

XVIII.—*On the Air of Towns.**

BY DR. R. ANGUS SMITH.

IT is now a long time since I first sent a short paper on this subject to the Chemical Society, showing that the existence of organic matter in the air capable of decomposing and actually undergoing decomposition was not a matter of theory but of reality. At a later time I was able to confirm the fact, but a mode of readily ascertaining it in all degrees of amount has always been a great desideratum, a problem for pure science as well as for science in its relations to the welfare of man; this latter department is one which will always be most interesting to the greater number, and one which receives a constant impulse from the great amount of sickness existing in the world. I wish now to give some further particulars of the principal points which I have been able to ascertain. One or two of these have been subjects of professional inquiry, *i.e.*, made to serve a purpose connected with the interests of individuals or corporations or the demands of the law, but the greater portion and that of most importance has been gradually accumulated; and whilst making this observation I think it well to add that it seems to me a most unfortunate oversight that whilst laws have been made with relation to the impurity of the atmosphere arising from many causes, neither those who made the laws nor those who administer them have ever taken pains to find out what it really was against which they combated, and what crime that was which they have been so anxious to punish. The same carelessness has been observ-

* This was written in such a manner as to enable me to compress the material into the usual size of a lecture; it will be found that many parts appear more like notes or fragments. On the more important points, such as the estimation of organic matter, it will be necessary to give a second paper, for which there is already material collected.

able on the side of those towards whom the law acted, and decisions have in reality been in the hands of those whose fancy or caprice have led them to take advantage of legal enactments; whilst great offenders have frequently escaped because no one has known the points on which they could be most easily assailed. The impurities of an atmosphere have been rudely guessed at and the actual sources have been found only in a general manner; when particular cases have demanded attention, there has been great difficulty in arriving at well defined conclusions. As to any attempt to measure the degree of impurity in the air, none has been made to my knowledge, unless we take that gauge to be the true one which, in the registrar-general's returns, presents us with a result where each percentage stands for nearly 300,000 human lives.

If science can define precisely the extent of the evils which so affect human life, it may at a second stage also attain the important desideratum of a cure, and at any rate any inquiry of the kind must be of interest to all, but especially so to the medical and chemical professions.

In studying the air of towns in which coals are burnt, no place can give such an opportunity as Manchester, being the largest manufacturing town of the world, and itself being the centre of a great manufacturing district. Geographically, it is not the centre, being near the outskirts of the district on the south-west side, whilst on every other side manufactures extend for, perhaps, on an average thirty miles. They extend from the south and south-east on the borders of Cheshire and Derbyshire onwards to Preston on the north, Liverpool on the west, and into Yorkshire on the east; and within this district are large towns, little known to fame, but containing a great working population, having, besides wealth, both intelligence and force. In this district, comprehending many hundred square miles, there is every variety of scenery, but I believe every portion of it may be shown to be influenced by the smoke of the workshops. The tinge of darkness in the atmosphere may be seen making a line of at least forty miles in length, and affecting the appearance of the sky and the landscape. At the same time, it is marvellous how rapidly nature asserts her superiority in all this accumulation made by art. The moment we leave a town we see every thing greener and fresher, and the people larger, broader, happier and cleaner; and so much so is this the case, that in the great district of which I have spoken,

there is no doubt in the minds of those who are only a little removed from towns, that they are breathing the purest air. Nor is it without difficulty that we can perceive that the towns to some extent affect the whole district. The eye becomes accustomed to the stunted trees and the many withered branches, and broken-up hedge-rows; nor is it customary to look at the state of the individual leaves to enquire how far degeneracy has advanced. The eye is sufficiently pleased to see the general aspect of pure green which refreshes us in every portion of the district, for the meadows in spring and summer seldom seem to suffer, but rather to be improved, partly by direct and partly by indirect assistance from the towns.

In some places, however, the destruction may be called total, the trees that exist being either absolutely dead or only capable of showing a very feeble life; and this it seems to me is very much the case in proportion to their height, although certain of them, such as the poplar, even if less tall, suffer on account of a greater delicacy. The smoke may either rise or fall or move horizontally. It rises when the barometer is high, the air dry, and the sky clear, at which time we perceive very little of the effects on the ground; it falls when the clouds are low, or the air laden with moisture; then the whole district is equally enveloped in the haze. In general there is a movement of the air and a motion of the smoke in a direction more or less horizontal, from the top of the chimney. By this means the highest objects are first attacked and the trees decay from the summit or from the upper part of that side exposed to the current. They resist the enemy for many years, keeping for a long time at least one side fresh; even the owners do not for years perceive that the district is gradually becoming unfit for plants of great height, but as they lose some favourite flower or shrub, they pitch on some person most easily attacked and blame him for all the mischief.

About St. Helens there are many miles of trees broken, stunted, and rotten, and between all the towns of Lancashire, according to the prevailing sweep of the wind, there are lines of dead hedges. Yet the young shrubs grow up without fear as fresh and beautiful as anywhere, and all faith in their prosperity not being lost, there fortunately are still found persons who plant them. Absolute destruction is caused only by the direct action of the smoke, but a general weakness may be seen extending far beyond this, resulting in a less capacity of resisting the attacks of external influences.

As to the effect on the inhabitants, the question becomes exceedingly complicated, but the registrar-general's returns are an unanswerable reply as to the result of the total influences of the district. Few people seem clearly to picture to themselves the meaning of a decimal place in the percentage of death, and few clearly see that there are districts of England where the deaths at least in some years, and when no recognised epidemic occurs, are three times greater than in others. When we hear of the annual deaths in some districts being 3·4 per cent., and in the whole of England 2·2, it is simply that 34 die instead of 22, whilst even that is too slightly stated, as the whole of England would show a lower death-rate if the towns were not used to swell it.

One of the conditions of health, and a most important, if not the most important of all, is to be found in the state of the atmosphere.

Combustion of the Carbon.

According to the best founded data, there are burnt in the course of a year in Manchester two million 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 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 0·16499 of the air. But as a medium will probably be correct, let us say 300 feet high, and the amount will then be 0·33 per cent. of carbonic acid thrown into the air.

The several cases will stand thus—

On a space of square miles, 60 feet high, there is an amount
of carbonic acid from coals . . . = 1·6499 per cent.

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,

CO ₂ from coal	. 0.16499
CO ₂ from breath	. 0.00362
Usual amount of CO ₂ in the air	. 0.06
Total at a given time	. 0.22861

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

CO ₂ from coals	. 0.08248
CO ₂ from expired air of inhabitants	0.0018
CO ₂ of the atmosphere	0.06
	0.14428

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—

CO ₂ from coals	. 0.033
CO ₂ of expired air	. 0.00072
CO ₂ of the atmosphere	0.06
	0.0937

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—

CO ₂ from coals	. 0.0091
CO ₂ from expired air	. 0.0002
Usual amount	. 0.06
	0.0692

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 0.03 of carbonic acid, which is rather above the amount found

on the sea-shore and the hills, and allowing the conditions in other respects to be the same as the last-mentioned, we have—

CO ₂ from coals	0·0091
CO ₂ from expired air	·0002
Natural CO ₂	·0300
	<hr/>
	·0393

This is less than the amount which I have at any time found, but may probably be taken as the extreme limit.

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. Repeated observations of various chemists have until lately pronounced the air of towns to be equal as far as composition is concerned, to that of the air most distant from human habitations. The experience of mankind has been against this, but it has been believed that science was unable to obtain a method of observation which could equal the test of the human lungs, or still more of the continued action of the lungs during the whole length of even a shortened life. These calculations agree so far with the results obtained that I am disposed to think that each is nearly true in its turn, and that times do occur when the percentage of carbonic acid is as high as in the first supposition. Such periods, however, are of brief duration, or have at least not occurred lately for more than a day at a particular place. When the carbonic acid was estimated by passing the air through a potash bulb apparatus very slowly for a whole day, a similar variety of results was obtained, but in equal weather and equal wind the results were very uniform. During the time the following experiments were being made, the weather was remarkably open and fine, and the results were lower than what I have sometimes calculated for the air outside the town.* When there is much wind blowing, they average from 0·045 to 0·08 per cent.; when less, they average from 0·10 to 0·12; making, the amount of carbonic acid decidedly different from the amount in the country. It still remains a question whether the amount is capable of affecting human life, and if the effect of the town atmosphere on health be not wholly attributable to other causes; I still believe that the carbonic acid is by no means the most important cause.

* I ought to have made simultaneous determinations of the air inside and outside the town. I hope to do so.

The carbonic acid was first determined by absorption with potash. The air was previously passed through sulphuric acid and chloride of calcium by an aspirator. The experiments were made at the Literary and Philosophical Society, George Street, Manchester.

No. 1.

February 25.

		Grs.
Total weight after experiment	. .	1906·50
„ „ before „	. .	1901·64
		<hr/>
Difference	. . .	4·86

14·63 cubic feet of air used. $\text{CO}_2 = 0·04095$ per cent.

No. 2.

March 16, 1853.

	Grs.	After experiment. Grs.
Potash bulbs, first . . .	335·280	324·200
„ „ second . . .	482·030	479·000
Chloride of calcium and tube .	865·555	879·100
Sulphuric acid apparatus . .	805·080	810·650
Total weight after experiment	. .	2492·950
„ „ before „	. .	2488·665
		<hr/>
Difference	. . .	4·285

9·2 cubic feet of air used. $\text{CO}_2 = 0·0573$ per cent.

No. 3.

March 18.

	Grs.	Grs.
Potash bulbs, first . . .	537·760	489·100
„ „ second . . .	413·220	452·60
Chloride of calcium tube .	875·00	895·90
Sulphuric acid apparatus . .	819·79	830·02
	<hr/>	<hr/>
Total weight after experiment	. .	2647·62
„ „ before „	. .	2645·77
		<hr/>
Difference	. . .	1·85

5 cubic feet of air used $\text{CO}_2 = 0·0455$ per cent.

No. 4.

March 19.

	Grs.	Grs.
Potash bulbs, first . . .	443·00	440·90
„ „ second . . .	490·00	489·60
Chloride of calcium tube . .	873·84	880·04
Sulphuric acid apparatus . .	829·66	831·73
		<hr/>
Total weight after experiment .	2642·27	
„ „ before „ .	2636·50	
		<hr/>
Difference . .	5·77	

4·6 cubic feet of air used. $\text{CO}_2 = 0·1544$ per cent.

No. 5.

March 20.

	Grs.	Grs.
Potash bulbs, first . . .	440·90	440·75
„ „ second . . .	489·60	489·77
Chloride of calcium tube . .	877·63	879·60
Sulphuric acid apparatus . .	829·66	830·72
		<hr/>
Total weight after experiment .	2640·84	
„ „ before „ .	2637·79	
		<hr/>
Difference . .	3·05	

6·91 cubic feet of air used. $\text{CO}_2 = 0·0544$ per cent.

No. 6.

March 22.

	Grs.	Grs.
Potash bulbs, first . . .	439·70	440·75
„ „ second . . .	474·90	489·77
Chloride of calcium tube . .	886·00	873·67
Sulphuric acid apparatus . .	830·80	832·43
		<hr/>
Total weight after experiment .	2636·62	
„ „ before „ .	2631·40	
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Difference . .	5·22	

5·38 cubic feet of air used. $\text{CO}_2 = 0·1195$ per cent.

No. 7.

March 23, 1858.		Grs.	After experiment. Grs.
Potash bulbs, first	. . .	417·575	413·070
„ „ second	. . .	393·560	393·342
Chloride of calcium tube	. . .	874·050	876·005
Sulphuric acid apparatus	. . .	809·200	814·290
Total weight after experiment		.	2496·707
„ „ before	„ .	.	2494·385
Difference		. . .	2·322

2·256 cubic feet of air used. $\text{CO}_2 = 0·0973$ per cent.

No. 8.

March 24.		Grs.	Grs.
Potash bulbs, first	. . .	393·342	392·950
„ „ second	. . .	413·000	412·450
Chloride of calcium tube	. . .	876·005	876·442
Sulphuric acid apparatus	. . .	814·150	817·012
Total weight after experiment		.	2498·854
„ „ before	„ .	.	2496·497
Difference		. . .	2·357

2·98 cubic feet of air used. $\text{CO}_2 = 0·0972$ per cent.

No. 9.

March 26.		Grs.	Grs.
Potash bulbs, first	. . .	442·57	462·57
„ „ second	. . .	501·28	472·40
Chloride of calcium tube	. . .	875·78	875·37
Sulphuric acid apparatus	. . .	487·56	499·00
Total weight after experiment		.	2309·34
„ „ before	„ .	.	2307·19
Difference		. . .	2·15

4·6 cubic feet of air used. $\text{CO}_2 = 0·0575$ per cent.

No. 10.

March 27.

	Grs.
Total weight after experiment	2311·42
„ „ before	2309·34
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Difference	2·08
3·83 cubic feet of air used. $\text{CO}_2 = 0·0668$ per cent.	

By these experiments we have—

No. 1	0·0409 per cent. CO_2
2	0·0573
3	0·0455
4	0·1544
5	0·0544
6	0·1195
7	0·0973
8	0·0972
9	0·0575
10	0·0668
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Average of carbonic acid = 0·07908

The amount of carbonic acid obtained on the hills near Preston was 0·022 per cent., and another time at Blackpool 0·03. If this were the mean of the air supplied to Manchester, the increase by smoke and other substances would be 0·049 in the centre of the town. I have not ascertained its average condition when it enters Manchester. 0·049 will represent the causes operating over all the county. High winds and stormy weather diminish the carbonic acid very much.

The next peculiarity in the air to which I will call attention, is the sulphurous and sulphuric acids, and to judge of this, I have here given analyses of various coals used in Manchester.

The Sulphur.

The table gives the analyses of 71 specimens, some of which have amounts of sulphur as high as 5 and even 6 per cent.; but leaving out these, we have from 66 specimens of coals used in Manchester, an average of above 1 per cent. of sulphur; or

let us say, 1 per cent. as the lowest amount we can be allowed to assume. If we take 1 per cent. as the average amount, we have 54.79 tons of sulphur burnt daily, producing 167.8 tons of sulphuric acid. This would give in the assumed space of 16 miles a grain of sulphuric acid to 101.72 cubic feet or 1 in 54,455 grains, or 0.0018367 per cent. by weight: by volume it would appear much less.

Amount of sulphur in coals used in Manchester.

		Sulphur per cent. = SO_3HO	
1.	A mixture of equal parts of 32 specimens of coal gave an average of sulphur =	1.4208 ..	4.3509
2.	Average in a mixture of coal used by 17 of the largest consumers	1.1512 ..	3.5255
3.	Average of another mixture of 9 coals	2.3488 ..	7.1957
4.	0.502 ..	1.5374
5.	2.399 ..	7.3487
6.	2.224 ..	6.811
7.	4.965 ..	15.2071
8.	2.034 ..	6.2303
9.	2.951 ..	9.038
10.	1.008 ..	3.087
11.	6.197 ..	18.9789
12.	5.308 ..	16.258
13.	2.158 ..	6.610
14.	0.602 ..	1.8462
15.	1.227 ..	3.7583
16.	0.508 ..	1.5557
Average of 71 specimens		1.665 ..	5.099
Average of 64 specimens, the 7 highest being removed		1.43 ..	4.37
1 per cent gives of sulphur =			3.0625

Sulphur-acids in the air.

In order to obtain the amount of sulphurous and sulphuric acids, I passed the air through acetate of lead. The mode of experimenting here also was to draw the air slowly through the solution, taking care to have a sufficient number of bulbs. Two of Liebig's potash apparatus were quite enough, the last bulbs having no deposit. The water drawn off by the aspirator was measured.

128·8 cubic feet of air gave, with acetate of lead, a precipitate of sulphate of lead = 0·2

60 cubic feet gave 0·095

The first is equal to 1 grain of sulphuric acid SO_3HO in 2000 cubic feet.

The second is equal to 1·076 grains in 2000 feet.

This by weight is 0·0000934 per cent., or say 0·0001 per cent.

These experiments were made whilst the air was dry, or at least whilst there was no rain; the passage of the air through the lead solution being always interrupted during wet weather. An experiment lasted generally for about two months.

The above were made at my laboratory at All Saints, in a situation where an average of the four miles of district might be expected.

Others in the centre of the town at the Literary and Philosophical Society gave—

1st. 0·0042 per cent. of SO_3HO

2nd. 0·0017 „ „

The first of these determinations was made whilst the weather was rather moist, and the acid would probably fall, whilst a moist atmosphere, with as much acid as it could obtain, would pass into the solution. The second is very little below the calculated amount. The amounts differ considerably, but the smell has long ago decided on the fact of a great variation in the amount. One is surprised after all at the minuteness of these figures.

At the same time, the amount calculated is, in my belief, much too high, for an average; and there is another method by which we may find an explanation of the fact that a smaller amount is obtained by experiment.

On examining some smoke from chimneys, I found that in 100 cubic feet (reduced to 65°F .), there were—

	Sulphur in grains.		SO_2		SO_3		SO_3HO
1.	8·13	=	16·26	=	20·325	=	24·05
2.	11·925	=	23·850	=	29·812	=	36·522
3.	2·666	=	5·322	=	6·653	=	8·150
4.	2·635	=	5·271	=	6·529	=	8·072
Average	6·328	=	12·651	=	15·829	=	19·1986

These I consider examples of the amount of sulphur from

chimneys giving out a large amount of sulphur, and others giving a comparatively small amount. As the amount of carbonic acid was not under 4 per cent., it is clear that all the sulphur actually in the coal is not made apparent in the smoke by this mode of burning. Mr. Spence, of Manchester, first called attention to the probability that the amount of sulphur-acids would greatly increase if the smoke were thoroughly burnt, believing that the sulphur is driven off at present uncombined in company with the carbon. In the few specimens of soot which I have examined, I have not found free sulphur. Sulphuretted hydrogen is rare, and cannot account for the absence of sulphurous acid, and sulphuret of carbon, which is produced by the distillation of coal, may be the chief mode by which sulphur is removed, as Dr. Bernays suggested to me. Although then it seems to be shown that a great portion, if not all, of the coal used is burnt in such a way as not to give its maximum of sulphur-acids, it is also true that from other circumstances, certain districts receive even more than their average into the atmosphere. I am ready to believe, then, that the evils arising from sulphur-acids in the air are somewhat diminished by the wasteful and offensive method of burning coals now practised; but the diminution is not sufficient to be looked on as a cure, and it may be even questioned whether the black carbon sent into the atmosphere does not produce worse results. I have spoken chiefly of sulphuric acid as the medium of removing the sulphur, and I am disposed to view it as the chief if not the only final product. I have always obtained the sulphur in that state; it seems to be rapidly oxidized when mixed with large volumes of air. At the same time it is well known that sulphurous acid is the form in which, to a large extent, the sulphur is removed by the chimneys, and that for a time it exists as such in the air. It requires no more than the ordinary senses to decide this question. The mode of oxidizing the sulphurous acid may be simply by the immense excess of oxygen supplied, but it may also be oxidized by the ozone of the atmosphere. In doing this, the ozone itself would be destroyed. This could be done by an amount equal to 8 of ozone for 49 of sulphuric acid, or say $\frac{1}{6}$, an amount sufficiently small to be readily supplied.

Ozone.

As to ozone, repeated attempts have failed to discover any indication near my laboratory and in one or two other parts of the

town. At the distance of nearly one mile, I found distinct indications in about two hours, at the sea-side (Blackpool) in less than half an hour, and even on the paper left on my bedroom table at that place during the night. This remarkable substance furnishes a sufficient proof of a distinct difference in the quality of the atmosphere in these places; its absence alone would completely destroy all attempts to prove that the air is not perceptibly altered by towns. At Rochdale, when the east wind was blowing from the hills, there were indications of ozone in a short time, or in less than an hour. At the same place, when the wind was blowing from the town and from the great manufacturing district west of it, no indications were perceived.

Acidity.

In pure air at the sea-side (Blackpool) litmus paper was made slightly less alkaline by exposure for a whole night during a sea-breeze. In other words, the blue colour was somewhat diminished.

In Manchester, blue litmus paper becomes red in half an hour at most, sometimes in ten minutes.

In my earlier papers on the rain, I mentioned that I frequently found alkaline rain. I did not at that time try the experiments so far in the town; my present laboratory is only a few hundred feet nearer the chimneys, but I see the effect distinctly. Polished brass apparatus does not keep so well, and the rain is always acid; of late years I have found no alkaline rain in any portion of what is actually the town. The amount of acidity is such as to colour blue litmus at once; one drop falling on the litmus at once reddens it. One may, by this very simple means, obtain a very clear idea of the extent to which the air is deteriorated by smoke from coals containing much sulphur.

The acidity of the rain in various districts of Manchester was measured for an object to which I was led in the course of my profession. An alkalimeter was used, and a solution of alkali containing one grain of carbonate of soda to 1000 grains of water. 1000 grains by measure of the water to be tested were used; it was boiled in a flask during the process of testing, and mixed with a solution of litmus, previously taking care to ascertain how much acid was required to acidify or redden the amount of litmus used, so as to subtract it from the number obtained. It is generally better that experiments to be compared

should be done by the same person, as the eye gets accustomed to its own peculiar method of judging of shades, and arrives at the results with wonderful constancy.

Pendleton, N.W. district of Manchester.

Wind blowing to the town, 1000 grs.		
of rain water rendered acid by .	6	of the alkaline sol.
Not directly from the town . . .	16	„
Wind touching the outskirts of the		
town	17	„
From town	36	„
From country	0	„
At All Saints' Church	24	„
„ „	38	„
For a short time	140	„
Then fell to	40	„
Next to	40	„
And lastly	16	„

Ancoats.

1000 grs. of rain saturated by .	44	of alkaline sol.
„ „	20	„
„ „	57	„

Average 40

The rain in these cases was collected in porcelain vessels. In the following it was merely taken out of stone-cisterns, which the inhabitants generally use for collecting water from the roofs of the houses. It will be seen that the results are similar. The weather being wet and the water frequently drawn off, there was little time for evaporation.

Water from cisterns in Pendleton.

No. 1. 1000 grs. of rain				No. 6. . . 64 of alk. sol.			
water required 6 of alk. sol.							
2.	„	„	5	„	7. . .	46	„
3.	„	„	6	„	8. . .	40	„
4.	„	„	10	„	9. . .	12	„
5.	„	„	38	„	10. . .	14	„

Average 25

Going over a part less built upon, the variations were according as chimneys were met with on the way or otherwise.

No. 1. 1000 grs. of rain				No. 5. . . 0 of alk. sol.			
	water required			24 of alk. sol.			
2.	„	„	21	„	6. . .	39	„
3.	„	„	0	„	7. . .	0	„
4.	„	„	27	„	8. . .	15	„
					9. . .	28	„

Average 17

Lower Broughton.

No. 1. 1000 grs. of rain				No. 5. . . 21 of alk. sol.			
	water required			8 of alk. sol.			
2.	„	„	33	„	6. . .	23	„
3.	„	„	26	„	7. . .	29	„
4.	„	„	12				

Average 21

Hulme.

No. 1. 1000 grs. of rain				No. 8. . . 29 of alk. sol.			
	water required			31 of alk. sol.			
2.	„	„	26	„	9. . .	36	„
3.	„	„	32	„	10. . .	28	„
4.	„	„	34	„	11. . .	47	„
5.	„	„	49	„	12. . .	34	„
6.	„	„	27	„	13. . .	41	„
7.	„	„	33	„			

Average 34

Salford.

No. 1. 1000 grs. of rain				No. 8. . . 21 of alk. sol.			
	water required			10 of alk. sol.			
2.	„	„	6	„	9. . .	22	„
3.	„	„	50	„	10. . .	28	„
4.	„	„	0	„	11. . .	20	„
5.	„	„	51	„	12. . .	22	„
6.	„	„	6	„	13. . .	23	„
7.	„	„	26	„	14. . .	10	„

Average 21

Ardwick.

No. 1.	1000 grs. of rain				
	water required	24	of alk. sol.	No. 3. . .	24 of alk. sol.
2.	„ „	40	„	4. . .	45 „
Average 33					

Ancoats, containing numerous mills and inhabited by a working population only.

No. 1.	1000 grs. of rain				
	water required	41	of alk. sol.	No. 5. . .	24 of alk. sol.
2.	„ „	28	„	6. . .	28 „
3.	„ „	60	„	7. . .	68 „
4.	„ „	40			
Average 41					

The average number is 28·5 for the whole.

Over 16 miles and with a rainfall of three feet deep we have 971, or say 1000 tons of sulphuric acid falling in rain during the year. As we had over this space burnt sulphur equal to 1 per cent. on the coal, it would give an amount equal to 61,245 tons of sulphuric acid in a year. The rain therefore returns only 1·588 per cent. of the quantity produced. But let us suppose that it rains one-sixth of the year, that time will receive all the benefit of the purification, equal to 9 per cent., and certain portions still more, according to the amount of stagnation of the air. As an example, the first rain falling on a particular occasion was at 140, but soon fell down to 40 of acidity; and during a violent shower, I found the carbonic acid of the air to be only 0·056 per cent., whilst the rain contained 7·23 cubic inches per gallon.

General Impurities in the Rain.

The rain was found to vary a good deal in the amount of its impurities. The following gives the number of grains per gallon:—

Rain at All Saints	7000 grs.	inorganic	0·265 grs.
Aug. 1857.	2000	organic matter	0·25
		inorganic	0·1
Ancoats	1000 grs.	organic	0·1
		inorganic	0·8

Pendleton	4000 grs.	organic matter	0.19
		inorganic	0.14
			<hr/> 0.33
Rain at All Saints	10,000 grs.	organic	0.34
		inorganic	0.66
			<hr/> 1.00

The inorganic matter consisted of:

Peroxide of iron	.	0.245
Sulphate of lime	.	0.177
and rest not ascertained	.	0.238
		<hr/> 0.660

The inorganic matter is chiefly coal ash. The organic matter is partly products of the distillation of coal.

Mode of Estimating Carbonic Acid.

When examining the action of the manganate and permanganate of potash on sugar, the points that seemed to me most remarkable were, first, that very minute quantities of carbonic acid converted the green into the red acid; and next, that very minute quantities of organic matter entirely destroyed the latter. When a carbonate of an alkali containing more than an equivalent of carbonic acid is added to the manganate of potash or soda, a decomposition ensues, and it is at once rendered red. This salt, called the mineral chameleon, has long been remarkable for this sudden change of colour. When poured into water, apparently and really pure for all ordinary purposes, its appearance is entirely changed, and it is in fact decomposed. But it will be found that water has only a limited power of changing it, and this power depends on the amount of carbonic acid and organic matter. If these be removed or if they be destroyed by the manganate, an additional amount may be employed without any change whatever taking place. Forchammer, in fact, proposed to use permanganate of potash as a mode of estimating organic matter in water, and the method has been less adopted than it deserves to be.

It occurred to me to use this susceptibility to carbonic acid as a mode of estimating its amount. When the manganate of potash is dissolved in water, enough being added to give a bluish purple tinge, the solution may be kept in well stoppered bottles without

apparent change for at least many weeks; but if frequently used it will not keep so long without change. The greatest care must be taken when working with it that no breath should blow upon it, and that no foul air of any kind should approach it. If it is to be transferred to another vessel, that vessel must previously be rinsed out with a portion of the manganate solution, as any water which can ordinarily be used for washing a bottle will destroy some of the solution, and it is better to throw that decomposed part away at once.

A standard solution of carbonic acid was made by filling a bottle with hot and well boiled water, and when cool, adding bicarbonate of potash. The solution first made contained 2·1227 grains of the salt in 5500 grains of rain water. (The bicarbonate of potash gave by analysis 44·98 per cent. of carbonic acid by the common method of loss of weight). This solution will then contain 0·48645 grains of carbonic acid not wanted to form the protocarbonate.

A solution of blue manganate was then made very dilute. Of this 50 grains were reddened by the bicarbonate solution—

1st	.	.	.	850 grains
2nd	.	.	.	846
3rd	.	.	.	848

As each grain of the solution is equal to 0·0000884 of a grain of carbonic acid, I look on these numbers as identical. It would be affectation, but no greater accuracy, to make them arrive within one grain each time.

Having obtained constant results on remarkably minute quantities, the test was then tried with gaseous carbonic acid; the results of these trials I do not find by me at present, but they were equally minute with those obtained by means of bicarbonate of potash, and more so than could be obtained without much trouble by the ordinary methods.

It was believed that the solution coming in contact with the acid of the atmosphere would cause a reddening with so much delicacy that the amount of carbonic acid might be estimated. The experiment was made in a bottle of known capacity, the air being extracted by pumping it out from the bottom and so allowing a fresh current continually to enter. It is safer to fill it by extraction than by forcing, as the pump is apt to communicate some modification to the air.

Another solution of bicarbonate of potash was prepared by dis-

solving 21·227 grains of the pure crystals in 5,500 of prepared water. Each grain would contain 0·001802 cub. in. of available carbonic acid. A solution of manganate of potash was made, 73 grains of which were reddened by 100 grains of the bicarbonate solution. The following experiments were then made to estimate the amount of carbonic acid in the air in the centre of Manchester.

87	cub. in. of air decomposed	35	grs.	$\text{CO}_2 = 0\cdot0981$	p. c.
87	„	„	36	„	$\text{CO}_2 = 0\cdot1012$ „
87	„	„	34	„	$\text{CO}_2 = 0\cdot0955$ „
87	„	„	32	„	$\text{CO}_2 = 0\cdot0899$ „
87	„	„	33	„	$\text{CO}_2 = 0\cdot0927$ „

In a closed up and crowded railway carriage 0·3484

These are my earliest results and seem rather high. I have nevertheless retained them; since that time there being a good deal of high wind or wet weather, no such amount has been obtained, and a few days ago I obtained the following very moderate results.

May 17th, a very wet day, gave with another solution, the following results:—The solution used had the following strength:

50 gr. of manganate solution -- 140 of the $\text{KO} \cdot 2\text{CO}_2 = 0\cdot01232$ of CO_2 or each grain of manganate equal to 0·000601 per cent. of carbonic acid when 87 cubic inches were operated on.

1. 87 grs. of manganate = 0·0522 of CO_2 in the air.
2. 91 „ „ 0·0546
3. 95 „ „ 0·0570
4. 94 „ „ 0·0564
5. 98 „ „ 0·0588
6. 97 „ „ 0·0583

Next day was very dry, the air clear, and the wind high, the amount is still small. The experiments were not made together.

7. 113 grs. of manganate = 0·0679 CO_2 in the air
8. 156 „ „ 0·0937
9. 118 „ „ 0·0709
10. 118 „ „ 0·0709

On a very wet day in a part of Manchester where a great deal of carbonic acid might otherwise be expected, viz., Oldham-road:—

11. 116 grs. of manganate = 0·0717 CO_2 in the air
12. 120 „ „ 0·0721
13. 110 „ „ 0·0671

An error of 1 grain by measurement makes a change only in the 3rd or 4th decimal place, so that it becomes inappreciable.

Experiment 1—6 inclusive were at my laboratory, the wind blowing towards the town; from 7 to 13 at the other side of the town; the wind blowing violently, and allowing no accumulation, we may conclude that the air of Manchester is very slightly affected by carbonic acid, and certainly this condition, which is equal to a diminution of between 0.04 to 0.08 per cent. of carbonic acid is very decidedly perceptible on the health and spirits: violent rain tends still further to remove the acid.

This mode of looking for carbonic acid of course makes no allowance for the other acids, but gives the total acidity. The other acids may be ascertained separately and subtracted. The amount is too minute to affect the value of these experiments. The method will be valuable in deciding exactly as to the quality of offensive matter issuing from manufactures, where it has hitherto been difficult to obtain sound chemical evidence, although the existence of even strong acid vapours could readily be detected by the senses. To separate the stronger acids from the carbonic I purpose using carbonates for absorption, boiling off the carbonic acid. The two experiments will give the amount of each. This, however, I have not yet put into practice.

The reaction with manganate of potash requires great attention to peculiar shades of colour, and in the hands of persons the most careful, not much accustomed to it, it may not lead to exact results. I have been desirous therefore of finding a substance very highly coloured, and yet capable of being decomposed entirely by carbonic acid. This I have found in the rosolates of the alkalies. Rosolic acid in combination with alkalies gives an amazing depth of colour. I published lately a short memoir* explaining its formation, and showing how it might be made in abundance. In attempting to fix the colour on cloth, it was found that however bright and beautiful it was at first, a short time completely converted it into the mere colour of the dark resin uncombined with alkali. This decomposition is effected by the carbonic acid of the air, and I am inclined to think that the method by the rosolates will meet with more general acceptance than that by the manganate, not because of greater exactness, but because of the more definite limit of the colour. As far as my present observations have gone, there is one drawback to the rosolates, and that is, the greater time

* *Memoirs of the Literary and Philosophical Society of Manchester*, vol. xv. p. 1.

needed to effect the thorough change; but this requires a little more experience.

Estimation of Organic Matter.

I have been trying for some time to obtain a measure of the amount of organic matter in the atmosphere. Among the first experiments with the manganates were the following, where an undetermined solution of permanganate of potash and manganate was put into a bulb-apparatus, the air being drawn through it by means of an aspirator. It was soon found that results under similar conditions could be relied upon as constant.

Although these first numbers may be of some use as comparative results, I am not inclined to rely on them as giving absolute differences, until it is known what effect pure air would have, acting in exactly similar relations. That the results are correct in a comparative point of view, is shown by the fact of the manganate of the first series of bulbs being more decomposed than that of the second series. I believe the time allowed by this method is not sufficient to enable the decomposition to take place so that a diminished result is obtained, and again I am not sure as to the actual effect as before stated of pure air. The results nevertheless have their value as an approach to the next part of the subject. The air was drawn by an aspirator through a solution of putrid flesh.

Air from putrefying matter.

- | | | | | | | |
|----|------|----------|------------|-----|------|--------------------------------|
| 1. | 0.38 | cub. ft. | decomposed | 250 | grs. | of a solu- |
| | | | | | | tion of permanganate of potash |
| 2. | 0.84 | | „ | 250 | „ | |
| 3. | 0.96 | | „ | 250 | „ | drawn through slowly |
| 4. | 0.96 | | „ | 250 | „ | |
| 5. | 0.96 | | „ | 250 | „ | drawn very slowly |
| 6. | 0.88 | | „ | 250 | „ | very rapidly |
| 7. | 0.57 | | „ | 175 | „ | |
| | 0.81 | | „ | 250 | „ | |
| 8. | 0.48 | | „ | 175 | „ | |
| | 0.68 | | „ | 250 | „ | |

In this case I did not attempt to approach more gradually than by steps of 25 grs., so that amounts nearly the same become as if identical.

Air from near an Ash-pit and Midden :—

- | | | | | | | |
|----|------|---------|------------|-----|-----|--|
| 1. | 1·15 | cu. ft. | decomposed | 250 | gr. | of the solution of permanganate of potash. |
| 2. | 1·15 | | " | 250 | | " |
| 3. | 0·76 | | " | 175 | | " |
| | 1·08 | | " | 250 | | " |

Air from the lungs was found to decompose the manganate with much greater rapidity than common air.

It occurred to me then to use it as a test, not upon paper like litmus, as it will not exist in contact with organic substances, but on pieces of porcelain, so that we might judge of the impurity of the air by the time required for decomposition; but the surface rapidly dried, at least when a soda salt was used, and the crust protected the under portion from any change. Afterwards nitrate of ammonia was added to keep it moist, but it was very difficult to obtain a surface on which the liquid would lie uniformly. It would be necessary to have it in constant revolution to keep up a uniform depth. By fixing a porcelain slab, moistened with permanganate to the centre of the hands of a clock, the effect might be obtained if wanted.

But in reality this is scarcely desired, as the method afterwards adopted is not a great exercise to the patience, neither could we make any exact approach towards estimating the amount of air that came in contact with the slab or surface as it would change with the state of the wind or other currents.

I came at last to the use of a single bottle or vessel of air, to be filled by means of an exhauster, as explained when speaking of the estimation of carbonic acid. In the stopper are two holes, each with tubes passing through them. As a stopper I have provisionally used only a cork, a very objectionable substance, but it is possible to avoid all contact between it and the manganate solution. One tube is fitted with a stopcock below, close to the stopper, the liquid is poured into the upper part and allowed to drop into the bottle by opening the stopcock as it is wanted. The lower part of the tube projects an inch or two into the bottle, and is drawn out to a fine point so as to keep it filled until pressed down by the column above, on the opening of the stopcock. The other tube has connected with it an india rubber tube and clasp. Its object is merely to allow a little air to pass out when the liquid is passing in. But the amount of liquid required is so small that this opening is scarcely necessary, the pres-

sure of the few inches of column being generally enough to secure an entrance. The stopcock and the mode of joining the tubes are matters of importance. The ordinary metals cannot be used as they so rapidly deoxidize the manganates, and glass is troublesome and apt to break. I have, therefore, obtained stopcocks of a substance not hitherto used by chemists generally, viz., hard vulcanised caoutchouc made by Mackintosh. I expect great advantages from its use in chemical apparatus. It resists the action of nearly all the agents against which we have to defend our vessels at the ordinary temperatures, and is hard enough to be used for any ordinary purpose. I put one of these stopcocks into a manganate solution, and could see no result till a much greater time had expired than the experiment will ever require. Nearly the same may be said of common india rubber, although it cannot be used for stopcocks; common vulcanised caoutchouc cannot be used for a moment for this purpose in contact with the manganates. It is sometimes convenient to close the upper part of the tube into which the liquid is poured. This is done at present by a gutta percha stopper, but a caoutchouc stopper or stopcock would be equally convenient, if not more so.

Having a little burette which held 140 grs., I poured the whole into the bottle and shook it, not knowing, of course, how much might be required. When the whole effect was supposed to be produced, the bottle was opened, and the air sucked out, not having at the time a suitable pump.

By this means it required the bottle to be filled 16 times before the whole of the solution was decomposed. The experiment when repeated still gave 16 times. It seemed therefore a result which could be relied upon as constant.

In extracting the air from the bottle so as to allow the entrance of the external air, a great excess is of course used, and the surface of the liquid is exposed to a little more decomposition than is just to the experiment, but the results are, independently of this, so fine that I have not practically seen the evil, although it was afterwards guarded against. Besides, as the same amount is used in every experiment, the comparative results are not injured even if it is not guarded against. After using a flexible pump or inverse bellows to extract the air, 140 grs. of the solution were always decomposed by 9 bottles of the air at my laboratory. This method was used in all cases afterwards. The air is pumped out and the external air fills the bottle. The use of the mouth for the purpose

is only mentioned to guard against an imperfect mode of doing the experiment, as well as a process very fatiguing to the lungs.

When air is exceedingly pure, as upon the hills or on the sea-shore, it may be impossible to perform the operation in one bottle, even by the use of the smallest convenient amounts of solution. But as the process will probably be wanted most where the air is impure, the same mode may be used as is described for trying the carbonic acid, and the whole experiment finished by one act instead of repeated fillings.

The first experiments give chiefly multiples of a full vessel of air, as I had no hope of arriving at anything finer. The actual limit of delicacy I am not yet able to give. To obtain a standard solution of permanganate for the purpose of testing organic matter, ten grains of sugar were boiled with acid to render it uncrystallizable; when neutralized it was dissolved in 10,000 grs. of water; 54 grs. of this solution, or 0·054 of sugar calculated as cane-sugar, decomposed 700 grs. of a dilute solution of permanganate of potash. Every ten grains of the permanganate are therefore capable of decomposing 0·000771 of sugar.*

This solution being used, the following results were obtained:—

1. Air from my laboratory, All Saints. 140 grs. of solution decomposed by 783 cubic inches of air, or 9 bottles.
2. Air from the front of the laboratory or open street. 140 grs. decomposed by 800 cubic inches, or $9\frac{1}{4}$ bottles.
3. Air from the back yard. 140 gr. decomposed by 658· cubic inches, or $7\frac{1}{4}$ bottles.
4. Air, from a bedroom (11 ft. \times 10 \times 12) up two stairs and looking to the back; at night. 140 grs. decomposed by 701 cubic inches, or 8 bottles.
5. In the morning, air from same room, in which a young man, aged 20, had slept, 614 cubic inches, or 7 bottles.
6. Front room of laboratory again. 833 cubic inches, or $9\frac{1}{2}$ bottles.
7. Back of Medlock river, behind some small and not clean houses. 140 grs. decomposed by 479 cubic inches, or $5\frac{1}{4}$ bottles.
8. St. Michael's-place, back Irk-street, a closely built neighbourhood, air from back yard. 140 gr. decomposed by 87·7 cubic inches, or 1 bottle.

* I now use oxalic acid, but the whole process must be separately described, and the standard employed fully explained.

9. Inside a rather cleanly kept house, in St. Michael-place, but close to the back yard. 175 cubic inches, or 2 bottles.
10. Front of the house. 438 cubic inches, or 5 bottles (nearly).
11. Air on the high grounds at Hoghton-tower, near Preston. 140 gr. decomposed by 1929·4 cubic inches, or 22 bottles.
12. 50 grs. decomposed by 701 = 140, for 22·4 bottles, or 1962.
13. Same air 50 gr. decomposed by 793 = 140 by 2084 cubic inches.
14. Same air 50 gr. decomposed by 701 cubic inches = 140 by 1962 cubic inches.
15. In a close carriage full of passengers, windows and doors closed, 140 gr. decomposed by 175 cubic inches, or 2 bottles.

Supposing the sugar and the organic matter in the air were decomposed by exactly the same amount of manganate, a supposition which cannot be perfectly true, but which from the minuteness of the amounts, leaves no room for a great error, the quantities of organic matter in the air would be in those cases such that the 140 grs. given would be decomposed by 0·0108 grs. of organic matter. When this is decomposed by 1 bottle or 87 cubic inches of air, it is equal to 1 gr. in 8058 cubic inches of air, or let us say 8000. The amount of organic matter in the atmosphere will then exist in the following amounts :—

	cub. in.
1. Air at All Saints inside the laboratory 1 grain in	72,000
2. Front of the house „	74,000
3. Behind the laboratory „	60,000
4. Bedroom looking to the back „	64,000
5. Same room in the morning after being slept in „	56,000
6. Front of laboratory again „	76,000
7. Back of the Medlock behind dirty houses „	44,000
8. St. Michael's-place behind a house „	8,000
9. Inside a house at St. Michael's-place „	16,000
10. The front of the house „	40,000
11. High grounds 30 miles north of Manchester „	176,000
12. „ „ „ „ „ „	183,000
13. „ „ „ „ „ „	209,000
14. „ „ „ „ „ „	183,000
15. Closely packed railway carriage „	8,000
16. When the strong smell of a sewer entered my laboratory „	8,000

Leaving out the three cyphers, we have a range of from 8 to 200, and I feel assured that we may go much lower than 8 as I entered no spots which were very revolting.

The amount from putrid matter, when coming after these, will shew the great evils to which we may be exposed, and the effects of different positions with regard to it.

The table of putrid matter on page 217, when put in this form gives,—

1.	One grain of organic matter in	20·5	cubic feet.
2.	”	”	45·0 ”
3.	”	”	51·9 ”
4.	”	”	51·9 ”
5.	”	”	51·9 ”
6.	”	”	47·5 ”
7.	”	”	43·7 ”
8.	”	”	36·6 ”

It will be observed how much greater it is at first, but how the passage of air over it diminishes it. I may mention that this putrid matter was liquid and the air bubbled through it, stirring it up. This removed the volatile matter and less than half the amount was after a time found in it. But on being allowed a little rest, the putrefaction began again and increased. The putrefying matter was not in an extreme condition, it was to a great extent expended when I began the experiments.

The air of ash-pits, or rather of middens and cesspools such as the repositories of fæces essentially are in Manchester, was tried, and gave

	grs.	cub. ft.
1.	1 of organic matter by the above mode of calculation in	62·2
2.	1 ” ” ”	62·2
3.	1 ” ” ”	58·9

The regularity here is remarkable; we may arrive at the exact amount sent into the air per day by these places, but this I have not yet done. Although, as I said before, I do not rely much on the experiments made on the putrid meat as far as amount is concerned, the results look probable and give a range between the purest air and that from putrid matter of from 20 to 180,000, or from 1 to 9000 units.

It would result from this that by breathing impure or putrid air, we might be inhaling 9,000 times more of some organic substance or other than we should be doing by inhaling the purest air.

But without taking such high numbers, and leaving out those experiments on which I have not professed a reliance, and leaving entirely such putrid matter as I have referred to, we have in different air breathed by people in the same county, a substance the amount of which in one case is 22 times greater than in the other, and in air breathed by people in the same town a difference which is as 9 to 22.

These differences are not greater than are required in order to enable us to account for the numbers which represent the deaths of the various districts. In the district in which the highest numbers here given were obtained, there were, in 1855, 4·5 deaths in a hundred, whilst the average is 2·2 for the country. In other words, the number 22 which I have obtained is not more than is needful to account for a state of things which destroys nearly 60 per cent. of the population. At the place where the number 9½ is given, the deaths are not so high as the average of Manchester.

I find the action of the manganates to be much more rapid on sugar when it has lost its *catamorphous** condition by being boiled with acid. The action is then instantaneous or nearly so. With crystallizable sugar it is slow. I have not yet decided how far this property may be relied on as a means of testing the amount of crystallizable sugar in a syrup. The action generally is more rapid on bodies tending to decay, and to some extent is a measure of the condition of bodies. This peculiar property may possibly be so far made use of as to enable us to arrive at some idea of the condition of a miasm or epidemic virus. If exceedingly active, it may act more rapidly than usual on organic matter. There is, therefore, a field of inquiry here.†

To prevent any doubt as to the fact of the vapours from putrid matter affecting this test, some mutton was put into a large closed vessel and allowed to putrify. Some of this vapour, even after many days, as well as some after many months, being collected in a tube and treated with manganate of soda, instantly decomposed a very large amount, leaving, as it appeared to me, some organic matter which was not entirely resolved into an oxide of carbon and water; but as these experiments are intended for another inquiry, it is enough for the present purpose to show that decom-

* I have used *catamorphous* in opposition to *amorphous*. Hitherto there has been no equivalent: crystalline and others are only so in a restricted sense. *Catamorphous* or *Katamorphous* includes crystallised and crystallisable substances and such as assume a recognisable form, whether well or ill defined.

† I have already found considerable diversity in this respect

position does take place when the only substance present is in the form of gaseous matter.*

I should not omit to say that we must not lay too much blame on the sulphurous acid of the coals. There can be little doubt that it acts as a disinfectant of the putrid matter in towns, and any one with an attentive smell passing through the streets of this country and the cleanest towns of the continent must feel how vastly superior our atmosphere here is in respect of putrid matter capable of affecting that sense: whilst then there is a great charge against the sulphur, we must not omit to speak so far in its favour. This was first mentioned to me by Mr. John Graham. It seems to me that the statistics of epidemics in large manufacturing towns fully bear out this belief, although a few cases present difficulties which my knowledge of the particulars does not allow me to clear up. It may, however, be said that if the ozone of the air oxidizes the sulphurous acid, it is rendered incapable of oxidizing the organic matter, and so the effect of the one is entirely lost, although the effect of the other is gained. Supposing they equally balanced each other, there still remains the sulphuric acid, to which we must attribute a disinfecting power of great value, although inferior to that of the sulphurous.

The sulphurous acid will readily act on sulphuretted hydrogen, and that gas is, I believe, largely removed from our atmosphere by the acid, and excepting near its sources, is not much to be complained of, never attacking the senses in the streets except in these circumstances. One of its most fertile sources in all towns is the gas-works.

Action of the air on the blood.

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, 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.

* Sometimes the term *manganate* is used when permanganate might be written, but it is not important, although the latter is preferred for organic substances.

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, as far as I know, been made to explain the reaction.

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 ozonized 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 ozonized air, was sufficient to give a maximum of brightness. The phosphorus 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 familiarized 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 the 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.

Many questions arise on this, and as I am not willing to speculate at present, even if I were sufficiently acquainted with physiology, but rather to inquire further, I shall venture only on a few remarks. In looking over the action of reagents on the blood, a great majority are said to make it red or vermilion. I was almost tempted to inquire whether observers had not mistaken the simple action of the air for that of the reagent, but I found that many acids and alkalies give this light colour under conditions when the action of the air alone could not explain it. May it not be that the abstraction of carbonic acid reddens blood, and that this is performed by the oxygen of the air taking its place in normal conditions, by alkalies absorbing it in certain experiments and by acids assisting its departure in other experiments, and in

the acid air of towns such as described? If so, 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.

Another supposition I am better pleased with. 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 inquiry, 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 oxidizing 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 0.01 per cent. at a time. I tried only a few hastily, with minute amounts, but got no such results as by sulphurous acid.

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 any one who does not accustom himself to it, may give fallacious results, as the effect is best seen after many comparative observations.

The test for the organic matter will include also sulphuretted hydrogen and some other gases, but I do not suppose that carbonic oxide will be affected by either of the methods. The great sources of evil seem, however, to lie in decomposing animal matter and the acids.

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 inquiry. 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 manganate 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 is quite possible that this test may be used as an indication of the state of the stomach. In a few cases, I found that its condition was correctly registered.

Oxygen.

As to the amount of oxygen in the air, I fear I have not made a sufficient number of experiments, finding it inconvenient to experiment on air from the centre of the town. I did not adopt the precaution of collecting a certain quantity from one district, and making several analyses of the same specimen; each analysis, therefore, refers to a different specimen in reality, and it was found by the other methods of analysis, that the air is constantly changing in composition, and that these changes were found in the second decimal place at least, and even in the first. Having broken my barometer at the time, I was reduced to use an old one, consecrated certainly by the hands of its former owner, Dr. Dalton, whose old laboratory I used for the time, but not thereby improved in its results.

Cub. C. Air used.	C.C. of H. added.	C. C. After explosion.	Per cent. of oxygen.
1. 40·731	73·176	— 24·113	= 20·868
2. 45·786	83·482	— 27·405	= 20·179
3. 33·125	74·582	— 20·677	= 20·807
4. 51·618	88·275	— 56·355	= 20·613
5. 93·107	170·240	— 112·150	= 20·793

These are of course the reduced numbers obtained. Leaving out the second, which is probably too small, the average will be 20·770 of oxygen. This will certainly agree with the numbers for carbonic acid very well, but I have, as I said, no evidence of the air being of a similar composition in any one case. I can scarcely call it an oversight, because, until I had become long familiar with the results, I had not become accustomed to view the atmosphere as so liable to frequent changes. The air outside the town should also be examined in connection with this, and until then no conclusion can be drawn as to the exact effect of the chimneys. Dalton's observations (quoted in Gmelin)* were, for Manchester 20·99, 20·95, 20·83; three miles from Manchester, 20·85. My experiments show a little deterioration. This, however, we may fairly say that it is not from any want of oxygen that the air of Manchester or similar towns can be less fitted for health. The amount varies much more with the changes of temperature and pressure, and it is not to be conceived that such a slight change in the proportions could have any effect on the blood or the health. Besides, I consider that the experiment here recorded, where the air was passed through caustic potash, and was found to have lost its peculiar effect on the blood, sufficiently shows that the peculiarity was not caused either by the amount or proportion of the oxygen, as both of them remained unchanged. The diminution of oxygen may be looked on as amounting to from 0·1 to 0·2 per cent. With gusts of smoke this must be higher.

Ammonia in the Air.

I find only one experiment in which the ammonia was directly taken from air. 21·341 cubic feet of air were passed through an acid solution, and on being treated with a platinum-salt, 0·0517 of ammonia were obtained. This is equal to 1 grain of ammonia in 412·42 cubic feet, or by weight 0·000453 per cent., or if saturated with sulphuric acid, 0·001758 of sulphate of ammonia. I have

* Handbook of Chemistry (translation), vol. ii., pp. 407, 408.

made no verification of this experiment; the place where it was made was in an open space at my laboratory, which with a south-west wind receives no smoke from factories. The ammonia here is sufficient to neutralise more than the amount of sulphuric acid found when the wind blows from the chimney districts. It shews, however, that, as in my earlier experiments, an alkaline atmosphere may occasionally be found. It happened, however, in this case that the air was acid, and that much more sulphuric acid was found than was needful to neutralize the ammonia. The sulphuric acid from the same air was intercepted by lead salt. The amount of sulphate of lead obtained was 0·6 grains=0·2041 of sulphuric acid, and equal to 1 grain in 104·6 cubic feet. This is nearly as much as the theoretical amount calculated for the assumed space of 16 miles, but it must be only occasional. The weather was moist, and in such a state of the air, the acid falls rapidly with the rain, the earlier rain clearing all that is in the air. I do not doubt that in the direct course of the smoke from a chimney, a much higher amount might be obtained, as it becomes then offensive to the smell, and sulphurous acid is strongly perceived. The utmost attainable limit in any given spot must be many times greater even than this.

Rain gave by the instrument, a short account of which I read to the Manchester Philosophical Society :—

Carbonic acid, 5·133 cubic inches per gallon.

„	7·233	„
„	7·233	„
„	7·46	„

By the use of the manganate, I obtained in one case 7·2, and in another the same amount as by the apparatus where the gases are removed by boiling, but the use of the former for water requires many precautions. To obtain correct results the organic matter must first be removed. If this method should turn out as successful as it has promised, the carbonic acid may be taken in 1000 grains of water in a few minutes.

Carbon in the Air.

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. Supposing it to be 60 tons, and the space alluded to filled equally with the vapour, with 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 (17·853). 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 5689, 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 have been removed, like the skeleton of the smoke alone, without the vitality of diffusion to decide upon any course.

Tarry Matters in the Air.

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:—

$\frac{1}{2}$	per cent. of the weight of coal used	given off as carbon.
$\frac{1}{2}$	given off as tar, and
	other volatile products of coal,	all of which may be included.
	Or 30 tons of tar per day and 30 of soot,	
	Or 0·83 to 1½ 0·83 to 1½ ..	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.

Smoke from common Fires.

1'604 cubic feet of smoke or gases from the chimney of a common fire, when the fire was smoking, gave—

Ammonia	0'0345
Sulphurous acid	0'1229

From a clear fire :

Ammonia	0'0326
Sulphurous acid	0'7047

In the first there is a grain of ammonia in 46'5 cubic feet ; in the second, a grain in 49 cubic feet. It is shewn that the sulphur is burnt in greater part after the smoke has ceased, as common experience indicates. The ammonia does not diminish so rapidly as we might expect. It is, in fact, very difficult to remove the nitrogen from coal or coke. Some experiments which I made on the subject gave a great diversity in the amount, but a low heat seemed to send off very little, and a high heat evidently destroyed entirely a large proportion.

Effect of the Atmosphere on Stones, Bricks, Mortar, &c.

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. gave 7·57 of BaOSO_3 , or 28·33 p. c. of sulphuric acid, = 48·16 per cent. of sulphate of lime.

It is not to be wondered at that iron oxidizes readily, and that galvanized 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 oxidized 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.

Carbon on the surface.

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 fine 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 the surface of Manchester buildings is very small. It collects on the rough surfaces principally, and when mortar is put roughly on, it soon becomes perfectly black, although the red of the brick should remain moderately clear. Rough bricks also take it up in greater quantity, and become black in proportion to their roughness.

4·4 × 4	or 17·6 square inches	gave of carbon	0·17 grains.
3· × 2·7	or 8·1	„	0·02 „

The last is equal to 0·45, or about half a pound in a house 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 should 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.

Can a Cure be found?

A cure for some of these evils ought certainly to be 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.

For the amount of sulphur in the air there has never been any attempt at a cure by any one, nor has it been generally looked on as an evil. A small beginning of this important subject occurred to me whilst investigating a patent obtained by Mr. Holme, of Manchester, for bleaching smoke. In this patent, Mr. Holme claims the use of lime and of common salt mixed with the coals, 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 bases.

Amount of sulphur driven off from a specimen of coal by distillation:—

Coal alone.

- | | |
|--------------------------|------------------------------|
| 1. At a low red heat, | 0·4692 per cent. of sulphur. |
| 2. At a higher heat, | 0·5655 ,, |
| 3. At nearly white heat, | 0·6755 ,, |

Coal with common Salt.

4. With 5 per cent. salt,	0·4526 per cent. of sulphur.
5. Ditto, at higher heat,	0·4843 „
6. Ditto, nearly white,	0·5557 „

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	..	0·4332 per cent. of sulphur.
From No. 6	..	0·5448 „

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:—

Coal distilled alone	.	0·4338 per cent. in the distillate.
Coal with 5 per cent. lime	0·1754	„
Coal with 10 per cent. lime	0·0511	„
Coal with ditto	..	0·0616 „

It results from this, that Mr. Holme had in his lime a cure for a great sanitary evil, but not knowing the effects, left it for a less valuable substance. I do not, however, propose lime as a bleacher of smoke, but as a remover of sulphur; and I think some mode of using this property may on inquiry be found. The mode of using it requires investigation, and many questions instantly suggest themselves. An inquiry by a public body would probably best find out the true mode of application, or bring to light a more efficient method.
