RIVER WATER.

(No. 2.)

A REPLY TO DR. FRANKLAND.

BY

CHARLES MEYMOTT TIDY, M.B., F.C.S.,

Master of Surgery.
Professor of Chemistry and of Forensic Medicine and Public Health at the London Hospital; Medical Officer of Health for Islington; and late Deputy Medical Officer of Health for the City of London.
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It may be within the memory of some present that in May last I submitted to the Society a paper on "River Water," which the Council did me the honour to print in the Journal. Amongst the many subjects I discussed was the power water possessed (in my opinion) of self-purification, passing from a state of impurity to a condition of purity after a flow of a few miles. I remarked that this change was effected by various agencies. Of these I mentioned three:—*first,* the subsidence of suspended organic matter. In this process of subsidence I included not mere deposition only, but a forcible carrying down (if I may so express it) of organic impurities by the admixture of the impure water with suspended mineral matters, a circumstance by no means infrequent in rivers; *secondly,* the scavengering propensities of fish, present in the water after it has attained a certain degree of purity; and, *thirdly,* the oxidation of the organic matter. On this latter point I said as follows:—"The dissolved oxygen is derived partly (and in the first instance entirely) from the air, and partly (more especially in the later stages, when the water has reached a certain purity, and vegetation begins to reappear) from plant life, vegetation constituting an important means whereby oxygen is set free in the water itself. Such oxygen is probably in a more than ordinarily active condition to effect the oxidation of the organic impurity" (p. 34).

I shall be excused repeating thus much of what I have said, because I desire to make clear that I neither directly nor indirectly suggested oxidation of the organic matter as the sole means, but only as one of the agencies at work, to render the water pure. And further, I neither directly nor indirectly suggested that the atmosphere was the only source of the oxygen for the oxidation of the organic impurities, but on the contrary, I distinctly stated that the oxygen set free by the river vegetation was the more potent agent in working the change.

Dr. Frankland has replied to this one part of my original paper in a paper of considerable length, published in extenso in the July (1880) Journal of the Society, and my experiments and deductions respecting the oxidation of peat have been further criticised in a second paper submitted to the society jointly by Miss Lucy Halcrow and Dr. Frankland. It was impossible for me on the evening these papers were read, owing to the short time at my disposal, and the practical difficulty of grasping offhand the real force of objections, to reply to Dr. Frankland's kindly criticisms, more especially when additional importance (if such could be the case) was given to the discussion by the remarks with which Professor Huxley and Professor Tyndall favoured the Society. I thank the Society for permitting me to bring the subject before them once more, and so far as I am concerned for the last time. The delay on my part in replying to Dr. Frankland's criticisms has been partly the result of private circumstances over which I was powerless, and partly because I desired to make my reply, in some small measure, a record of new work.

And FIRST—*as to the oxidation of peat.*

In my paper I detailed numerous analyses of Shannon water for the purpose of showing the disappearance of peaty matter in the onward flow of a river. I remarked that—although along the whole of the fifty mile course of the Shannon I described, and with which I am intimately acquainted, more or less peaty drainage is to be seen flowing in at frequent intervals—at certain places deep black streams, the drainage of hundreds of acres of bog land, discharge themselves into the river, increasing notably, even to the naked eye, the peaty condition of the water generally. I then showed by analysis that between certain spots, choosing intervals where as far as I could judge the peaty drainage discharged into the river was comparatively little (although let me add it would have been impossible to have chosen any interval where there was none), the quantity of peat manifestly lessened after a certain run, the organic carbon in one instance (viz., after the flow of a mile from Killaloe downwards) decreasing from 0·8 per 100,000 parts to 0·48, and in a second case (the famous Doonas falls being situated in the interval) from 0·84 to 0·59.

Dr. Frankland in answer to this argues first of all that the river Shannon is not well fitted for experiments (to use his own words) either "in proof or disproof of oxidation," because (and the reason is important) it exhibits "such sudden bounds" in the proportion of the organic elements.

I chose the Shannon for the study of the results of flow on peaty matter, partly because the opportunity occurred to me for a more than ordinarily careful and systematic inspection of the river, but chiefly because the Shannon was a highly peaty river, absolutely free from any suspicion of sewage, full of fish of the most delicate kind (for Shannon trout are known all the world over), and luxuriant with vegetation.

Regarding Dr. Frankland's objections to the Shannon in the light of an intimate knowledge of the river, I am disposed to think that the Shannon may not (for the reasons he states) be well fitted to disprove the oxidation of peat in the course of its flow if there be no oxidation, but that it is admirably adapted to prove oxidation, should such a process be going on. Admit the ever recurring entrance into the river of feeders of peaty water, perpetually increasing the already large quantity of peaty matter present,—if I can show that in spite of this constant addition and addition, there is after a short run a manifest lessening of the peat present in the water, then my experiment, considering that all the chances and conditions are against me, becomes a hundredfold more conclusive than it would otherwise be.

I grant, then, that in the Shannon the conditions were unfavourable (because so severe) to prove oxidation, and impossible for the purpose of disproving oxidation, because it was impossible to say that the quantity of peat discharged into the river might not be greatly in excess of the amount got rid of. In other words, the conditions required to disprove are not necessarily the conditions required to prove. Hence, when Dr. Frankland points out, referring to my analyses, that at certain points of the river, viz., at Portumna, at Killaloe, at O'Briensbridge, and above the junction of the Mulcaire, there is a certain uniformity in the quantity of organic elements present, he merely indicates that the quantity of peaty drainage that has found its way into the river at or near the spots where the samples were taken, succeeded in restoring again and again the amount of peat that had been removed in the course of the flow. Thus far then, I contend that the Shannon, considering all circumstances, was singularly well suited for an investigation as to the fact whether the oxidation of peat and its removal by other agencies did take place in a running river, although it was ill-fitted to determine the extent to which that oxidation occurred.

I would venture, however, to press Dr. Frankland's criticisms one step further, and to use them as valuable confirmatory evidence of my contention. Admit that at four points along a course of forty miles (viz., at Portumna, Killaloe, O'Briensbridge and at the junction of the Mulcaire) the organic elements do indicate a fairly uniform quantity of peat in the water, it is indisputable that in this forty mile flow floods of peaty matter have poured themselves into the river. What has become of it all ? Dr. Frankland says the results of the analyses show uniformity. So be it. But how comes the uniformity ? How is it, in short, that the quantity of peat in the river does not, yard by yard as the water flows onward, increase in amount, if there be no oxidation or no inherent power in the running water of self-purification. Starting with a brown water at Killaloe, and noting stream after stream of peaty drainage being discharged into it, the river water surely ought to be black over and over again before it reaches Limerick. It wants no analysis to prove that this is not the case; and I venture to urge this fact as a powerful argument in favour of the view I am advocating. Whether, then, I take a reach of the river where the peat drainage emptying itself into it is comparatively small (although not inconsiderable) in quantity, and show that in that reach (Dr. Frankland has been good enough to coin for me the phrase "an oxidising section" for this reach, though I am sorry to decline it, for I am convinced the whole river is one entire oxidising section)—and show that in that reach a considerable diminution occurs in the quantity of organic matter present, or whether I take a long run of river—say forty or fifty miles—and show the organic matter at the two extremes to be practically identical, knowing all the time that the river has received the drainage of some thousands of acres of bog land, matters nothing to my argument. Either way the case is the same. Given a certain run of river, the peat disappears.

Dr. Frankland now starts a new criticism, and one well worthy of an adroit critic.

I have stated that in a reach of the River Shannon extending from one mile below Killaloe to O'Briensbridge (a distance of five miles), the organic carbon shows a rise from 0·48 to 0·84, the increase being a manifest result of certain black peat streams that join the river in this section. In another place I have stated that two grains of peat render the water, as seen though a stratum of two feet, black. Peat, says Dr. Frankland, contains 60 per cent. of carbon. Hence if this increase from 0·48 to 0·84 indicates the actual increase of the peat in the entire river, it follows that the volume of the Shannon must have been augmented within the five miles by more than one-third its volume of peaty drainage. 'Impossible!' says Dr. Frankland, and I agree with him. Dr. Frankland's argument, however, is based on the statement made by me, that two grains of peat would render the water as seen in a two-foot tube black. But Dr. Frankland overlooks the fact that directly you have reached a tint in your two-foot tube, indicated by the word "black," the black discoloration will be very little affected by the addition of a further quantity of peat. There are no degrees of a true black. I did not say that the water to appear black in the two-foot tube must contain no more than two grains of peat per gallon, but that it must not contain less. Jealous as I am for the indications of the two-foot tube as a controlling element in a water analysis, I am free to admit that it is at this spot in a peaty water (viz. about two grains per gallon) that the two-foot tube ceases to be of value. But its real value is not overthrown became it has limits of value. A balance that will not weigh pounds may be a very useful instrument with which to weigh milligrammes.

To test this matter further I desired a trustworthy man to collect for me six samples of peaty drainage as it was being discharged into the Shannon, three samples being what he would roughly call "strong drainage" and three "weak drainage." All six samples were collected in the reach between Killaloe and Castle Connell, and at a spot 2 or 3 feet distant from the junction of the peat stream and the river. The following were the results of my analyses of these six samples, expressed in parts per 100,000 :—

	Strong Samples.			Weak Samples.		
	1	2	3	4	5	6
Organic Carbon ...	6·11	9·89	4·62	2·21	2·48	2·90
Organic Nitrogen ...	0·44	0·78	0·21	0·10	0·16	0·11

The fact is that peat drainage may be practically of any strength. I have thus, I think, disposed of this argument of Dr. Frankland.

Since Dr. Frankland read his paper I have had three samples of Shannon water collected and forwarded to me for examination. They were taken, I have reason to believe, with most scrupulous exactitude. One sample was taken from the river one mile below O'Briensbridge, the second one mile below Castle Connell, and the third just above the town of Limerick. Each sample was an admixture of four separate samples taken at different spots in a line across the river, and in each case one foot below the surface. The river on the day on which the samples were taken was five inches above mean ordinary summer level. The following are the results of the analyses :—

	Total Solids.	Nitrogen as Nitrates.	Chlorine.	Oxygen required to oxydize the organic matter.	Organic Carbon.	Organic Nitrogen.
	grains.	grains.	grains.	grains.	parts per	100·000.
River one mile below O'Briensbridge	16·18	None.	1·008	0·626	1·014	0·061
River one mile below Castle Connell	16·58	None.	1·008	0·231	0·681	0·056
Town of Limerick	17·01	None.	1·008	0·180	0·342	0·030

I had no doubt that my original samples (which I had myself collected) fairly indicated the condition of the water; but in order to meet the criticism, that the only way to judge with perfect accuracy the state of a river water is to take several samples in a line across the river and to examine a mixture of the several samples—a statement in regard to which I am at one with Dr. Frankland—I have had this done. I cannot question for a moment, with these facts before me, that peat is got rid of in the course of the flow of a river, and that oxidation is one of the agencies concerned in its accomplishment.

At this point I am led to say a few words of criticism on the paper presented to the Society jointly by Miss Lucy Halcrow and Dr. Frankland on "The action of air on peaty water."

The first series of experiments recorded by the authors are analyses of the air that had remained for a year, or thereabouts, in the same bottle with some peaty water, without agitation. A variety of difficulties occur to me in drawing from these experiments conclusions of any value in reference to the oxidation of peat in rivers. At any rate, take them as they are—that is, simply as analyses of the air that for one year had been allowed to remain without motion in accurately stoppered bottles containing peaty water. I confess, remembering the conditions (viz., an undisturbed water in the same bottle with undisturbed air), I should have been rather astonished if any appreciable oxidation of the organic matter by the atmospheric oxygen had taken place. If it occurred, the action must have been limited, it seems to me, to a fractional layer of the still water, and a fractional layer of the still air; that is, where actual contact of air and water occurred. I have arranged the first four analyses, given by Miss Halcrow and Dr. Frankland, of the air in the four bottles experimented upon, and in the fifth column I have given the mean of the four analyses :—

	1 Expt. 1.	2 Expt. 2.	3 Expt. 3.	4 Expt. 4.	5 Mean.
CO_2	0·10	0·15	0·19	0·15	0·147
O	20·78	20·81	20·83	20·69	20·777
N	79·12	79·04	78·98	79·16	79·076
	100·00	100·00	100·00	100·00	100·000

The results are remarkable; but surely Dr. Frankland will admit that 14·7 parts of carbonic acid per 10,000 of air is rather excessive. Dr. Frankland would scarcely contend that the air when it was first corked up in these bottles contained that amount of carbonic acid. 4·5 parts of CO_2 per 10,000 parts of air is, we regard, a fairly normal proportion, and I think we are indebted to Dr. Frankland himself for proving that at high altitudes the CO_2 in the air is—to use his own words—rather in excess; for he found in one case (viz., at Chamounix) 6·3 parts per 10,000, and in a second (on Mont Blanc) 6·1 parts per 10,000. But here, according to Dr. Frankland, we have 14·7 parts per 10,000. But, says Dr. Frankland, supposing the carbonic acid is in "*slight*" excess (I should call it enormous excess), look at the oxygen. If I were hypercritical, I might argue that the oxygen was *slightly* below the normal; but I am far from suggesting that the free air in the bottle supplied the oxygen for the carbonic acid; for

Dr. Frankland has given us no analysis of the dissolved gases in the water, *first*, at the commencement of the experiment, when the bottle was put on the shelf, and *secondly*, at the end of the year, when the air in the bottle was removed for analysis. Oxygen is not a very insoluble gas, and unless Dr. Frankland can show that the dissolved oxygen was the same at the beginning and at the end of the inquiry, and that the amount of organic matter in the water at the beginning and at the end were the same (for these data are wanting), I have a perfect right to assume, that the organic matter in the water was slowly oxidized at the expense of the dissolved oxygen. I need scarcely add that I join issue with Dr. Frankland when, arguing on these experiments, he states, "*These results* prove that the extent to which the peaty matter of the upland water was oxidized in periods of more than a year was very small indeed."

The next series of experiments conducted by Dr. Frankland and Miss Halcrow is even still more remarkable. 250 c.c. of a peaty water was placed in a well-stoppered bottle of 500 c.c. capacity, and well shaken for various periods. This done, the air (which had never been changed during the shaking process) was examined, and the following are the results of the four experiments :—

	After 19 hours' with shaking.	After 64 hours' with shaking.	Steam engine experiment.	After 192 hours' with shaking.	Mean.
CO_2	0·23	0·23	0·12	0·20	0·195
O	20·77	20·77	20·75	20·79	20·775
N	79·00	78·98	79·13	79·01	79·030
Total ...	100·00	100·00	100·00	100·00	100·000

Again I point out 19·5 parts of carbonic acid per 10,000 is considerable, and I ask Dr. Frankland where did it come from ? I cannot think the suggestion is very far fetched that the carbon came from the peat, and the oxygen from the oxygen dissolved in the water.

For a moment, apart from criticism, consider broadly how far the results of Dr. Frankland's and Miss Halcrow's experiments of shaking together a little water and air in a bottle can be deemed comparable with the flow of a river and the changes resulting.

My case is this :—our river is one of great volume, in free and open contact with a sea of air, no one moment the same as the moment preceding it, but ever shifting and changing as winds and currents shift and change. The water is moving onwards towards its ultimate destination at great rapidity. But the motion of flow is neither perfectly uniform nor unbroken. Along its whole course the flow is more or less broken by the friction of the water with the bed of the river, the disturbance increasing as the roughness and unevenness of the river bed increases. But other and more active physical influences are at work to disturb the even run of the water. At every few yards certain slight mechanical obstructions to the flow will probably occur, sufficient to agitate the water considerably, whilst at certain points in its course there may be found disturbing influences of a more intense kind (such as the Doonas falls on the Shannon), where the water is lashed by the violent action to which it is subjected into a head of foam. Be the disturbing influences, however, in our river great or small, they all tend to bring *fresh surfaces* of the water, second after second, into a more absolutely complete contact with *fresh* supplies of unused air, conditions not unfavourable for the exertion of chemical activity. Our river, too, is luxuriant, for at any rate a large part of its course, with vegetable life (*i.e.*, an oxygen producing life), and it also abounds with fish (*i.e.*, an organic matter consuming life), both lives acting as river purifiers, the one life purifying by furnishing active nascent oxygen to the dissolved organic matter, the other life purifying by consuming the dead organic matter as food for itself. Such is the river.

Dr. Frankland's experiment with which he would compare all this complication of actions is the shaking up of 250 c.c. of water with 250 c.c. of air. That is the whole experiment. No animal or no vegetable life present, but merely the same dribble of water brought ever and anon into contact with the same few bubbles of air.

Supposing for the moment that fish and vegetation were unimportant factors in the problem, and supposing that all turns on air and water, I fail to see the possibility of comparing together Dr. Frankland's experiment, and the effects produced in nature by the flow of a river.

Unwilling to disturb the continuity of my remarks on oxidation, this occurs to me to be as the most suitable place to refer to the courteous criticisms of Dr. Mills. I have already and at great length endeavoured to prove to the Society (I am egotistical enough to think proved) that the results obtained by the combustion process in water analysis closely accord, and generally agree, with the results obtained by the oxygen process. I confine myself to these processes because a not very limited experience compels me unwillingly to believe that they are the only two processes that have been as yet suggested on which reliance can be placed for the determination of the organic matter of a water. Dr. Mills, however, points out that taking the numerous analyses I have recorded in my original paper

$$\frac{\text{the oxygen required}}{\text{the sum of the organic C and N}}$$

yields an inconstant number. Hence he concludes that the oxygen process does not indicate the actual quantity of organic matter present. Believing that the estimation of the organic carbon (I say nothing about the organic nitrogen) supplies us with valuable information as to the organic purity of a water, it is impossible to forget that its indications are not absolute. I have already stated a case in illustration. Send for analysis a sample of distilled water containing sufficient hydrocyanic acid to poison a household; the chemist would, judging it by the combustion process, report the water to be perfectly wholesome and free from matter of an injurious nature. What I mean is, that the combustion process takes no cognizance at any rate of that which I am disposed to think may be the most important constituent in a water, viz., the volatile organic matter. I do not propose discussing this matter further. Believing firmly that both the oxygen and combustion processes are valuable, but still leaning to the oxygen process as the better of the two, it seems to me unwise to forget that the indications of the one are no more absolute than the indications of the other. The general agreement of the two (judged from a practical point of view) is the strong argument in favour of the value of both.

SECONDLY. I now come to a far more serious question, viz., *sewage pollution, or the contamination of running water with animal matter.*

Dr. Frankland, admitting that some purifying action may occur in the course of the flow of a river, contends that it is so infinitesimal in quantity, as practically to amount to nothing. Given a water once polluted, no matter what the volume of dilution may be, no river in England, in his opinion, provides a run long enough to bring about, by oxidation or otherwise, such a removal of the organic impurities as to render the water wholesome and fit for domestic use.

On the other hand, I contend if sewage be discharged into a river in such proportion that the pure water is at least 20 times the volume of the sewage, that after a moderately rapid flow of a few miles the whole of the impurities will by oxidation and other agencies disappear, and the water be again restored to its original state of purity.

In my paper on "River Water," I carefully and intentionally abstained from quoting authorities. I regret that Dr. Frankland has adopted a different course. He remarks that the "intrinsically absurd" notion (these are his own words) that a once polluted river should again become pure, in other words, that water possesses any such power as that of self-purification, had its origin with, and was supported by, two classes of people: 1st, *river polluters,* who found a difficulty in dealing with their refuse, and were glad to discharge it into the nearest water-course; and, 2ndly, *water companies* who derived their supply from rivers. The notion, says Dr. Frankland, of self-purification, oxidation, and the like, was *popular* for a time because it was *comfortable*. Forgive me if I suggest that the non-purification of river water, of which hypothesis Dr. Frankland may claim to be both parent and apostle, is at the present time popular (certainly not because it is comfortable but) because it comes before the public launched and perpetually buoyed up by the influence of a deservedly great name.

I desire to make one remark on the authorities (or rather I should say authority) Dr. Frankland has called in favour of non-purification by flow. Professor Brodie in 1865 questioned, says Dr. Frankland, the old fashioned comfortable idea of water polluters and water companies. "To think," says Brodie, "of getting rid of organic matter by exposure to the air for a short time is absurd." I will not quote the whole passage ;—some of us (at any rate) know it very well, for we have heard it very often. Now I venture to ask Dr. Frankland this question, and I ask it confidently because I know what the answer must be. Can Dr. Frankland point me to a single experiment, or to a single recorded observation by Brodie bearing on this subject, upon which his condemnation of the then accepted theory was based? Not a single experiment (I say this advisedly) was, so far as I can find, ever made or recorded by Brodie on the subject of oxidation on running water. So that after all, this great chemical witness, called by Dr. Frankland in support of his view, is simply a witness summoned to state a hypothesis, and not the witness of exact experiment.

It is worth recording that the Thames has, notwithstanding the sewage, been approved of as a source of water supply by various Committees and Commissions after hearing evidence, as *e.g.*, by the Scientific Commission of 1850, by a Select Committee of the House of Commons of 1867, and by the Royal Commission on Water Supply in 1869. Thus in 1867, the Select Committee reported that they were "satisfied that both the quantity and quality of the water "supplied from the Thames is so far satisfactory, that there is no "ground for disturbing the arrangements made under the Act of 1852, "and that any attempts to do so would end in entailing a waste of "capital, and an unnecessary charge upon the owners and occupiers "of property in the metropolis."

We now come to the change in the popular feeling, resulting, as Dr. Frankland remarks, from certain researches undertaken by himself. These researches, which Dr. Frankland repeats in his paper, without so far as I can see, any further experiments, it now becomes my duty to refer to.

Dr. Frankland's experiments were conducted on the Irwell, the Mersey, and the Darwen. The total number of observations on these rivers for the purpose of disproving the then accepted theory were five in number. First of all, I should wish to know how the samples were collected? Were they simply single samples of the water of the river, because if so (and I have Dr. Frankland's authority in his criticisms on my samples for saying this), they are of very little value. On this point Dr. Frankland is silent. I happen to know all three of these rivers well, and I confess, three rivers more pre-eminently ill-fitted for experiments of this nature, it seems to me, it would be impossible to find. The constant inflow of polluted matter into these rivers along their course is well shown by Dr. Frankland's own experiments. Thus in the Irwell, after an eleven miles run, Dr. Frankland finds an actual increase in the organic elements present, in one case the organic carbon rising from 2·15 to 2·37 per 100,000 parts, and in a second the organic nitrogen rising from 0·248 to 0·304. In the third Irwell experiment the organic nitrogen is also recorded as undergoing a slight increase. And these are the only three experiments recorded on the Irwell. I can scarcely believe Dr. Frankland's antipathy to the notion of water improving by the flow of a few miles would lead him to suggest that it deteriorates unless by the reception of fresh impurities. But, says Dr. Frankland, 'you must not interpret these results too strictly.' I was curious to understand the exact meaning Dr. Frankland attached to that phrase, and this was not far to seek. In one experiment on the Irwell recorded by Dr. Frankland, the organic carbon, after a run of eleven miles, decreased from 2·134 to 1·502, or by nearly 30 per cent. In the observation on the Mersey the organic elements decreased from 0·815 to 0·648, or by 21 per cent., whilst in the case of the Darwen they decreased from 2·422 to 1·430, or by 41 per cent., this latter experiment having been made in the cold month of March.

I have now referred to the whole of Dr. Frankland's experiments on these three rivers. But I venture to think that these observations, pre-eminently successful as I regard them in proof of the possibility of a river water regaining purity in the course of its flow, Dr. Frankland will admit are scarcely illustrations of what I mean. A sewage water represented by 2·352, 2·394, 2·373, and 2·422 of organic elements per 100,000 can hardly be regarded as a sewage *freely diluted*, and that these highly contaminated waters should undergo a process of purification to the extent that Dr. Frankland's experiments indicate, is singularly satisfactory.

But there is another point about these rivers which I should have dwelt upon more fully had not Dr. Frankland himself, as a faithful recorder, done so. I will quote his own words ("Sixth Report," p. 135): "The rivers upon which they [these experiments] were made are *notoriously* much polluted by sewage and *other refuse organic matters.*

So intense indeed is their pollution *that ordinary aquatic life is entirely banished from their waters.*" And now we are in possession of the facts respecting these experiments on the Irwell, the Mersey, and the Darwen—and it is well to put them together—Notoriously polluted sewage rivers, contaminated with other refuse organic matters besides sewage, vegetation banished because of their extreme impurity (so much Dr. Frankland admits), and to this account I would add rivers constantly receiving fresh supplies of impure matters and absolutely devoid of fish—everything, in short, against the regaining of purity—and yet, in spite of all this, and I thank Dr. Frankland for the illustration, he shows that after a few miles' run a decrease of organic elements occurs, to the extent in one case of 41, and in a second case of 30 per cent.

Dissatisfied as Dr. Frankland seems to have been with these experiments—and I am far from astonished at the dissatisfaction—he says :—"We deemed it therefore desirable, in order to *complete* this part of our investigations to ascertain the effect of a flow of some miles upon the water of a river less polluted, and in which animal and vegetable life still flourished" (Sixth Report, p. 135) ; and so Dr. Frankland turns to the Thames. I happen to know something of this river and the work that has been done in connection with it. I was perfectly familiar with the analyses of Dr. Frankland and Dr. Odling of the two samples collected by Dr. Pole—the one at Lechlade and the other at Hampton—and to which Dr. Frankland refers as unsatisfactory because of the sixteen days' interval between the collection of the samples. I agree with Dr. Frankland in some respects in this criticism, but not entirely. But so far as I can understand Dr. Frankland's statement, that "it would require a flow of seventy miles to destroy the organic matter present in the Thames after its junction with the Kennet" (and even here Dr. Frankland apologizes for supposing such a distance as seventy miles of run would be sufficient), this conclusion is based on a single experiment, recorded on page 6. Over and over again I have experimented on the Thames, and before long it may be necessary to arrange the facts in my possession. Let me, however, state the results of one very carefully conducted experiment. A sample of Thames water (a mixture of four samples taken across the river) was collected one mile below Oxford. Within two hours (1 hour 45 minutes exactly) of this sample being taken, a sample of the river water was in a similar manner collected at Hampton. The Oxford sample gave 0·712 parts of organic carbon and 0·081 parts of organic nitrogen per 100,000 parts, and required 0·268 grains of oxygen to oxidize the organic matter ; whilst the Hampton sample gave 0·234 parts of organic carbon and 0·035 parts of organic nitrogen per 100,000 parts, and only required 0·071 grains of oxygen. But, says Dr. Frankland, "*The Thames is not well* *adapted for the study of oxidation by flow in any part of its course, but it lends itself most readily to the support of ad captandum arguments about oxidation.*" This criticism is a curious one, seeing that Dr. Frankland himself selects the Thames for one experiment, and on this one observation he founds certain conclusions. But Dr. Frankland has made other experiments on rivers, and although he has not included them under that portion of the Sixth Report devoted to the subject of oxidation, perhaps he will allow me to transpose certain experiments to their proper place in the Report. The River Tees, says the Sixth Report, p. 399 (and correctly), is polluted by the sewage of Barnard Castle, a village of 4,000 inhabitants, also by several other villages, and also by refuse from dye-works and fellmongers' premises. After a flow of about sixteen miles it reaches Darlington, at which spot is the intake of the Darlington and of the Stockton and Middlesborough Water Companies. Dr. Frankland records an analysis of the River Tees water taken on October 6th, 1870, above the town of Darlington. It gave 0·183 of organic carbon and 0·020 of organic nitrogen, and in spite of all this sewage and the pollution of the river with dye-stuff and fellmongers' refuse within sixteen miles, no previous sewage contamination was found in the water at all. And further, let me read Dr. Frankland's opinion on this very water after filtration (Sixth Report, p. 399): "The water at the time our sample was taken was of *unimpeachable quality:* it was clear and bright and nearly as palatable as deep well or spring water." A truly magnificent illustration of the power of running water to purify itself ! !

I will not burden this paper with further cases of a like kind from the Sixth Report, but they are not wanting.

Dr. Frankland's criticisms on my experiments on Severn water suggest one or two remarks. From the quantity of sewage that my analyses show was got rid of by the flow of the river for a few yards, Dr. Frankland contends that the whole of the organic impurities ought to have disappeared very soon. Thus, he says, "If the Severn samples prove anything about oxidation they prove too much : for comparing sample No. 2 with sample No. 3, both taken just below Worcester, it appears that a flow of thirty yards reduced the organic elements in 100,000 parts of the water from 1·103 to 0·751 or by 32 per cent. At this rate the Severn would be absolutely free from organic matter before it covered the distance of 100 yards, and it is therefore somewhat disappointing to find that after a further flow of a mile it had scarcely lost 12 per cent. more" (page 11). To this I answer—if sewage was a material of constant composition and not the complicated body we know it to be, there would be much force in the criticism, but bearing in mind the complicated nature of the admixture called sewage, I was not disappointed, as Dr. Frankland expresses himself, at the result, seeing that I expected that the materials in the sewage of easy oxidation would yield first and rapidly, leaving in the water those portions of the organic impurities most difficult to be disposed of.

In discussing the peculiarities of Severn water Dr. Frankland criticises my statements about chlorine. I remarked that starting with a freshly polluted sewage river the chlorine would be large, but that the quantity would gradually decrease until it reached an amount equal to about one grain per gallon, the quantity commonly found in a good river water. I said probably the excess of chlorine was taken up by vegetation. Dr. Frankland disputes this. Dr. Mills remarks that "the weeds in a river's bed into which sewage is poured constitute a natural sewage farm, *capable of absorbing chlorides*, phosphates and organic matter in the ordinary way. A great deal of the purification of the Thames must be owing to this cause. I should regard with grave apprehension any proposal to dredge the bed of the Thames above London."

I should wish to add that by supplying some growing watercresses for twenty-four hours with a water to which I had added forty grains of common salt per gallon, I succeeded in increasing the quantity of chlorides present in the watercresses by 220 per cent.

With respect to Dr. Frankland's criticisms on my experiments with sewage water in troughs I have only one word to say (for as far as I can see they remain absolutely unanswered) and that is that the sewage used was filtered.

I should wish to express my regret that in quoting Dr. Frankland's report respecting the River Wear at Durham as "of good quality," I omitted, unintentionally, the succeeding words which I certainly ought to have quoted, "considering its source." I would suggest, however, that, without considering its source, Dr. Frankland's analysis of the water (0·166 of organic carbon, no previous sewage contamination being found in the turbid unfiltered water, and 0·082 of organic carbon in the filtered water) would not, even by Dr. Frankland, be regarded otherwise than as satisfactory.

I now approach that part of my subject where, in addition to Dr. Frankland, I have to meet the criticisms of Professor Huxley and of Professor Tyndall. Let me therefore endeavour to state my precise line of argument, and then attempt to answer their objections.

The Thames admittedly receives sewage in certain parts of its course. At Hampton the Water Companies take their supply from the river, and the filtered water is drank by nearly the whole of London.

This, says Dr. Frankland, in effect, is the cause of all your troubles and much of your disease. The supply of chalk water to London "would be a priceless boon, and would at once confer upon it absolute immunity from epidemics of cholera."

My reply is, What are the facts ?

First. I have shewn on the statistics of ten years, that English towns supplied by pure well water are, as regards health (indicated by the general death-rate), no better off, and as regards zymotic diseases, a trifle worse off, than towns, excluding London, supplied with river water. (Table XV.)

Secondly. I have shown on the statistics for ten years that the inhabitants of those parts of London supplied with that "priceless boon" the deep chalk water of the Kent Water Company are, as regards health, indicated by the total death rate and the death rate for zymotic diseases, no better off than the inhabitants of those parts of London supplied with what Dr. Frankland regards as the hopelessly polluted Thames and Lea. (Table XVII.)

Thirdly. I have shown that, accepting the carbon and nitrogen determinations of Dr. Frankland to indicate the relative purity (or impurity) of the river water supplied to London, that the death rate is lowest when Dr. Frankland records the organic matter as above the average, and highest when Dr. Frankland records the organic matter as below the average. (Table XVI.)

I was curious to see how Dr. Frankland would meet these statistics. Purposely I presume (for so skilful a debater would do nothing unadvisedly, more especially in a written reply of great care and thought) he has left them alone. And yet not quite alone. "How fallacious," he says, "statistics are." On a small scale I admit statistics are fallacious, but not on the scale such as I have ventured to place before the Society. On an extensive scale the fallacies of statistics vanish. I certainly was surprised, however, to find Dr. Frankland attempting to meet and answer the statistics of years and years, such as I quoted, by the remarks of the late Registrar General on the death returns of a single week. I will not discuss those remarks. I was amused (perhaps pained would be the better word) to read them in an official document when they first appeared, but I certainly never expected to see them quoted as of scientific importance.

Nor do I think Dr. Frankland helps his position by quoting an outbreak of disease at Millbank to prove the injurious effects of drinking river water. I know it was said that an outbreak of disease at the prison was due to this cause, and that it disappeared when well water was substituted for river water. I have before me the whole of the facts and reports bearing on this matter, the truth being that Millbank is and has been since 1874 supplied by the Chelsea Water Company, since which time the medical officer has periodically reported on the excellent health of the prisoners.

I purposely abstained in my paper on river water from discussing the possible cause (germs or chemical poisons) of zymotic diseases. "Chimerical," as Professor Tyndall remarks my suggestion may be, that germs should suffer destruction in water by endosmic action, there is nothing imaginary in the statement—for it will bear without yielding one whit the hard and severe test of extensive statistics, that zymotic diseases do not necessarily result (sewage or no sewage) from drinking river water, any more than they can be kept away by drinking chalk water.

I am quite prepared to undergo the severe cross-examination to which the learned professor of physics at the Royal Institution would desire to subject me on this point, viz., that be the "materies morbi" what it may, it does not increase or multiply in the course of the flow of the river, but on the contrary disappears. The how, or the why, is not so much the point we are discussing as the fact. And I am content to accept Professor Tyndall's explanation: "If germs once communicated to river water do not increase and multiply, it is simply because the conditions necessary to their nutrition are not present."

And here a word on the interesting remarks of Professor Huxley. If he failed in acting as the perfect biological filter some of us might have wished, at any rate we feel sure that no one could have done better or so well. I admitted in my paper that in three diseases at any rate (viz., remittent fever, splenic fever, and pig-typhoid), the evidence in favour of certain organisms (Professor Huxley says they are of the nature of bacteria, but that does not signify) as the cause of these diseases seems established. In these disease germs, says the Professor, "the whole source of damage rests." One drop of Pasteur's solution added to a gallon of water might produce terrible results to the unfortunate water drinker, and we have high authority for the fact that such a quantity would be undiscoverable by any process of analysis. Then said Professor Huxley, "If it be proved that sewage has been once mixed with water, there is a great chance that the excreta of some diseased person may be there too." With that I agree, and I say further that a sewage-contaminated water is absolutely unfit for drinking. But in the face of the statistics I have ventured to submit to the Society, standing as they do uncontradicted, is Professor Huxley prepared to state that because these germs are once in the water they are for ever in the water, and that under no conditions can that water be purified? I submit this fact to Professor Huxley for his consideration:—London has been drinking this water for many years, and yet it is the healthiest large city in the known world. The inhabitants have not been swept away by disease in spite of the water consumed having once received sewage. If disease-producing bacteria must once (as Professor Huxley says they must) have found their way into the water along with the sewage, is it not next to certain, therefore, that nature has provided either a way of escape for these germs so that they should never reach us, or a plan of destruction so that if they do, they should previously be rendered harmless ? And I submit with all confidence this further question to the learned Professor :—Can he give me one single well-authenticated case where a drinking water in which the chemist failed to detect manifest contamination has caused disease ?

Lastly, I venture to submit two or three questions to Dr. Frankland. I do so with some courage, yielding to no one in my admiration for the love of truth that has imbued all his work.

1. Seeing that the dangerous element in a water is entirely outside his ken or detection, what in his judgment is the good of water analysis at all ?

2. Seeing that 100,000 active bacteria, each bacterium being capable of imparting disease, may be present in a gallon of water, and yet be incapable of detection by the most refined of refined chemical processes, under what conceivable conditions can Dr. Frankland (as he is wont to do) report any water to be wholesome and fit to drink ?

3. What are Dr. Frankland's grounds for reporting a water containing 0·1 of organic carbon to be of good quality, and a water containing 0·4 of organic carbon to be of inferior quality, seeing that the first may contain a few odd millions of bacteria which may be entirely absent from the second ?

I must even press Dr. Frankland a little further. On page 220 of the Sixth Report he records some interesting experiments, showing the effect of spongy iron filtration on Thames water. In one sample, for instance, the organic carbon is reduced by filtration from 0·230 to 0·060, and the organic nitrogen from 0·047 to 0·008 ; and there are many other experiments recorded equally successful. And now let me read his remarks on these experiments (page 221) :—" We desire it to be distinctly understood that, although this purification of water polluted by human excrements may reasonably be considered, on theoretical grounds, to be some safeguard against the propagation of epidemic diseases, there is not in the form of actual experience a tittle of trustworthy evidence to support such a view."

I confess myself puzzled with all this. Why, I ask, should "absolute immunity from epidemics of cholera" be promised if only London would drink chalk water, which after all is merely rain-water that must have fallen in certain parts on heavily manured land, but filtered naturally by passage through chalk ; and yet, in the case of this artificially-filtered water, where, judging from analysis, the filtration is far more complete than with natural filtration, Dr. Frankland should say, No, the water after filtration is no more safe than before filtration—" there is not a tittle of trustworthy evidence to show this [filtration] to be any safeguard against the propagation of epidemic diseases."

Given, then, a fairly rapid river that has received sewage in quantity not exceeding, say, one-twentieth part of its volume, is there evidence to prove that, after the run of a few miles, the water of that river will regain its purity, and become wholesome and good for drinking? To this question I answer unhesitatingly, There is undoubted evidence of such purification; and the evidence I have adduced has been of a fourfold nature: physical, chemical, medical, and statistical.

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