perature of 65° C. for 30 minutes, be used, there will be but slight if any differencee in the results, provided that the solution of citrate be slightly ammoniacal, and the digestion be made in a closed flask. To the use of *acid* solutions of citrate, either acid at the beginning or the digestion or becoming acid during its continuance, are probably due more than to any other cause, the discrepancies in this analytical process.

XVIII. THE CONTAMINATION OF THE NEW YORK WATER SUPPLY.

BY DR ALBERT R. LEEDS

In this journal (vol. III, 98,) I have given under the title "Relative purity of city waters in the United States," the analyses of the drinking waters of many of our large cities, as performed by the same methods, and as made upon samples collected at or near the same date. As a necessary deduction from these results, the drinking waters arranged themselves in a series, in which the Brooklyn water occupied the place of honor as the purest, the Boston water the lowest place, being the most foul. The New York water, No. VI, fell in the middle of the series. The water supplies (judged by the samples analyzed) of Brooklyn, Rochester, Philadelphia, Baltimore and Washington, were pronounced satisfactory. Those of New York, Newark, Jersey City, Hoboken, Cincinnati, Oswego, Wilmington (Del.) and Boston, were stated to be contaminated. There was no question as to the contamination of the Newark (Passaic) water (No. 7), because its impurities were established by a multitude of analyses. And with regard to the Croton water the evidence of contamination was found in the facts :

1st. That its various impurities exceeded the limits which are allowable in a pure drinking water. 2nd. That its composition approached very closely to that of the Passaic, of whose contamination there could be no question ; 3rd. That the water shed of the Croton contained sufficient sources of pollution to account for the figures obtained by analysis.

The results above summarized have been accepted without cavil, except in the case of the New York water supply. The pollution of the Boston water was so great that I could not credit my results without making a personal inspection of the sources of the Boston supply. This I did at a later season, and found not only were the first results more than confirmed but that the sources of pollution were unmistakable. They were in the first place, the impurities thrown in by manufacturing communities; and secondly, the enormous expansion of certain organic growths at favorable seasons of the year. The results relating to the water supply of Wilmington, Del., which were at first objected to by the uninformed local press of that city, were later on acknowledged by the health and city authorities to be warranted by the actual facts, and I am at present engaged on behalf of this city to investigate the precise nature and extent of the pollution.

Dr. E Waller in this journal (Vol. IV, p. 15), has objected to the New York water supply being characterized as contaminated, on the ground that his results, for a term of years, differ greatly from my own and are of a more favorable character. Also, for the reason that the Croton water shed cannot originate any sources of contamination; and finally, that the health of New York is not, and has never been, such as to indicate the presence of any contamination in the water supply.

In reply, I would state:

1st. That the analyses of the New York water, on which Dr. Waller bases his opinion, have been incomplete, and data have been lost sight of, which are of the greatest value in forming a judgment as to the fact of contamination.

2nd. That certain of his methods are erroneous, and of necessity give a more favorable impression of the water than its true character warrants.

3rd. The analysis of the Croton water made June 23rd, 1881, does not stand alone, but is confirmatory of other previous analyses which led me to the conclusion that the Croton water was contaminated. It was not made, as Dr. Waller intimates, upon water turbid with sediment, but upon clear water drawn from the continuously flowing faucet in the Barclay street ferryhouse. I took it from this source as affording a very fair average sample of Croton water and I have no reason for supposing that it was otherwise.

4th. The condition of the Croton water shed is such as to account for the figures given in my analysis. The table of population given by Dr. Waller for the water sheds, does not explain the facts which exist; for Rochester with but *half* the density of population on its Hemlock Lake water shed, that exists upon the Croton, has a far superior water supply, while Brooklyn on the other hand, with *double* the population, has a better supply than either.

5th. There are no data given, or so far as I know ever have been given, upon which conclusions can be based connecting the vital statistics of New York with the character of its water supply. The face of Dr. Waller's assertion is not to be measured by its positiveness, but upon the appositeness and weight of the data upon which it is based. and of such data he has given us none.

It is extremely difficult, unfortunately, to connect the various diseases of a great community with the more or less deteriorated condition of its water supply. Unfortunately because if the connection were one easily proved, both chemist and medical practitioner could arrive with ease and certainty at results which are now open to more or less criticism. Whilst reiterating in various communications the statement that the water supply of Newark and Jersey City is contaminated, and in seasons of excessive drought like the summers of 1881 badly so, I have repeatedly challenged the physicians of these towns, to connect by irrefragible demonstration, the impurity of the water with any case of disease, and they have professed their inability so to do. I mention this, more particularly because the chemist is often accused by the physician of reporting a long series of decimals, without satisfactorily showing that any of the substances reported are specific causes of disease. Here, on the other hand, is a case where for years the water has been reported, on the strength of the oxidizable and oxidized nitrogenous matters which it contains, to be contaminated, and yet no physician has proven, or attempted to prove, that any particular case of disease has been due to drinking it. In the population of 50,000 people inhabiting the town of Paterson, which pours all its sewage into the Passaic at a point 16 miles above the intakes of the pumping stations of Newark and Jersey City, and in the city of Passaic with its considerable population likewise sewering into the Passaic river at a point only half this distance, there must be many cases of typhoid fever, yet no one has ever shown that a single case of typhoid fever has been originated in Newark and Jersey City by drinking the Passaic water. From what source then does Dr. Waller derive the statistics upon which he infers the purity of the Croton water from the death-rate of the New York population?

To return, however, to the more serious sources of error in the methods made use of by Dr. Waller.

1st. With reference to the incompleteness of his analyses. He has charged me with indefiniteness in the use of the terms "nitrites" and "nitrates." I fail entirely to see any indefiniteness. I used these terms because they were the proper generic names of the sub-

substances I determined. Nitrites and nitrates were the bodies meant, and the bodies whose determination *as such*, I regarded as of importance. The results were calculated to nitrites (HNO_2) and nitrates (HNO_3). and not to the "nitrogen in nitrites or nitrates," because I had no interest in knowing the amount of the nitrogen, could not have made use of it in my calculations, and think its estimation would have been absurd. If the organic carbon and nitrogen had been determined instead of the free and albuminoid ammonia, then indeed I should have calculated out the nitrogen in the nitrates, in order to compare this amount with the organic nitrogen, and thus to find the correct ratio between the organic carbon and nitrogen in the organic compounds present.

There is no evidence that Dr. Waller had estimated the nitrates until the summer of 1881, nor does he appear to have remarked the fact that the Croton water at times gives the reaction for *nitrous acid*. The latter I regard as a most important fact (although too frequently overlooked), because it is very rare that a decided reaction for nitrites is obtained in drinking-waters, and only when the oxidizable nitrogenous compounds are caught in the very transient stage of nitrites, and before their oxidation to the final condition of nitrates. If in *river-waters* the reverse transformation is effected, and any nitrites undergo reduction to nitrites, I have not yet in my own experience encountered a case in which there was any reason for supposing that such a reverse transformation has been effected.

An unusual opportunity for noting this oxidation of albuminoids into nitrites and finally into nitrates, is afforded when the pure mountain-stream waters of the Passaic, after leaping the Great Falls, come straightway into contact with the sewage of the population of 500,000 inhabitants of Paterson. The albuminoid ammonia increased *per saltum* (Sept. 6th, 1881) from 0.014 parts per 100.000, to 0.225 parts. It then fell away in the course of 2 miles flow to 0.133 parts, and at the end of 7 miles flow had fallen to 0.0215 parts per 100,000. The nitrates at the corresponding points were 0.185 parts; 2.183; 0.722; 1.388 parts. The nitrites at the same points were none; a considerable amount the quantitative determination of which was lost): a trace; none.

It is seldom, however, that the reaction for nitrites is obtained in the Passaic water after it has flowed 16 miles, and has been delivered by the pumping-stations to the consumers in Newark and Jersey City. The oxidation of the albuminoids during its long flow is so complete, that while the amount of nitrates is always high, the amount of nitrites is usually quite inappreciable. And the same is generally true of river-waters. In the 23 samples of water, the composition of which is given in the table upon the "Comparative Purity of City Waters in the United States," no reaction for nitrites happened to be obtained in the case of any one of them. And yet the Boston water required the enormous amount of 1.77 parts to the 100,000 of oxygen to effect the oxidation of its organic matters, and contained 1.24 parts of nitrates. Sometimes the Passaic water does afford the nitrous reaction, but in these cases I have attributed it to the non-oxidized sewage of Newark which has only to flow upward for a couple of miles before reaching the intakes of the pumping-stations, rather than to the sewage of Paterson, which has to flow downward 16 miles before reaching the same points, and is usually completely oxidized in its long exposure.

The fact that at times I have obtained from the Croton the nitrous reaction, in one instance a reaction strong enough to admit of quantitative estimation, has had due weight in forming my estimation of its purity.

2nd. The method which Dr. Waller employed of determining the amount of oxygen required to oxidize the organic matter is erroneous, and of necessity gives a more favorable impression of the water than its true character deserves. In making this statement, I am aware of what has been written in favor of this method, but see no reason for regarding it otherwise than erroneous. If it is desired to oxidize the organic matter in drinking waters by potassium permanganate, so as to destroy the possibly noxious substances, the water to which the potassium permanganate has been added is boiled before it is thought entirely safe to drink. If the potassium permanganate is to be standardized by an organic substance, as it is in this instance by oxalic acid, the standardization is performed by heating the solution of oxalic acid or of permanganate. But in making the determination of the organic matter in the water itself, this method proposes it should be done at a variable common temperature. Of course, only a small amount of the organic matter in the water is thus oxidized, and although it is claimed that this part is the really dangerous putrescible portion, the fact still remains that only a part of the organic matter is attacked by the permanganate in the cold, and the method is thus far erroneous. Still more, the method lacks the essentials of an accurate volumetric estimation, and does not admit of arriving at a certain end reaction. I tried the method, and after satisfying myself of its essential inaccuracy, adopted what appeared to me the most satisfactory and definite process, that of titration at the boiling point.

Dr. Waller's objections to this method are fanciful rather than real. The impurities in the permanganate and sulphuric acid, though actually present, are too small to admit of any appreciable influence on the results. The objection that the chlorides in the water would affect the results has greater weight, but in practice these errors, like those produced in Tidy's modification of the permanganate process by the presence of chlorides, are too small to be considered. I am fortunately able to bring an experiment, performed on polluted natural waters themselves, to confirm this statement. The Passaic water (Sept. 6th, 1881), at the intake of the Jersey City Reservoir, contained in 100,000 parts:

	LOW TIDE.			HIGH TIDE.		
	E. Bank.	Middle.	W. Bank.	E. Bank.	Middle.	W. Bank.
Chlorine,	5.73	2.10	3.40	55.88	44.75	59.75
Oxygen required	0.55	0.44	0.44	0.47	0.60	0.76

The differences had been caused by the backing up of the river waters, on meeting the tidal wave from the sea.

On the same day, and upon the same two tides, the differences for the corresponding points at the Newark Reservoir, three-quarters of a mile further up stream, were :

	LOW TIDE.			HIGH TIDE.		
	E. Bank.	Middle.	W. Bank.	E. Bank.	Middle.	W. Bank.
Chlorine,	2.185	1.17	2.505	21.13	25.00	14.50
Oxy. required,	0.62	0.56	0.43	1.80	2.82	2.82

But it may be objected to these results, that the oxydizable organic matters in the various samples varied greatly, so that no inference could be drawn with regard to the influence of the chlorine itself.

This objection does not apply to the water taken at intervals of about a mile down the stream, upon the same day (Sept. 6th, '81).

Beginning at a point below the Tail Race at Passaic Mills, we found for samples :

No.	75	76	77	78	79	80	81	82	83	84
Chlorine,	0.585	0.60	0.94	0.57	1.57	1.005	0.90	0.56	1.10	2.505
Oxy. required	0.53	0.50	0.49	0.52	0.51	0.59	0.47	0.47	0.47	0.43

It may be objected, however, that all these results merely raise a strong presumption in favor of the proposition that the small amounts of salt in river water do not appreciably raise the figures obtained by permanganate at the boiling point. They do not prove it ; for proof a suitable experiment is demanded.

In the first place, it was necessary to determine the coefficient of change, when potassium permanganate is boiled with *distilled* water. Both in the cold and on heating, potassium permanganate changes in acidulated solution with distilled water, and the amount of this change has to be determined in both cases, and a corresponding deduction has to be made from the amount obtained with an equal volume of the natural water under examination, I thus obtained :

100 cc. distilled water, Miller's method (3 hr.) 0.9 cc. permanganate.
100 cc. distilled water, Kubel's method, – — 0.9 cc. permanganate.

Another test by Kubel's method on a different sample of distilled water, and with different permanganate solution, gave 1.15 cc. permanganate.

In other words, in order to make use of Miller's method with any expectation of arriving at accurate results, it is necessary in each case to apply a correction due to the alteration of the permanganate alone. . This and many other precautions C. M. Tidy (Jour. London Chem. Soc., xxxv., p. 46) has tak-n, in order if possible to bring Miller's process into a form capable of yielding regular and accurate results. But I do not find in Dr. Waller's account of Miller's method, as he employed it, that any such precautions were taken. The influence of these corrections may be perceived by considering the great changes thereby produced in Dr. Waller's results. For instance, for the first five months of the year 1879, he obtained 0.072 parts of oxygen per 100,000, as the amount required to oxydize the organic matter. This would be equal to 0.72 cc. permanganate solution used. If, as is possible, he had obtained 0.9 cc. as the correction to be applied by the use of the permanganate solution with *distilled water alone*, there would have been left a minus quantity, or 0.018 parts of oxygen, as the amount required to oxydize the organic matters in the Croton water.

Dr. Tidy has endeavored to find the extent to which the chlorides affect the accuracy of the modified Miller's method, and has found that their influence, even when present in very large quantities, practically *nil.* A similar remark applies to the effect of chlorides on the results afforded by Kubel's method. For instance, in an experiment in which 100 cc. of *distilled water* decolorized on boiling 1.15 cc. permanganate, it decolorized only 1.3 cc. permanganate after sodium chloride had been added in amount corresponding to 400 parts in 100,000. This amount vastly exceeds any quantity likely to exist in river or well waters.

In practice, however, I think it is better, whenever the amount of sodium chloride is large, to add the percentage indicated by the chlorine determination to the distilled water used in making the blank experiment. If the results thus obtained differ from those obtained by the use of the 100 cc. distilled water alone, they can be deducted, instead of the latter, from the number of cubic centimeters of permanganate obtained with the natural water.

3d, and finally. The analysis of the Croton water made June 3d, 1881, does not stand alone, but is confirmatory of other previous analyses, which led me to the conclusion that the Croton water was contaminated.

The best proof of this statement is to be found in an analysis made only three days before the one intended for the general comparison with the drinking water of all the other large cities, or on June 20th, 1881. It was made upon unfiltered but perfectly clear water, drawn from the same place, the constantly running faucet in the Barclay street ferry-house. It shows by its close agreement with the results obtained on the sample taken June 23d that neither analyses gave high figures by accident, but both are to be regarded as mutually confirmatory results of the actual average condition of Croton water at that time.

	CROTON WATER.	PARTS PER 100,000.
	June 20th, 1881.	June 23d, 1881.
Free ammonia	0.004	0.0027
Albuminoid ammonia	0.027	0.0270
Oxygen required	0.73	0.810
Nitrites	None.	None.
Nitrates	0.6105	0.8325
Chlorine	0.26	0.35
Total hardness	3.00	3.30
Permanent hardness	2.60	----
Temporary hardness	0.40	----
Total solids	9.00	11.80
Mineral matters	2.50	5.00
Organic and volatile matters	6.50	6.80