RIVERS POLLUTION PREVENTION ACT, 1876.

REPORT

TO

THE LOCAL GOVERNMENT BOARD

BY

DR. R. ANGUS SMITH, F.R.S.,

ONE OF THE INSPECTORS UNDER THE ACT.

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

LONDON:
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TO THE LOCAL GOVERNMENT BOARD.

My Lords and Gentlemen,

The position which I hold as Inspector under the Rivers Pollution Prevention Act has led me to consider the question of water, pure and impure, from various points of view, and I have brought forward several investigations which I hope will be found of value.

The first part relates to questions which rise to the surface when we study the analysis of water, and mainly to the decompositions which take place in the organic matter often found in water.

The second part relates to the action of air on sewage, and to the mode of treating sewage so as to hasten the aeration. This includes remarks on the relation which the results bear to the latest microscopic inquiries on the germs of disease, and water supply.

The third part relates to the purification of sulphureous liquids draining from alkali works, and to the action of air upon them; with a few experiments on effluents from several other works.

These inquiries are continuations of work done in former years, the earliest going back to 1846, if not prior.

It is really so difficult to come to a conclusion on the many questions that rise before one, that I have not attempted to do much more than to give my results, and leave them to do their work in ripening the mind. Further inquiries may modify all I have to say here, but I think the matter will in any case be of use on the road of progress. I would call attention to the decomposition of organic matter by nitrates, or vice versâ; the use of the oxygen of the nitrates in oxidizing organic matter, and giving out the nitrogen of the nitrates; to the use of aeration in preventing putrefaction, and to its use in throwing down the sulphur from the sulphides under the given conditions; that is, by the constant sulphuration and oxidation of manganese.

If in some cases I may appear to bring forward earlier papers in quotation too often, I may be forgiven, as there is always an inclination to put aside earlier work, and occasionally one feels inclined to show that in former times he has been on the right track, and not in the way of misleaders.

PART I.—WATER AND SEWER WATER.

No. I.

SOME CHANGES EFFECTED BY OXYGEN ON ORGANIC BODIES IN WATER—FORMATION AND DESTRUCTION OF NITRIC ACID.

In a magnificent paper by Messrs. Gilbert and Lawes (Phil. Trans., 1861, p. 508), read in 1860, it is said, when speaking of the sources of nitrogen in vegetation,—

" Other investigations to which we have to call attention will throw some light upon the character of the molecular forces by which the decomposition of nitrogenous organic compounds is effected under such circumstances as we have been considering. These forces might be one or both of two kinds:—

" 1. They might be of an oxidising character, analogous to that of the action of chlorine upon ammonia by which free nitrogen is evolved.

" 2. They might be of a reducing character similar to that of a great number of substances upon the oxygen-compounds of nitrogen, by which the oxygen of the latter is appropriated, and free nitrogen given off.

" 3. These two actions may operate in succession the one to the other.

" It is well known that an oxidising action may be so intense as to deprive a nitrogenous organic compound of all its carbon and hydrogen, converting it into oxygen compounds, as is done by permanganic acid. The converse action of the transformation of oxygen-compounds of nitrogen into ammonia is also very well known. An intermediate stage in either of these converse actions may give free nitrogen."

In the account of an investigation " on the amount and " composition of the rain and drainage waters collected at " Rothampstead, 1881," written by Dr. Lawes, Dr. Gilbert, and Mr. Robert Warrington, Part II., page 25, speaking of the loss of nitrates in the soil, it is said,—

" This reduction in question has been effected by the organic matter of the soil, and has resulted in the formation of carbonic acid gas. A part of the nitric acid has probably been reduced to ammonia, while a considerable part of the nitrogen has most likely taken the form of nitrogen gas."

The idea of such a decomposition of nitric acid by organic substances as to give out free nitrogen has been very clearly in the minds of the authors named.

I shall add some of my early work and opinions, as well as a page or two of my latest work, showing that in some conditions free nitrogen is given off by organic substances with great rapidity. It may seem at first that such a point was not of very great importance, but it has such an important bearing on the pollution of rivers question, or, we may say, rather on the purification of water, that it might have been treated even more fully than it

is, and it seems to be a suitable point with which to begin the subject afresh.

In a previous paper read in 1867, and in one in which I spoke of the importance of examining the condition of the organic matter in water, and not merely showing its amount, I divided the organic matter into seven sections:—

1. Organic matter decomposed or putrid.
2. Organic matter readily decomposed and ready to become putrid.
3. Organic matter slow to decompose.
4. Recent organic matter.
5. Old organic matter.
6. Vegetable organic matter.
7. Animal organic matter.

I might express it differently now.

I was interested in showing the development of organisms in the water as a mode of finding the quality of the organic matter. The idea was correct, and the use of the microscope for the purpose correct abstractly; but the subject must pass into the hands of men who have devoted much time to the study of the changes which take place in minute forms. I think I was right in my mode of dividing the organic matter chemically; and, seeing that the study of the germs of disease threatens to be difficult and complicated for a long time to come, it seems to be the duty of chemists to do something towards examining questions relating to organic matter in their own way. I have turned, therefore, more of late to the study of changes which take place in the ammonia, nitric acid, and oxygen, so as to find by chemical methods if the water has active organic matter in it. I do not say that I have made much progress in it, but I have done some work which seems to me interesting.

I may as well here give an extract showing some of my previous views and procedure (Mem. Lit. and Phil. Soc., Manchester, 1867):—

" If the water contains organic matter in solution, or in a condition approaching in all appearance to solution, it may be wholesome or unwholesome. The mere existence of organic matter is no proof of impurity. We must know if it brings animalcules or vegetable life, or products of putrefaction. We must know the quality as well as the quantity. If the matter is peaty, consisting of the ordinary humous class of acids and salts, the colour may be very dark, and the water very unpleasant to look at, without being in any way, so far as I have ever heard, injurious to health, although such water cannot be quite so wholesome as pure water, since the oxygen of solution is diminished. The taste and other sensible qualities will be the chief guides.

" If the matter is wholly or nearly colourless it may still be wholesome or unwholesome. It may, for example, contain the juices of plants of a wholesome character. If these juices are fresh they may do no injury; but they will not remain long fresh; they will putrefy. Water containing organic matter ready to putrefy ought to be avoided, as we cannot tell when the moment of danger begins, whilst the quality at best is never known to us exactly.

"To ascertain the nature of the organic matter the water is allowed to stand for a day or two, in which case it may be found that *organised bodies* show themselves. Sometimes plants seem completely to fill the vessel, having come out of a moderately clear solution. When standing in this case, the water must be prevented from evaporating, and it must be in glass, so as to be exposed to light; a temperature suiting vegetation is also to be given. Animalcules may appear in great numbers. They are an indication of nitrogenous matter, and one proof of the presence of substances capable of putrefaction. It may be that some form of putrefaction will be the only result; but whether this occurs alone, or along with organised forms, an excess of organic matter is proved. Water that will not bear the test of standing will in most cases be rejected at once. If no other can be obtained, it ought to be used before the putrefactive process has begun; but this is very dangerous. The next best method is to wait till after putrefaction has terminated, and the products are separated as much as possible. This is popularly known to be the case when the water has for some time become clear and colourless and free from smell and taste."

"Water with green organic matter in suspension or semi-solution is generally full of germs of living things and nauseous to the taste."

The oxidation of nitrogen has been a favourite subject of late, but I should not think of attending to it for that reason. However, it may be known to a few chemists that I made nitric acid or nitrates from organic matter in water at a very early stage, as seen in the vol. of the Trans. of the British Association in 1848. I cannot, therefore, be said to be late in the field.

Regarding the deoxidation I may quote from my paper of 1867, speaking of nitrates: "the oxygen seems to be removed as the oxygen of the air is, leaving the nitrogen to pass off as gas." Mem. Lit. and Phil. Soc. of Manchester, vol. 4, ser. 3rd, 1867–8.

When doing the first experiments, spoken of in 1848, I used generally, if not always, yeast for the organic matter, and I did not give to the organised bodies the credit of producing the action. The interesting experiments, however, of M. Schloesing and of Mr. Robert Warington cause every one to reconsider the subject. I suppose M. Pasteur gave the first idea of the action of organisms in nitration, but, so far as I remember, did not prove it. The idea that all nitrates are the products of oxidation of animal matter in contradistinction to vegetable is another point which compels us to think over the subject. I have always objected to the opinion that they were the products special of animal life, knowing that my own experiments were made with vegetable substances. It has, on the other hand, been said (long ago in conversation with me,—I do not remember

if the idea is published,) that probably the vegetable life passed into animalcular life, and thus nitrates were the products only of animals. This is hypothesis, and cannot go far. We must separate the idea of the action of Bacteria, or any vegetable or animal forms of organic matter, from the idea that organic matter must itself be animal before it passes into nitrates, leaving it possible that nitrates may come from decomposing vegetation.

Then, again, the drainage of peat lands frequently contains nitrates, as we know, in great abundance, but the peat itself is not remarkable for much animal life. I find Mr. Warington thinking otherwise as to the amount of nitrates. Our experiences differ on this point, but there is a great variety of peat.

On account of these facts the conditions become complicated, and I am now led to believe the following:—

1. Animal or vegetable matter containing nitrogen produces nitrates by oxidation under certain conditions, with and without organisms.
2. Inorganic matter containing ammonia does the same.
3. Albuminous matter in a putrefactive condition, if exposed to excess of air, produces an oxidation of organic nitrogen.
4. A peculiar putrefactive condition produces a deoxidation of nitrates. In some conditions nitrogen is eliminated as gas, the oxygen going to the carbon in whole or in part. In weak solutions, containing water enough to absorb the carbonic acid, nearly pure nitrogen may be obtained with considerable rapidity.

Two of these propositions may require proof and explanation.

It will be seen in this view of the question that nitrogenous bodies have a peculiar power of taking up oxygen and of giving it out. They seem to be used for the purpose of purifying organic substances as transferrers of oxygen. Although we have not proved, so far as I know, that in cases of great impurity nitrates are formed, and their oxygen given out afterwards, yet we can prove that when the impurity is moderate nitrates are formed, and in any case they give out their oxygen in this way.

The escape of free nitrogen from solutions of organic matter has been noticed, by Reiset, for example, (see Jahresbericht der Chemie fur 1856, p. 806,) and by myself, in the examination of the gases of putrefaction, in 1863 (Mem. of the Lit. and Phil. Soc. of Manchester). I certainly doubted if this was an action of organisms when I found that the nitrogen was evolved at a heat equal to 160° F. or 71·1° C. At the same time there was evolved a great deal of sulphuretted hydrogen. The putrefactive substance then used was blood very much diluted.

I had occasion lately to examine several mixtures. One was formed by the evaporation of urine, and some fæces in the process for drying employed by Mr. Alfred Fryer; and, reverting to my old idea that nitrogen might be given off, I put nitrate of potash in various amounts into solutions of the substance. Bubbles of gas began to escape next day, and in some cases came off with great rapidity and regularity, *i.e.*, about one bubble in three seconds.

Many organic fluids were tried, but the same rapidity was not obtained by any other, although nitrogen was found escaping from blood, egg albumen, and flour paste in the same conditions, that is, the putrefactive state being overpowered by nitre. It did not, however, escape so rapidly as in the liquid mentioned previously.

The nitrate of potash was decomposed and ceased to be found in the liquid. Sulphuretted hydrogen did not exist, or it was found in minute quantities only, and it was clear that the action was entirely different from putrefaction simple. The gas was frequently collected, and it was found to contain very little, sometimes only a trace of, carbonic acid. At the same time the action seemed to be caused by organisms; that is, it took place in liquids which had a great amount of organic matter ready to assert itself. And this I leave for the moment, under a belief that this may be a kind of fermentation; certainly it seems a very new kind. It is like a reversed putrefaction, and must be carefully noted down in considering the action of disinfectants.

This action not being quite the same as that found by Schloesing, where nitrogen was slowly given out, I put down here many of the experiments to illustrate it:—

1. 1100 c. c. of water, containing 10 per cent. of excreta (concentrated by Fryer's apparatus), gave off no gas during 2½ months.

2. 1100 c. c. of water, containing 10 per cent. of excreta and 2 grms. KNO_3 (nitre), commenced to give off gas after 12 hours. The gas collected over water

$$N = 98{\cdot}4 \text{ per cent.} \quad CO_2 = 1{\cdot}6 \text{ per cent.}$$

3. 1100 c. c. of water, containing 10 per cent. of excreta and 2 grms. nitre, gave off 218 c. c. of pure nitrogen when collected over water. (2 grms. nitre contain 220·6 c. c. N.)

4. 340 c. c. of water, containing 10 per cent. excreta and 0·5 grm. nitre per litre, gave off 14·8 c. c. nitrogen collected over water. (The nitre contained 18·7 c. c.)

5. 350 c. c. of water, containing 10 per cent. excreta and 1·0 grm. nitre per litre, gave off 38 c. c. N collected over water. (The nitre contained 37·4 c. c.)

6. 350 c. c. of water, containing 10 per cent. excreta and 0·25 grm. nitre per litre, gave off 7·5 c. c. N collected over water. (The nitre contained 9·6 c. c.)

7. 340 c. c. of water, containing 10 per cent. excreta and 1·0 grm. nitre per litre, gave off 38·0 c. c. N collected over water. (The nitre contained 37·4 c. c.)

8. 320 c. c. of water, containing 10 per cent. excreta (fresh sample) and 0·5 grm. nitre, gave off 53·8 c. c. N collected over water. (The nitre contained 55·1 c. c.)

9. 330 c. c. of water, containing 10 per cent. excreta (as No. 8) and 1·0 grm. nitre. Gas collected over water was nitrogen only.

10. 560 c. c. of water, containing 10 per cent. excreta (as No. 8) and 5·0 grms. nitre. Gas collected over water was nitrogen only.

11. 1100 c. c. of 10 per cent. excreta from a previous experiment, with 2 grms. nitre, were mixed with a second quantity of 2 grms. nitre. Decomposition commenced at once, but continued very slowly. A gas evolution went on for many days. Liquid became strongly alkaline.

12. 1100 c. c. of 10 per cent. excreta from a previous experiment, with 2 grms. nitre, were mixed with 1,980 c. c. of water containing 2 grms. nitre in solution. Decomposition commenced at once; the gas collected over water was pure nitrogen. Liquid became strongly alkaline.

13. 440 c. c. 10 per cent. excreta (as No. 8) and 5 grms. nitre were heated to (120°–125° F.); gave off gas after 30 hours. The speed was much slower than in those experiments at the ordinary temperature.

14. Putrid blood + 3 grms. nitre gave off much gas, which, when collected over water, consisted of pure nitrogen.

15. 1100 c. c. urine + 0·5 grm. nitrate of ammonia + 5 c. c. putrid urine, gave off 70 c. c. of N.

16. Egg and water with nitre gave off gas consisting of 77·8 per cent. N and 22·2 per cent. CO_2, collected over mercury. When collected over water the nitrogen only was obtained.

17. Blood with $(NH_4)_2CO_3$ carbonate of ammonia and KNO_3 gave off gas, which consisted of nitrogen when collected over water.

18. To a quantity of blood which had stood several weeks without apparent change about 2 grms. KNO_3 were added; a brisk evolution set in after 2 days.

Gas collected over mercury = N = 92·6, CO_2 = 7·4.

Gases collected over Mercury.

19. 406 c. c. blood + 1 grm. KNO_3 gave off gas consisting of 96·1 per cent. N and 3·9 per cent. CO_2.

20. Flour paste + 1 grm. KNO_3 gave off gas consisting of N = 97·0, CO_2 = 3·0.

21. Flour paste + 2 grms. KNO_3 gave off gas consisting of N = 6·4, CO_2 = 93·6.

22. Egg diluted with 8 vols. of water KNO_3 gave off gas slowly for some days, N = 85·4, CO_2 = 14·6.

23. 205 c. c. blood + 1 grm. KNO_3 gave off gas consisting of N = 84·5, CO_2 = 15·5.

24. 1150 c. c. blood + 1 grm. KNO_3 gave off gas consisting of N = 48·5, CO_2 d 51·5.

25. 235 c. c. bloo + 2 grms. KNO_3 gave off gas consisting of N = 94·4, CO_2 = 5·6.

In experiments Nos. 8, 9, 10, and 13 the gas evolution did not commence until the third day, the earlier specimen yielding gas much more readily. Urine when fresh did not yield gas at first, unless a small quantity of old urine was mixed with it. The solutions became more alkaline according to the amount of nitre decomposed, forming carbonate of potash.

In 11 and 12 the nitrogen came off slowly at first, but when the first charge was exhausted a second charge of nitre caused the gas to come off instantly.

The gases were exploded with oxygen and hydrogen separately, and also in conjunction with water gases. They were also treated with caustic potash and pyrogallic acid, but only carbonic acid and nitrogen were found.

No. II.

ABSTRACT OF WORK BY KUHLMANN, BOUSSINGAULT, SCHLOESING, AND REISET.

In 1848 (Jahresbericht, vol. 1, page 385) Kuhlmann found that nitrates were formed by heating sulphate of ammonia and sulphuric acid with bichromate of potash, peroxide of manganese, brown peroxide of lead, minium, or barium peroxide; also that by heating sulphate of ammonia with nitrate or chlorate of potash, the ammonia is completely converted into nitrous gas.

Kuhlmann (Ann. Chim. Phys., 1847, XX., p. 223 et seq.) produces ammonia from nitrates by—

a. The reducing action of zinc or similar metal and dilute sulphuric acid.

b. The action of sulphuric acid, or, better, hydrochloric acid, upon sulphide of iron in contact with a solution of nitrate.

c. The action of sulphide of arsenic dissolved in solution of caustic potash.

d. Gradually acidulating a solution of sulphide and nitrate of potash. Ammonia is formed after some days.

e. The reducing action of ferrous or stannous oxide. After several days a notable amount of ammonia is formed.

f. Passing a current of sulphuretted hydrogen through a solution containing $SbCl_3$ and KNO_3.

He also obtains nitric acid from ammonia by the following reactions:—

1. By distilling a mixture of bichromate of potash, strong sulphuric acid, and sulphate of ammonia.

Equation:—

$$8\,(2\,CrO_3,\,KO) + 3\,(SO_3\,NH_3\,HO) + 32\,(SO_3\,HO)$$
$$= 8\,(3\,SO_3,\,Cr_2O_3) + 8\,(SO_3\,KO) + 3\,(NO_5\,HO) + 41\,HO.$$

(There seems some error here.)

2. By distilling a mixture of weak sulphuric acid, binoxide of manganese, and sulphate of ammonia.

Equation:—

$$8\,MnO_2 + SO_3\,NH_3\,HO + 7\,SO_3\,HO$$
$$= 8\,SO_3\,MnO + NO_5\,HO + 10\,HO.$$

3. By heating a mixture of nitrate or chlorate of potash with sulphate of ammonia. The whole of the ammonia is converted into nitrous gas.

4. By acting with strong sulphuric acid on a mixture of peroxide of lead and sulphide of ammonium.

5. By acting with strong sulphuric acid on a mixture of peroxide of barium and sulphate of ammonia.

6. As published in 1838, by passing air mixed with ammonia through red-hot tube of porcelain, a small quantity of hyponitric acid and binoxide of nitrogen is formed.

These important facts, which I have confirmed by similar experiments, show at least that nitric acid may be formed by inorganic agents in solution, as well as by explosions in which nitrogen gas itself is oxidized.

In the *Compt. Rendu*, vol. 66, 1868, p. 177, M. Reiset speaks of the production of *nitrous gas* when the liquids become too slightly acid during the fermentation of beetroot solutions.

M. Th. Schloesing, vol. 66, 1868, p. 237, speaking of putrefaction in tobacco juice, says: "Nitrates decompose so rapidly "that the solutions may be found to differ from day to day. "When the nitrates are decomposed, the gases, when carbonic "acid is absorbed, leave a residue containing protoxide of "nitrogen. Urine was found to give out protoxide and binoxide "of nitrogen." The decomposition of nitrates has not been observed when the solutions remained acid. When they became neutral or alkaline the decomposition commenced, and developed with such activity that all the nitrates disappeared in a few days.

In *Compt. Rendu*, vol. 76, p. 22, 1873, M. Boussingault speaks of the disappearance of a small amount of nitrogen in soils, but his chief object is to show that nitrates were not formed from the nitrogen of the air.

Vol. 84, *Compt. Rendu*, p. 301. MM. Th. Schloesing and A. Muntz give experiments to show that the destruction of germs by chloroform prevents the formation of nitrates. This result seems in conformity with other results, but Mr. Otto Hehner thinks a large amount necessary, as I understand him. *Chem. News*, vol. 39, pp. 26 and 53.

A fuller abstract of M. Schloesing's work may be usefully published, as below:—

Study of Nitrification in Soils (Abstract.)

(*Compt. Rendu*, vol. 77, pp. 203 and following.)

The necessary conditions for nitrification are known, namely, a nitrogenous substance capable of slow combustion, sufficient aeration, a carbonated base, a certain amount of moisture and heat.

These may be arranged in several categories:—

1. Conditions pertaining to the soil:—Mineral composition and the physical properties resulting therefrom, nature and proportion of the saline constituents soluble and insoluble, nature and quantity of organic matters, amount of cultivation.

2. Conditions resulting from the joint action of the soil and atmosphere:—Humidity proportion of O and CO_2 in the atmosphere contained within the soil, interchange of gas between the soil and atmosphere.

3. Purely physical conditions:—Heat, light, electricity.

Influence of the Proportion of Oxygen in a Confined Atmosphere.

Five quantities of a calcareous earth were placed in long glass vessels, at the ordinary temperature; the only difference being the composition of the atmosphere in each, which consisted of air and nitrogen as follows:—

No. - - -	1	2	3	4	5
Per cent. by vol. of O	1·5	6·0	11·0	16·0	21·0

Humidity of the earth 15·9 per cent.

Nitrogen in the moist earth = 0·263 per cent. A fertile earth rich in humic matter.

The air was deprived of CO_2 and NH_3 before reaching the earth. The experiments lasted from 5th July till November 7th, 1872.

The following are the mean results for the CO_2 in the atmosphere expelled from the soils during the months of July and August. The temperature varied between 21 and 29°.

No. - - -	1	2	3	4	5
Mean T. - -	24·3	24	23·1	24·2	25·2
Mean CO_2 formed in 24 hours per kilo. of earth -	Mgm. 10·4	Mgm. 16·6	Mgm. 16·1	Mgm. 15·1	Mgm. 19

In the last four cases the combustion of the organic matter appears almost independent of the amount of CO_2; and in the 1st case, where the O = only 1·5 per cent., the amount of CO_2 formed is 60 per cent. of that formed in the others;—a difference between the slow oxidation in soils and ordinary combustion.

The determinations of CO_2 made in September and October at temperatures between 14° and 18° call for the same remarks. They show, moreover, that temperature has a considerable influence. The production of CO_2 at 16° is only half that at 24°.

The following are the results of HNO_3 determinations calculated in mgms. per kilo. of earth:—

No. - - -	1	2	3	4	5
7th Nov. 1872 -	151·8	201·8	238·6	352·7	268·7
5th July 1872 -	106·1	106·1	106·1	106·1	106·1
HNO_3 formed -	45·7	95·7	132·5	246·6	162·6

The nitric acid present at first = 1528 mgm.

Schloesing fancies Nos. 4 and 5 have been transposed.

The production of HNO_3 depends on the amount of oxygen in the atmosphere.

Second series, differing only in the degrees of humidity which was raised to the maximum, viz., 24 per cent. In No. 1 pure nitrogen alone was used, and Nos. 2, 3, 4, and 5 had atmospheres containing 6, 11, 16, 21 per cent. oxygen.

The experiments lasted from 18th November 1872 till 3rd July 1873.

The CO_2 determinations in November and December gave as means—

No. - - -	1	2	3	4	5
Per cent. oxygen in atmosphere -	0	6	11	16	21
T. - - -	14·3	14·5	15·0	16·1	14·2
CO_2 formed mgm. per kilo. earth -	9·03	15·09	16·0	16·6	16·0

The results are similar to the earlier ones. The excess of moisture favours the oxidation; the temp. being lower.

Estimation of the Nitric Acid.

No. - - -	1	2	3	4	5
3rd July 1873 - -	0·00	263	286	267	289
8th November 1872 -	64·0	64	64	64	64
Nitric acid lost -	64·0.				
Nitric acid formed -	-	199	222	203	225

In No. 1 the nitrates have been destroyed, doubtless by the organic matter. In the others the nitrifications have been fairly equal.

The mode of decomposition of nitrates, when the medium is a soil deprived of oxygen, has not been to my knowledge the object of an exact examination. To 12 kilos of earth from Boulogne were added 7·5 grms. KNO_3 in dilute solution, the whole was placed in a 10 litre vessel with a gas evolution tube.

Moisture of the earth = 17·46 per cent.

Nitric acid	originally present =	0·844
	added as KNO_3 =	4·0095
		4·8535

The experiment commenced on the 20th November 1872.

The mercury at first rose in the evolution tube, owing to absorption of oxygen and CO_2. After five days the pressure began to increase again. On the 9th December it was equal to that of the atmosphere. On the 19th a violent evolution of gas commenced. Some gas was lost. At the end of the experiment 4200 c. c. of N and 1300 mgm. CO_2 were collected, other gases were absent.

Analysis of the earth:—

Nitric acid - - - -		0·00
NH_3 in 100 grms.	before experiment -	0·51 mgm.
	after „ -	1·35 „
Gain - - -		0·84 „

2nd experiment, conducted chiefly to measure initial and final volumes of N:—

Weight of earth - - -	11·4 kilos.
Moisture - - -	18·2 per cent.
Temperature (surrounding) -	5·5°.
Air introduced - - -	5·0215 litres.
Do. do. (corrected) - -	4·8904 „
N =	3·8732 „
O =	1·0172 „

Contraction at first as in previous experiment.

Analysis of the earth :—

Nitric acid		0·00
NH_3 in 100 grms. earth	before experiment	1·35
	after „	3·04
Gain		1·69

Gain of NH_3 for 11·4 kilos earth	192·7 mgm.
NH_3 equivalent to 7·5 grms. KNO_3	126·2 „

Analysis of the gas	CO_2	N
Vol. at 0° and 760, recovered by the pump	3484·2	4088·5
Do., disengaged during experiment	89·1	809·4
	3573·3	4897·9

Nitrogen present in air at first	= 3873·2
Nitrogen present in 7·5 grms. KNO_3	= 828·0
	4701·2

There is thus more N given off than is present in the air and KNO_3.

Boussingault has shown that in a confined oxygenated atmosphere the gaseous nitrogen does not go to form nitrates, which indeed lose a small quantity of their combined nitrogen. Schloesing confirms this last result.—Th. Schloesing, *Compt. Rendu,* vol. 77, p. 353.

No. III.

OXIDATION OF AMMONIA.—EXPERIMENTS MADE SOME YEARS AGO, AND LEADING UP TO THE POINTS EXPLAINED IN No. I.

The peculiar effect of the nitrates has led me into the question of the oxidation of ammonia further than previously because there seemed to be a question as to the action of organisms. I am desirous of saying that I have no desire to give my experiments in opposition to Mr. Warrington. The only experiments that were caused by reading Mr. Warrington's were those relating to the action of light and those where heat was used. I so far corroborate Mr. Warrington that light does retard the action in organic solutions. The action of organic matter is further shown by the fact that where there is most organic matter in a state readily decomposed by caustic potash that is in so-called albuminoid matter, the action of decomposing nitrates is strongest.

Pure water containing 0·1 per cent. milk, filtered through sand free from nitrites and nitrates.

The sand was about 8 inches deep and 1 inch broad.

1st. 100 c. c. filtered from December 2nd to December 20th, 1874, contained 0·185 mgm. HNO_3.

2nd. 100 c. c. filtered from December 2nd to December 20th, 1874, contained 0·170 mgm. HNO_3.

100 c. c. not filtered, kept in a bottle half a day, 0·148 mgm. HNO_3.

100 c. c. of the water used, filtered through sand from December 2nd to December 20th, 1874, 0·00.

The remaining portions were again filtered till 28th January 1875, making altogether 46 days, when—

1st gave	0·163 mgm. HNO_3.
2nd „	0·148 „ „
Pure water	0·00.

There is a slight increase in the amount of HNO_3 by filtration from the 2nd to 20th December, 18 days; and afterwards to January 28th, the 46th day, there is a decrease, but not so small as in the unfiltered portion. Pure water filtered through the sand for 46 days remained free from HNO_3.

Ozonized air with phosphorus vapour aspirated through water containing milk, and water made alkaline containing yeast. January 1875.

Through 50 c. c. water containing 0·01 per cent. yeast for 85 hours, extending over 5 days, strong reaction to ozone paper	No HNO_3 formed.
Through 50 c. c. water containing 0·01 per cent. milk for 69 hours; 50 c. c. of caustic soda pure were then added, and the ozonized air again aspirated for 95 hours, altogether 164 hours	No HNO_3 formed.
After passing the 0·01 per cent. milk solution, the ozonized air was aspirated through water containing 0·01 per cent. yeast for 164 hours	No HNO_3 formed.
And finally through another 50 c. c. water containing 0·01 per cent. yeast for 164 hours	No HNO_3 formed.

When the aspiration was stopped all the liquids were alkaline, and gave a reaction with ozone paper. Each portion smelled of phosphorus.

Permanganate and yeast :—

0·3 grm. yeast mixed with water and permanganate of potassium, stood 20 days	No HNO_3 formed.

Hydrogen peroxide and yeast :—

0·3 grm. yeast mixed with water and 25 c. c. H_2O_2 solution, stood 20 days.

The 0·3 grm. yeast contained before mixture	0·444 mgm. HNO_3.
After mixture with H_2O_2 solution, and standing 20 days	0·193 mgm. HNO_3.

There has been a decrease in the amount of HNO_3.

Reduction of Nitrate by H_2S in slightly Acid and in slightly Alkaline Solutions.

Both columns were determined in the same way by reduction with aluminium.

——	Milligrammes HNO_3.	
Bottle, loosely closed with paper, December 5th, 1875. 200 c. c. KNO_3 solution + 1 c. c. NaHS solution + 0·5 c. c.	1st day.	39th day.
Acetic Acid. Acid - - - - - -	8·750	8·214
Bottle stoppered do. do. do. -	8·750	8·214
Open, December 5th, 1875, 200 c. c. KNO_3 sol. + 0·5 c. c.		41st day.
NaHS sol. + 0·1 c. c. $\bar{A}$ - - - -	8·750	8·177
Stoppered do. do. do. -	8·750	8·326
Open, December 5th, 1875, 100 c. c. KNO_3 sol. + NaHS sol.		42nd day.
0·25 c. c. Alk. - - - - -	4·375	4·012
Closed do. do. do. -	4·375	4·012
		38th day.
Open, 200 c. c. KNO_3 sol. + 1 c. c. NaHS sol. Alk. - -	8·750	8·194
Closed do. do. - -	8·750	8·342
Open, 200 c. c. KNO_3 sol. + 2·5 c. c. NaHS sol. Alk. -	8·750	8·405
Closed do. do. -	8·750	8·105
Open, 200 c. c. KNO_3 sol. + 5 c. c. NaHS sol. Alk. - -	8·750	8·356
Closed do. do. -	8·750	8·504
Open, 200 c. c. KNO_3 sol. + 5 c. c. NaHS sol. + 0·5 c. c. $\bar{A}$ Alk.	8·750	8·357
Closed do. do. do. -	8·750	8·504
Open, 200 c. c. KNO_3 sol. + 10 c. c. NaHS sol. Alk. - -	8·750	8·561
Closed do. do. - -	8·750	8·483
Open, 200 c. c. KNO_3 + 0·25 c. c. NaHS + 0·1 c. c. $\bar{A}$ Alk. -	8·750	8·153
Closed do. do. do. -	8·750	8·301
Closed, 200 c. c. KNO_3 + 10 c. c. NaHS sol. + 0·7 c. c. $\bar{A}$ Alk.	8·750	8·561
H_2S was found in two bottles only. After 39 days these were alkaline.		

I have spoken of the oxidation of organic nitrogen, and we must now consider the oxidation of inorganic nitrogen or ammonia.

Oxidation of Free and Acid Solutions of Ammonia by Hydrogen Peroxide.

——	Milligrammes HNO_3 in the Total Liquid.	Milligrammes HNO_3 in the Re-agents before mixing.
10 c. c. NH_3 solution + 50 c. c. H_2O_2 solution. Tested after eight months - - -	14·45	0·77
10 c. c. NH_3 solution, acidified with 20 c. c. dilute H_2SO_4 + 50 c. c. H_2O_2 solution. Tested after eight months - - - - -	3·56	1·29
10 c. c. NH_3 solution, acidified with 10 c. c. dilute H_2SO_4 + 20 c. c. H_2O_2 solution. Stood exposed to light from February 20th, 1874, to March 24th, 1874 - - - - -	1·48	0·89
30 c. c. NH_3 solution + 55 c. c. H_2O_2 solution -	12·28	0·81

LATER EXPERIMENTS.

OXIDATION OF AMMONIA.

PERMANGANATE of POTASH, AMMONIA, and SULPHATE OF AMMONIA. Oxygen and Nitric Acid in Parts per Million.
5 p. c. sol. 5 p. c. sol. 2 p. c. sol.

——	20 c.c. $KMnO_4$ sol. 3 c.c. $(NH_4)_2SO_4$ sol. 1 c.c. NH_4OH sol. } Water to 1 litre.			20 c.c. $KMnO_4$ sol. 1 c.c. NH_4OH sol. } Water to 1 litre.			20 c.c. $KMnO_4$ sol. 3 c.c. $(NH_4)_2SO_4$ sol. } Water to 1 litre.			40 c.c. $KMnO_4$ sol. 6 c.c. $(NH_4)_2SO_4$ sol. 2 c.c. NH_4OH sol. } Water to 1 litre.			60 c.c. $KMnO_4$ sol. 9 c.c. $(NH_4)_2SO_4$ sol. 3 c.c. NH_4OH sol. } Water to 1 litre.		
	HNO_3 formed.	Available O lost.	Days in Contact.	HNO_3 formed.	Available O lost.	Days in Contact.	HNO_3 formed.	Available O lost.	Days in Contact.	HNO_3 formed.	Available O lost.	Days in Contact.	HNO_3 formed.	Available O lost.	Days in Contact.
1880.															
1st Oct.	9·34	16	1	11·12	8	1	—	0	1	16·3	32	1	21·49	79	1
29th ,, -	20·00	—	29	—	—	—	—	—	—	—	—	—	59·3	—	29
1st Nov.	—	—	32	—	—	—	—	—	—	—	—	—	—	—	—
4th ,, -	23·2	47	35	15·2	63	35	2·67	0	35	23·8	126	35	104·5	379	35
12th ,, -	17·0	47	43	14·1	63	3	6·67	16	43	23·0	632	43	206	491	43
1881.															
19th Jan.	81·5	95	111	55·6	103	111	4·45	32	111	97·8	660	111	267	679	111
29th Mar.	52·6	289	180	34·7	281	180	7·71	211	180	77·8	680	180	278	1,158	180

OXIDATION OF AMMONIA.

Permanganate of Potash, Ammonia, and Sulphate of Ammonia. Oxygen and Nitric Acid in Parts per Million.
5 p. c. sol. 5 p. c. sol. 2 p. c. sol.

—	80 c. c. $KMnO_4$ sol. 12 c.c. $(NH_4)_2SO_4$ sol. 4 c.c. NH_4OH sol. } Water to 1 Litre.			160 c. c. $KMnO_4$ sol. 24 c.c. $(NH_4)_2SO_4$ sol. 8 c.c. NH_4OH sol. } Water to 1 Litre.			160 c. c. $KMnO_4$ sol. 24 c. c. $(NH_4)_2SO_4$ sol. } Water to $\frac{1}{2}$ Litre.			160 c. c. $KMnO_4$ sol. 8 c. c. NH_4OH sol. } Water to $\frac{1}{2}$ Litre.			160 c. c. $KMnO_4$ sol. 24 c. c. $(NH_4)_2SO_4$ sol. 8 c. c. NH_4OH sol. } Water to $\frac{1}{2}$ Litre.		
	HNO_3 formed.	Available O lost.	Days in Contact.	HNO_3 formed.	Available O lost.	Days in Contact.	HNO_3 formed.	Available O lost.	Days in Contact.	HNO_3 formed.	Available O lost.	Days in Contact.	HNO_3 formed.	Available O lost.	Days in Contact.
1880.															
1st Oct.	22·24	32	1	28·16	759	1	7·41	—	1	17·8	751	1	21·5	711	1
1st Nov.	63·0	—	32	68·2	—	32	8·52	—	32	72·6	—	32	75·6	—	32
4th „ -	203·1	601	35	308	1,959	35	11·12	—	35	661	3,397	35	1,127	4,898	35
12th „ -	241	664	43	—	2,214	43	20·7	632	43	667	3,607	43	1,082	4,962	43
1881.															
19th Jan.	427	980	111	623	2,708	111	35·6	948	111	1,112	4,424	111	889	5,530	111
29th Mar.	454	1,092	180	845	3,438	180	32	3,016	180	949	5,601	180	1,601	6,808	180

OXIDATION OF AMMONIA.

Specimens heated Previously. Solutions as before.

1·606 grm. $KMnO_4$, 50 c. c. $(NH_4)_2SO_4$ sol., 50 c. c. water. After heating to 100° C. for 2 hours— $HNO_3=390$, Permanganate still present.	2·339 grm. $KMnO_4$, 50 c. c. NH_4OH sol., 50 c. c. water. After heating to 100° C. for $\frac{1}{2}$ hour— $HNO_3=830$, Permanganate still present.	Between 1 and 2 grm. $KMnO_4$ (about 1 grm.), 100 c. c. NH_4OH sol. After heating to 100° C. for 1 hour— $HNO_3=734$, Permanganate all reduced. Considerable evolution of gaseous nitrogen.	50 c. c. NH_4OH, 50 c. c. $(NH_4)_2SO_4$, 1·5 grm. $KMnO_4$, 100 c. c. water. After heating to 100° C. for 2 hours— $HNO_3=216$, Permanganate all reduced.	5 c. c. NH_4OH, 5 c. c. $(NH_4)_2SO_4$, 5 c. c. $KMnO_4$. The permanganate was in one limb of a bent tube and the ammonias in the other; after being heated to 100° C. for 2 hours the solutions were mixed— $HNO_3=38·91$ mgm. NH_3 present=333·65 mgm.
				5 c. c. NH_4OH, 5 c. c. $KMnO_4$, Treated as above— $HNO_3=26·7$ mgm. NH_3 present = 297·5 mgm.

OXIDATION OF AMMONIA.

Results given as Nitric Acid in Parts per Million.

—	Mixed 31st May. 10c.c.1°/oKMnO$_4$sol., 5 c.c.(NH$_4$)$_2$SO$_4$ sol. 100 c.c. Pure Water.	Mixed 31st May. 5c.c.1°/oKMnO$_4$sol., 5c.c.(NH$_4$)$_2$SO$_4$ sol. 100 c.c. Pure Water.	Mixed 11th June. 200 c.c. KMnO$_4$ sol., 25c.c.(NH$_4$)$_2$SO$_4$sol., 50 c.c. NH$_4$OH sol. Water to 1 Litre. KMnO$_4$ all reduced to MnO$_2$ in 1 Day.	Mixed 27th August. 200 c.c. KMnO$_4$ sol., 25c.c.(NH$_4$)$_2$SO$_4$sol., 10 c.c. NH$_4$OH sol. Water to 400 c.c. Alkaline. Exposed to Daylight.	Mixed 27th August. 200 c.c. KMnO$_4$ sol., 25c.c.(NH$_4$)$_2$SO$_4$ sol. Water to 400 c.c. Neutral. Exposed to Daylight.	Mixed 30th August. 200 c.c. KMnO$_4$ sol., 25c.c.(NH$_4$)$_2$SO$_4$sol., 10 c.c. NH$_4$OH sol. Water to 400 c.c. Alkaline. Kept in Darkness.	Mixed 30th August. 200 c.c. KMnO$_4$ sol., 25c.c.(NH$_4$)$_2$SO$_4$ sol. Water to 400 c.c. Neutral. Kept in Darkness.	Mixed 1st Sept. 25 c.c. H$_2$O$_2$ sol., 25c.c.(NH$_4$)$_2$SO$_4$ sol. Water to 400 c.c.	Mixed 1st Sept. 25 c.c. H$_2$O$_2$ sol., 25c.c.(NH$_4$)$_2$SO$_4$sol., 10 c.c. NH$_4$ OH. Water to 400 c.c.
1880. May 31	0·62	0·16							
June11	..	..	2·43						
„ 15 (about)	..	..	446·0						
Aug. 27	..	..	..	6·0	6·0				
„ 30	..	..	667	1057	..	6·0	6·0		
Sept. 1	..	..	..	..	..	..	..	0·8	0·8
„ 2	5·41	4·45	630	1123	6·0	806	..		
„ 17	5·58	4·84	756	1297	7·4	..	88·9		
Oct. 1	..	..	741	1601	10·6	889	96·4	8·3	80·0
Nov. 4	..	..	783	..	..	..	..	8·21	88·9
1881. Jan. 19	..	..	623	1660	48·9	1037	..	16·7	21·3
Mar. 29	..	..	..	2490	68·9	2501	..	7·26	9·04

OXIDATION OF AMMONIA.

Blood Solutions.—Heated.

0·969 grm. KMnO$_4$ and 100 c. c. 1°/o blood solution were heated separately to 100° C. for 2 hours and then mixed, 23/9/80. After 4 days' contact—

HNO$_3$=37·36 parts per million.

0·819 grm. KMnO$_4$ and 100 c. c. 1°/o blood solution with 1 c. c. pure NaOH solution were heated separately, as above, 23/9/80. After 4 days' contact—

HNO$_3$=29·65 parts per million.

0·480 grm. KMnO$_4$ and 100 c. c. 1°/o blood solution treated as above. After 4 days' contact—

HNO$_3$=28·91 parts per million.

There is here mentioned a series of trials made some years ago by passing air drawn over phosphorus, first through pure water, to find if the reagents were pure. After 60 hours the water was found to contain in 10 c. c. 0·01 mgm. of ammonia, and that in contact with phosphorus contained no nitric acid.

This phosphorised or ozonized air drawn through water for a week produced no nitric acid in it.

Then through dilute ammonia for three days no nitric acid.

Another (time not recorded) gave 0·22 mgm. HNO_3.

It will be seen from the tables that an ammoniacal condition greatly increases the nitric acid. This action of the alkaline state is probably an explanation of the effect produced in the presence of chalk, and may explain the amount of nitrates in at least some chalk waters. The water having organic matter in it and percolating through the chalk has this matter more readily oxidized.

It is clear then that oxidation is continually ready to take place when organic substances are in water, and oxygen is presented to them. This takes place rapidly if air is abundant, but it takes place much more rapidly when oxygen is presented in a concentrated form, as in nitrates, &c.

It may be asked how far this may affect the estimation of albuminoid ammonia when heat is used with permanganates.

The consequence also is that waters with little oxygen in solution are under suspicion, or rather they have or have recently had substances in solution taking up oxygen. I brought this subject forward many years ago, but it is not sufficiently attended to by chemists. Dalton saw the importance of this examination long before our time. It is a merit, which only the best water has, to have the oxygen and nitrogen in the relation of not less than 33 per cent. of the first. Sometimes I have found as much as 35. I do not know if this is an error of experiment; I think not.

We learn that by a natural process all traces of organic impurity may be removed from water, and that part of this may be done more rapidly by the assistance of organisms probably, but that it may be concluded without organisms. The only usual bodies that cannot be removed are the alkalies and their salts, of which chloride of sodium is the most striking. The attention paid to this salt has been too small. I brought this forward also in 1867, but that also was attended to before me by Dalton, and forgotten. I hope it will not again be left out. It is remarkable with what complacency many chemists look on an amount of chlorine in water far above that of the natural drainage of the district.

No. IV.

FURTHER ON THE ACTION OF THE AIR ON COMBINED NITROGEN.

The air oxidizes the nitrogen which is found in nitrogen compounds. That this is done when organic substances are absent, is not proved, so far as I know. The oxidation of inorganic nitrogen seems to require higher oxides than the air. But we may ask, will not the ozone of the air prove to be one of these higher oxides? It is probable, and we may expect it to show its power in some cases, but it cannot be expected to reach far into sewers or heaps of refuse. We may, however, be certain that it acts as a purifying agent on substances which it meets in the atmosphere.

On the table of changes in the condition of nitrogen in water containing organic matter (p. 26) it will be observed that the nitric acid column shows a general increase in every case. There is, however, a curious observation to be made; at a certain time there is a slight diminution, and this has taken place in every instance. It cannot be supposed that amongst so many analyses no error should have occurred, but such a consistent error is not probable. Besides, a similar backward movement occurred on the occasion of a previous inquiry; and, indeed, it agrees with my early observations of the destruction of nitrates, and the later ones on the elimination of nitrogen. Those in the table given all occurred from the end of June to the beginning of August, as if the heat had been an agent, the peculiar putrefaction spoken of having occurred, I suppose.

The nitrogen of the three kinds increases (*i.e.*, of free ammonia, albuminoid ammonia, and nitric acid), being supplied by the residual albumen, which is supposed to be continually decomposing.

The effect of darkness has been to increase the growth of nitrates in the Manchester water, and the stronger blood solution. This is not apparent in the weaker.

It seems correct, from the results already given, to believe that nitric acid has a mode of growth in nature quite independent of organic matter, as well as one which is intimately associated with organic matter. We have arrived at a very firm stage, but we require to learn more. The inorganic matter from which nitric acid is made contains ammonia, and we require now to learn in what conditions nitrogen is used for forming ammonia if there be any methods of doing so.

Schönbein showed long ago that ozone caused the formation of nitric acid; and Luca (Jahresbericht, 1855, p. 318) said that, after passing air ozonized by contact with moist phosphorus for three months over potassium and potash, there was formed nitrate of potash in quantities enough to crystallize; from 7,000 to 8,000 litres of air being used. With these views before us we may look to the formation of nitrates from inorganic nitrogenous compounds in cases where ozone exists, and that may possibly be found in very pure water, where the oxygen exists to the nitrogen in a proportion above 33 per cent. Alkalinity seems an essential condition in many of the mixtures, but I have found nitrates in wells very acid. This was remarkable in the case of a polluted well, the use of which had probably caused death to more than one of a family in Manchester before the present water supply was universal in the city.

Inferences.

We may then come to the following conclusions:—Bodies containing protein compounds, when in abundance of water and in common air, may oxidise and form nitric acid.

The same organic bodies in a state of decomposition and in water may oxidise at the expense of nitrates, and give out nitrogen.

The first condition is that in which a certain amount of sewage is in the water, but is overpowered by the amount of air.

The second is when the sewage is in excess, and overpowers the nitrates.

Nitrates may be formed by the oxidation of inorganic nitrogen, but not so far as we know by contact with common air under ordinary conditions. The oxygen must be presented in a more concentrated or more active condition.

Putrefaction destroys organic matter without the influence of oxygen; it breaks up organic compounds, and destroys organisms. The evidence seems to indicate that it destroys even those bodies that produce disease, but that in certain conditions it produces others. This is a point not to be enlarged upon without more knowledge, but it is evident that by putrefaction we get rid of an enormous amount of offensive matter. Oxygen cannot enter under the surfaces of actively putrefying bodies; but whenever it is allowed to enter by the putrefaction being less active, an action begins which in time completes the destruction of the body. We are not, therefore, to suppose that the germs of disease can resist all these efforts of nature to destroy noxious things, nor are we to suppose that an invisible germ of disease can pass on from stage to stage unaffected by the putrefaction of sewage and the action of air. We must believe, for the present, that it is not so. In water we see perfect purification, nitrogen itself being lost.

In ordinary putrefaction sulphuretted hydrogen comes off in abundance, with much carbonic acid and some nitrogen. Oxygen resists this action, and if the oxygen is supplied in a concentrated condition a change takes place, nitrogen is evolved as the principal gas, and a decomposition of nitrogen compounds takes place. Nitrogenous bodies are thus destroyed, in one manner by their voluntary putrefaction, in another by oxidation. Up to a certain point not determined the greater the amount of nitrogenous bodies the more rapid is their decomposition.

The oxygen of the nitrate passes in part to the carbon; some will be retained, forming a carbonate. I have not estimated how much, or if all, is taken by the carbon.

If the solution is weak the nitrogen takes up the oxygen, and does not allow it to pass away, thus forming nitrates.

Putrefaction and oxidation are two well-known modes of destroying organic bodies at ordinary temperatures. The second is not proved to be connected with organisms.

How far then can oxidation or a great supply of air be employed to destroy putrefaction or to purify.

The bearing it has on the analysis of water will be clearly seen by chemists. The bearing on the sewage question is also interesting. Substances and living things may be carried by the rapid sewage system into the range of a new activity before undergoing that putrefaction which breaks them up in proximity to us or in the sewers themselves. It seems to point to a plan of causing the destruction of organisms by putrefaction and subsequent oxidation or by chemical action. At least it seems to me that we require to learn if it be true that any of the germs of disease, or which germs of disease, will live in an abundance of good air. We know that abundant dilution will render them all ineffective. It is probable that there will be a difference amongst them in this respect, whilst all will yield to the double action of first putrefaction and then oxidation.

On oxidation, more will follow, when speaking of its application to sewage, &c.

Changes in the condition of NITROGEN in WATER containing ORGANIC MATTER.

The figures indicate parts per million.

Date.		Days Standing.	Manchester Water in Light.				Manchester Water in Darkness.				0·25 c. c. Blood per 10 ltrs. H_2O Light.				0·25 c. c. Blood per 10 ltrs. H_2O Dark.				2·5 c. c. Blood per 10 ltrs. H_2O Light.				2·5 c. c. Blood per 10 ltrs. Water Dark.			
			N as Free NH_3.	N as Alb. NH_3.	N as HNO_3.	The 3 N's.	N as Free NH_3.	N as Alb. NH_3.	N as HNO_3.	The 3 N's.	N as Free NH_3.	N as Alb. NH_3.	N as HNO_3.	The 3 N's.	N as Free NH_3.	N as Alb. NH_3.	N as HNO_3.	The 3 N's.	N as Free NH_3.	N as Alb. NH_3.	N as HNO_3.	The 3 N's.	N as Free NH_3.	N as Alb. NH_3.	N as HNO_3.	The 3 N's.
May	13	Fresh	0·033	0·074	—	—	0·041	0·079	—	—	0·099	0·326	—	—	0·0088	0·212	—	—	0·650	3·33	—	—	0·79	3·66	—	—
	17	4	0·039	0·069	—	—	0·041	0·079	—	—	0·086	0·219	—	—	—	—	—	—	0·728	3·50	—	—	0·833	3·05	—	—
	21	8	0·026	0·072	—	—	0·026	0·074	—	—	0·206	0·454	—	—	0·201	0·461	—	—	1·186	3·97	—	—	1·25	4·20	—	—
	26	13	0·062	0·079	—	—	0·039	0·076	—	—	0·382	0·420	—	—	0·425	0·413	—	—	3·26	3·46	—	—	5·80	2·27	—	—
	31	Fresh	—	—	0·069	—	—	—	0·069	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
	31	18	—	—	0·566	—	—	—	0·526	—	—	—	0·666	—	—	—	1·11	—	—	—	2·30	—	—	—	2·46	—
June	7	25	0·036	0·072	0·564	0·672	0·059	0·056	0·577	0·692	0·652	0·298	0·664	1·614	0·665	0·283	0·617	1·565	5·106	1·853	1·33	8·289	7·11	1·78	1·18	10·07
	23	41	0·044	0·066	0·535	0·645	0·106	0·058	0·555	0·719	0·848	0·160	0·544	1·452	0·889	0·173	0·295	1·357	7·11	1·845	1·23	10·185	8·56	1·15	1·03	10·74
July	1	49	0·036	0·079	0·535	0·650	0·119	0·072	0·544	0·735	0·889	0·168	0·513	1·570	0·889	0·168	0·371	1·428	7·71	1·37	1·01	10·09	8·89	0·79	0·624	10·304
August	10	89	0·015	0·058	0·608	0·681	0·021	0·051	0·699	0·771	0·058	0·182	1·540	1·780	0·029	0·015	1·70	1·744	4·61	0·972	4·81	10·392	4·87	0·74	5·06	10·67
	20	99	0·016	0·041	0·657	0·714	0·013	0·044	0·739	0·796	0·130	0·130	1·69	1·950	0·030	0·10	1·73	1·860	3·56	0·91	4·94	9·41	5·45	0·76	5·43	11·64
September	2	112	0·018	0·057	0·666	0·741	0·016	0·064	0·764	0·844	0·099	0·017	1·72	1·836	0·180	0·16	1·74	2·080	4·48	0·89	5·13	10·50	6·26	0·77	5·34	12·37
	17	127	0·020	0·053	0·699	0·772	0·016	0·059	0·732	0·807	0·118	0·132	1·89	2·140	0·036	0·087	2·01	2·133	4·28	0·74	5·51	10·53	5·60	0·59	5·93	12·12
October	1	141	0·021	0·053	0·695	0·769	0·018	0·062	0·967	1·047	0·140	0·112	1·85	2·102	0·036	0·092	1·87	1·998	4·28	0·64	5·50	10·42	6·09	0·54	5·76	12·39
	26	166	0·023	0·054	—	—	0·025	0·059	—	—	0·145	0·109	—	—	0·043	0·100	—	—	4·45	0·724	—	—	6·69	0·504	—	—

No. V.

OXIDATION AND DECOMPOSITION IN SEWAGE.

I feel almost ashamed to bring so many figures to prove so little, but we know that wisdom grows slowly; and the enormous amount of labour spent on sewage within 40 years shows that it has happened also to all my contemporaries as well as predecessors to give their labour to an extent which seems too great for the result. Still, I by no means think that my time is lost if I prove no more than this,—that by agitation with air sewage is kept without smell for two or more weeks in ordinary weather, and even in warm weather. This is probably the most practical portion of this part of the inquiry. The numerous analyses are not at all so telling as I expected them to be, but they do tell something of the chemistry of the sewage in relation to its loss of organic matter, and the change to inorganic. This inquiry, like all inquiries on the subject hitherto, has a preliminary character; but it is a progress in my opinion, and I am desirous of publishing it, whether found at present practicable or not, because I think that in time the practical may be forwarded by it.

The gases from putrefaction I examined some years ago, as elsewhere quoted. In 1877 I made some experiments simply to find if abundance of water increased or diminished the offensive emanations, and these I supposed to be measured by the ammonia. It is shown that the air is much more tainted with ammonia when water is not present: see "Gases from Sewage, &c.," p. 29. Another inquiry going further may follow this, but I look to the microscopist rather than the chemist to complete it.

Gases from Sewage.

The gases from sewage are continually receiving attention, but they are not frequently examined in a quantitative manner. It is very difficult to obtain them in the condition and state of dilution in which they cause illness. It has been observed that these gases produce more cases of illness than gases from excreta not producing sewage, and it seemed of some importance to know what was the proportionate amount of decomposition. The result is insufficiently shown in the table to follow. The atmosphere over the dry matter contains much more ammonia than over the wet. This may not mean that more organic matter is decomposed; it may mean only that the water keeps the ammonia in solution. Still, bubbles of gas rise rapidly from sewage in water; no such rapid evolution takes place with the dry matter. The quality of the decomposition is different. Water assists greatly the growth as well as decay of most organisms, and to these we must attribute the more rapid escape of gas. Still, the experiments point only in one direction, and we have to learn much regarding the quality of the air which comes out so rich in ammonia. This much we learn: that water assisting the conditions of putrefaction causes a much greater destruction of organic matter, and the emanations are offensive, but we do not hear much of decided injury when they are mixed with a great profusion of air. Perhaps the paper by Dr. Henderson of Helensburgh has touched this point more directly than anything that has been done.—*See* "The Sewage Question in its relation to the Gareloch and neighbourhood."—Helensburgh.

It was natural, however, to inquire what advantage could be got out of this knowledge; and at a later period, after obtaining Dr. Storer's apparatus, a fresh stimulus was given to the inquiry in a new direction.

It will be observed that in the "dry" column the ammonia rises much higher than in the "wet." This would show that, however much gas may escape from sewage, it is never very full of the products of decomposition of animal matter. The result of breathing sewage gases, if more hurtful, must be attributed either to the supply being more abundant, although more dilute, or of a more offensive kind. This latter explanation seems the more probable one, but requires examination. This is quite in agreement with the statement elsewhere made as to the rapid destruction of organic matter in water. We know that there is a stage of great danger, and, when the sewage is diluted and exposed to the atmosphere, a rapid and remarkable stage of diminished, if not disappearing, injuriousness. The materials were kept in carboys holding about eight gallons (36·5 litres); the carboys were kept closed, a certain measure of air drawn out and washed, and an equal amount of fresh air allowed to enter.

Gases from Sewage and Sewage Materials.

Date.	T°C.	NH_3 from the Solid.			NH_3 from the same in Water.			——
		Free.	Saline.	Albmd.	Free.	Saline.	Albmd.	
1877.		Mgm. per cubic metre of air.						
October 17th - -	—	* 8·5			* 7·0			Nesslerised direct without distillation.
„ 18th, 10 a.m. -	—	*10·0			* 7·5			
„ „ 1 p.m. -	—	*10·5			* 7·5			
„ „ 5 p.m. -	—	*10·0			* 5·0			
„ 19th, 10 a.m. -	16°	* 8·0			* 3·5			
„ „ 2 p.m. -	—	* 6·5			* 5·0			
„ „ 5 p.m. -	—	*10·5			* 5·0			
„ 20th - -	16°	* 8·0			* 7·5			
„ 22nd, 10 a.m. -	18°	*20·75			*17·5			Wet, smells strong and repulsive. Dry, more intense than at first.
„ „ noon -	—	*20·0			*19·5			
„ „ 3 p.m. -	18°	*22·5			*25·0			
„ 23rd, 10.30 a.m.	15°	*22·5			*17·5			Corks on carboys removed, and caoutchouc caps substituted.
„ „ 3 p.m. -	17°	*23·5			—			
„ 24th, 10 a.m. -	17°	*31·0			*17·5			Smell from the wet becoming less intense. (The wet had another 50,000 grns. water added.)
„ „ 3.30 p.m.	15°·5	*33·0			*12·5			
„ 25th, 10 a.m. -	16°	*37·5			*22·5			Distilled from alkaline permanganate.
„ 26th, noon -	15°	*52·6			*15·5			
„ 27th, 10 a.m. -	16°	*80·0			*10·0			
		67·5			*6*		—	Free. Till 24th no distillations.
„ 29th, 10.30 a.m.	17°	*140*		4·0	*15*		3·5	
„ 30th, 10.30 a.m.	17°·5	*105*		24·0?	*12·5*		2·5	
					0	15·0	1·5	(Acidulated water used to collect the ammonia.)
„ „ 3 p.m. -	18°	*100*		1·6	—	—	—	
„ „ 3.30 p.m.	—	*110*		2·0	8	0	0·8	
„ 31st, 10.30 a.m.	—	0	180	2·0	4·8	0	2·0	
November 1st, 10 a.m. -	16°	170	0	2·5	4·0	0	1·0	
„ „ noon .	17°	140	0	?	—	—	—	
„ 2nd - -	16°5	160	1·5	3·0	4·6	0·2	1·0	
„ 3rd, 10 a.m. -	16°	160	1·0	7·5	2·6	0	1·0	
„ 5th, 9.30 a.m.	16°·5	160	0	2·5	4·4	—	?	
„ 6th, 9.30 a.m.	16°·5	*162·5*		2·0	5·0	—	1·4	
„ 7th, 9.30 a.m.	19°	*225*		—	—	*16*		Soda lime.
„ „ 12.30 p.m.	—	*250*		—	—	*6*		Not distilled.
„ 8th - -	17°	*200*		—	—	*6*		
„ 9th, 10 a.m.	—	*195*		—	—	*5·6*		Do.
					—	*8·2*		Soda lime.

* Asterisks show the three ammonias given; italics the first two.

Date.	T.	Ammonia from the Solid.			Ammonia from the same in Water.			—
		Free.	Saline.	Albmd.	Free.	Saline.	Albmd.	
1877.								
November 10th, 9.30 a.m.	17°	-	200	—		4·4	—	Not distilled before Nesslerising.
						8·6	—	Soda lime.
„ 12th, 9 a.m.	16°	*230*		27·5	*12·0*		3·0	Acidulated water used to obtain the ammonia.
„ 13th	15°·5	*150*		7·5	*8*		2·0	
„ 14th, 9.30 a.m.	14°·5	*170*		—	*8·0*		2·6	
„ 15th	16°	*215*		4·5	—	8·4	—	
„ 16th	16°	*275*		3·0	*7·0*		0·6	Surface.
		290		2·0	—		—	1 ft. from surface.
„ 17th	—	*230*		5·5	*5·2*		1·2	
„ 19th	15°	*200*		6·0	*5·0*		1·0	
„ 20th	12°·5	*155*		2·5	*4·8*		1·0	
„ 21st	12°	*162·5*		6·0	*4·2*		0·8	
	12°	*149*		—	—		—	By Frankland's method.
„ 22nd	12°	*250*		6·0	*5·2*		1·6	
		200		—	—		—	Soda lime.
„ 23rd	12°	*212·5*		7·0	4·0		1·0	
		200·0		—	—		—	Soda lime.
„ 24th	12°	*225·0*		6·0	*7·0*		0·0	
„ 26th	10°	*187·5*		2·5	*2·2*		0·4	
		175·0		—	—	2·4	—	Dry by soda lime.
December 1st	12°	—		—	—	—	—	
„ 3rd	—	*252·0*		2·5	3·0		1·0	
„ 4th	13°	—		—	5·0		trace	
„ 5th	12°·5	*250·0*		0·5	—		—	
„ 10th	—	—		—	*2·2*		1·0	
„ 11th	14°	—		—	*1·6*		0·8	
„ 14th	12°	*175·0*		—	*0·0*		*4·0	*Albuminoid and saline? Wet carboy well agitated.
„ 17th	12°	*250·0*		—	—	—	—	
„ 19th	12°	*275·0*		—	0·0	3·6	0·0	
„ 29th	11°	—		—	0·6		—	
„ 31st	10°	*300·0*		2·5	1·0		0·0	
1878.								
January 2nd	10°	*300·0*		—	—	—	—	
„ 21st	15°·5	700	10·0	9·0	10·1	1·2	5·4	
„ 25th	10°	*600*		7·5	8·0	2·4	1·0	
February 1st	14°·5	700	8·0	7·5	11·2	0·0	1·6	
„ 2nd	1°	190	4·5	10·5	8·8	4·2	2·6	
„ 4th	1°·5	200	8·0	5·5	3·8	0·2	lost	
„ 12th	Freezg.	0·00	124·0	4·5	3·6	0·2	1·2	

Flesh, Decomposition of.

Date.	T.	Ammonia.			—
		Free.	Saline.	Albmd.	
1877.					
November 22nd	—	2·4		0·4	Flesh newly bought, when washed with water, showed ammonia by Nessler's test.
„ 24th	12°	0·8		0·2	
„ 26th	10°	6·6		?	Flesh tainted.
„ 28th	—	2·2		—	
„ 29th	—	2·0		—	
December 1st	12°	3·0		—	
„ 3rd	—	4·4		0·2	Flesh smell putrid.
„ 4th	13°	5·6		Trace	
„ 5th	12°·5	11·0		1·0	
„ 7th	—	13·0		1·4	
„ 8th	12°	11·8		2·0	
„ 10th	—	12·8		Trace	
„ 11th	14°	13·0		Trace	
„ 12th	—	19·0		Trace	
„ 13th	11°	14·2		—	
„ 14th	12°	18·0		—	
„ 15th	11°	13·0		—	
„ 17th	12°	22·0		—	
„ 19th	12°	22·0		—	
„ 20th	5°	21·0		0·8	
„ 29th	11°	16·0		Trace	
„ 31st	10°	16·0		Trace	
1878.					
January 2nd	10°	22·0		—	
„ 21st	15°·5	54·0	12·4	3·0	
„ 25th	10°	70·0	2·4	2·0	
February 1st	14°·5	70·0	1·6	3·0	
„ 2nd	1°·0	34·4	0·2	4·4	
„ 4th	1°·5	34·0	1·0	2·6	
„ 12th	Freezing	15·2	0·4	0·2	

No. VI.

Earlier Results—Aeration in Water.

This subject has long interested chemists, and aeration has formed one of the plans of engineers from a long date back. The mode of bringing down the water from the hills to Manchester has been founded on the idea that aeration is of value, the artificial falls being constructed with that view. It has also been a common saying that water is best when aerated, and mineral waters aerated to an excessive extent have been in use since first invented by Thomas Henry, F.R.S., of Manchester, last century; although it must be remembered that "aeration" by carbonic acid, as is the case in prepared waters, does not carry out the true idea of aeration which is by the oxygen of the air. "Aeration" by carbonic acid gives a secondary meaning. In order to test the purity of water to be used in distilleries, it was said jocularly in a publication, some 20 years ago, in Scotland, that one asked, "How many falls does the water fall? Talisker water comes down 80 falls." (*See* Highland Drovers, Douglas, Edinburgh.) The idea of purification by air must be considered then as a general one.

Of course water, as such alone, is not oxidized by the air, although a certain amount is held in solution; but water does not exist quite pure or unmixed in nature, and we really mean the destruction of organic matter in it when we speak of purifying water. I am obliged to approach this matter very cautiously because there are already men (one or more) who claim the aeration of water under certain conditions of impurity, such as sewage, as their invention; and it is not for me to state what is their legal position; but it is a long time since I began to study air and water, as well as sewage, as my published papers show, and I may be allowed to save myself trouble by making a few more quotations, instead of writing the old facts in new words, from a paper on "The Mud of the Clyde," Glasgow Philos. Soc. 1880, by R. Angus Smith.

P. 17.—"This raises a question to be solved, but the fact is certain that fevers have not been traced to the escape of gases of putrefaction when there has been a large amount of water and exposure to the air. But they have been found when the water is not very great in amount and the decomposition is made under cover, as in sewers. The question arises, is this owing to the concentration or to the difference of decomposition in darkness, or to the better supply of oxygen? The effect of sunlight in warm countries does not allow us to suppose that the daylight always produces in vapours an innocent state, although it has a great effect in that direction when there is little water . . . . . . With us, at least, innocence in the atmosphere seems to be rather something connected with the abundance of air in proportion to the impurity. This air may act in two ways. It may act by rapid oxidation of the substances in water, or by dilution of the gases when formed; and the destruction of putrid matter in water is really very rapid when plenty of air is allowed. This air is brought to the Clyde by the water, and also by the waves, both artificial and natural, exposing a great deal of surface. The air may act also merely by rapid dispersion of the gases. Still we must not forget that these gases or vapours are not reported to us to produce any marked type of disease over the Clyde, even when they come in a state so concentrated as to produce sickness; whereas gases from sewers, in a condition which may not produce immediate illness, may produce in time typhoid fever, as we are credibly informed.

"We must conclude then that it is not mere dispersion, but that it is a more thorough putrefaction and oxidation, which takes place in the Clyde, and a more complete destruction of the organic substance by the abundance of air, than can take place in sewer water, whatever the senses may indicate to us. Of course we must ever give some credit to the flow of air up the river and the ever fresh breezes that come from the Atlantic as well as the mixture of air with water caused by steamers.

"Whilst then there is oxygen enough at present to prevent disease, there is not enough to prevent smells which disgust and cause sickness for the time."

In connection with the purification of streams the following quotation may be added, although written previously:—

"When azotized compounds decompose and form ammonia, how long is this ammonia retained in the water? On examining a very putrid stream, I estimated the amount of ammonia at the most putrid portion, where carburetted hydrogen was passing off in great volumes, and where a cubic foot could be obtained in a very few minutes by stirring.

"In the sewage stream of which I have spoken the amount of ammonia was from 0·5 to 0·7 grain per gallon. After going 14 miles the amount was only 0·07, and after 20 miles none at all was found.

"The mud of the same stream was in a state of putrefaction, and contained per cent.:—

"Ammonia - - -	0·797
" a mile lower - -	0·420
" at second mile -	0·171

"The ammonia rapidly disappeared, and the mud itself diminished very greatly in amount.

"I estimated that one grain of ammonia evaporated in some seasons from every square foot per hour.

"In taking sewage water to the land, I think it very important that the movement should be as rapid as possible.

"The water in its passage of 20 miles has lost its valuable ammonia, and that within two or three days. This is a sufficient proof that we must not trust to the ammonia as an indication of the amount of the organic matter which has been, as it is as rapidly removed as the organic matter is decomposed;

that is to say, the length of time necessary for complete putrefaction is, under favourable circumstances, no greater than the time afterwards required for the removal of its products. In this water there was no life to be observed; but the estimation of the organic matter would have shown no difference, whether vitality had been present, and the substance had been capable of entering into active and unwholesome forms, or had been ready to break up into instantaneous putrefaction, or had been preserved, like a mummy, in carbolic acid for a thousand years.

"From this observation regarding the ammonia we are clearly led to beware, in our schemes of irrigation by sewage water, that the land shall be overflowed before the ammonia is thoroughly formed, or else, if the ammonia is formed, that it shall not be subjected to loss by long exposure to evaporation.

"We see also that nature provides here for the complete obliteration of organic matter. It ceases altogether to be found in the water. It may be traced, either as such, or as ammonia and carbonic acid, long after the bubbles of carburetted hydrogen have ceased to appear, until at last it dwindles down to an amount which is rather difficult to remove from water, and which, so far as we know, may be utterly disregarded.

"In the passage of organic matter we may observe, from figures soon to be quoted, that the volatile and organic matter diminished from 9·33 grains per gallon down to 5·04, even when there was an increase of fixed matter, and that the decomposing matter in solution diminished still more rapidly, in the ratio of 283 to 17.

"The organic matter having left the water, we may next inquire whether any trace of its existence remains behind. That trace we do find in the increased amount of alkalies, sulphates, and chlorides."—Memo. Lit. and Phil. Soc., Manchester, 1867-8; also "Chemical News."

I had also, even so early as 1848, shown the oxidizing action of filtering (report to the British Association already quoted), and in a paper to the Glasgow Philosophical Society, "On the Mud of the Clyde," the following words were used:—

Page 8.—"The long stretch of water lying between Ardmore and Dumbarton is not all an accumulation of mud, which is an indication that the mud falls down,* and is carried off mainly by the deep channels. The channel is inclined to act as a depositing reservoir, and the northern side receives the overflow water. The mud remaining in the channel is carried partly by nature, and as the water leaves the channel it becomes less disturbed and clearer. This water has, of course, some of the lighter and the soluble parts which the putrefying river brings down, but it really seems as if a very large portion of this were rapidly oxidized; and, whilst the neighbourhood is freed from the mud which might continue sending out more gases, the water is rapidly undergoing purification, and it flows then down as the tide goes out, bringing unexpectedly good water below, although, of course, not perfect. We see a rapid change of water about this district.

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* *i.e.* before spreading.

"I feel inclined, therefore, to repeat that this space is of great value to all the watering-places of the Clyde, and if it is ever made into arable land they will suffer severely. I only wish it were still larger."

This led to an examination of the effects on the atmosphere of the putrefaction in sewage and sewage materials. The examination, as usual, is not complete, except in one direction; and although the work was done four years ago, I have brought it forward in this Report: see p. 29. It may convey some idea of the work going on when putrefying substances in water are exposed to air in the conditions mentioned. The amount of organic matter becoming decomposed is great, and ammonia is continually given off. The experiments might be continued with advantage by finding the total amount of nitrogen and carbon from a given quantity of material in various conditions, but it is not easy to do this in conditions purely natural.

REPORT to the LOCAL GOVERNMENT BOARD by DR. ANGUS SMITH, F.R.S., as to TREATMENT of SEWAGE.

SIR,

As requested (by Mr. Sclater Booth, M.P., late President of the Board) I have the honour to submit a careful examination of five effluents, the results of processes for the purification of sewage water by different methods.

1st. From Aldershot, where irrigation alone is used.
2nd. From Coventry, where precipitation by alum and iron is used first and irrigation afterwards.
3rd. From Birmingham, where precipitation by lime is employed.
4th. From Burnley, where also lime is employed.
5th. From Aylesbury, where the A B C process is used, a precipitant of alum being employed, also of clay.

I shall extract from the larger tables some of the more important points; but I wish it to be observed that in this report I refer to quality of effluents only, and not at all to the ultimate value of processes.

FREE AMMONIA A.

——	Ammonia abstracted.		Ammonia remaining.	
	Parts per 100,000.	Per Cent. of Total.	Parts per 100,000.	Per Cent. of Total.
Aldershot *b.* - -	11·755	97·96	0·245	2·04
Coventry - - -	2·06	79·8	0·520	20·2
Aldershot *a.* - -	2·913	80·10	0·747	19·9
Aylesbury - - -	2·980	74·5	1·020	25·5
Birmingham *a.* - -	1·425	52·9	1·275	47·1
Burnley - - -	0·120	8·3	1·330	91·7
Birmingham *b.* - -	added 0·520	added 18·8	3·220	118·8

NOTE.—Aldershot (*a*) specimen collected in wet weather; (*b*) in dry weather Birmingham (*a*) after lime precipitation and irrigation; (*b*) after lime alone.

Free includes saline ammonia. Its existence is of no disadvantage so far as the effect on the atmosphere is concerned, but its absorption by the soil is important as manure. Its amount indicates decomposition. So far as free ammonia is concerned, the first on the list is Aldershot during dry weather, specimen *b*; Coventry stands second. There two processes are used, both precipitation (with alum, iron, and lime) and irrigation. The next is Aylesbury. The lime processes are certainly behind.

ALBUMENOID AMMONIA B.

—	Ammonia abstracted.		Ammonia remaining.	
	Parts per 100,000.	Per Cent. of Total.	Parts per 100,000.	Per Cent. of Total.
Aldershot *b.*	5·195	98·95	0·0550	1·05
Coventry	1·639	96·40	0·061	3·60
Aylesbury	0·59	89·40	0·070	10·60
Aldershot *a.*	1·543	88·2	0·207	11·8
Birmingham *a.*	0·840	80·0	0·210	20·0
Burnley	0·895	74·6	0·305	25·4
Birmingham *b.*	0·390	37·1	0·660	62·9

On this table Aldershot on a dry day stands best, but not on a wet day. Coventry with its double system is next best. Aylesbury with precipitation alone is almost the same as Coventry with its double purification.

Perhaps this expression "on a dry day" is not quite fair, it is meant to show that in a case of an overflow there may be little or no purification, but there is generally some unless the flood be great.

RESIDUAL AMMONIA C.

—	Ammonia abstracted.		Ammonia remaining.	
	Parts per 100,000.	Per Cent. of Total.	Parts per 100,000.	Per Cent. of Total.
Aldershot *b.*	11·36	100·0	0·00	0·00
Coventry	5·081	99·8	0·009	0·2
Aylesbury	4·269	88·0	0·581	12·0
Aldershot *a.*	3·177	83·0	0·636	17·0
Birmingham *a.*	3·351	61·4	2·109	38·6
Burnley	1·200	29·65	2·503	70·35
Birmingham *b.*	1·530	28·0	3·930	72·0

Aldershot (dry) is again the best; Coventry next; Aylesbury third.

The two chief ammonias in a sanitary point of view are the albuminoid and residual. They are therefore added here.

TABLE D.—TOTAL ORGANIC AMMONIA, *i.e.*, Albuminoid and Residual; both Ammonias may be called "possible."

—	Ammonia abstracted.		Ammonia remaining.	
	Parts per 100,000.	Per Cent. of Total.	Parts per 100,000.	Per Cent. of Total.
Aldershot *b.*	16·555	99·65	0·055	0·35
Coventry	6·720	98·97	0·070	1·03
Aylesbury	4·859	88·20	0·651	11·80
Aldershot *a.*	4·720	84·84	0·843	15·16
Birmingham *a.*	4·191	64·40	2·319	35·60
Burnley	2·095	42·73	2·808	57·27
Birmingham *b.*	1·920	29·60	4·590	70·40

TABLE E.—TOTAL AMMONIA.

Aldershot *b.*	28·31	98·9	0·300	1·1
Coventry	8·78	93·6	0·590	6·4
Aldershot *a.*	7·633	82·7	1·590	17·3
Aylesbury	7·839	82·4	1·671	17·6
Birmingham *a.*	5·616	60·9	3·594	39·1
Burnley	2·215	34·9	4·138	65·1
Birmingham *b.*	1·400	15·2	7·810	84·8

It is seen here that the lowest effluent for total ammonia is from the irrigation farm in dry weather. Coventry next. Aldershot (wet) and Aylesbury stand third nearly the same.

NITRIC ACID.

—	Sewage.	Effluent. Parts in 100,000.
Aldershot *b.*	-	9·23
Aylesbury	None	3·19
Aldershot *a.*	Traces	2·79
Birmingham *b.*	None	2·13
Coventry	"	1·22
Burnley	-	1·20
Birmingham *a.*	None	1·19

Nitric acid shows the effect of oxidation on the organic matter, and here the great action of air and of a porous soil shows itself remarkably.

The best seems to be irrigation in dry weather when the land has full opportunity to act.

The use of precipitation has a decided advantage in wet weather as it raises the purity of the Coventry water above the Aldershot wet specimen. The precipitation method by alum, &c., at Aylesbury, is better in Tables B and C than in A. It is the best of the single processes in wet weather. Aldershot is the best of the single processes in dry. Coventry, the double process, is the best in wet weather. Those which are good in wet weather would probably show still better in dry if the specimens were taken frequently during the year. The precipitation processes, when alum or iron is used, have an advantage in wet weather, since the act of precipitation becomes an act of disinfection.

The effluent specimens were examined as to their tendency to putrefy. Those with lime changed most. Ammonia is in parts per 100,000.

——	Date.	Free Ammonia.	Albumenoid Ammonia.
Burnley, collected 16th June 1879.	June 20, 1879	1·33	0·305
	July 17, „	1·92	0·23
	„ 28, „	1·92	0·30
	Sept. 4, „	2·15	0·13
Birmingham, collected 23rd June 1879.	June 25, „	3·222	0·662
	July 17, „	4·20	0·336
	„ 18, „	4·15	0·336
	„ 28, „	3·90	0·324
Aldershot (wet) Effluent C.	„ 7, „	0·44	0·144
	„ 28, „	0·364	0·130
	Sept. 4, „	0·140	0·092
Aldershot, dry	„ 16, „	0·245	0·055
	Oct. 11, „	0·240	0·058
Aylesbury, collected 2nd July 1879.	July 23, „	0·96	0·067
	„ 26, „	1·00	0·06
	„ 29, „	0·96	0·06
	Sept. 4, „	0·045	0·035

Birmingham (effluent) ammonia rose in 23 days from 3·222 to 4·2; then began to fall; Burnley in 27 days from 1·33 to 1·92; Aylesbury changed none in a week; Aldershot (dry) may be said not to have changed in 25 days.

The capacity to froth when shaken is a very useful mode of finding the comparative sewage matter rapidly. It may be said to have been absent in Aldershot (dry), Coventry and Aylesbury effluents. The clearness of the liquid is very important although a popular indicator. In the Coventry sample there were a few white floating particles; when these fell the water was clear and colourless. Three specimens—Aldershot (dry), Coventry and Aylesbury were without colour, when they had stood in a large colourless glass vessel.

The total solids are in this order in the effluents; the advantage is in being low, of course :—

	Parts per 100,000.
Aldershot average of three (wet)	29·5
Aldershot dry	41·0
Burnley	54·0
Coventry	70·5
Aylesbury	89·5
Birmingham (*b*)	100·5
Birmingham (*a*)	112·5

During floods the volatile solids stand thus :—

Coventry	12·0
Aylesbury	13·5
Aldershot average of three (wet)	13·8
Burnley	14·0
Birmingham (*a*)	24·0
Birmingham (*b*)	26·5

The chlorine has not been found the same in the effluent as in the sewage. This, I suppose, is owing to the effluent being from sewage of a time previous to that of the specimen of sewage.

The question has been asked which of these specimens is most suited for passing into a sewer. None of these effluents can be called sewer-water in the ordinary sense. The Birmingham stream froths readily, and is not pleasant to the eye. The Burnley water was pretty clear, would do well in appearance as a stream by itself, but caused a milky deposit in the river into which it fell, arising, we may suppose, from the free lime taking up some carbonic acid from the river water and precipitating carbonate of lime. The Coventry effluent went into a stream which was very impure, and it might, so far as appearance went, pass into a shallow mountain stream without being noticed. So of Aldershot and Aylesbury.

In every respect we may say that the best result has been obtained by irrigation when the weather is not so wet as to cause overflowing. Still it was not found highly successful after lime at Birmingham.

That the precipitation with alum or alum and iron compounds is next.

That in wet weather there is an advantage in precipitation, because the action is largely, if not wholly, independent of dilution.

That the lime process is valuable but not equal to the above precipitation processes.

I must repeat that in this report I allude only to the merits of the effluent. There is a good deal to be examined before pronouncing on the ultimate value of the processes, and of course a large proportion, if not all of the remaining reasoning, must be left for Mr. Rawlinson, for whose report this may be considered as a preparation.

I am, &c.

Manchester, 16th October 1879. R. Angus Smith.

ANALYSES of SPECIMENS of WATERS from SEWAGE WORKS, expressed in Parts per 100,000.

SAMPLES from Burnley, collected 16th June 1879. (See Report by Dr. R. Angus Smith.)

—	Organic Carbon.	Free Ammonia.	Albumenoid Ammonia.	Residual Ammonia.	Total Ammonia.	Nitric Acid.	Sulphuric Acid.	Chlorine.	Mineral Solids.	Volatile Solids.	Total Solids.	Alkalinity as SO_3.	Hardness	
													Before boiling.	After boiling.
Stream above sewage works -	0·476	0·025	0·024	0·075	0·124	0·50	3·26	1·28	17·0	3·0	20·0	7·50	10·81	7·78
Do. below ditto - -	0·987	0·236	0·070	0·219	0·525	0·56	4·12	1·90	18·5	6·5	25·0	9·00	12·64	8·22
Crude sewage - -	20·33	1·45	1·20	3·703	6·353	—	6·35	5·12	49·0	148·0	197·0	13·00	—	—
Effluent from sewage works -	3·737	1·33	0·305	2·503	4·138	1·20	7·38	5·06	40·0	14·0	54·0	13·00	20·92	17·16

SAMPLES from Birmingham, collected 23rd June 1879.

—	Organic Carbon.	Free Ammonia.	Albumenoid Ammonia.	Residual Ammonia.	Total Ammonia.	Nitric Acid.	Sulphuric Acid.	Chlorine.	Mineral Solids.	Volatile Solids.	Total Solids.	Alkalinity.	Hardness	
													Before boiling.	After boiling.
Crude sewage - -	20·44	2·70	1·05	5·46	9·21	None	25·23	24·16	102·0	75·0	177·0	13·83	45·0	44·0
Effluent from settling tanks -	4·09	3·22	0·66	3·93	7·81	2·13	22·65	15·78	74·0	26·5	100·5	11·32	39·0	38·4
Effluent after irrigation -	2·303	1·275	0·21	2·109	3·594	1·19	18·19	11·46	88·5	24·0	112·5	37·77	66·8	29·22

SAMPLES from Coventry, collected 24th June 1879.

—	Organic Carbon.	Free Ammonia.	Albumenoid Ammonia.	Residual Ammonia.	Total Ammonia.	Nitric Acid.	Sulphuric Acid.	Chlorine.	Mineral Solids.	Volatile Solids.	Total Solids.	Alkalinity.	Hardness	
													Before boiling.	After boiling.
Crude sewage - - -	11·33	2·58	1·700	5·09	9·370	None	11·33	6·85	59·0	31·0	90·0	25·35	32·80	9·85
Effluent from settling tanks (filtered).	1·418	2·40	0·245	0·511	3·156	None	7·21	6·30	55·0	16·0	71·0	22·34	37·15	15·62
Effluent after irrigation (drain out of order).	0·587	2·12	0·205	0·895	3·220	1·22	13·21	5·73	58·0	19·0	77·0	12·62	35·45	17·65

SAMPLE from Coventry, collected 10th July 1879.

—	Organic Carbon.	Free Ammonia.	Albumenoid Ammonia.	Residual Ammonia.	Total Ammonia.	Nitric Acid.	Sulphuric Acid.	Chlorine.	Mineral Solids.	Volatile Solids.	Total Solids.	Alkalinity as SO_3.	Hardness	
													Before boiling.	After boiling.
Effluent after irrigation -	0·618	0·52	0·061	0·009	0·590	1·62	13·39	4·99	58·5	12·0	70·5	21·0	40·70	19·72

SAMPLES from Aldershot, collected 1st July 1879 (wet weather).

—	Organic Carbon.	Free Ammonia.	Albumenoid Ammonia.	Residual Ammonia.	Total Ammonia.	Nitric Acid.	Sulphuric Acid.	Chlorine.	Mineral Solids.	Volatile Solids.	Total Solids.	Alkalinity as SO$_3$.	Hardness	
													Before boiling.	After boiling.
Crude sewage - - -	16.90	3·660	1·750	3·813	9·223	Traces	2·04	4·38	41·0	32·0	73·0	9·40	6·71	3·38
Effluent "A" from sewage farm	—	0·920	0·276	0·803	1·999	2·69	5·14	4·54	24·0	16·0	28·0	2·50	6·57	6·29
Effluent "B" from sewage farm	2·282	0·880	0·200	0·788	1·868	1·59	1·89	3·14	15·0	11·0	26·0	1·90	5·00	4·57
Effluent "C" from sewage farm	2·240	0·440	0·144	6·318	0·902	4·08	2·40	3·20	20·0	14·5	34·5	0·70	7·86	6·86
Collected 13th September 1879 (dry weather).														
River Blackwater - -	0·536	0·0108	0·03	0·0542	0·095	1·56	2·40	2·46	32·50	6·00	38·50	20·40	28·55	10·88
Crude sewage (camps) - -	43·76	12·00	5·25	11·36	28·61	—	4·29	12·08	48·0	93·0	141·0	16·40	11·70	4·43
Effluent from sewage farm -	0·970	0·245	0·055	—	0·286	9·23	4·48	15·53	24·5	16·5	41·0	Neutral	12·28	12·00

SAMPLES from Aylesbury, collected 2nd July 1879.

—	Organic Carbon.	Free Ammonia.	Albumenoid Ammonia.	Residual Ammonia.	Total Ammonia.	Nitric Acid.	Sulphuric Acid.	Chlorine.	Mineral Solids.	Volatile Solids.	Total Solids.	Alkalinity as SO$_3$.	Hardness	
													Before boiling.	After boiling.
Crude sewage - - -	6·76	4·00	0·66	4·85	9·510	None	14·42	7·97	76·0	29·0	105·0	35·50	48·6	20·0
Effluent from 3rd settling tank -	0·633	1·02	0·07	0·581	1·671	3·19	30·21	5·44	76·0	13·5	89·5	11·20	66·40	37·1

No. VII.

MECHANICAL AERATION OF SEWAGE, &c. TABLES OF EXPERIMENTS.

In the experiments noted in the previous chapter we see how long and persistently the sewage matter, both with and without water, gives out ammonia, and we must add at the same time putrid, and therefore offensive, gases and vapours. The substances were preserved in vessels partially exposed to the air; that is, the air filled the vessel excepting a very small space occupied by the sewage matter, and fresh air occasionally allowed to enter. These carboys resembled the sewers themselves and similar confined places.

I now go to entirely different conditions, and give to the sewage matter abundance of air, examining the results. I must, however, add, that in the following experiments the amount of air supplied is greater than that usually given to sewage water in nature. It does, however, show qualitatively the result of aeration, and I look on the effect as a parallel to that which we see in the Clyde and elsewhere. The oxygen does its work, and the gases of putrefaction are to a large extent modified, and in time the action itself ceases.

The aeration experiments were all made by Dr. Storer's and Mr. Cranston's apparatus, lent me for the purpose.

It consists of an earthenware vessel containing 3·3 gallons; into this was put about 2½ gallons of liquid, or about 14 litres, having an archimedean screw in the centre, acting on a vertical shaft, and driving the water down a cylinder which does not reach to the bottom of the earthenware vessel, called the converter. Along with the water air is driven, and the mixed water and air rise up on the outer side of the centre cylinder ready to flow in again, and so continuously. The screw was driven by a gas engine of half-horse power.

OXIDATION, TABLE I.

We see here the condition of the sewage before treatment. It is to be observed that it is not fresh organic matter, but matter which has undergone decomposition as well as oxidation. We see the first by the ammonia in solution, and the second by the nitric acid. The amount of free oxygen shows the source.

It is a curious fact, observed frequently, that one hour's aeration removes oxygen as well as carbonic acid, and there is a general coincidence of rise and fall with free exposure. The cause of this is not clear. It may be that the motion causes the oxygen to combine; there is also a slight rise of temperature, part of which may be due to friction and part to oxidation. After an hour's aeration the effect diminishes considerably, and oxygen tends to assume its original amount. The analyses were made a day after the aeration. There were many done immediately after aeration,—that is, after allowing the bubbles to rise,—but the differences were too great to allow of any useful table, the gases coming probably at varying speeds from the liquid. It was decided, therefore, to wait for a day.

After standing for 16–18 days, we find that the oxygen of the non-aerated has greatly diminished, and the other shows itself in proportion to the amount of aeration. Aeration, therefore, has preserved not only from all sensible signs of putrefaction, but from such chemical action as these gases would indicate.

The nitrogen being 19·97 in one case is very high, and in another 22·7 c.c. in a litre; indeed all the nitrogens are very high, but I suppose the liberation of nitrogen in the experiments already given may account for this. Perhaps time was required to remove the nitrogen, or some may have been evolved during the extraction of the gases, but it was found frequently when there was no reason for believing in a mistake.

The carbonic acid, as we may suppose, is driven out by aeration, but gradually increases on standing.

The free ammonia diminishes uniformly with aeration, but more rapidly in the first hour, as we might expect. After standing there is no difference in the free ammonia of the specimens aerated for one and for more hours.

The albuminoid ammonia is not much changed in any of the aerated specimens, and this we might expect from the absence of putrefaction; some organic substance, however, has evidently decomposed, forming free ammonia. This organic substance is that which produces the residual ammonia—the nitrogenous matter not decomposed in the treatment for albuminoid ammonia. In the non-aerated the albuminoid ammonia has increased decidedly.

The nitric acid has in every instance disappeared, when kept long, and, no doubt, has given out nitrogen in conformity with previous experiments. This is an important point in the purification of rivers, and settles the question, do rivers purify? but not the time.

In this inquiry Wanklyn's process for ammonia and albuminoid ammonia was used, but I divide the nitrogenous matter into three parts, the ammonia free and saline as one, the albuminoid, and the residual. We do not follow the stages of decomposition when we estimate the total nitrogen only.

OXIDATION.—TABLE I.

DISSOLVED GASES, MANCHESTER SEWAGE:—1st GROUP of FOUR examined the DAY after AERATION.
„ „ „ 2nd „ „ 16–18 DAYS „

—	Oxygen, c. c. per Litre.	Nitrogen, c. c. per Litre.	CO_2, c. c. per Litre.	Total Gas, c. c. per Litre.	Oxygen, % of Total.	Nitrogen, % of Total.	CO_2, % of Total.	% O, O+N=100.	% N, O+N=100.	Ammonia and Nitrate. Free NH_3, per Million.	Albd. NH_3, per Million.	HNO_3, per Million.
Manchester Sewage, from Miller's Lane, received 12/1/81:—												
Before treatment .... Examined 13/1/81	4·00	18·24	21·51	43·75	9·22	41·59	49·19	18·28	81·72	48·0	10·8	4·07
Aerated 1 hour, 12/1/81 .... „ „	2·71	19·03	12·86	34·60	7·69	54·09	38·22	14·76	85·24	34·0	—	4·63
„ 2 hours, 13/1/81 .... „ 15/1/81	4·42	18·15	18·12	40·69	10·88	44·57	44·55	19·6	80·4	31·68	10·6	3·71
„ 4 hours „ .... „ „	3·94	18·18	21·37	43·49	9·00	42·20	48·80	17·59	82·41	25·6	10·4	5·19
Manchester Sewage, from Miller's Lane, received 12/1/81:—												
Before treatment .... Examined 31/1/81	1·88	20·80	31·73	54·41	3·45	41·67	55·88	8·27	91·73	32·0	13·4	0·00
Aerated 1 hour, 12/1/81 .... „ „	1·94	19·97	—	21·94	8·84	91·02	—	8·86	91·14	32·0	8·40	0·00
„ 2 hours, 13/1/81 .... „ „	2·76	22·70	19·4	44·86	6·15	50·61	43·24	10·84	89·16	33·0	7·70	0·00
„ 4 hours „ .... „ „	3·48	19·15	30·40	53·03	6·57	36·12	57·31	15·4	84·6	33·0	8·52	0·00

Table II.

Glasgow Sewage.

After half an hour's oxidation the carbonic acid fell as before. The oxygen was very low at first, and rose but only a little. The addition of lime has lessened both the carbonic acid and oxygen. This in part explains the increase of nitrates when lime is used. There seems no increase of nitrates by aeration with the apparatus alone, but there is some formed after 22 days.

The free ammonia diminishes by the use of agitation, but increases by standing.

TABLE II.

Dissolved Gases, Glasgow Sewage partly with Lime:—1st Group of Four examined the Day after Aeration.
„ 2nd „ Two „ 22 Days „

—			Oxygen, c. c. per Litre.	Nitrogen, c. c. per Litre.	CO_2, c. c. per Litre.	Total Gas, c. c. per Litre.	Oxygen, % of Total.	Nitrogen, % of Total.	CO_2, % of Total.	% Oxygen, O + N = 100.	% Nitrogen, O + N = 100.	Ammonia and Nitrate.		
												Free NH_3, per Million.	Albd. NH_3.	HNO_3.
Glasgow Sewage, received 8/2/81:—														
Before treatment	Examined	9/2/81 -	0·44	12·16	30·10	42·70	1·03	28·47	70·50	3·5	96·5	91·9	—	—
Aerated ½ hour alone, 9/2/81	„	10/2/81 -	0·71	13·69	20·10	34·41	2·04	39·64	58·32	4·9	95·1	91·9	—	—
„ „ and ½ hour with lime.	„	„	0·12	17·12	0·02	17·26	0·70	99·19	0·11	0·71	99·19	88·0	—	—
„ „ and 1 hour with lime.	„	„	0·06	18·23	4·40	22·69	0·25	80·36	19·39	0·30	99·7	85·0	—	—
Glasgow Sewage, received 8/2/81:—														
Before treatment	Examined	4/3/81 -	1·39	19·21	97·88	118·48	1·17	16·21	82·62	6·77	93·23	114·0	7·34	2·96
Aerated ½ hour alone, and 1 hour with lime, 9/2/81.	„	„	1·91	12·94	39·47	54·32	3·52	23·82	72·66	12·87	87·13	86·4	13·6	9·78
Glasgow Sewage, received 8/2/81:—														
Aerated without, and then with, lime...	Examined	4/3/81 -	1·38	18·44	0·15	19·97	6·92	92·34	0·74	6·97	93·03	66·0	7·0	7·41

TABLE III.

DISSOLVED GASES, GLASGOW SEWAGE.

	Oxygen, c. c. per Litre.	Nitrogen, c. c. per Litre.	CO_2, c. c. per Litre.	Total Gas, c. c. per Litre.	Oxygen, % of Total.	Nitrogen, % of Total.	CO_2, % of Total.	% Oxygen, O + N = 100.	% Nitrogen, O + N = 100.	Ammonia. Free NH_3 per Million.
Glasgow Sewage, received 5/2/81:—										
Before treatment .... Examined 5/2/81 -	6·41	19·15	34·68	60·24	10·64	31·78	57·58	25·09	74·91	87·29
Aerated 1½ hours, 6/2/81, without air tubes „ 7/2/81 -	4·75	15·86	45·04	65·65	7·23	24·16	68·61	23·04	76·96	87·00
„ „ and ½ hour with 12 gr. CaO, 6/2/81. „ „	2·40	16·51	0·40	19·31	12·4	85·5	2·1	12·66	87·34	82·90
„ „ and 1 hour with 12 gr. CaO, 6/2/81. „ „	2·06	18·18	0·75	20·99	9·81	86·58	3·61	10·2	89·8	80·7
Glasgow Sewage, received 5/2/81:—										
Before treatment .... Examined 3/3/81 -	0·66	14·24	0·04	14·94	4·42	95·31	0·27	4·43	95·57	108·0
Aerated 1½ hours alone, and 1½ hours with lime. „ 4/3/81 -	4·58	15·40	0·04	20·02	22·89	76·93	0·18	22·93	77·07	114·0

Summary of Experiments, Aeration of Sewage.

In all cases putrefaction is delayed by aeration. The oxygen recovers itself in the aerated specimens better than in the non-aerated. Nitrates are formed more readily in the aerated than in the non-aerated specimens.

Ammonia is lost by agitation, but specially by the previous addition of lime. The amount of lime added was ·171 gr. per litre, or 12 grains to the gallon.

TABLE IV.

The experiments were made with water containing 10 cub. cent. of putrid blood to 15 litres.

They show a diminution of oxygen and of carbonic acid by agitation; also an increase of nitrates.

The peculiarities came out more strongly here than with the sewage.

The diminution of oxygen is not observed so much in the aerated as in the non-aerated.

The oxygens rise and fall very irregularly; this may come from changes of temperature.

The nitrates always rise up to a certain point, and then begin to fall.

It appears as if the putrid matter were able to contain nitrates to a certain extent before the action which destroys them both commences. This also depends on temperature.

TABLE IV.

DISSOLVED GASES. PUTRID BLOOD. EFFECT of TIME AND AERATION.

Standing various Lengths of Time.

—	Oxygen, c. c. per Litre.	Nitrogen, c. c. per Litre.	CO_2, c. c. per Litre.	Total Gas, c. c. per Litre.	Oxygen, % of Total.	Nitrogen, % of Total.	CO_2, % of Total.	% O, O + N = 100.	% N, O + N = 100.	Ammonia and Nitrate.		
										Free NH_3, per Million.	Albd. NH_3, per Million.	HNO_3, per Million.
10 c. c. putrid blood + 15 litres Manchester water, mixed 28/12/80:—												
Not treated .... Examined 29/12/80 -	—	—	—	—	26·33	62·07	11·60	29·8	70·2	5·5	6·00	0·00
„ .... „ 3/1/81 -	1·84	17·10	7·34	26·28	7·01	65·05	27·94	9·74	90·26	5·8	5·80	—
„ .... „ 14/1/81 -	4·80	17·5	3·50	25·8	18·6	67·8	13·60	21·53	78·47	—	—	—
„ .... „ 28/2/81 -	2·49	19·04	4·05	25·58	9·66	74·56	15·78	11·47	88·53	10·8	2·85	1·8
Putrid Blood aerated for one hour. Effect of Time.												
10 c. c. putrid blood + 15 litres Manchester water, mixed 28/12/80:—												
Aerated 1 hour, 28/12/80 .... Examined 29/12/80 -	7·56	15·61	3·91	27·08	26·76	57·74	15·50	32·53	67·47	6·3	6·3	0·00
„ „ .... „ 3/1/81 -	3·43	14·46	5·49	23·38	14·65	61·87	23·48	19·16	80·84	7·2	4·8	—
„ „ .... „ 13/1/81 -	1·79	15·85	2·23	19·87	9·01	79·77	11·22	10·15	89·85	5·8	6·86	7·84
„ „ .... „ 28/2/81 -	4·03	13·21	1·88	19·12	22·19	68·12	9·69	24·57	75·43	8·0	3·84	3·50
10 c. c. putrid blood + 15 litres water, mixed 28/12/80:—												
Aerated 2 hours, 28/12/80 .... Examined 29/12/80 -	3·33	16·79	1·70	21·82	15·28	76·94	7·78	16·56	83·44	6·46	6·08	0·00
„ „ .... „ 3/1/81 -	3·15	15·77	2·85	21·77	14·48	72·44	13·08	16·60	83·40	6·40	7·00	7·41
„ „ .... „ 13/1/81 -	2·31	18·50	3·28	24·09	9·59	76·78	13·63	11·11	88·89	7·20	6·75	7·84
„ „ .... „ 28/2/81 -	5·27	12·73	2·12	20·12	26·20	63·25	10·55	29·29	70·71	7·00	4·80	3·15
10 c. c. putrid blood + 15 litres water, mixed 28/12/80:—												
Aerated 3 hours, 30/12/80 .... Examined 31/12/80 -	5·21	35·78	5·06	46·05	11·31	77·70	10·99	12·71	87·29	6·93	6·05	0·15
„ „ .... „ 5/1/81 -	4·06	17·30	4·54	25·90	15·67	66·76	17·57	19·00	81·00	5·80	6·4	16·31
„ „ .... „ 28/2/81 -	6·14	12·48	6·23	24·85	24·71	50·23	25·06	32·97	67·03	11·10	4·48	5·19
10 c. c. putrid blood + 15 litres water, mixed 30/12/80:—												
Aerated 4 hours, 30/12/80 .... Examined 30/12/80 -	5·00	11·66	3·36	20·02	24·96	58·25	16·79	30·0	70·00	6·00	6·00	2·52
„ „ .... „ 5/1/81 -	1·90	15·00	4·15	21·05	9·04	71·23	19·73	11·26	88·74	4·80	4·80	11·12
„ „ .... „ 14/1/81 -	3·03	13·55	4·59	21·17	14·32	64·04	21·64	18·28	81·72	—	—	—
„ „ .... „ 28/2/80 -	4·95	14·94	1·61	21·50	22·98	69·52	7·50	24·84	75·16	4·70	3·76	5·22

TABLE V.

DISSOLVED GASES. WHITE of EGG. EFFECT of TIME AND AERATION.

—	Oxygen, c. c. per Litre.	Nitrogen, c. c. per Litre.	CO_2, c. c. per Litre.	Total Gas, c. c. per Litre.	Oxygen, % of Total.	Nitrogen, % of Total.	CO_2, % of Total.	% Oxygen, O + N = 100.	% Nitrogen, O + N = 100.	Ammonia and Nitrate.		
										Free NH_3, per Million.	Albd. NH_3, per Million.	HNO_3, per Million.
White of one egg in 15 litres water, mixed 31/12/80:—												
Not treated .... Examined 3/1/81 -	4·44	13·56	0·49	18·49	24·04	73·36	2·60	24·68	75·32	0·80	15·12	5·18
„ .... „ 6/1/81 -	2·67	20·33	2·45	25·45	10·48	79·88	9·64	11·69	88·31	—	—	—
„ .... „ 4/3/81 -	2·77	14·00	4·96	21·73	12·78	64·38	22·84	16·69	83·31	8·08	10·40	2·70
White of egg in 15 litres water, mixed 31/12/80:—												
Aerated 1 hour, 31/12/80 .... Examined 3/1/81 -	3·86	10·83	0·61	15·30	25·25	70·73	4·02	26·31	73·69	0·84	17·60	5·92
„ „ .... „ 6/1/81 -	4·40	20·74	4·30	29·44	14·90	70·46	14·64	17·49	82·51	0·90	16·50	20·01
„ „ .... „ 1/3/81 -	4·94	19·00	7·06	31·00	15·92	61·29	22·79	20·62	79·38	5·70	6·30	2·00
White of egg in 15 litres water, mixed 31/12/80:—												
Aerated 4 hours, 31/12/80 .... Examined 3/1/81 -	3·34	20·50	0·24	24·08	13·89	85·11	1·00	14·01	85·99	0·90	12·00	0·00
„ „ .... „ 1/3/81 -	4·97	16·76	1·70	23·43	21·20	71,52	7·28	22·87	77·13	2·12	9·40	2·89

TABLE VI.

Dissolved Gases. Yolk of Egg. Effect of Time and Aeration.

—	Oxygen, c. c. per Litre.	Nitrogen, c. c. per Litre.	CO_2, c. c. per Litre.	Total Gas, c. c. per Litre.	Oxygen, °/₀ of Total.	Nitrogen, °/₀ of Total.	CO_2, °/₀ of Total.	°/₀ Oxygen, O + N = 100.	°/₀ Nitrogen, O + N = 100.	Ammonia and Nitrate.		
										Free NH_3, per Million.	Albd. NH_3, per Million.	HNO_3, per Million.
Yolk of one egg in 15 litres water, mixed 3/1/81:—												
Not Treated .... Examined 4/1/81 -	2·51	20·60	1·90	25·01	10·04	82·36	7·60	10·86	89·14	1·25	12·00	—
„ .... „ 7/1/81 -	0·65	16·18	2·89	19·72	3·28	82·09	14·63	3·85	96·15	0·48	18·00	—
.... „ 3/3/81 -	0·84	9·26	4·20	14·30	5·87	64·75	29·38	8·32	91·68	1·12	11·25	2·30
Yolk of one egg in 15 litres water, mixed 3/1/81:—												
Aerated 1 hour, 3/1/81 .... Examined 4/1/81 -	3·76	18·75	2·64	25·15	14·94	74·58	10·48	16·69	83·31	0·60	12·50	—
„ „ .... „ 7/1/81 -	1·58	15·06	1·79	18·43	8·55	81·74	9·71	9·47	90·53	8·08	13·30	7·86
„ „ .... „ 3/3/81 -	2·12	15·35	4·82	22·29	9·49	68·86	21·65	12·12	87·88	0·96	11·10	3·59

No. VIII.

Connection of this Work with recent Microscopic Inquiries.

It may be interesting to connect this subject with the progress of microscopic inquiry. After writing the opinion on the action of the air, I made many examinations of aerated sewage, and found that the act of aeration prevented putrefaction. It was my intention to examine the matter microscopically; and, indeed, I did so, but not with that fullness which a professed microscopist attains. I am therefore glad to allow this part of the subject to pass into other hands, so far as sewage is concerned, remarking that the chemical change is most decided. An hour's aeration will enable sewage water to keep without putrefaction from two to three weeks in weather ranging from the beginning of July 1880 in Glasgow, where experiments were first made with Storer and Cranston's apparatus, till the end of spring 1881 in Manchester.

My idea was to kill, by the use of air, the germs of disease and of putrefaction, whatever they might be, judging this possible simply from the fact that they seemed to be killed when they passed from sewers into rivers. Still it seemed to me that it would be better if putrefaction were allowed its full action first, so that many organic substances should be decomposed, and some living organisms themselves broken up into gases or into intermediate products, when afterwards the oxygen would purify the solution. This is a plan that may be adopted systematically in some places, as I believe it is adopted practically, but without intention, in many places. The action in the sewer rivers is partly this; and when there is little water, that is, when there is a strong solution of sewage, this is probably the chief action. The oxidation takes place very rapidly after putrefaction. (On this point I have only general observation, not measured results.) As we seldom can allow the water to stand long enough to undergo complete putrefaction, it comes from the sewers into the rivers in a condition which, so far as I know, has not been characterised microscopically with a knowledge up to the latest date. I am therefore speaking somewhat vaguely when I say that we may take it for granted that the minute organisms causing disease have not been acted on sufficiently to be destroyed. It would be interesting to know at what stage they are destroyed. The mixture of the sewage water with river water will produce an act of oxidation to a certain extent, and will, in cases where there is water enough, produce an act of purification so far, but a further flow will increase the oxidation. In the Clyde and the Irwell (as in the Thames before the new sewage system was adopted) the action of putrefaction is decided, but it is limited by the struggle with oxidation. A part of the river becomes a reservoir for putrefaction, although probably not to its utmost, and the lower part a place for oxidation. For this reason it is suitable to have the first part confined, so as to allow the putrefaction full force, and after this to have the water spread as much as possible to allow of fullest oxidation. A fair example of this is given in the action in the Clyde above Dumbarton as putrefactive, then down to Greenock as oxidizing; and if by no means satisfactory, it is so far good and nature's method.

This is entirely regardless of the comfort of the inhabitants, who may object to the part of the river being devoted to purification by putrefaction. That, however, brings in another subject. For irrigation purposes *solely*, rapid transference is best.

To return again to the effect of oxidation, I wish to introduce an extract from two papers; and, first, that already quoted, Chem. News, 1865, p. 304.

“ *On the examination of Water for Organic Matter.*

“ The gases of pure water contain 34 per cent. of oxygen.

“ Dalton found cistern water almost deprived of its oxygen, and I have found every per-centage of oxygen, from 34 downwards. I go further into this point in my chapter on water, which I hope to bring out soon. Meantime, I may say that the examination for oxygen is a very important one.

“ The loss of the oxygen with peaty matter and no vegetation indicates, as already said, the formation of carbonic or a bitter acid. The loss of oxygen with evolution of sulphuretted hydrogen indicates putrefaction. But there are two conditions which externally resemble each other very much,—the growth of vegetable matter with diminished oxygen, and the growth of vegetable matter with excess of oxygen. Water in the first of these conditions may contain, as I imagine, the most dangerous ingredients. Germs of all kinds may exist in such waters,—we do not know to what extent; and as we are very ignorant on the subject, it is well to be alarmed at the conditions until we have examined them and made distinctions.”

I add, perhaps too cautiously, but still believing in the power of oxidation, even when vegetable matter was growing in it:—
“ We do not know much about the second of these two (*i.e.* with excess of oxygen), and if I think it is less dangerous it is perhaps more from a prejudice in favour of the abundance of vital air, and of those hill waters which do not contain bitter peat.”

So far as this purification by air is concerned, it is interesting to see that it is the right track, and to be able to look at the researches of Pasteur and of Dr. Ogston as confirming the views, if indeed there was any need of confirmation of the fact that air purified. Yes, there was need, as there have been questions as to organisms on the hills acting with all their poisoning vigour after being brought down by rivers. It was my belief at one time that the effect of dilution was the purifier, but now that may be supplemented by the effect of oxygen preventing putrefaction as proved here, and by weakening the action of the germs, at least of some diseases, as shown by Pasteur in his researches on the chicken cholera, and by Dr. Ogston on the micro-organisms in pus.

The words of Pasteur may be given here, as introduced into the “ Chemical News,” 22nd April 1881:—

“ *On the Attenuation of the Virus of Chicken Cholera, by L. Pasteur.*

“ Now that we have arrived at this point, a question presents itself which relates to the cause of the attenuation of virulence.

“ The cultivations of our virus must take place in contact with air, because our virus is aerobian, and, without air, its development becomes impossible. We are then naturally led to ask whether the attenuation of the virus is not due to contact with the oxygen of air? Would it not be possible that the small organism which constitutes the virus, when left in contact with the oxygen of pure air, in the medium of cultivation in which it has developed, may have been modified, and the change remains permanent, even after the organism has been withdrawn from the modifying influence? We may also inquire whether some chemical principle in the atmosphere, other than oxygen, does not intervene in this phenomenon, the singularity of which almost justifies my hypothesis.

“ It is easy to understand that the solution of this problem, in case it depends on our first hypothesis, that the phenomenon is due to the oxygen in the atmosphere, may be tried by experiment. If oxygen is in reality the cause of the attenuation of virulence, we may have, to a certain degree, a proof of it by noting the effect of suppressing it.

“ To test this, let us conduct our cultivations in the following manner:—We may take a certain quantity of our chicken broth and place in it the most virulent virus, and fill with it a series of glass tubes up to two-thirds, three-quarters, &c. of their volumes. These tubes may then be closed over the lamp. By the presence of the small quantity of air left above the liquid the development of the virus may be started, which is ascertained by the increasing turbidity of the liquid. The development of the cultivation gradually absorbs all the oxygen contained in the tube. The turbidity then diminishes, the growth is deposited on the sides of the tube, and the liquid becomes limpid. This takes places generally in two or three days. The microscopic organism is then deprived of oxygen, and will remain in this condition as long as the tube is not opened. What will become of its virulence? To be sure of our results we will have prepared a great number of such tubes and an equal number of flasks, which last will continue to be left in contact with pure air. We have already spoken of what becomes of cultivations carried on in presence of air. We know that they experience a progressive attenuation of their virulence, and we will not return to this subject. Let us now only pay attention to the cultivations in closed tubes. Let us open them —one after an interval of a month, another after three months, and so on until we open one that has stood ten months. I have not gone any further at the present time. It is a remarkable circumstance that the virulence in all these cases is of the same degree as that of the liquid which served to fill up the tubes. As to the cultivation exposed to the air, they are found either dead or in a condition of feebler virulence.

"The question we have proposed is then solved: it is the oxygen of the air which attenuates and extinguishes the virulence.

"To all appearances we have here what is more than an isolated fact. We must have reached to a general principle. We may suppose that an action which is inherent to atmospheric oxygen, an agent present everywhere, has the same influence on other viruses. At any rate it is worthy of interest that possibly a general cause of attenuation exists dependent on an agent which is in a manner cosmical. Can we not suppose even now that it is to this cause that we can attribute in the present, as in the past, the limits set to great epidemics.

"The facts which I have had the honour to communicate to the Academy suggest many proximate and remote inductions. From all these I must hold back with reserve. I will not feel authorised to present them to the public unless I make them pass into the domain of demonstrated truths."

Also from Dr. Alex. Ogston:—

From a "Report upon Micro-organisms in Surgical Diseases," by Alexr. Ogston, M.D.—Brit. Med. Jour., *12th March* 1881.

"The results, so far as has been gone, may be summed up as follows:—

"Cold abscesses contain no micro-organisms, and their pus is harmless.

"Acute and pyæmic abscesses always contain micrococci.

"Pus whose micrococci have been killed by carbolic acid or high temperature is harmless.

"Pus containing micrococci is resisted by animals if the dose be minute, or if it be injected into the peritoneal cavity.

"Doses of one or two minims injected into the sub-cutaneous tissue may cause death by blood-poisoning, or may cause sphacelus of the site of the injection, or may be resisted by an unusually insusceptible animal.

"As a general rule such doses produce acute inflammation, accompanied by blood-poisoning and ending in abscess.

"The third part, upon micrococci in wounds and suppurations, is summed up as follows:—

"Suppurating wounds contain micrococci, whose numbers and activity are proportionate to the intensity of the suppuration.

"Listerian dressings prevent micro-organisms from gaining access to wounds.

"Micrococci in wounds withstand most antiseptic applications.

"Where no micrococci are present in wounds, no pus is produced; the discharge is serous.

"Micrococci exist wherever pus occurs, save in chronic suppurations, such as cold abscess, chronic acne vulgaris (?), &c. Micrococci in man produce the same varying effects as in animals; they may produce blood-poisoning without suppuration, they may cause suppuration, or they may be resisted by strong individuals under favouring circumstances. Lastly, there are possibly micrococci that do not produce suppuration."

An account of numerous cultivation experiments is given, and the certain opinion is expressed that the cultivation must go on in absence of air, *i.e.* air is hurtful to the cultivation of the micrococci. The most successful method employed was to inject the seed fluid to the end of an egg opposite to a very small puncture made to admit a hollow needle. Antiseptic precautions were taken to exclude the action of germs in the air.

"To sum up, micrococci do not produce putrefaction. They develope best when removed from the atmosphere. The preceding facts prove that they are able, under suitable circumstances, to give rise to blood-poisoning, to acute inflammation, and to suppuration."

The experiments described in this report show that aeration delays putrefaction. If it does not kill the germs or organisms which produce putrefaction, it weakens them, as in the case of chicken cholera. It is possible that they may die in the oxygen, and the liquid may be again supplied with them.

A curious question arises then: What is the value of a process of oxidation which does not kill the germs of putrefaction; because, if they are not killed, what proof have we that the germs of diseases are not killed? We have no proof; but we are told by Pasteur that in certain cases at least the germs of disease are made innocent by the action of air; and I find as a distinct fact that the germs of putrefaction are also so weakened that they produce no effect until after considerable time. If the germs of all diseases are affected as the germs of chicken cholera and putrefaction are, then we must look to the aeration as a cure for a certain time. This time being long and measured by weeks, the water or sewage treated can in almost all cases be far removed, and sent to places where its impurities will be without effect.

The proposal quoted from my previous papers to examine the growth of the organisms in water was not carried out by me, but I am glad to have been in the right track so long ago, and to have been so also with the inquiries as to oxidation. The results obtained by Koch and also by Klebs must receive the attention of chemists. It does not appear that even if we kill all the germs in any process that much advantage is gained if they are so readily producible from neighbouring sources. The discussion whether those which are most common in smaller numbers, and are in constant supply, can by some peculiar change of condition not visible to us become so abundant as by their very bulk to be deadly, or by their character to be active, virulent, and equally destructive to higher animal life, is one which must greatly interest chemists as well as biologists. As chemists we can only exhibit the inorganic phenomena so far; we have no test for vitality. The advice I gave so long ago has not been sufficiently taken by myself, but Koch has made it easier to take, and we must not forget the results. At the same time, as chemists we must not forget that chemical tests are not exhausted, and the two inquiries must go on until they blend into one, as they must ultimately do.

These researches do not prove to us that the germs spoken of are destroyed by oxygen; they become weak, and this weakness increases to absolute ineffectiveness, and so far we are guarded; and they may be supposed also from their weakness to increase less rapidly, or to cease to play a prominent part, but ready to begin again. Analogy, however, would lead us to believe in complete annihilation of the first series in the case of those that produce putrefaction, so that they may make room for their successors. The relation of the succeeding to the first is not known to me at least, and I judge simply from analogy that the first organisms will be destroyed with the mass of the organic matter which is destroyed. The destruction is so great in the case of putrefaction that we see the bubbles rising before us rapidly. The gases given out were found to be carbonic acid, hydrogen, carbonic oxide, carburetted hydrogen, nitrogen, sulphuretted hydrogen.

Still there is a limit to the proportion of organic matter destroyed by putrefaction, and I suppose oxygen to be the body which comes and concludes the process. There is, however, an oxidation going on slowly in sewage before putrefaction, but it is not a very rapid one, and what time is required to destroy the several classes of organisms has not been made out. I see no reason as yet for Pasteur's hypothetical influence other than oxygen.

When Dr. Storer came to me with a proposal to use his apparatus for aerating sewage, we had not the investigations of Pasteur to go upon, but I was very glad to have the opportunity of using the means put into my hand, Dr. Storer's apparatus, as the time for considering the subject was come.

To the opinions and facts already given we must add the remarkable observations recorded in the Royal Agricultural Society's Journal, No. XXXIII, Part 1, 1881, by Dr. W. S. Greenwood.

It seems clearly proved there that sewage that has undergone no putrefaction or sufficient oxidation is a very destructive agent when it contains the débris of diseased action in the human body, in some if not all cases. It seems to follow as a natural conclusion that as disease has not been observed to any very decided extent as following the use of sewage irrigation, that the putrefaction and oxidation are in ordinary cases sufficient. But there are some differences of opinion, and it is pro-

bable that these processes of destruction and purification, in cases where the sewage has been injurious, have been interrupted too rapidly by the rapid transfer of the sewage to the soil. On this point we must refer to the remarkable inquiry by Dr. Greenwood. At a mill near Bingley, woolsorters' disease and malignant pustules had broken out amongst those who worked with mohair coming from Van. To disinfect the wool it was exposed to the air on a field. In a few days a cow died in a field which received the village sewage, and next month cattle and sheep were attacked by anthrax. In a series of experiments carefully made it was abundantly proved that the disease had come from the wool,—which, however, seemed to have been roughly torn, let us hope, from dead animals, as some of the skin and flesh was at times found attached.

In relation to this report on impure wool, I may mention a circumstance elsewhere mentioned, as it occurred many years ago. I was rather disgusted on entering into a room in a large paper-works where rags were sorted. These rags came from all the miserable wretches that lived or died in unwholesome dens, prisons, workhouses, and hospitals on the continent, and seemed to call up every variety of human bodily misery lingering in our civilisation. What diseases might not be there, and must have been there. In this room dust was flying about, and a number of young people, about 20 years of age, were working diligently there. I certainly never had seen more healthy looking specimens of young women. They were much beyond the average in bulk, their cheeks were ruddy, and their complexions remarkably good, different decidedly from other women in the neighbourhood. I asked if none of them were ever poisoned: no such thing was said to be known. I asked the owner to weigh them, but he never did so. I have often considered this case, and every time I came to the conclusion that the original substances had undergone a transformation, and the germs of disease had been destroyed by an act, if not of putrefaction, of at least some analogous chemico-organic change. This first showed me the value of putrefaction as a purifying agent.

I may add here that I had attempted by the use of fermentation to estimate the amount of vitalized matter in the air, and I give one attempt here from the 10th Annual Report, pp. 42 and 43.

“ Some Hidden Qualities of Town Air.

“ Some years ago I made inquiries into the effect of various substances in preventing putrefaction; the effect was measured in one set of trials by the amount of sulphuretted hydrogen produced, and in another set by the amount of gases of decomposition evolved. Lately I made another series, with the view of finding if there were any bodies in the atmosphere of large towns which prevented decomposition, the opinion being that the sulphurous gases must have this effect. I did not find that putrefaction was a satisfactory method of trial, and I trusted to fermentation of sugar as one more under our command. The air of the town was used first, that is, a certain quantity of air was washed with pure water, and a fixed quantity of sugar and yeast added. At the same time there was a control experiment made, lest the yeast should be peculiar. This was done simply by using the same amount of yeast and sugar in pure water, without adding any air washing. There was no mode, however, employed of preventing ordinary contact of the air.

"The results of these experiments are very irregular, as one might suppose when dealing with such substances as yeast. Sometimes the amount of carbonic acid obtained was almost nothing, and at other times, apparently with the same quality of substance, there would be disengaged a large amount. It was needful, therefore, to make numerous experiments, satisfied that absolute exactitude could not otherwise be gained, if it even then could. The results, however, are not without interest if we take the averages, because in them we see a uniformity which it is impossible to see in the list of apparently struggling individual experiments.

"These trials were made at various times, occupying a portion of every month for two years. It seemed to me that nothing would come of them, and that much time was lost. However, I did venture to look the whole in the face lately, and summed them up, bringing out the averages. It is apparently true that the air of a town influences fermentation in sugar to a certain extent."

The researches of Dr. Koch, however, have obtained results far superior, and by using a part of his process I am obtaining very promising results, to be spoken of later.

Having now given some of the principal points connected with the idea of aerating sewage, I may make some further investigation into the result. What advantage would it be to any city to send down its sewage into a river in a condition in which it did not putrefy, if it were to have an appearance of impurity as great as ever? I think this would be an advantage, but not a sufficient one. We demand more. Indeed, the appearance is quite as much a matter to be considered as any other point. Perhaps we may say that it is the chief point. We have not proved any disease to occur from the sewage below Glasgow or Manchester, although I see that some one has observed a peculiarly poisonous centiped in the mud opposite Liverpool, and fears, probably too well founded, are rising on the Clyde where shores are left bare. The appearance of a river is, to say the least, a prominent point; next, the fish question is an important one; and although sewage is an excellent feeder of fish, these do not enter the places where sewage is very strong, and, I believe, never at all where there is putrefaction going on. We do not effect enough by removing the tendency to putrefy. We may then ask, what more is to be done?

A very common plan is to allow the sewage to pass into tanks and deposit. This generally brings on purification by putrefaction, and of course would receive favour if it were thorough enough,—which it seldom, if ever, is. In most cases there is not ground enough for it; and although some towns take room enough by converting the rivers into putrefying tanks, and so purifying the water, the result has the objection already mentioned. This certainly has the advantage of being well tried. I might have mentioned. as an argument in favour of putrefaction and the destruction of diseases in rivers, that the Thames water not along ago was considered the best of all water on ships, after it had stood and putrefied in the barrels on board. I am therefore on safe ground. Still it is desired that the water be cleared, and it is cleared at present very slowly by putrefaction and subsequent subsidence or filtration in its own bed.

For sewage filtration unaided has been given up, but filtration with lime is a possible process.

The use of lime has now been long tried, and it has been shown to pcssess many good qualities. It clears the sewage with great rapidity to a certain extent. But I need not describe its action, as this has been done so well before, and especially by Dr. Wallace, of Glasgow, who has made a special study of the subject. It may be even asked, what advantage can we obtain by any aeration if lime is used?

It is agreed that the effluent from lime is still liable to putrefaction, although this action is postponed for a time and diminished also. Dr. Wallace finds that if the sewage water of Glasgow is allowed to mix with twelve times its bulk of river water, the result will be entire freedom from further smell or putrefaction. It has, among other results, been oxidized. By aeration, then, we seem to do that something for a time which twelve times its bulk of water does to the sewage permanently. The absence of putrefaction after aeration, even with the solid remaining in the water, is a very great result; still I am unable to decide on its ultimate value from present results.

Lime, then, does not disinfect in such a way as to prevent putrefaction as long as aeration does, but it clears the liquid much more thoroughly, and if the effluent is removed the putrefaction resulting cannot be equal to that which would take place in the aerated sewage after a certain time.

This seems to lead to another point, namely, is it not best, then, to use both processes—the lime and the aeration? The lime to clear, and the aeration to delay putrefaction, until the water flowed out of the reach of danger. It is probable that in this case none would take place whatever in any of the circumstances usually found at towns.

The results here are given of a great many experiments, but they are all laboratory experiments, we must remember. I have, however, seen enough of lime precipitation to make me believe that something more would not be a disadvantage.

This account does not contradict anything already said of the great value of the use of alum and iron salts, and it is thought well to append the account previously given, 1879. The matter of expense must be settled by others.

No. IX.

Extraction of Ammonia.

I have not at all considered independently the expense of aerating in this manner a million gallons, but I am assured that the apparatus is not at all likely to be too expensive. It has been considered that one hour's agitation will be enough. This has been variously calculated for expense. I shall not give my calculation. The amount arrived at is much higher than that obtained by Messrs. Storer, but I must yield to their greater experience.

When sewage is aerated, and left with the solid matter in it, there is, of course, abundant room for a new succession of putrefactive material, and it is necessary to remove that rapidly by precipitation or by filtration. These methods may both be useful; namely, precipitation, so as to allow the deposit to leave a considerable amount of clean water above. After this filtration may be used for collecting the deposit, if it is true that the improvements in filters is as great as I hear of. I have read and been told of several inventions in this direction which seem to me of great value, but I have not entered practically on a study of them. It was found, however, that ammonia came off more readily when the sewage was not filtered.

This subject has baffled all engineers. Sewage has caused expense, and it has failed to produce profit in cases which cannot be called exceptional. It is not proposed to describe the causes, but one certainly is pre-eminent,—the great bulk of water now used for supplying towns. And this cause allies itself to another, namely, the great mass of water as rain, which in some parts of the country weakens the sewage in its flow or in the fields already drenched. The amount of ammonia, as we have long known, is great in sewage, but we have not known how to remove it. It has truly almost as little weight in proportion to the sewage itself as a man has to a castle.

In working with the apparatus described, a constant loss of ammonia was observed, and sometimes this was found by the smell itself. The sewage was tossed about; the volatile part was carried up by the currents of air, and it had no opportunity of returning. Here it was thought was at last a method of obtaining a revenue from sewage ammonia. If we take a grain of ammonia only out of a gallon of sewage, we have from a million gallons a million grains, equal to 142·81 lbs.—let us say 140 lbs.; let us for a large city like Glasgow multiply this by 50, and we have 7,000 lbs. of ammonia daily = 1,140 tons per annum, which at 60*l.* a ton, its present market price, is 68,400*l.* per annum.

Can we obtain a grain out of every gallon? In the laboratory with a small apparatus this has not always been done unless lime has been used in sewage containing six grains of ammonia. In an experiment made in Glasgow with one hour's aeration without lime, two grains per gallon were obtained from sewage containing about nine grains of free ammonia, I am told. This appeared as a very remarkable and cheering fact, opening up a new field of action for sewage operations.

The question now came to be, How is this to be proved? We know that the world will not spend its money unless it can receive the hope of a good reward.

There is, however, a second question :—When the ammonia is removed from the liquid, how is it to be retained; that is, removed from the air which carries it up, and held in a concentrated form? The present ideas on that point have not been subjected to proof, but it is contemplated using either an acid or a solution of a salt, such as chloride of calcium; from which the ammonia, which has always carbonic acid along with it, would throw down carbonate of lime in fine division, and leave chloride of ammonium in solution.

It was found, however, that the idea had come into the mind of another, ten years before, and had been thrown away. A provisional specification of a patent had been made out in 1870: was it right to throw it aside, and must I do the same? The first projector seems to have acted from theory, and his plan was so imperfect that success, we can easily see, was impossible. A liquid must be very rich in ammonia before such a process would allow any important quantity to be taken from it, as there is simply a bubbling of air through the liquid. Another patent was taken out by Messrs. Hills and Biggs in 1872, one by Messrs. Welch and Scott in 1876, and one by W. L. Wise in 1878. I have tried only Dr. Storer's, and it is not for me here to give opinions of the comparative merits.

Ammonia removed by Exhaustion.

In aerating water by the method just spoken of, there is a removal of the gases contained in the water, and a substitution of air. This displacement is not effected without using a large amount of air, as one part of fresh air removes a small part only of the gases contained in the water. We cannot expect to do the same work with a small amount of air, unless we first remove the gases contained in the water. This may be done to a large extent by pumping them out; and it is remarkable how much is done, at least on a small scale, in a minute or two by this method. I cannot say that a result has been obtained equal to that by a current of air; and we must remember that the current acts oxidizingly as well as by displacement; but a great deal is effected by the exhaustion methods. The gases come off in a condition strongly impregnated with organic matter, and very disagreeable to the senses. They may in this case be sent through a disinfecting process; say, by passing a fire, if necessary, and if the ammonia cannot be got out. It was expected that by taking them out in a concentrated state they could be passed through acid, and the ammonia removed in a small absorbing space, thereby getting over the difficulties which may be expected when we try to absorb the ammonia from a large amount of air. This, however, was not found in practice to take place to a sufficient extent; very little ammonia was removed by pumping. At the same time it is very probable that the failure was not inherent to the process, but caused by the limited and imperfect scale of laboratory operations. In some cases it is probable that ammonia would come off in this way; that is, when the sewage was stronger than usual.

If this method of pumping out the gases were adopted it would only be as a preliminary to aeration, which might be effected either by allowing the air to enter, or by assisting it.

How far this plan will be found useful in extracting ammonia from various decomposing bodies, other than sewage and solutions, is still to be found out, but it certainly seems to recommend itself in many cases where putrefaction causes offence, because the putrid gases are drawn out at once and may be treated by fire or otherwise rapidly. It is a new mode of disinfection.

Some enquiry is necessary to enable us to find if the plan of previous pumping out of the gases, and then aerating, is to be recommended in any case in preference to the mode of passing air through the sewage at first. My belief is that such cases may exist when ammonia is to be extracted, but they are more likely to be found where purification and oxidation are required, without looking to the ammonia. This pumping simply draws out the gases which are not very soluble in water, and makes room for others to enter. Those which enter are expected to be oxidizing agents; and first we must look to the action of common air. The sewage could be rapidly deprived of its gases, and then simply allowed to flow away. When it had flowed even a very short time the air would enter, and during this period more would come out. Still it cannot be denied that if the sewage smells badly, the removal of all the air that can be pumped out does not quite remove the smell. It does, however, diminish the quantity of gases and of vapour capable of causing a smell. If, however, the oxygen were caused to enter in a condition more concentrated than it is in the atmosphere, the oxidation would go on more rapidly. It has been a matter of thought to supply the concentrated oxygen to the sewage. We know what wonderful effects are caused by permangates and by peroxide of hydrogen; and I have shown also what high oxides, such as nitrates, effect. A careful study is not required to show that pure oxygen gas, at the present price of materials, is too expensive if we are to saturate the solution, or to give it two per cent. only. At the same time I have not ascertained how much would be necessary. Unless the action were very rapid, the amount would be rapidly diminished by diffusion, and the most active agents for supplying solid oxygen are too expensive.

Viewing the matter in this way, I have called to mind my experiments made long ago in the absorption of gases by charcoal. It was at one time apparently certain to me that oxygen could be filtered out of the air by charcoal; and, indeed, I have found large proportions of oxygen taken out by charcoal. Indeed, we may say that the amount removed from the air is, as a rule, 30 to 40 per cent. I have not, however, been always successful in obtaining it back by pumping it out of the charcoal; occasionally it has come out very well, but it has sadly disappointed me. The plans, however, are not all exhausted. The amount of gas regained from the charcoal usually has less oxygen than the air.

Finding that charcoal was not manageable I have had recourse to water. Pure water absorbs air, not exactly as such, but in the proportion of one-third and a little over of oxygen, the rest being nitrogen—leaving out traces. In other words, we can pump out of water a mixture containing about 13 per cent. more oxygen than common air contains. Such a great increase of per-centage of oxygen has a remarkable influence in most cases, and I suppose will have in oxidizing the substances in sewage. There is a disadvantage, namely, in the small total volume of air that can be obtained from water. In some cases this is of little consequence, because the water is abundant, and one would suppose a stream to be an endless source. In other places, it may be said, where should we obtain water? Water may be used as a filter; when it has been exhausted of air it begins again to drink it in; and the same may be repeated for ever. The question, however, remains, How rapidly can this be done? For example, what surface and depth of water are necessary to supply continuously a cubic foot of air having 13 per cent. of oxygen added to its present amount.

If this mode of obtaining oxygen were manageable, the water would not require to be pumped high; but it could be put into a closed vessel, first pumped for a short time and then allowed to pass off for more air. This water would require to be kept pure.

These are ideas regarding the supply of oxygen:—Methods of hastening the action of the air. Another method consists of using pressure to save time. The water, after being exposed to a vacuum, might be exposed to pressure with common air, or with the richer air from water, and pressure might be applied so as to finish the process very rapidly.

These methods of increasing the purifying action going on in water, are based on the first facts; namely, that a large and important effect is produced in one hour by passing air alone; next, that a more rapid effect is produced by pumping out the gases before passing air. The next effects, namely, of stronger mixtures of oxygen, may be considered certain, although not tried. The mode of obtaining oxygen is certain also, as shown by Mallet. The analysis of the air at successive stages is very interesting (*see* 2nd Supplement, Watts's Dict., "Oxygen"), but the practicability of the process on a large scale has not been put to the test to my knowledge.

I send out these writings, being chiefly the description of work done, but partly of course looking forward to work that may be better done after the information here has been considered.

No. X.

Aeration of Water Supplies.—Drinking Water.

This is not quite a new subject to speak of, but it has not been well worked out. These latest ideas as to the value of air compel us to turn to it again. We have now many towns supplied with surface water without filtration. The result of this must be that there is some deposit in the reservoirs if the solid matter is not carried away. Perhaps I ought first to have said that there is solid matter in all cases nearly, although there may not be much. Indeed it is not easy for us to suppose that the washings of fields can be free from floating bodies. When the water sinks deep into the ground we have magnificent filtration and clear brooks. The experience described by Dr. Greenwood must not be forgotten, and we do not know the limits of the impurities from manured land. As a rule, however, it seems to be true that the organisms washed from fields, and coming into our streams, rivers, and reservoirs, are not hurtful. And why not? When we have the great dilution and the great aeration together we may draw the conclusion that the researches of Pasteur empower us, even when no opportunity is given for that destruction by putrefaction which has received favour in this Report. Still we have this fact before us, that in great floods there is little time for change, and the increase of material washed off the soil is equal to a diminished opportunity of oxidation. In such cases it does seem right that water should have some time to undergo its changes, or that it should be filtered, or both. The reason for filtration is twofold: we remove solids by it; and in removing visible although minute floating matter, we remove also much which is insensibly small. But this is not all: the act of filtration in a good filter is really an act of oxidation,—in sand, for example, and other porous bodies. Of this I have spoken elsewhere. When this is not sufficiently done, would it not be well to use mechanical aeration for drinking water? It is certain that many of our water supplies do not contain as much oxygen as the best water. Manchester, for example, has a fair supply of water, but it varies considerably, and the oxygen passes down to 27 in the 100 of air dissolved in it. How far this could be remedied by the use of more oxygen, it is not possible to say without trial, and it is a trial which ought to be made. This is rather an addition to the duties of waterworks proprietors, but it is one which comes with the changes of our habits. Our reservoirs are filled by streams in flood, and we do not wait until these rough waters pass away leaving the calmer streams to be taken, such as are filled by the water filtering through the soil, and thereby purified, as they often are, to brilliancy.

In speaking of this it is natural to consider the use of lime now proposed in so many cases of purification. In Clark's process, for example, lime is used, and carbonic acid is removed. It has been asked if it would not be an improvement to add carbonic acid. I believe it would, and probably air also. In the first proposals for purifying water by Thomas Henry, F.R.S., Manchester, about a century ago, lime was used, and after precipitation carbonic acid was employed to neutralize any excess. Any excess of carbonic acid over the lime would be an improvement. Free lime in water is a great disadvantage. Indeed we must look carefully to our streams whenever lime is used for precipitation. It is a favourite poison for fish, and I have seen a clear solution from a precipitating tank become suddenly whitish on entering a river of clear water. The process was intended for purifying the river, but it was in reality adding a new poison. It was in small amount, certainly; but who knows the amount used at times? There must be a careful measure of lime used in all cases where a river may be affected.

This use of carbonic acid might take place along with air, and if the water were to be used rapidly, it would add sapidity, which is not obtained at once by natural aeration, and is best obtained by deep filtration or by carbonic acid.

We may now consider generally the effect to which the newest information regarding germs of minute life seems to point. Water, when standing long with germs of life, has, or may have, according to the weather, increase of growth. Abundance of air will be preventive of or destructive to many of the lower forms, certainly those that cause putrefaction. Stagnation allows the oxygen to be consumed. Filtration and aeration ought to take place just before the water is to be used, according to these views.

Note on Depositing or Filtering.

The use of filtration is great, and nature provides us by its means the best of water, which comes through soils holding many unlovely things, and still without carrying them forward. It is an important matter to imitate this, and we try with some success, but we cannot imitate the self-purification in nature as fully as we wish; we cannot remove the carbon so rapidly and make it from a constituent of a filthy compound to one of a pure gas. The consequence is that our filters become covered with much impurity, and through this all our water supplies go, that is, there is an accumulation which is not an imitation of nature as we find it on the soil, but a simple product of art. I do not ignore the fact that various modes have been devised of removing this, and I have much good feeling towards the oxidizing filters used, especially those with compounds of iron or iron itself, and I will not say that in some cases they are not by far the best to be chosen; but it has seemed to me that it would be a fine thing if we could avoid completely the passage of water through an impure deposit, and if we could remove that as rapidly as it is formed.

Note on Mechanical Filters.

There are several such filters, and a remarkable one is devised by Mr. Bowing, in which the liquid to be filtered is pressed against canvas or cloth, which lies on a perfectly plain surface of metal. The water works its way between the metal and the cloth. The fact is curious, and the result is remarkable. How far this would do for finer water I do not know; it would probably be insufficient; but when applied to less pure liquids the result has been shown to be excellent. The advantage of the mechanical filter is that the deposit is removed rapidly, and is not allowed to remain and putrefy; and if it could be so applied for drinking water as to produce an unobjectionable result, we should rejoice.

I doubt if Mr. Bowing's filter, as made by Manlove and Alliott of Nottingham, has been tried otherwise than for sewage; but even then it seems as if it were too much to expect a good result on account of the enormous bulk of sewage to be passed. If, however, a deposit were allowed first, and mechanical filtration applied to it afterwards, we might obtain something of more value. I have nothing to say of new modes of filtration, but am looking to experiments on a large scale. I have not given sufficient attention to the spongy iron filter of Mr. Bischof's, but having got remarkable results from iron filings in 1848, I am much in favour of iron, and must give it more attention.

Precipitation in Drinking Water.

The very great attention given to precipitation of sewage has led us from its application to drinking water, and it is remarkable that we have so much neglected the possibility of improvement in this direction. We know the old methods of adding alum to muddy water and finding it rapidly cleared,—a method used in India and China, and probably elsewhere. We send many tons of alum to Asia for this purpose, as it is supposed. But then these Easterns are so careless that the water which they call not good for drinking is to us horrible, and that which they consider just drinkable is bad to our sight and smell. The wonderful clearness obtained by throwing down flocculent precipitates has been often remarked. It has occurred, however, to Mr. Peter Spence, of Manchester, to use an aluminous salt not merely for sewage and very impure waters, but also for town supplies, and the results obtained in Manchester water are remarkable. The substance he uses is called by him alumino-ferric cake, but the name can be readily altered. The salt is a sulphate of alumina, with only 0·7 per cent. of oxide of iron in it. It is made from bauxite, a mineral containing chiefly soluble alumina.

The Manchester water is considered a very fair one; it is a little yellow, being coloured by peaty matter. But when this sulphate of alumina is added, and it is allowed to settle for two days, it obtains a brilliancy equal to all we can desire. I made some experiments, and obtained the following results. The estimation of the purity of tint made by my new method is as follows. (*See* for explanation, No. XI., p. .)

The highest numbers show the greatest transparency; but it must be remembered that it is only proportionate, that is, the same water does not give the same numbers on every trial, because the amount of light differs.

People may be afraid that alum will remain in solution, but there is rarely a water to be found so free of lime as not to decompose the small amount of sulphate of alumina used in these experiments. This point must of course be remembered, and the water kept alkaline.

TRANSPARENCY of the specimens, WEAK LIGHT, but STRONG SOLUTION to COMPENSATE, 50 c. c. contained 0·3 grm. SO_3 + 0·5 grm. KI.

——	Pure distilled Water.	Manchester Water purified by Alumino-Ferric Cake.	Manchester Water purified by Ferric Chloride.	Manchester Water after filtration through Blotting Paper.	Manchester Water.
1881.	c. c. $Na_2S_2O_3$ sol.	c. c. $Na_2S_2O_3$ sol.	c. c. $Na_2S_2O_3$ sol.	c. c. $Na_2S_2O_3$ sol.	c. c. $Na_2S_2O_3$ sol.
November 9th - - -	25·0	25·0	21·0	17·4	13·6
„ 10th - - -	16·0	16·7	12·1	7·9	7·0
„ 14th - - -	4·2	3·9	2·9	—	1·1
„ 15th - - -	26·9 26·9	25·0	23·0	—	19·1 19·0
„ 16th - - -	30·5	29·0	26·0	—	22·0
„ 17th - - -	50·0	48·5	46·0	—	41·0
„ 18th - - -	23·5	21·0	19·5	—	13·0

Could this result be obtained in great reservoirs? I can only say that it is so remarkable that it ought to be tried. It is certainly far beyond any method of filtering used at any waterworks seen by me; it can only be compared to deep soil filtration. I must mention that I have not seen either the recent iron filters or the carbide of iron ones at use on a large scale, and cannot allude to them.

The alumina which falls takes down organic colouring matter, but the alumina may be dissolved out of the precipitate. How far it can be dissolved again is a point into which I have not entered. Mr. Spence says that for sewage this may be done repeatedly; in treating drinking water it may not act so well a second time, but the expense is certainly small. The amount I used for Manchester water was ·7 grains per gallon, or about a ton for 20,000,000 gallons, which ton would cost 50*s.*

The results of analysis are as follows, but it is enough to judge by the effect in brilliancy already mentioned:—

EFFECT of PRECIPITANTS on MANCHESTER WATER.
Analyses in Parts per 100,000.

——	Total Solid Matter.	Free Ammonia.	Albumenoid Ammonia.	Alkalinity as SO_3.
Manchester water - - - -	6·75	0·0022	0·0076	1·37
Do. with 1 part alumino-ferric cake per 100,000.	7·00	0·0028	0·0056	1·03
Do. with 1 part ferric chloride	7·16	0·003	0·0060	1·00

If this turns out as well as it promises it will be necessary to give up the method of using surface or flood water. Hitherto it has been allowed to settle only, but we can neither remove the colour nor the germs of life by any such process, and by precipitation we remove at least the visible part of the organic matter, and some of the very material on which organisms live. That none should be left, considering the very vital conditions of the world, it is not for us to expect even if it is to be hoped for.

Examination of this process continues. I look on it as chiefly valuable for throwing down fine floating matter, taking, however, some organic matter out of solution.

No. XI.

MODE OF ESTIMATING THE TRANSPARENCY OF WATER.

In the 16th Report under the Alkali Act, also in the proceedings of the Royal Society, Vol. XXX., I gave an account of a mode of measuring the amount of light over any given period of time. Finding that the idea had previously occurred to Dr. Leeds of the United States, I gave him of course the credit. However my intention was to pursue it for the purposes of my office, and to endeavour to measure the influence which fogs natural and artificial or smoke have on the amount of light transmitted through the atmosphere immediately above us. I also said that I intended to apply the knowledge to an estimation of the transparency of water. I have in my 16th Report under the Alkali Acts given a table of the total comparative light during six hours of the day for above a year, and in this report under the Rivers Pollution Prevention Act I now describe the same method as applied to water.

A few of the results are given, and I may say that they are most satisfactory. The order of the figures is certainly the same as I made out for myself by using the tube when the waters were brought to me without description; but I had a difficulty in judging. Time was required, a certain amount of anxious consideration was needful, and in one case I gave a decision, which, on reconsideration, I reversed. I may add that I am not behind in this mode of judging; two pairs of young eyes belonging to chemists were less correct and constant in their judgment, at least on this occasion. I say this to show the accuracy of the experiment, and to prove that we save time by it and uncertainty. However, this is not all. We obtain exact numbers, such as the eye cannot pretend to determine.

I always use of course as a standard distilled water, and it is necessary to have it in the experiment, as the numbers are not definite in a continuous sense on account of the constant change of light. They are comparable with pure water; but the same numbers do not occur on every experiment, as the light differs. The exact law of change requires study, but the order is certain.

Every new idea has its own mode of development, and I cannot tell how this may grow, considering all the various changes of colour in impure streams in this country; but for natural streams there is less difference of shade. I have here used it to define the brightening effect of precipitation as an example unless otherwise mentioned.

TRANSPARENCY.—TABLE I.

TRANSPARENCY of WATER with different PROPORTIONS of CHLORIDE of IRON in SOLUTION.

50 c. c. Solution containing 0·060 grm. SO_3 + ·5 gramme Potassium Iodide.

—	Pure Water.	Pure Water + 2½c.c. Fe_2Cl_6 per Litre.	Pure Water + 5 c. c. Fe_2Cl_6 per Litre.	Pure Water + 10c.c. Fe_2Cl_6 per Litre.	Pure Water + 20c.c. Fe_2Cl_6 per Litre.	Pure Water + 50c.c. Fe_2Cl_6 per Litre.	Pure Water + 60c.c. Fe_2Cl_6 per Litre.	Pure Water + 70c.c. Fe_2Cl_6 per Litre.	Pure Water + 80c.c. Fe_2Cl_6 per Litre.	Pure Water + 100c.c. Fe_2Cl_6 per Litre.
	c. c. $Na_2S_2O_3$ sol.	c. c. $Na_2S_2O_3$ sol.	c. c. $Na_2S_2O_3$.	c. c. $Na_2S_2O_3$.	c. c. $Na_2S_2O_3$.	c. c. $Na_2S_2O_3$.	c. c. $Na_2S_2O_3$.	c. c. $Na_2S_2O_3$.	c. c. $Na_2S_2O_3$.	c. c. $Na_2S_2O_3$.
July 5th, 1881. 8.45 a.m. to 6.15 p.m. - -	5·7	4·9	4·4	3·4	3·0	2·5	1·5	1·4	1·0	0·8
July 8th, 1881. 8.45 a.m. to 6.15 p.m. - -	3·0	2·0	2·0	1·0	0·7	0·3	0·0	0·0	0·0	0·0
July 9th, 1881. 8.40 a.m. to 2 p.m. - - -	4·0	3·5	3·0	2·0	1·0	0·5	0·0	0·0	0·0	0·0
July 11th, 1881. 8.35 a.m. to 6.30 p.m. - -	3·4	3·0	2·6	1·8	1·0	1·0	1·0	0·8	0·5	0·4

1 c. c. Standard Ferric Chloride = 10·15 mgms. Fe_2Cl_6 + 6·75 mgms. HCl (free).

TRANSPARENCY.—TABLE II.

TRANSPARENCY of WATERS with CHLORIDE of IRON in SOLUTION. DUPLICATE EXPERIMENTS showing CONSTANCY.

50 c. c. Solution containing 0·060 grm. SO_3 + ·5 gramme KI in 50 c. c. Flasks.

—	Manchester Water.	Manchester Water + 5 c. c. Standard Fe_2Cl_6 per Litre.	Manchester Water + 10 c. c. Fe_2Cl_6 per Litre.	Manchester Water + 20 c. c. Fe_2Cl_6 per Litre.
	c. c. $Na_2S_2O_3$ sol.	c. c. $Na_2S_2O_3$ sol.	c. c. $Na_2S_2O_3$ sol.	c. c. $Na_2S_2O_3$ sol.
York Place— June 28th, 1881 - - -	59·5 57·0	57·75 55·75	52·50 53·50	47·25 49·00
June 29th, 1881 - - -	8·85 8·85	8·5 8·5	7·7 7·7	7·0 7·1
June 30th, 1881 - - -	6·8 6·8	6·5 6·5	5·9 5·9	5·3 5·3
July 1st, 1881 - - -	12·1 12·0	11·4 11·5	10·9 10·9	10·55 10·6
July 2nd, 1881 - - -	8·55 8·5	8·3 8·2	7·9 7·75	6·5 6·5

TRANSPARENCY.—TABLE III.

TRANSPARENCY of MANCHESTER WATER and PEATY WATER.

50 c. c. Solution containing 0·060 grm. SO_3 + ·5 gramme Potassium Iodide.

—	Distilled Water.	Manchester Water.	Peaty Water.
	c. c. $Na_2S_2O_3$ sol.	c. c. $Na_2S_2O_3$ sol.	c. c. $Na_2S_2O_3$ sol.
1880, May 26th. After 21 hours - - - - -	12·0	7·0	4·5
May 27th. - - - - - -	3·2	3·0	1·6
May 28th. After 3 hours - - - - -	4·2	1·2	0·6
May 29th. After 16 hours - - - - -	2·0	1·0	0·0
May 29th. After 44 hours - - - - -	50·0	38·5	27·2
May 30th. After 3 hours* - - - - -	47·0	28·0	18·0
May 31st. After 24 hours - - - - -	14·5	9·6	4·6
June 1st. After 1½ hours - - - - -	10·7	6·5	4·2
June 1st. After 5 hours - - - - -	7·5	5·6	2·6
June 2nd. After 16½ hours - - - -	7·1	5·5	2·4
June 2nd. After 7 hours - - - - -	6·9	3·7	1·6
Average - - - -	15·0	9·96	6·1

* This contained 0·300 grm. SO_3.

TRANSPARENCY.—TABLE IV.

MANCHESTER WATER and PEATY WATER. LITTLE LIGHT and MODERATE LIGHT.

Solution 50 c. c. containing 0·300 grm. SO_3 + ·5 grm. Potassium Iodide.

——	Manchester Water.	Manchester Water containing 1 °/₀ Peaty Water.	Manchester Water containing 2½ °/₀ Peaty Water.	Manchester Water containing 5 °/₀ Peaty Water.
	c. c. $Na_2S_2O_3$ sol.	c. c. $Na_2S_2O_3$ sol.	c. c. $Na_2S_2O_3$ sol.	c. c. $Na_2S_2O_3$ sol.
1880, June 9th. After 2 hours, 11 a.m. to 1 p.m. - - -	7·1	6·4	4·2	1·3
June 9th. After 2½ hours, 1 p.m. to 3.30 p.m. - -	5·9	6·0	3·1	1·1
June 10th. After 3 hours, 9.45 a.m. to 12.45 p.m. - -	6·46	6·0	4·4	1·8
June 11th. After 3 hours, 12 noon to 3 p.m. - -	6·7	4·5	4·2	2·0
June 12th. After ½ hour, 10 a.m. to 10.30 a.m. - -	8·6	8·6	5·6	1·6
June 12th. After 17 hours - - - - -	25·8	25·8	20·0	17·3
June 10th. After 21¼ hours, 1.45 p.m. to 11 a.m. - -	17·3	16·5	15·0	12·0
Average - - - -	11·2	10·5	8·07	5·3

One per cent. is very decided on the average numbers, but has variations. After one the result is decisive.

TRANSPARENCY.—TABLE V.

MANCHESTER WATER with SEWAGE and PEATY WATER. A FAIR AMOUNT of LIGHT.

Same Solutions in each.	Manchester Water.	Manchester Aq. containing 3·52 °/₀ Sewage.	Manchester Aq. containing 7·04 °/₀ Sewage.
	c. c. $Na_2S_2O_3$ sol.	c. c. $Na_2S_2O_3$ sol.	c. c. $Na_2S_2O_3$ sol.
1880, June 5th. After 18 hours - - - - - -	10·4	7·0	3·2
June 6th. After 45 hours - - - - - -	25·5	18·3	10·7
	Manchester Water.	Manchester Water containing 2·8 °/₀ Peaty Water.	Manchester Water containing 5·6 °/₀ Peaty Water.
1880, June 2nd. After 23 hours - - - - - -	5·5	3·2	2·0
June 4th. After 24 hours - - - - - -	13·0	3·0	2·0
June 5th. After 24 hours - - - - - -	9·4	4·0	2·7
June 6th. After 45 hours - - - - - -	26·4	13·0	10·0

TRANSPARENCY.—TABLE VI.

DISTILLED WATER. MANCHESTER WATER and SEWAGE in WEAK LIGHT.

Solutions containing in 50 c. c. 0·06 gramme SO_3 + ·5 gramme KI.

——	Pure Distilled Water.	Manchester Water.	Pure Water + 2½ °/₀ Sewage Water.	Pure Water + 5 °/₀ Sewage Water.	Pure Water + 50 °/₀ Sewage Water.
1880, June 22nd. 10 a.m. to 4.30 p.m. - - -	2·5	2·0	1·7	0·7	0·3
June 23rd. 10 a.m. to 4 p.m. - - -	1·45	1·39	1·1	0·9	0·4
June 24th. 10 a.m. to 4 p.m. - - -	5·0	4·1	3·8	3·6	0·3