17. Ash (Fraxinus excelsior).
18. White poplar (Populus alba).
19. Italian poplar (Populus fastigiata).
20. Aspen (Populus tremula).
21. Tuya orientalis.
22. Vine (Vitis vinifera).
23. Plum trees of different varieties (Prunus domestica).
24. Apple „ „ (Malus communis).
25. Pear „ „ (Pyrus communis).
26. Cherry „ „ (Cerasus vulgaris).
27. Currant bushes of different kinds (Ribes rubrum, Ribes aureum, Ribes sanguineum, &c.).
28. Rose bushes (Rosa gallica, &c.).
29. Lilac (Syringa vulgaris).
30. „ (Philadelphus coronarius).
31. Raspberry bushes (Rubus idæus).
32. Meadowsweet of different kinds (Spiræa ulmaria, Spiræa lanceolata, Spiræa bella).
33. Hops (Humulus lupulus).
34. Alder (Alnus communis, Alnus incana).

The colour of the marks on different plants varied from a livid white to a pale yellow, or from a brownish to a brown, yellowish brown, reddish, cinnamon, grey brown, or blackish grey. On the leaves, for example, of the oak, the stains were of yellowish white or pale brownish colour. Their colour approached brown on the leaves of the elm or yoke elm, the plum tree, and the apple tree, reddish for those of the nut, the vine, the rose, and the hazel nut, and blackish brown and blackish grey for pear trees, &c.

There was always a very great difference between the woody

plants, according to the degree of sensibility of the leaves. Accordingly the yoke-elm and the wych-elm, of all plants which we have observed, are the most severely attacked, so that on examining the effects produced by a chemical work they are the first species which show a change. Also, on removing to a distance from a chemical work, these plants are seen to be injured when all other kinds have entirely escaped.

The alders, on the contrary, are of all plants those which best resist the action of acid gases, as we have often seen common alders at a small distance from chemical works, in situations very much exposed to acid gases, showing very few marks on their leaves, and suffering apparently very little from the neighbourhood of the works.

Oaks, hazels, and birches are very sensitive to the noxious influence of these gases, and so are many varieties of apple, pear, and plum trees. We have proved by many observations that trees of these kinds are very different in the state of their vegetation, according to the varieties to which they belong. Apple trees, for example, of the variety called *court-pendu*, were found more vulnerable than those of other varieties.

Now we have sometimes remarked that there is a striking analogy between congener plants in the facility with which they are injured by acid gases.

It is thus that different varieties of the poplar, such as the aspen, *Populus tremula*, the white poplar, *Populus alba*, the Italian poplar *Populus fastigiata*, &c., &c., resist generally the action of the acid gases, so far, that they show no damage, where many other woody plants show it, and they must be very much exposed to acid emanations before they are sensibly affected.

As to the circle within which woody plants are observed to suffer from chemical works it is impossible to fix it in an absolute manner. It depends, in fact, upon a variety of local and variable circumstances, and even if these circumstances were constant the radius would differ in some degree for every species of plant.

The following will be convincing as to the truth of this:—

1st. The noxious action of the gases from chemical works

ought to be observed at a greater distance, according to the magnitude of the works, all other things being equal, and according as the processes for condensing and retaining the acid gases are more or less perfectly practised.

2nd. The action of the acid gases ought to be perceived at a distance which will be greater according as the configuration and undulations of the soil prevent the currents from moving and extending themselves freely in all directions.

3rd. The radius of a noxious influence in a given direction, depends not only on the more or less frequency of the wind which blows in that direction, and the amount of rain which it brings, but also very often on the height of the soil in relation to the centre of escape of the gases.

4th. Consequently the gas extends itself in proportion to the greater or less height of the chimneys.

5th. In a given direction the state of the vegetation shows that the radius of damage extends farther according as the plants which grow in that circle are more or less susceptible of damage from these gases.

6th. Some meteorological circumstances, not ascertained, but apt to change from year to year, may extend or shorten the radius in any direction.

To sum up, we observe that numerous circumstances, for the most part variable, contribute to form the radius in which damage is done by the gases from chemical works. To fix this radius in an absolute and general manner is therefore impossible, but it is possible to determine in every given case to what distance in various directions around the chemical works the gases have caused damage to woody plants.

In many cases we have seen pear, apple, and plum trees, of the variety called mirabelle, notably damaged at a distance of a kilometre to the south-east of a focus of acid gases, whilst at the north-east the effects of the same kind are observed at a distance of 1,200 to 1,500 mètres.

Sometimes in the direction of the prevailing wind we have observed the yoke-elm and wych-elm (*les charmes* and *les charmilles*) damaged at a distance of about 2,000 mètres, whilst, as to other plants, the radius of noxious influence was not so great, although the oak, the birch, the sycamore, the hazel, the maple, &c., were injured in a very decided

manner at a distance of 1,100 to 1,200 mètres from the point of escape of the gases.

In the direction of the less frequent winds the radius in question was always shorter than in that of the prevailing winds.

It is important now to examine the results of the damage done to the foliage of trees. Let us see what they are physiologically.

None of the two classes of stains which we have pointed out as due to the mediate or intermediate influence of acid gases give rise, properly speaking, to an organic malady of the plants, and so far as the vitality is concerned, their results are the same in quality, and only differ in their degree of importance. The continuous marginal stains which are more or less large, and which we have seen as the products of the immediate and direct action of the acid gases, extend themselves generally farther than the stains due to the indirect action of the gases. The consequence is that they cause more important results, although the nature is in reality the same.

The leaves being organs of fundamental importance every cause which tends to hurt them, to destroy them, or to cause a premature fall, injures greatly the nutrition of the plant. They draw out of the atmosphere certain principles, especially gases which are necessary to the life of plants; they constitute the organs of respiration, and they represent, according to the ingenious comparison of an illustrious English botanist, the lungs of the plants. As by the function of respiration among animals, the blood acquires the necessary properties for nourishing the diverse organs, so the sap rising in the plants is elaborated in the leaves and becomes fitted for nourishing the whole. But this elaboration does not result merely from respiration, the sap rises and is deprived in the leaves of its excess of water by transpiration and exhalation. By this latter phenomenon the ascension of the sap is accelerated most efficaciously. The leaves then serve for excretion. These diverse functions of the leaves are of such importance that, along with the roots, they must be considered the principal organs of nutrition. By being covered with marks on a portion of their surface and sometimes on the whole, the leaves,

without immediately falling, cease to fulfil their functions in a perfect manner.

In all that portion of the parenchyma under the stains, the chlorophylle is completely destroyed, or at least very much damaged, leaving no trace of vital activity. Every physiological function is found to be completely destroyed. On the other hand, the parts which have preserved their green colour continue their functions with more or less activity. Consequently, all the functions performed by the leaves are subjected to a retardation which has the effect of depressing the vital energy and notably clogging the progress of the plants when they are attacked frequently.

The deterioration of the leaves of young sprouts may consequently result in their destruction.

The Damage to the Buds and Branches.

Notwithstanding their protective covering, the buds and their rudiments are not saved from the influence exercised by the acid gases.

In the spring time we have remarked on various species a number of buds which were formed in 1854, so as to expand and contribute to the early spring of 1855, but which perished with the twigs which sustained them.

During this autumn also we have found a number of young sprouts and their buds dead, whilst others dwindled away. In this case, at the end of the twigs the buds were completely dead; lower down they were damaged only on the surface, whilst at the base they were still sound. Circumstances of the same kind were observed on the herbaceous envelope of the bark.

In the examination of the damage to the buds, young sprouts and branches of every size, we have constantly attended, as in the case of the foliage, to their position and relation to the chemical works.

In proportion as we have observed the varieties which were most sensitive to the action of acid gases or plants most exposed to be reached by them, we have seen the deterioration of the branches more or less distinctly. Sometimes the plants entirely perished.

On inquiring into the causes of this damage we have also taken into consideration the nature of the soil, the subsoil, and

all the circumstances of a nature to guide us in a question of this kind.

We shall not enter here into the long details, but shall confine ourselves to a summary of the observations which we have made on plants in the neighbourhood of chemical works.

It is sufficient for the moment to remark that in the case of trees completely dead, analogy ought to be taken into serious consideration.

We understand that if by the side of trees which have suffered damage in all their characteristics, such being due to the action of acid gases, we find others of the same kind which are quite dead, it is very probable that these have perished from the same cause; it being understood that no particular circumstance, such as age, the state of the trees, &c., is opposed to such a conclusion. Observation may in each special case furnish sufficient indications on this point.

Plants not woody.

We have not observed on herbaceous plants damage so distinct and great as we have found on trees.

We have observed that in some cases ferns, nettles, different kinds of natural grasses, spring barley, and a field of clover had greatly suffered from acid emanations, and showed damage which we have not observed except in the neighbourhood of chemical works.

It is important to notice that the stains produced on the leaves by the acid gases did not coincide with the damage to the tissues, except locally and in a restricted manner, and had in no case produced organic disease.

We have remarked on the leaves of the potato, bean, clover, lucerne, turnip, cabbage, beet-root, &c., in the neighbourhood of chemical works, certain whitish marks, slightly yellow, having all the characteristics of those which we have obtained artificially by the use of water containing a slight amount of chlorhydric acid.

In general the number of these stains was small, and did not appear in any way to have affected the vigour of the plants.

One point of the highest importance for the appreciation of these stains is this, that when they have been produced by the

action of an acid they never at any time develope cryptogamic parasites, epiphylls and biogens (that is, those which are developed on the leaves of another plant during a state of vital activity).

We have not come to this conclusion solely because of our observations in the neighbourhood of chemical works, but also because of the experiments we have made directly on many of the plants, on the leaves of which we have caused spots by means of hydrochloric and sulphuric acid in very small quantities.

When water slightly acidulated caused the stains, the vital function of the part was completely destroyed, and we have never at any time observed vegetable biogenous parasites; still more, we have very rarely seen even necrogenous parasites so long as the leaves remained attached to the plant.

Besides, the first fact would be admitted by theory, for it is clear that the little vegetable cryptogams produced on the leaves by vital action could not find the conditions necessary for their development physiologically when these leaves were killed by the action of either acid.

Whilst it is impossible to admit that the biogenous parasites can exist on the spots produced by the action of gases, we have here important facts for the diagnosis of the stains, and the search for the causes which have produced them. As cryptogamic parasites play a great part in the production of stains on many vegetables we must always in our investigations have regard to this circumstance.

Monsieur Kickx, professor at the University of Ghent, member of the Academy of Sciences, and author of excellent works on the cryptogamic plants of Brabant and Flanders, has taken the trouble of precisely determining and classifying a long series of cryptogams, known as the cause of stains on the leaves of many plants. Many persons in the province of Namur have mistaken the marks caused by parasites solely, for the effect of acid gases.

Conclusion.

1. Acid gases hurtful to a certain number of plants escape from a chemical work.

2. The effect is very unequal on the various ligneous and

herbaceous plants natural or cultivated; some resist the acid gases to a considerable extent, whilst others are deteriorated by them in different degrees.

3. Of these latter, some cease to be affected at a small distance from the works, whilst others continue to feel the effects at a great distance.

4. The radius of the noxious influence of these gases depends on many circumstances, but it may be determined practically by observing certain plants such as the wych-elm.

5. Examined in this way the radius will be found to differ for every establishment, and for various directions from the establishment depending on the dominant winds.

6. In the direction of the dominant winds the radius appears to be 2,000 mètres at the most, and 600 mètres as a minimum.

ARRANGEMENT OF WORKS.

The classes are mixed in the French list. Here they are separate; this will account for apparent irregularities.

Arrangement of Works in France, 1867.

FIRST CLASS.

Names of Manufactures.	Cause of Complaint.
Public slaughter-houses	The smell and tainting of water.
Arsenic Acid (manufacture of), by means of Arsenious Acid and Nitric Acid.	
1. When the nitrous products are not absorbed	Injurious emanations.
Acid, Hydrochloric (production of), by the decomposition of the Chloride of Magnesium, of Aluminium, &c.	
1. When the acid is not condensed . .	Injurious emanations.
Acid, Oxalic (manufacture of).	
1. By nitric acid.	
A. Without destruction of injurious gases	Fumes.
Acid, Picric.	
1. When the injurious gases are not burnt	Injurious vapours.
Acid, Stearic (manufacture of).	
1. By distillation	Smell and danger of fire.
Acid, Sulphuric (manufacture of).	
1. By the combustion of sulphur and pyrites	Injurious emanations.
Refining of gold and silver by acids . .	Do.
Aldehyde (manufacture of) . . .	Danger of fire.
Matches (manufacture of), with detonating and explosive substances . . .	Danger of explosion and fire.
Starch Works.	
1. By fermentation	Smells, injurious emanations, and pollution of the waters.
Explosive powders (manufacture of) . .	Danger of explosion.
Potash, Arseniate of (manufacture of), by means of Saltpetre.	
1. When the vapours are not absorbed .	Injurious emanations.
Fireworks (manufacture of) . . .	Danger from fire and explosion.
Tarpaulings (manufacture of).	
1. By using oil	Danger of fire.
Mud and Impurities (dépôts of), and Sewers	Smell.
Gut manufactures (working of fresh intestines for all purposes)	Smell; injurious emanations.
Carbonising of animal matters in general .	Smell.
Pearl Ashes.	
1. With discharge of fumes outside . .	Smoke and smell.

Arrangement of Works in France, 1867.

FIRST CLASS—*continued.*

Names of Manufactures.	Cause of Complaint.
Flesh, débris, and (*offal* dépôts of), arising from the slaughter of animals	Smell.
Dogs (infirmaries for)	Smell and noise.
Chrysalides (workshop for extracting the silk)	Smell.
Coke (manufacture of).	
1. In the open air, or in kilns not smoke-consuming	Smoke and dust.
Glue (manufacture of)	Smell; polluting of waters.
Burning of marine plants in permanent establishments	Smell and smoke.
Greaves (manufacture of)	Smell and danger of fire.
Patent leather (manufacture of)	Smell and do.
Cyanide of Potassium and Prussian Blue (manufacture of).	
1. By the direct calcining of animal matters with potash	Smell.
Fat or thick oil, for the use of chamois leather dressers and curriers (manufacture of)	Smell; danger of fire.
Cleaning of tissues and waste wool by oils of petroleum and other hydrocarbons	Danger of fire.
Fatty Waters (extraction of the oils contained in) for the manufacture of soap and other purposes.	
1. In open vessels	Smell; danger of fire.
Scalding-houses.	
1. For the industrial preparation of animal remains	Smell.
Printing ink (manufacture of)	Smell; danger of fire.
Manures (manufacture of) by means of animal matters	Smell.
Manures (dépôts of) from Middens.—Animal Remains.	
1. Not prepared or in uncovered stores	Smell.
Skinning of animals	Smell; injurious emanations.
Ether (manufacture and dépôts of)	Danger from fire and explosion.
Quick matches (manufacture of) with explosive materials	Do. do.
Felts and patent shades (manufacture of)	Smell; danger of fire.

Arrangement of Works in France, 1867.

FIRST CLASS—*continued.*

Names of Manufactures.	Cause of Complaint.
Fulminating Mercury (manufacture of) .	Danger of fire and explosion.
Tars (special processes for the boiling of) from various sources	Smell; danger from fire.
Tars and vegetable resins from various sources (elaboration of)	Do.
Fat in the naked flame (melting of) . .	Do.
Carriage grease	Smell; danger from fire.
Roasting of sulphurous minerals . . .	Smoke; injurious emanations.
Guano (dépôts of).	
1. When the quantity exceeds 25,000 kilograms	Smell.
Oils of Petroleum, of Schist, and of Tar and other hydrocarbons employed for lighting, heating, manufacture of colours and varnishes, the cleaning of cloths and other purposes.	
1. Manufacture, distillation, and work on a great scale	Smell and danger of fire.
a. Very inflammable substances, that is to say, emitting vapours liable to take fire (1) at a temperature of less than 35 degrees	Do.
1. If the quantity stored is, even temporarily, of 1,050 litres (2) or more . .	Do.
b. Less inflammable substances, that is to say, emitting vapours liable to take fire, at a temperature of 35 degrees and above.	
1. If the quantity stored is, even temporarily, 10,500 litres or more . . .	Do.
Neatsfoot Oil (manufacture of).	
1. With employment of matters in putrefaction	Do.
Fish Oil (manufacture of)	Smell; danger of fire.
Resinous oils (manufacture of) . . .	Do.
Oils and other fatty bodies extracted from the remains of animal matters (extraction of)	Do.
Oils (mixed, hot or boiled).	
1. In open vessels	Do.
Red oils (manufacture of), by the extraction of greaves and fatty remnants, at a high temperature	Do.

Arrangement of Works in France, 1867.

First Class—*continued.*

Names of Manufactures.	Cause of Complaint.
Lignites (incineration of)	Smoke ; injurious emanations.
Menageries	Danger from animals.
Nitrate of Iron (manufaeture of).	
1. When the injurious vapours are not absorbed or decomposed . . .	Injurious emanations.
Ivory Black and Animal Charcoal (distillation of bones or manufacture of).	
1. When the gases are not burnt . .	Smell.
Archil (manufacture of).	
1. In open vessels	Do.
Bones (torrefaction of) for Manure.	
1. When the gases are not burnt . .	Smell and danger of fire.
Fresh bones (dépôts of) on large scale .	Smell; injurious emanations.
Phosphorus (manufacture of) . . .	Danger of fire.
Piggeries	Smell ; noise.
Powder and fulminating substances (manufacture of)	Danger of explosion and fire.
Dessicated nightsoil (manufacture of), and other manures from animal matters .	Smell and polluting of waters.
Resins, gallipot and common resin (work on a large scale for melting and purifying)	Smell and danger of fire.
Red, Prussian and English . . .	Injurious emanations.
Retting in quantity, hemp and flax . .	Do. and polluting the waters.
Sabots (workshop for smoking), by the combustion of the horn or other animal matters in the towns	Smell and smoke.
Blood.	
1. Workshops for separating fibrine, albumen, &c.	Smell.
2. (Dépôts of) for the manufacture of Prussian blue and other industrial products .	Do.
3. (Manufacture of powder of) for clarifying wines	Do.
Bristles of swine (preparation of).	
1. By fermentation	Smell.
Raw soda, from sea-weed (manufacture of), in permanent establishments . . .	Smell and smoke.
Brown tallow (manufacture of) . . .	Smell; danger of fire.

Arrangement of Works in France, 1867.

FIRST CLASS—*continued.*

Names of Manufactures.	Cause of Complaint.
Tallow candles (melting-houses for) using naked flame	Smell; danger of fire.
Bone fat (manufacture of)	Smell; pollution of waters; danger from fire.
Sulphate of ammonia (manufacture of), by distillation of animal matters	Smell.
Sulphate of copper (manufacture of) from roasting pyrites	Injurious emanations.
Sulphate of mercury (manufacture of).	
1. When the vapours are not absorbed	Do.
Sulphate of soda (manufacture of).	
1. By the decomposition of common salt by sulphuric acid, without condensation of the hydrochloric acid	Do.
Sulphuret of carbon (manufacture of).	Smell; danger of fire.
Do. (manufactures in which they employ on a large scale the)	Danger of fire.
Tobacco (calcination of the midribs of)	Smell and smoke.
Taffeta and glazed or waxed cloth (manufacture of)	Smell and danger of fire.
Pyritous and aluminous earths (roasting of)	Smoke; injurious emanations.
Turf (charring of).	
1. In open vessels	Smell and smoke.
Olive oil-cakes (preparation of) by sulphuret of carbon	Danger from fire.
Triperies annexed to the slaughter-houses	Smell and pollution of waters.
Fat varnish (manufacture of)	Smell and danger of fire.

SECOND CLASS.

Arsenic Acid (manufacture of) by means of Arsenious Acid and Nitric Acid.	
2. When the nitrous products are absorbed	Injurious vapours.
Hydrochloric Acid (production of) by the decomposition of the Chloride of Magnesium, of Aluminium, &c.	
2. When the acid is condensed	Accidental emanations.
Oxalic Acid (manufacture of).	
2. By sawdust and potash	Vapour.

Arrangement of Works in France, 1867.

SECOND CLASS—*continued.*

Names of Manufactures.	Cause of Complaint.
Pyroligneous Acid (manufacture of).	
1. When the gaseous products are not burned	Smoke and smell.
Pyroligneous Acid (purification of) . .	Smell.
Stearic Acid (manufacture of).	
2. By saponifying	Smell and danger of fire.
Alcohol (rectification of)	Danger of fire.
Artificial Fuel or Bricks of Coal (manufacture of).	
1. With fat resin	Smell; danger of fire.
Starch Works.	
2. By the separation of the gluten, and without fermentation	Pollution of waters.
Arseniate of Potash (manufacture of) by Saltpetre.	
2. When the vapours are absorbed . .	Accidental emanations.
Asphalts and bitumens (working of) by the naked fire	Smell and danger of fire.
Tarpaulings (manufacture of).	
2. Without boiling in oil	Danger of fire.
Baryta (decolorising of sulphate of), by hydrochloric acid in open vessels . .	Injurious emanations.
Carpet beating on a large scale . . .	Noise and dust.
Bleaching.	
1. Of yarns, of cloths, and of pulp for paper by chlorine	Smell; injurious emanations.
3. Of yarns and woollen fabrics, and silks, by sulphurous acid	Do.
Carbonisation of woods	Do.
1. In the open air, in permanent establishment, and otherwise than in the forest .	Smell and smoke.
2. In close vessels, disengaging into the air the gaseous products of distillation .	Do.
Indiarubber (working of), employing essential oils or sulphuret of carbon . .	Smell; danger of fire.
Ditto (application of coatings of) . .	Danger of fire.
Pearl Ashes.	
2. With combustion and condensation of the smoke	Smoke and smell.
Chamois Leather Factories.	

Arrangement of Works in France, 1867.

SECOND CLASS—*continued.*

Names of Manufactures.	Cause of Complaint.
Silk hats or other preparations, by means of a finish (manufacture of) . . .	Danger of fire.
Lime Kilns.	
1. Permanent	Smoke; dust.
Chlorine (manufacture of).	
1. On a large scale	Smell.
Alkaline chlorides, eau de javelle (manufacture of)	Do.
Cocoons.	
1. Treatment of coloured cocoons . .	Pollution of water.
2. Spinning of cocoons (see spinning).	
Coke (manufacture of).	
2. In smoke-consuming kilns . . .	Dust.
Currying Works.	
Hairs and pigs' bristles (preparation of) without fermentation (see also pigs' bristles by fermentation) . . .	Smell.
Raw leather and fresh hides (dépôts of) .	Smell and dust.
Cyanide of Potassium and Prussian Blue (manufacture of).	
2. By employing matters previously carbonised in close vessels	Smell.
Fatty Waters (extraction for the manufacture of soap, and other uses of oils contained in).	
2. In close vessels	Smell; danger of fire.
Manures (dépôts of) from Middens.—Animal Remains.	
2. Dried or disinfected, and in covered stores when the quantity exceeds 25,000 kilograms	Smell.
Crockery (manufacture of).	
1. With kilns not smoke-consuming . .	Smoke.
Felt (tarred), manufacture of . . .	Smell and danger of fire.
Forges and boiler works for great works employing machine hammers . .	Smoke; noise.
Furnaces, blast	Smoke and dust.
Laces and cloths of gold and silver (burning on a great scale of), in the towns . .	Smell.

Arrangement of Works in France, 1867.

SECOND CLASS—*continued.*

Names of Manufactures.	Cause of Complaint.
Gases for lighting and firing (manufacture of)	Smell.
1. For the public use	Smell and danger of fire.
Tars (treatment of) in gas manufactures	Do.
Tars and Bituminous Fluid Matters (dépôts of).	
Oils of petroleum, of schist, and of tar, light oils, and other hydrocarbons employed in lighting and heating and in the manufacture of colours and varnishes, cleaning stuffs, &c.	Do.
a. Very inflammable substances, that is to say, emitting vapours liable to take fire (1) at a temperature of less than 35 degrees (or 95° Fahr.) on approach of a lighted match	Smell and danger of fire.
2. If the quantity above 150 litres does not reach 1,050 litres	Do.
b. Less inflammable substances, that is to say, emitting vapours liable to take fire only at a temperature of 35 degrees and above	Do.
2. If the quantity stored above 1,050 litres does not reach 10,500 litres	Do.
Neatsfoot Oil (manufacture of).	
2. When the matters employed are not putrefied	Smell.
Oils (mixing by heat or boiling of).	
2. In close vessels	Smell and danger of fire.
Dairies on a large scale in the towns	Smell.
Oils burning, when alcohol and essential oils are used	Danger of fire and explosion.
Engines and Waggons (workshops for construction of)	Noise; smoke.
Murexide (manufacture of), in close vessels, by the reaction of nitric acid, and of the uric acid of guano	Injurious emanations.
Nitro-benzine, aniline and matters derived from benzole (manufacture of)	Smell, injurious emanations, and danger of fire.
Animal charcoal from refineries and sugar works (revivification of).	Injurious emanations; smell.
Lamp black (manufacture of), by the distillation of oils, tars, bitumens, &c.	Smoke; smell.

Arrangement of Works in France, 1867.

SECOND CLASS—*continued.*

Names of Manufactures.	Cause of Complaint.
Ivory and Animal black (distillation of bones or manufacture of).	
2. When the gases are burnt . . .	Smell.
Onions (drying of), in the towns . .	Do.
Bones (torrefaction of) for manure . .	Smell and danger of fire.
Ditto when the gases are burnt . .	Do.
Parchment factories	Smell.
Tobacco Pipes (manufacture of).	
1. With kilns not smoke-consuming . .	Smoke.
Plaster (kilns for).	
1. Permanent	Smoke and dust.
Salted fish (dépôts of)	Unpleasant smell.
Porcelain (manufacture of) . . .	Smoke.
Potash (manufacture of), by carbonising the residue of molasses	Smoke and smell.
Protochloride or salt of tin (manufacture of)	Injurious emanations.
Sugar refinery and manufacture . .	Smoke and smell.
Rogue (dépôt of brine used for salting) .	Smell.
Skins or fur of hares and rabbits (cleaning of)	Do.
Retting (on a great scale) of hemp and flax by the action of acids, of warm water, and of vapour	Injurious emanations and polluting waters.
Salt provisions (establishments for) and smoking of fish	Smell.
Sardines (preparation of preserved), in the towns	Do.
Sausages (manufactures on a great scale of)	Do.
Sal ammoniac and sulphate of ammonia (manufacture of), by employing animal matters	Smell; injurious emanations.
Sal ammoniac extracted from the waters of gasworks (special manufacture of) .	Smell.
Sulphur (fusion or distillation of) . .	Injurious emanations; danger from fire.
Tallow candles (smelting-houses for).	
2. In the water bath or by steam . .	Smell.

Arrangement of Works in France, 1867.

SECOND CLASS—*continued.*

Names of Manufactures.	Cause of Complaint.
Sulphate of Mercury (manufacture of).	
2. When the vapours are absorbed . .	Slight emanations.
Sulphate of peroxide of iron (manufacture of), by sulphate of protoxide of iron and nitric acid (nitro sulphate of iron) . .	Injurious emanations.
Sulphate of Soda (manufacture of).	
2. With complete condensation of the hydrochloric acid	Do.
Tobacco (manufacture of)	Smoke and dust.
Tanneries	Smell.
Enamelled Earths (manufacture of).	
1. With kilns not smoke-consuming . .	Smoke.
Stripping of flax, hemp, and jute on large scale	Dust and smoke.
Oilcloths for packing cloth, tarred cords, tarred papers, pasteboards, and bituminous tubes (manufacture of).	
1. By hot method	Smell and danger of fire.
Cooperage on a large scale, working on casks impregnated with fatty and putrescent matters	Noise, smell, and smoke.
Resinous torches (manufacture of) . .	Smell and danger of fire.
Turf (carbonisation of).	
2. In close vessels	Smell.
Slaughter-houses	Smell and danger from the animals.
Varnish, with spirits of wine (manufactures of)	Smell and danger of fire.
Glassworks, Crystalworks, and manufactures of Mirrors.	
1. In kilns not smoke-consuming . .	Smoke and danger of fire.

THIRD CLASS.

Nitric Acid	Injurious emanations.
Oxalic Acid (manufacture of).	
1. By nitric acid	
b. With destruction of injurious gases .	Accidental fumes.
Picric Acid.	
2. With destruction of injurious gases .	Injurious vapours

Arrangement of Works in France, 1867.

THIRD CLASS—*continued.*

Names of Manufactures.	Cause of Complaint.
Pyroligneous Acid (manufacture of).	
2. When the gaseous products are burned.	Smoke and smell.
Sulphuric Acid (manufacture of).	
2. Of Nordhausen by the decomposition of sulphate of iron	Injurious emanations.
Steel (manufacture of)	Smoke.
Albumen (manufacture of) from the fresh serum of blood	Smell.
Alcohols other than from wine, without works for rectification . . .	Pollution of the waters.
Ditto (agricultural distillery) . . .	Do.
Artificial Fuel or Bricks of Coal (manufacture of).	
2. With dry resin	Smell.
Ammonia (manufacture on a large scale of), by the decomposition of ammoniacal salts	Do.
Refrigerating preparations.	
1. By ammonia	Do.
2. By ether, or other similar and combustible liquids	Danger of explosion and fire.
Asphalts, bitumens, resins, and bituminous solid matters (dépôts of) . . .	Smell; danger of fire.
Thrashing, carding, and bleaching woollens, hairs, and feathers for bedding . .	Smell and dust.
Thrashing hides (hammer for) . . .	Noise and disturbance.
Thrashing and washing (spacious workshops for) worsteds, hairs, and waste of woollen and silk threads in the towns . .	Noise and dust.
Gold and silver beaters	Noise.
Bark beaters in the towns	Noise and dust.
White of Zinc (manufacture of), by the combustion of the metal . . .	Metallic fumes.
Bleaching.	
2. Linen threads and tissues, hemp and cotton, by the alkaline chlorides (hypochloride)	Smell; pollution of waters.
Candles of paraffin and others of mineral origin (moulding of)	Smell; danger of fire.
Candles and other articles in wax and stearic acid	Danger of fire.

Arrangement of Works in France, 1867.

THIRD CLASS—*continued.*

Names of Manufactures.	Cause of Complaint.
Button makers and other metal embossers by mechanical means	Noise.
Breweries	Smell.
Brickworks with kilns not smoke-consuming	Smoke.
Wash-houses	Pollution of waters.
Coffee (roasting on a large scale of) . .	Smell and smoke.
Flints (kilns for calcining) . . .	Smoke.
Carbonising Wood.	
2. In close vessels, with combustion of the gaseous products of distillation . .	Smell and smoke.
Pasteboard makers	Smell.
Goldsmiths' waste (treatment of by lead) .	Metallic fumes.
Ceruse, or white lead (manufacture of) .	Injurious emanations.
Candles (manufacture of)	Smell; danger of fire.
Yards for firewood in the town . .	Injurious emanations; danger of fire.
Felt hats (manufacture of) . . .	Smell and dust.
Wood carbon in the towns (dépôts or stores of)	Danger of fire.
Lime Kilns.	
2. Not working more than one month in the year	Smoke and dust.
Rags (dépôts of)	Smell.
Chloride of Lime (manufacture of).	
2. In works manufacturing at most 300 kilograms per day	Do.
Chromate of potash (manufacture of) .	Do.
Sealing-wax (manufacture of) . . .	Danger of fire.
Ammoniacal cochineal (manufacture of) .	Smell.
Cotton and greased cotton (bleaching waste of)	Pollution of waters.
Copper (solution of) by acids . . .	Smell; injurious emanations.
Red prussiate of potash	Injurious emanations.
Cotton waste (dépôts of), on a large scale, in the towns	Danger of fire.
Distilleries in general; spirits, gin, kirschwasser, absinthe, and other alcoholic liquors	Do.

Arrangement of Works in France, 1867.

THIRD CLASS—*continued.*

Names of Manufactures.	Cause of Complaint.
Gilding and silvering of metals . . .	Injurious emanations.
Scalding-houses.	
2. For the preparation of parts of animals proper for food	Smell.
Enamel (application of) on metals . .	Smoke.
Enamels (manufacturing), with kilns not smoke-consuming	Do.
Manures (dépôts of) from Middens.—Animal Remains.	
3. Dried or disinfected, and in covered store, when the quantity is less than 2,500 kilograms	Smell.
Fattening of fowls in the towns (establishments for)	Do.
Sponges (washing and drying of) . .	Smell and pollution of water.
Quicksilvering of mirrors	Injurious emanations.
Earthenware (manufacture of)	
2. With smoke-consuming kilns . .	Accidental smoke.
Whalebone (working)	Unpleasant emanations.
Starch manufactories	Smell; pollution of waters.
Tin plate (manufacture of) . . .	Smoke.
Spinning of cocoons (workshops on a large scale, that is to say, employing at least six winders)	Smell; pollution of waters.
Foundries of copper, brass, and bronze .	Metallic fumes.
Foundries for the second fusion . .	Smoke.
Founding and rolling of lead, zinc, and copper	Noise; smoke.
Cheeses (dépôts of) in the towns . .	Smell.
Gases for lighting and heating (manufacture of).	
1. For particular use	Smell; danger of fire.
Gasometers for particular uses, not adjoining manufacturing works . . .	Do.
Gelatine for food, and gelatines derived from fresh skins and dressing, and fresh hides	Smell.
Guano (dépôts of).	
2. For sale by retail	Do.
Herrings (salting of)	Do.

Arrangement of Works in France, 1867.

THIRD CLASS—*continued.*

Names of Manufactures.	Cause of Complaint.
Hungary leather tanneries	Smell.
Oil-works and oil-mills	Smell; danger of fire.
Oils (purification of)	Do.
Bacon (workplaces for smoking) . .	Smell and smoke.
Coal-washing	Pollution of waters.
Wash-houses for wool	Do.
Litharge (manufacture of) . . .	Noxious dust.
Morocco leather manufactories . . .	Smell.
Massicot (manufacture of) . . .	Noxious emanations.
Leather-dressing establishments . .	Smell.
Red lead (manufacture of) . . .	Injurious emanations.
Mills for grinding lime, flints, and pouzzolane	Dust.
Nitrate of Iron (manufacture of).	
2. When the injurious vapours are absorbed or decomposed	Injurious emanations.
Mineral charcoal (manufacture of), by pounding the residue of distillation of bituminous schists	Smell and dust.
Olives (pickling of)	Polluting of waters.
Archil (manufacture of).	
2. In close vessels, and employing ammonia to the exclusion of urine . . .	Smell.
Wadding (manufacture of) . . .	Dust and danger of fire.
Paper (manufacture of)	Danger of fire.
Paper pulp (preparation of), by means of straw and other combustible matters .	Pollution of waters.
Sheepskins (drying of)	Smell and dust.
Perchloride of iron, by solution of peroxide of iron (manufacture of) . . .	Injurious emanations.
Mechanical pounding of drugs . . .	Noise and dust.
Tobacco pipes (manufacture of).	
2. With smoke-consuming kilns . .	Accidental smoke.
Gypsum (kilns for).	
2. Only working one month . . .	Smoke and dust.
Porcelain (manufacture of).	
2. With smoke-consuming kilns . .	Accidental smoke.
Earthenware (manufacture of) with kilns not smoke-consuming	Smoke.

Arrangement of Works in France, 1867.

THIRD CLASS—*continued.*

Names of Manufactures.	Cause of Complaint.
Puozzolane, artificial (kilns for) . .	Smoke.
Salting and preparation of meats . .	Smell.
Salting (dépôts for), in the towns . .	Do.
Soap works	Do.
Salt of soda (manufacture of), with sulphate of soda	Smoke; injurious emanations.
Glucose and syrups from fecula (manufacture of)	Smell.
Sulphur (pulverising and sifting of) . .	Dust; danger of fire.
Sulphate of protoxide of iron or green copperas, by the action of sulphuric acid on old iron (manufacture on a large scale of)	Smoke; injurious emanations.
Sulphate of iron, alumina, and alum (manufacture of), by the washing of roasted pyrites and aluminous earth .	Smoke and pollution of waters.
Pasteboard snuff-boxes (manufacture of) .	Smell and danger of fire.
Oak bark (mills for)	Noise and dust.
Dyeing	Smell and pollution of waters.
Dyeing of skins	Smell.
Enamelled ware (manufacture of).	
2. With smoke-consuming kilns . .	Accidental smoke.
Oilcloths for packing textures, tarred cords, tarred papers, pasteboards, and bituminous tubes (manufacture of).	
1. By cold method	Smell; danger of fire.
Painted cloths (manufacture of) . . .	Smell.
Plates and polished metals	Smell and danger of fire.
Wire-drawing works	Noise and smoke.
Tileworks, with kilns not smoke-consuming	Smoke.
Cowhouses, in towns of more than 5,000 inhabitants	Smell and drainage of urine.
Glassworks, Crystalworks, and Manufactories of Mirrors.	
2. With smoke-consuming kilns . .	Danger of fire.

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LONDON: PRINTED BY
SPOTTISWOODE AND CO., NEW-STREET SQUARE
AND PARLIAMENT STREET

TIGHT GUTTERS.