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Contributors

Spencer, Thomas.
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THE SEWAGE COMMITTEE OF 1864, AND ITS REPORT AS TO THE PURIFICATION OF RIVER WATER FROM TOWN SEWAGE.

By THOMAS SPENCER, F.C.S.

ETC., ETC.

LONDON:
EDWARD STANFORD, CHARING CROSS.

1865.

Price One Shilling.

"No efficient artificial method has been discovered to purify water for drinking and culinary purposes, which has been once infected by town sewage."

(Vide *Report of Select Committee on Metropolitan Sewage—Session* 1864.)

"Mr. Spencer's is certainly a very powerful agent of purification, and, it may be added also, is the only discovery made in this century of a new substance of really great importance for the filtration of water; it is also one of the most important discoveries as tending to elucidate the principles of filtration or purification, as distinct from mere straining."

(Vide *Report of Jurors to Her Majesty's Commissioners*, 1862, *in awarding their Medal.*)

RIVER WATER

AND ITS

PURIFICATION FROM TOWN SEWAGE.

PREFATORY.

On reading the first passage on the opposite page, quoted from the Sewage Report of 1864, it would scarcely strike one continuing to read on, that that which follows it, from the Jurors' Report of 1862, could also be true, for, though in different language, both equally refer to the purification of water. At the same time, the reader would naturally assume the more recently drawn up passage to be nearer the truth, inasmuch as he could hardly entertain a doubt that the Committee of 1864, before arriving at such a sweeping conclusion, had satisfied itself of the inefficiency of the discovery referred to by the Jurors of 1862. He would therefore deem himself justified in concluding that matters rested pretty much where they did in 1830, when a Government Commission reported, in terms almost similar, as to the hopelessness of the long-sought purification of water.* If given to reflection, he might consider it singular that in the midst of so much advancement in other things, sanitary science had in this respect stood so still. But while indulging this train of thought, he would never imagine that the process thus referred to by the Jurors of 1862—is not only in successful operation, in several parts of the kingdom—FOR PURIFYING RIVER WATER INFECTEDBY TOWN SEWAGE; —but that evidence to this effect when proffered, was not even allowed to be heard before the Parliamentary Committee of 1864.

But what, it will be asked, could have induced such a line of

* "Whatever substances may be employed for filtering, water cannot be deprived of matter held *in solution* by any practical modification of the process of filtration."—*Report of the Government Scientific Commission*, "*On the supply of Water to the Metropolis*," 1830. To this it may be added that all attempts at water *purification*, imply freeing it from sewage matters in solution, whether derived from the drainage of a town or a manured field; without which, the term purification would be a misnomer.

conduct on the part of the Committee; especially, since the terms of its instructions leave no doubt that the subject came within the scope of the inquiry? Unless, indeed, it had been reported a failure, it is difficult to see why the Committee should not have been anxious to learn something of a process endorsed so strongly by the Jurors of 1862; especially as a copy of their Report along with several other reliable statements in its favour were forwarded to the Chairman.

Though it scarcely comes within my province to conjecture why the Committee should have followed this course, yet since it has reported that water containing sewage matter is incapable of purification, I, as being responsible for the invention referred to, felt it incumbent on me to publish the facts connected with the refusal of evidence, which would have shown the contrary to be true.

Having large works for the purification of such water in progress at the time the Report was published, I need hardly say that it affected my interests most prejudicially. Extracts from it were circulated in the local newspapers, and commented on, little to the advantage of the process—which by implication was impugned—or the professional reputation of myself as its author.

Whilst deeming this but a poor public return for the labour and thought I had bestowed on the subject for so many years, I saw at the same time that unless the Committee's statements were contradicted on the meeting of Parliament, a measure founded on them might pass into law. At first I thought of addressing a public letter to the Home Secretary, pointing out the slender evidence on which the conclusions of the Report are grounded; and show also, that to free river water from sewage matters, is a fact already accomplished, and in practical operation. This intention however, was almost frustrated by serious illness during the better part of four months. Indeed, but for learning that the noble Chairman had obtained leave to bring forward a Bill founded on this Report, the idea of publication would have been abandoned. Finding however that the matter would not admit of delay, I determined to throw the circumstances together in a pamphlet form in the best manner I could, trusting it would not be too late to be considered in any legislation that might ensue.

The facts connected with the Committee's refusal to hear the evidence referred to are briefly the following:—

Immediately after its nomination, I had the honour to receive a

message from its noble Chairman, conveying his desire that I should give evidence of my views, as to the disposal of town sewage. I assented, on condition that I should be allowed to do so at a more advanced stage of the inquiry, as I had other engagements which were then pressing. It was so arranged accordingly.

Soon after I received a second visit from the gentleman who conveyed his Lordship's message, from whom I took no pains to conceal my views on the sewage question, as I had gathered from newspaper reports of the evidence, that they differed from those held by the Chairman, and a majority of the Committee. For my own part, as a chemist, I had never been able to see the economy that was to arise from drenching the land with mere dirty water —though termed sewage—and holding a very doubtful pennyworth of fertilising matter in a ton of the raw material. However, whilst expressing readiness to lay my views, such as they were, before the Committee, I intimated that his Lordship, and the witnesses whose evidence I had seen, all seemed to be unacquainted with a process for purifying river water, even if it contained sewage; and which I stated, could be so effectually freed from all such matters, as to be used for domestic purposes. I also said, that having erected works for the purpose, I could furnish the Committee with evidence derived from several of them in actual operation—adding, that though some seven years ago this would not have been possible, yet now, nothing was more easy or inexpensive. Moreover, that were it deemed advisable, I should confine my evidence to this subject. At the close of the interview, I was given to understand, that the following Monday, (I think), would be set apart for my evidence, I undertaking meanwhile to furnish an abstract of it for the use of the Chairman. However, a day or two after, when the abstract was finished, and about to be forwarded, I received a letter from my recent visitor to say that the day he had named had been set apart for the evidence of another witness, in mistake, but that I should be informed when a vacant one occurred.

After this, I heard no more from the Committee, though its sittings were continued until nearly the close of the session.

It is of course impossible for me to say what was reported to the Chairman by the gentleman entrusted to act for him, but from the mode of breaking off communication, one need not go very far into the regions of conjecture to offer a surmise. Doubtless he would be made acquainted with the tone of my heretical views

as to his favourite mode of disposing of the sewage; whilst it would be felt that, were I allowed to furnish adequate evidence—*that sewage water could be effectually purified,* it would do much to cut away the ground for the contemplated measure of next session—as it would be difficult to ignore the fact in the Report. Since its publication, this is rendered more than probable, seeing that its avowed object is, to induce Parliament to pass a measure to prevent municipal authorities from allowing sewage to have its outfall in rivers—and this mainly on the ground that it is "*impossible to remove such matters from the water by any process now known to science,*"—but which is simply untrue.

However, as I had no very strong desire at the time to be heard, on the receipt of the letter I felt quite contented to consign my abstract to the waste paper basket, and so let the matter rest; and in this frame of mind, I should have doubtless continued, but for the subsequent circulation of newspaper paragraphs selected from the evidence, affirming the absolute impossibility of purifying water, however slightly contaminated with sewage.

It so happened that at the time, I was busily engaged in carrying into operation the arrangements for the purification of the water supply to Wakefield. As the processes were devised by myself, and besides were novelties in the district, I felt, perhaps, more than ordinary responsibility. And when the extremely impure quality of the water of the Calder is taken into account—it receives THE WHOLE OF THE TOWN SEWAGE into its comparatively small volume, as well as the waste water from a number of public works, BEFORE it is taken into the Company's reservoirs for purification—the task to supply from it, something like a million gallons of *pure water* daily, was by no means a light one. Still, having achieved success in other places where equal difficulties had to be contended with, I bore up tolerably well against a local newspaper opposition, supported by the Board of Health of the Town. Of course, without the entire confidence of the Water Company, such opposition might have stayed the completion of the works. However, the Directors fortunately, were firm, and I was confident—and, let me add, a successful result has justified both Still, in the midst of our efforts to carry a portion of the works into premature operation in order to meet the untoward circumstance of the drought of last summer and autumn, out came damaging extracts from evidence given on the Sewage Committee by Professor Acland, Mr. J. T. Way, and others, affirming, that all attempts to purify

river water that had once contained sewage matters, and make it fit for domestic uses, would turn out delusive. Now as this was precisely what we were preparing to do at Wakefield, our opponents in the town fastened on these authoritative extracts, and had them reprinted, with approving comments, in the local newspapers. Though I had previously felt myself tolerably able to encounter local misstatements, yet I confess I felt it less easy to meet those coming with the authority of an Oxford Professor, and a well known chemist, surrounded, as both were, with the *prestige* of a Parliamentary Committee.

It was in vain I knew both gentlemen to be entirely in error, and that they spoke of a state of things that no longer exists—others were not to be so easily persuaded.

On finding the mischievous effect of these extracts, I sent a copy of a Wakefield paper containing one of them, to Lord R. Montagu, as Chairman of the Committee; and in a letter called his attention to their inaccuracy, and the injury they were inflicting on me professionally, as well as the Water Company. I also reminded him, that at his request I had prepared evidence on the subject, which it was only fair I should now be permitted to bring forward; adding, that I could furnish reliable proof from works in operation, that river-water could be thoroughly freed from impurity arising from sewage, or any other organic matters—even when held in chemical solution. Week after week elapsed, still the Committee sat and heard evidence, but no reply came to my letter. Having no ambition to appear publicly as the owner of a grievance, I again concluded to let the matter drop, though like most people under similar circumstances, I had some thoughts of writing to the *Times*.

Several weeks after, when the whole of the evidence became accessible, by the publication of the Blue Book, I found the Report stated most positively that all modes of purifying water from sewage matters were to be looked on as delusive. But what made the matter worse, the Report was reprinted in the *Times*, with editorial comments drawing attention to its more prominent features, especially that in regard to the hopelessness of purifying foul river water.*

* That this is the main object of the Report there can be no doubt; for without appearing to prove this much, the Committee could not have recommended any course of action. Besides the measure subsequently brought before Parliament by the Chairman is founded on this allegation.

The effect of all this in regard to myself, was to stay fresh applications to apply the process, whilst some who had concluded to do so wrote to say—they "thought of waiting." One intended work was even countermanded, after estimates and drawings were prepared.

This state of things at length determined me on addressing a letter to the editor of the *Times*. In doing so, I pointed out the inaccuracy of the Committee's Report, and its injurious effects on me professionally; whilst I trusted to his sense of justice to be permitted to show the grounds on which I did so. I informed him also, that the Committee had applied to me for evidence in the first instance, yet subsequently it refused to hear any, *when coupled with the purification of water*. However, as no kind of notice was taken of this letter, I wrote again, but in a less deprecatory tone than before. This time I dwelt on the injustice I was suffering in consequence of its articles, and that allowing them to remain uncontradicted, was nothing short of an editorial misleading of the public, as to a matter of fact. This, as I almost expected, did not quite meet with the fate of the other—but still it was not inserted.

Of course no sane man asks an editor to furnish space in his own paper to contradict himself on general topics, or matters of public opinion, but where he happens to be misled in respect to matters of practical science which can hardly be expected to come within the scope of his personal experience, surely he is bound to afford the means of setting the public right.

Be this as it may, whoever was in charge of the "leading journal," when my letters were received, clearly thought otherwise, for instead of affording them space, on the receipt of the latter one, he penned a disparaging paragraph which appeared a day or two after. I have no means of referring to it whilst I write, but in substance it ran thus:—"Mr. Thos. Spencer writes to us to say that he proffered evidence to the Sewage Committee of Lord Robert Montagu as to the purification of water, which the Committee refused to hear;" but which is a mild version of it.

Such a paragraph, given without the qualification that the Committee had applied to me in the first instance, was calculated to injure me professionally as showing apparently, the estimation in which the process was held by its members. At all events such a use was made of it, for, where I had works in progress, it was

put into the local newspapers by way of showing what was thought to be their chances of success by those deemed most capable of judging. Indeed a Wakefield editor made it a peg on which to hang a leading article to show why the works I had in progress for the Water Company ought not to be allowed to proceed.

In this state of the case, seeing that no better means of redress offered, I determined on making the matter public in its present shape; but it was obvious that, unless accompanied by some reliable statement as to the nature of the process so strongly impugned, the affair would be looked on as a mere personal one between the Committee and myself. I have therefore drawn up a brief account of its discovery in my hands, its physical principles, and a short account of some of its results. The first I have made as little technical as possible, whilst I have confined the latter to a few cases, exhibiting the powers of the purifying material under different circumstances.

What I have stated in regard to the Committee, or perhaps its Chairman, is purely in self defence. Of a professional man, all know, that his reputation is to him everything, but on finding it published in the leading newspaper, that a Committee of the House not only refuse to hear, but in its Report, strongly condemn, by implication, the process he is doing his best to carry into operation —he must naturally feel assailed in the most tender point. Having in the course of a long Parliamentary practice, given evidence before, I daresay fifty Committees, I certainly felt it damaging to have it said, even in this instance, that I was refused a hearing, though, after all, my object was quite as much to set the Committee right with the public, as myself.

PROGRESS AND PRINCIPLES OF THE DISCOVERY.

Before explaining the principles of the process, it may be well to prefix a brief account of the circumstances which led to its discovery.

In 1846 I had the honour to be called on to report to the

Corporation of Liverpool in reference to a new water supply to that town. My instructions led me to make a personal inspection of the water-bearing districts of the neighbourhood, and also of those in the adjoining counties. I found that as a rule, wherever spring water formed the staple supply, people generally complained that it was gradually becoming scarcer, and that what was left, was obtained with more difficulty than formerly; some wells having become dry, whilst others were sunk deeper, which involved additional steam power in getting up the water. Moreover, in rural districts, the ordinary yield of brooks was also said to be not nearly equal to what it was formerly. On extending my inquiries into other counties, a generally similar state of things seemed to prevail, not only as to spring water, but even in respect to that of rivers, especially the smaller streams. Although the average quantity of water in them was not considered to have suffered much reduction, taking the entire year, yet the flow of water in fair weather was in most cases observed to have undergone considerable diminution, as compared with what was formerly observed—whilst floods of short duration, were now of more frequent occurrence.

An additional fact of hardly less importance, applicable both to wells and streams was, that the quality of the water from each source was found to be deteriorating. Apart from domestic observation, showing increasing hardness, wherever previous analyses could be referred to, such deterioration was confirmed, on comparing them with analyses of a more recent date. In short, spring and river water had become harder, whilst river water, *immediately after rain,* was remarked to be much softer than formerly. For this state of things there was no doubt some adequate cause, though it certainly did not fully present itself at first sight. Of course in urban localities much of this might be set down, vaguely, to the increase of town population, but this could hardly afford an adequate explanation, when applied to the deterioration of well and stream water in rural districts, where population and building had not increased.

Ultimately, these inquiries rendered it evident, that this growing scarcity of water was mainly, if not altogether, due to the greater prevalence and efficiency of modern drainage. And looking from this point of view, it is difficult to see what other result could be expected; for, the foundation of all improvement of land commences with thorough drainage, which literally means

—leading the rainfall off it as speedily as possible, into the nearest stream, there to find its way into the next estuary. It was not therefore difficult to see why wells were becoming dry, since the greater part of the rainfall that was formerly allowed to slowly percolate the substratification, and issue at lower levels, as spring water, was now, by means of the universal drain pipe, carried off the surface into the adjoining brooks in the course of a few hours.

The difference between this state of things and that existing formerly may be likened to what one often sees in the country after a shower of rain has fallen on two roofs—the one thatched, the other slated. For hours after the shower, water will be found trickling from the eaves of the thatch, whilst in a few minutes, all trace of moisture is gone from the slates. Not only does this illustrate the diminution of spring water in the wells, but it applies *a fortiori* to the rapidity with which small streams become flooded, after even moderate rain. Hence, those which were seldom known without some flow of water, are now, in even moderately fair weather, all but dry, whilst the effect of a day's rain swells them into comparative flood. This view, as to the bearing of drainage on water supply, was published in my Report of that year; and I may mention that it subsequently met with the concurrence of Dr. Buckland, and Sir Henry De la Beche, to whom it was submitted by the Corporation.

Without here entering on the views I then arrived at as to the effects of increased drainage on the climatology of these islands, from that time I became convinced that:—*the supply of water available for domestic and manufacturing uses is diminishing; and that, in an almost similar ratio, its quality is deteriorating.* Since that period I have had opportunities of examining most of the water bearing districts in this country, which entirely confirm the views I then formed in regard to modern drainage. At present I believe, most who have had occasion to consider the subject, concur with me in these views.*

When it thus became evident that the quantity, as well as the

* I do not mean positively to affirm, that the average general rainfall has become much less, though I believe that if reliable rain-gauge data, for the last fifty years could be referred to—which unfortunately cannot—a considerable diminution would be found, especially as regards the more cultivated districts. And if, as Mr. Glaisher has just shown, our mean annual temperature during the last century has gradually increased by 2 degrees, this would bring along with it increased evaporation, which would of course lessen the available quantity of water.

general quality of the water supply, in cultivated districts was both diminishing and deteriorating, it became equally so that an efficient mode of effecting its purification would be attended with great public benefit. I was therefore induced to commence an investigation of the subject, in the hope that it might terminate in the discovery of some method which would be available on a large as well as a small scale. At the commencement, no process of purification existed beyond mere straining or filtering through sand and gravel, but which does not do more than remove matters visible to the eye, in suspension (charcoal being a temporary makeshift)—whilst the organic impurities held in chemical solution, are left altogether untouched. Still, I felt assured that nature must have some mode of freeing water from these impurities though *in solution*; for in several districts, springs, evidently deriving their supply from impure surface sources—all surface waters being more or less impure—yield comparatively pure water. That is to say, many springs yield water with much less organic impurity than the surface water by which they are known to be fed. Consequently, the stratification through which it percolates must exercise some occult power of purification altogether beyond the mere straining effects of sand and gravel. From the different degrees of purity of spring waters in different districts it is also clear that some stratifications possess this power in a much higher degree than others, though the depth of the wells and amount of percolation may be equal in each instance. Malvern water, for example—even that of the shallower springs—is colourless, and almost free from organic matter; whilst in the deepest well water from new red sand stone, organic matter abounds, though the surface impurity in each case may be much alike.*

A variety of similar instances thus pointed to the existence of a power in some stratifications, which was denied to, or but feebly exercised by others. That this power, however related, was not chemical—in the ordinary sense—but physical, appeared clear from the commencement. Taking its existence for granted, my first step was to ascertain its nature, with the view of imitating

* The purity of spring water, as compared with that found in brooks, always gives rise to a popular belief, in a locality, that the spring water is derived from some district indefinitely distant—but never from the one immediately adjoining. On several occasions I have had to show the fallacy of this belief by direct experiment.

it artificially, if possible; but of course this was only to be arrived at through experiment. A series was accordingly undertaken, in which the mineral constituents of most stratifications were employed along with various samples of impure water. These were continued at intervals, over several years, though without any marked success. Their results, though in many respects interesting, as illustrating the philosophy of filtration, need not be detailed here. Suffice it that most of experiments involved the labour of a chemical analysis of the water—often before as well as after—in order to compare the effect produced by each.

As ordinary oxide of iron enters into the constitution of all stratifications more or less—though its presence was always previously thought to injure water—it was one of the first substances tried, but without its affording any promise of success. Nevertheless, after the trials with most other mineral substances were partially exhausted, oxide of iron again suggested itself, but now luckily under a different aspect. This substance, I may observe, exists naturally in two states of oxidation. The peroxide (ordinary rust), which I had tried at first, is diffused everywhere, whilst the protoxide (magnetic oxide) is, by comparison, found sparingly. When the question arose, as to whether the purifying principle I was in search of might not reside in magnetic oxide, I had good grounds for thinking so, for on looking over my bulky memoranda of the trials, I found that those made with rocky or mineral materials, into the constitution of which the largest proportions of this oxide entered, always appeared to give slightly purer water. To put the idea beyond doubt, some natural magnetic oxide was procured, and after being granulated, its effect was tried upon coloured bog water. Though thus led to expect better results from this experiment than the former ones, yet, at its close, neither the oxide nor the water betrayed any signs of alteration. This was unfortunate; for a variety of reasons suggested themselves favouring the belief that magnetic oxide must be the substance sought. Though I had always found that the water of Silurian stratifications contained little organic matter, it had not struck me, that their iron existed chiefly as protoxide. Still, the negative result of this experiment with natural oxide, seemed to set this view of the case aside; whilst, to obviate doubt as to its purity, it was tested, but without affording any apparent reason for failure on this score. Nevertheless, from time to time, the idea

recurred that the purifying power must reside in this oxide, though possibly in combination with another substance.*

Happening soon after to call on a friend, I saw on his chimney-piece a large and very fine specimen of what is known in mineralogy as Spathose iron ore. Though of a creamy white colour, I knew it to consist of pure protoxide of iron (magnetic), in combination with carbonic acid. It is known to chemists as "proto-carbonate of iron." Here then, was the oxide in combination with another body, and which possibly, might lead the way to the result I had been so long seeking. I certainly had a strong desire to have the specimen I saw, in my laboratory, but on observing me take it up, my friend began to dilate on it, as being the finest piece of the ore he had seen—which did not encourage me to ask him to break off a piece, and let me have it for experiment. However, I contented myself with begging him to procure for me, if possible, some smaller pieces from the same quarter, which he kindly did. I was accordingly supplied with a few pounds which, on being granulated, was tried on some highly coloured bog water. The results of the first experiment were not encouraging—or rather, they were doubtful. That is to say, the water, though it appeared a little less coloured than before its percolation through the ore, yet had acquired a milky turbidity, which made it difficult to say whether the improvement was real, or only apparent, for the turbidity was not removed on subsequent filtration. Notwithstanding the doubtful nature of this experiment, it seemed to afford hopes of better success if tried in mixture with some other material, and with a thicker layer of ore than I had then at command. Soon after I obtained an additional quantity, and on granulating it, impure water was passed through a two feet layer of equal parts ore and sand. Subsequent analyses showed that by this treatment the water was deprived of a small portion of its organic matter. Modifications of this experiment were repeated, with other specimens of impure water, until it became evident, that although the water acquired the turbidity it did at first, yet this ore possessed a feeble power of destroying organic matter—which afforded hopes that a practical solution was at hand. It now seemed reasonable to believe, that were impure

* It was not until some time after that the cause of failure was found to lie in the crystalline structure of natural magnetic oxide. The same applies to magnetic sands which are crystalline, and therefore non-absorbent.

water to percolate through a natural stratification containing even a small per centage of a similar oxide, freedom from organic matter would result, though possibly the carbonate of iron of the ore might be found in its place.

Though I considered theoretical success to be thus far established, still it was evident that a process of purifying water, available for every-day purposes, was not yet arrived at, for the great thickness of ore which appeared to be necessary, even were it sufficiently plentiful, would form an important drawback in practice. Still, when it appeared certain that the purifying property belonging to one stratification and not to another was due to the presence or absence of magnetic oxide, a considerable step was gained. On the other hand, the negative results of the previous experiments with natural oxide alone, led me to believe that it would only act when combined with another body, as it exists in some natural strata. I was then unaware that its failure to act arose from its solid *crystalline* character. But for this misapprehension the discovery would have been available for practice at least twelve months earlier.

At the commencement of the experiments, numerous trials were of course made with various specimens of sand, when I observed, that one of them seemed to give very feeble purifying results, whilst all the others were inert. Long subsequently, when I had arrived at something like certainty that the purifying property belonged to magnetic oxide, it occurred to me to examine the character of the iron in the exceptional specimen. The search resulted in finding several magnetic grains along with some of ordinary inert peroxide—which appeared to account for the feeble purifying property I had formerly observed. Still it was difficult to see why these isolated granules of oxide should produce an effect which the natural magnetic oxide subsequently tried did not. Nor was the difficulty lessened, on finding the chemical constitution of the magnetic oxide belonging to the sand, when analysed, to be quite equal in purity to the crystalline oxide formerly used. However, on examining these grains with the microscope, they exhibited no appearance of crystalline form; and what was apparently of more importance, their specific gravity was found to be less than the grains of the crystalline oxide; whilst another still more important difference was observed, viz.;—that the grains of crystalline oxide were non-absorbent, while those collected from the sand were highly absorbent.

Could it be after all, that in this difference of physical structure resided the cause of the important difference of property manifested by the two equally magnetic bodies? To solve this question, several other specimens of crystalline oxide were procured, but none gave on trial, any better results than that tried at first—though all were equally magnetic. On the other hand, a small quantity of the amorphous oxide procured by means of a magnet out of several hundred weights of the exceptional sand, gave more favourable results. It then suggested itself, that possibly, a plentiful supply of uncrystallized oxide might be obtained from the Spathose ore, for if so, it would only be necessary to drive off its carbonic acid, by applying heat, and a magnetic oxide ought to result. Accordingly, some of this ore was put into a retort and subjected to moderate heat in a furnace. On cooling, a black, highly magnetic, and *absorbent* oxide was found. It was now evident that this was the nearest approach to practical success yet made, for in this experiment I saw the means of procuring the magnetic material to any extent.

At the time, having no very impure water in my laboratory, and being impatient to test the properties of the promising oxide I had just obtained, I added an equal bulk of water to a glass of port wine, and on putting a few ounces of the oxide into laboratory filtering paper, passed through it the diluted wine, when, to my great satisfaction, *it came through nearly colourless.* On making further experiments with this oxide, its remarkable power showed that at length the means were acquired of concentrating the purifying property of perhaps hundreds of feet of the most favourable natural stratifications—and this, too, within the compass of a few inches,—and with the additional advantage of being able to avoid such mineral matters as act prejudicially on the water in percolating through some strata. In short, the means of manufacturing, so to speak, spring water, even on the largest scale, appeared thus within reach.

Though at this stage I had no longer any doubt as to the practicability of the process, several important questions in relation to its future now arose. The chief were, as to how long the oxide would maintain its powers of purification unimpaired, and what was the true nature of those it exercised. To give a detailed account of the experiments made to determine these points, would not only take up more space than is desirable, but would

bring the reader within the technical arcana of the laboratory, which all along, I have endeavoured to avoid. I shall therefore confine myself as nearly as possible, to a brief *résumé* of the more successful results which immediately followed.

Regarding the durability of the material, and the maintenance of its power, consideration showed that the first would greatly depend on the nature of the power. Ultimately, several experiments resulted in showing this to be substantially so. In short, it was found, that the purifying property of the oxide was due to its power of attracting oxygen to its surface, BUT WITHOUT THE SURFACE BEING ITSELF ACTED ON. There could thus be no reason why this power should ever decay; and though chemists are taught to believe that protoxide of iron has a tendency to become peroxide, this was found, in the course of the experiments, to be not only a fallacy, but the *very reverse* of true. It also appeared by the same experiments, that the oxygen, when thus attracted to the surface of the magnetic oxide, becomes CHANGED INTO OZONE; or at least a body having its properties. That is to say, oxygen, whilst under the attractive influence of the magnetic substance, becomes ozonised, or perhaps, more correctly speaking POLARISED, by which it acquires properties altogether different from oxygen in its ordinary state. Just as a needle, when attracted by a powerful magnet becomes in like manner, polarised, and thereby is not only possessed of new properties, but in the language of physics—is no longer a needle, but a magnet, because it has acquired the double property of attracting iron—and pointing north and south. This analogy affords a good illustration of the case, inasmuch as the oxygen when attracted to the surface of the oxide, acquires, like the needle, new properties, and is therefore no longer oxygen, but ozone, which body is after all only polarised oxygen—while, on the other hand, the magnetised needle, is only polarised steel. In proof of this conversion of oxygen into ozone, I cannot forbear from giving below one of the most striking experiments I know of in chemistry. It is however only one of several, equally conclusive, in manifesting the change the oxygen has undergone.*

* Gum Guiacum dissolved in ordinary alcohol gives a deep red translucent solution. Slips of paper dipped in it are used by meteorological observers to discover the presence of ozone in the atmosphere. The reddened papers are hung out in exposed places, and when their colour is found changing to blue, ozone is known to be present; in such case the air is considered to be healthful. Often the slips have to remain weeks before

The question yet to be determined was, as to the part played by the ozone, in the process of purification, its functions in the economy of nature being not then settled. Ever since this body was observed by Schonbein in, I think, 1839, neither its true nature, nor its functions were thoroughly determined by philosophers; at all events, both were regarded as doubtful. Its discoverer had at first set it down as a new elementary body associated, somehow, with electricity, and occasionally met with in the atmosphere. Since the publication of my experiments in 1857, he seems to recognise the body itself to be polarised oxygen. With regard to its leading function in nature, there is now no room for doubt, that it enters into combination with all noxious organic matters, whether in the atmosphere or in water, and converts them into harmless carbonic acid.

Subsequent experiments went to show, that the destruction of the organic matter is effected combustively, by the mere *presence* of the magnetic body. For example:—Organic matters consist chiefly of hydrated carbonaceous matter, in a species of chemical combination with water. These are constantly liable to putrefactive decomposition, in stagnant or sluggishly flowing water; whence are generated those malarious gases which give to both water and air their unhealthy character. Whenever then, such matter, however noxious, comes in contact with the ozone or

even a slight change of colour is observed. For my own part I have never seen them, even under favourable circumstances, to be more than slightly blue. At all events, when they become blue in an atmosphere free from acid vapours, ozone is known to be present. It was whilst in search of a mode of proving the nature of the purifying action of the magnetic oxide—believing as I did that ozone was the agent—that it occurred to me, that if so, it would necessarily change the red guiacum solution into blue. Some was prepared accordingly, and passed through about 10 or 12 ounces of the black oxide obtained from the spathose ore. I do not know that I was ever afforded more real gratification, than when I saw the solution, trickling out of the funnel had assumed — a deep, rich, indigo blue. In this one experiment, more ozone was indicated than a meteorologist could have seen in a lifetime. Another corroborative experiment was made subsequently by dropping water filtered through the oxide into a starch solution of iodide of potassium—another test for ozone,—which also became faintly blue; but the former is by far the more striking experiment. In both instances the solutions in time regain their original colour. These experiments were first shown at the meeting of the British Association in 1859.

polarised oxygen, the two bodies combine, when the organic matter disappears, and carbonic acid results—which gas in water, is one of its most healthful ingredients. It imparts that peculiar freshness and sparkle which is popularly ascribed to oxygen, a gas which is by no means healthful in the stomach—the lungs being its proper place.* The chemist will thus see, that the purifying material acts on organic matter, COMBUSTIVELY. That is—singular as it may appear in a medium of cold water—the noxious matter is destroyed, just as it is in the ordinary process of combustion, inasmuch as the physical influence of the magnetic material forces it into combination with the free oxygen present in the water, whilst carbonic acid is the product in each case. A fuller explanation of the principles brought to bear in the operation would occupy us too long; whilst it would not add much to the information of the general reader to be told that it was due to an occult physical force known to philosophers as CATALYSIS.† A better idea is obtained therefore by showing that the action of the purifying medium on the organic matter is strictly analogous to, if not identical with, ordinary COMBUSTION. I may mention here a result that was arrived at in the course of the experiments, fully equal in philosophical interest to any I have referred to, and which well illustrates the nature of the purifying power. On placing guaicum test papers at some distance from a surface of moistened oxide, and inducing a current of air to pass over it, in the direction of the papers—they were soon found to become blue; whilst on repeating the experiment without the oxide, no change of colour was observed. We may thus see how winds blowing over some stratifications, bring with them increased salubrity as compared with others.‡

* Carbonic acid in chemical language, contains constitutionally two atoms, or equivalents of oxygen, and one of carbon—the two equivalents of oxygen when polarised, may therefore be said to form *one* equivalent of ozone.

† This term was first used by the Swedish chemist Berzelius, to explain certain cases in which two or more bodies are made to combine with each other, by reason of the mere presence of another body, but which itself, remains unaffected by the change. Such is strictly so in the present instance.

‡ The proportion of magnetic oxide entering into the constitution of some of the rocks of the western islands of Scotland for example, is known to be sufficient to very slightly affect the compass needle of passing vessels.

I have now to relate another important step towards practical application. As just stated, the results in verification of the theory were arrived at with magnetic oxide obtained from Spathose ore. Though found in several parts of this country, it is by no means plentiful, and owners of mines value it highly to mix with poorer ores. In short, it became evident that were the process to be dependent on this source for oxide, it would probably never get beyond filters for domestic purication. I was therefore induced to search for some other means of obtaining it. In doing so, I could hardly fail to look towards Cumberland—with an unlimited supply of the purest iron ore, though in a state unavailable for my purpose. I need hardly say it exists as a peroxide (the well known hematite), which, as we have seen, is altogether inapplicable for the purposes of purification. Besides, the ore itself is of so refractory a nature as to have baffled all direct attempts at smelting it alone. Hence it is chiefly used in small quantities for intermixture with poorer ores. Indeed so difficult is the expulsion of its oxygen, with even the highest degree of heat, that I am not aware of any instance of its *direct* reduction to the metallic state by ordinary means. As my object was not to eliminate the whole of its oxygen, but only one part out of three, I thought it might possibly be accomplished in retorts at a comparatively low heat by putting carbonaceous matter along with it, whilst I felt satisfied that if successful in driving about this much off, a magnetic oxide would be the necessary result. The theory of the action I expected to ensue, was:—that the carbon, at a low red heat would combine with one part of the oxygen, and that both would be eliminated as carbonic oxide. Granulated Cumberland ore was accordingly put in an iron retort along with saw dust, and the whole luted up, leaving a small aperture for the escape of moisture —and the carbonic oxide, should it be formed. The retort was put into an appropriate furnace, and on arriving at a low red heat, gas was found escaping, at the aperture. On applying to it a light, I had the satisfaction to observe that it gave the unmistakeable blue flame of carbonic oxide, which showed that the hematite ore was being deoxidised—to the extent anticipated. On discharging the retort after cooling, the formerly inert red ore, was found, converted into a black, highly magnetic, granulated oxide; which for all purifying purposes, was found as powerful as that from the Spathose ore; whilst it was obtained at half the cost, and the supply of the raw material, unlimited.

A number of experiments were now made to determine the most economical manner of applying the heat, its degree, and the period of its duration, &c., &c., all of which resulted in establishing the rules now recognised in the manufacture of the substance —commercially known as, Magnetic Carbide. They further showed that this hitherto refractory iron ore is most easily reduced to metallic iron—after its conversion to magnetic oxide; a result of considerable importance in the manufacture of iron.

I may here state the reason why the new purifying substance is termed Magnetic Carbide. The word "Carbide" is adopted because it is associated with a practical improvement in the manufacture, and to a very slight extent it denotes the composition of the material. Having only a limited quantity of oxide at command for my first experiments, it had to be frequently removed from one filtering apparatus to another. This, and occasional mixing with other ingredients, made it evident that its brittle granules were liable to be reduced to a powder, so fine, as would interfere with the passage of water. It thus became desirable to devise some mode of rendering them harder and less friable in the event of their requiring to be washed. All know that a small proportion of carbon hardens metallic iron into steel. With this analogy in view, it seemed worth while trying whether a similar result might not be obtained with an oxide. I confess my hopes of success were not great, as I knew of no instance of carbon, as such, entering into chemical combination with a metallic oxide. However, the experiment was tried by adding charcoal powder to magnetic oxide, and subjecting both in a retort for several hours to a dull red heat—as is practised in the cementatory process for the formation of steel. At the termination of the experiment the oxide was found to have not only absorbed carbon, but to have lost its objectionable brittleness. In short it had become thoroughly hardened into—what I may be allowed to term—an oxide steel; but without any tendency to further oxidation on exposure to moisture, or, at any temperature less than a high red heat. In short this compound body, is found to be practically, as indestructible under all possible atmospheric influences, as gold or platinum. At first, I was led by some of the experiments to believe its combination with carbon, took place only in atomic proportions. This I now find is not invariably so; nor is it necesssary, because a comparatively small proportion of carbon—from 2 to 3 per cent.—effects the object desired; more

being detrimental to its purifying power. With regard to the word "magnetic," it is prefixed to that of carbide, because, but for the power of the substance to attract oxygen to its surface, the purifying combustion of the organic matter would not take place. The commercial term "Magnetic Carbide" therefore, seems appropriate, inasmuch as it designates its primary essential property, as well as an element of its composition; though according to strict chemical nomenclature its technical name ought to be, Ferrosoferric-carbide, but which is by far too pedantic for a commercial invoice, even were the material invariably to combine in atomic proportions.

In this brief statement I have referred to the more successful experiments only—or rather to those which led more directly, though gradually, to this really natural mode of purifying water; but it must not therefore be supposed they include all that were made in the course of so lengthened an inquiry. At first, numerous failures almost necessarily occurred, and although many of them, as often happens, were highly instructive, an account of them would be out of place here; though some of the most remarkable which have led to results in other directions, will shortly be embodied in another publication.

Simple and obvious as this application now appears, as regards water, it was far from being so at the commencement, or, even for a considerable time after the nature of the principle had been discovered. Most know, that there is often more real labour—and certainly greater responsibility—in bringing an abstract principle into practical operation, than in unfolding the principle itself. In the present instance its nature had not only to be discovered, but it was first necessary to find out whether there was any principle to be discovered—for all authority, as has already been shown, pronounced against its existence. That is to say, it was pronounced impossible to deprive water by filtration, of any substances which it held in chemical solution, just as mechanical authority, not improperly, continues to pronounce in regard to perpetual motion. In short to chemists the one seemed almost as much a dream as the other. Had it previously been recognized that nature possessed an undefined power of depriving water of organic matter, apart from mere mechanical filtration, my labour would have been considerably lighter. I should have made my experiments more confidently, and occasional failure would scarcely have produced the discouraging effect, which it often

did. Before becoming quite certain, that in magnetic oxide resided the principle I was in search of, the failure of an experiment had not unfrequently the effect of leading me to think the object of my search was after all imaginery. It is true I resumed it on falling back on the strong reasons which induced me to undertake the inquiry, though not always with increased confidence. There is a vast difference in searching for land in a direction where previous navigators have assumed land ought to be, than in searching for it in a part of the ocean, where all authorities have pronounced none to exist. Since reaching it however, I am certainly surprised that a natural principle so obvious should have been so long over-looked, not only in regard to water, but in respect to its operation throughout almost every department of nature.

I cannot help observing, that even now, seven years after its publication, so little acquainted are many members of my own profession with this process, that it is spoken of by them as an application of "magnetic oxide" to the *decolorization* of water, and but few seem to have any idea of its capability of destroying organic matter. The fact being so new and unexpected, it is no doubt difficult to believe. But even were a chemist, in order to satisfy himself, to put it to the test of experiment, and use natural magnetic oxide for the purpose—which he is almost sure to do—the experiment will turn out a failure—because such oxides are usually crystalline. Indeed, so feeble are its powers, in this state, that a lifetime might be spent on a natural stratification of it, without discovering even accidentally, that it had any purifying properties over ordinary sand. On the other hand, were he to make an experiment with laboratory prepared oxide, which hitherto has been used only for medicinal purposes, it also would be found practically inapplicable, even where its properties were previously known. Its ordinary mode of preparation is in the first place exceedingly costly—perhaps a dozen pounds were never made at once—but even then, it is only to be obtained in the state of a powder so fine, as to prevent the passage of water. In short, before this discovery could be rendered practically available, everything connected with it had to be devised anew from the commercial preparation of the material itself,—by far the most important part—to the mechanical arrangements necessary to admit of the percolation and purification of the water in one and the same process.

Although the mechanical part was in some degree rendered necessary by the character of the material, it has fortunately terminated in introducing a most economical arrangement, which may be applied with great advantage, to any mode of filtration whatever—and on any required scale.

I have now touched on the main points connected with what I may term, the history of the discovery—for the practical part of which I have taken patents. I have adopted this course advisedly, as the best means of securing its credit, if nothing more. In this country, apart from profit—which I am by no means insensible to—a patent is almost a necessity, even in respect to an invention of this class. Here, we have no public body like the French Academy of Sciences, on whom devolves the duty of reporting to the public as to the merits of inventions, and the claims of their discoverers. Without a patent, such discoveries are often laid hold of by the mere trader, whose first object is to set about making some colourable modification of the process or the material, purposely to secure to himself a patent, which is then advertised as his invention, or at any rate as "a practical improvement" on the original, even should the discoverer's name not be altogether ignored. Modifications of this class, in the majority of instances, are not improvements, but merely pegs on which to hang patents for the purpose of gaining a march over fellow traders; or in other words, to avoid their competition. This, however, is no argument against the existence of patent laws, though it implies a necessity for a modification of them, applicable in such instances. Without the hope of their protection—with the experience I have had—I should most certainly never have undertaken this long and really laborious inquiry; and I am disposed to think few would. After all, nothing so much conduces to call out and sustain the latent energy of a man, as a reasonable assurance that the fruits of his labour shall—in one shape or other—be his own; but were these laws not to exist, what chance of such a result would be afforded him? But for their protection, and for which he pays roundly, he would be left to scramble for his rights, and probably be deprived of them by the prying trader.

How unseemly this contrasts with the care taken by the law to hedge in literary property. A clever stringer together of sentences, without, it may be, one original thought, has his writings secured to him for double the period of an inventor, even should he be a Watt or an Arkwright. The verses of a Close are more jealously

guarded by the law, than would be the patent of a Stephenson. In my own case, for the discovery of the Electrotype,* I took no patent. Within twelve months after the publication of my pamphlet first describing it, at least a score of patents were taken for as many of its applications, and, had I been desirous of practising any one of them, I should have been precluded by law from doing so—even with my own apparatus, as none of the patentees had invented any. But this was not all, for it became the interest of each to have it inferred that but for his "practical improvement," the invention would have remained a mere philosophical toy in my laboratory. Patentees of the most obvious applications of a discovery, to which they seldom contribute anything, generally endeavour to make it appear that it does not emanate from one or two, but is an accidental result derived from several—not the least being their own "practical application." When property has a number of owners, the appropriation of a share of it is felt to be easier than where ownership is confined to one. But had I patented this discovery when I might, I should at least have secured some share of its profit, and have stood no chance of being deprived of its full credit. However, the lesson I was then taught determined me to act differently, should I ever be afforded another opportunity.

RESULTS IN PRACTICE.

It now remains to add a brief account of the progress of the invention since its application to practice. I shall confine myself to a few instances only, each illustrating a case which may be taken to represent several others.

When the qualities of the domestic filters were first made public —few would believe in their capability of purifying water to the extent they professed. It is true, those who tried them, saw that they rendered impure, coloured water, bright and colourless, whilst its disagreeable flavour was entirely gone. Although this was

* The name electrotype, which was at first popularly applied to this discovery, includes the whole scientific art of electro-metallurgy, much of which I have not yet made public.

looked on as most satisfactory, compared with the usual results of filtration, no verbal assurance or explanation, could make it clear that the organic matter *was literally destroyed.* To prove this to demonstration, an analysis by an independent chemist was necessary, but which of course few would be at the expense of having made, whilst assurance derived from the analyses of other waters, was not always deemed sufficient to apply to the particular water which happened to be in question. In short those who went the length of believing the water could be deprived of its organic matter at all, always considered that it must be retained in the layer of filtering media—and that some day, it would either require to be washed out, or, the filter would ultimately stop. To believe that it was *burnt out in cold water,* required more faith than most people were found to possess. Even eminent chemists "could'nt see it," notwithstanding the analogous operation going on in the lungs at every inspiration. They certainly believed that I had discovered that magnetic oxide possessed some indefinite power of oxidising organic matter, which they recognise under the name of "decolorization," and which merely expresses that colour is got rid of somehow: but the fact has not, I believe, led to any inquiry into the chemical character assumed by the bleached matter after its oxidation. Its elimination as carbonic acid seems to have been overlooked, though were not such a transformation of organic matter in constant operation throughout nature, every lake and sea, would long ere this, have degenerated into vast cess-pools. Moreover, the cause of the corelative phenomenon of the same acid being found at the bottom of deep wells, seems also to have been overlooked, for beyond doubt this gas has its origin, not in any deep seated decomposition of the strata itself, but is the result of the combustive destruction of the organic matter belonging to the water, and is brought into existence by the purifying influence of the stratification where it is so found.

For these reasons the filters were not received with unlimited confidence at first, simply because they promised so much—yet when their principle of operation is understood it is difficult to say less for them. Mr. Simpson, of the Chelsea and Lambeth Waterworks, was one of the first supplied with some of the new material for a domestic filter for his own family. Though this is now nearly seven years ago, it maintains, as stated by this gentleman, its purifying power undiminished, and has not undergone anything

in the shape of cleansing or disturbance—so also in regard to the one used in my own house for a still longer period. Though at present, instances might be multiplied by thousands in respect to the success of the domestic filters—without a single exception—yet I have no doubt many who possess them, retain an idea that the day must come when they will stop, or require cleansing, such being their experience of all former modes of filtration. Still, it is admitted that these retain their power longer than any others.

A few words are now necessary, as to the conditions under which this filter destroys organic matter. Most natural waters, however pure, possess some traces of this matter, but where a quantity exists not exceeding half a grain to the gallon, if of *vegetable origin*, it is harmless, especially if it imparts neither colour nor taste. But water containing from one to three grains or upwards, if arising from river impurities, is open to great objection, for such quantities give rise to the putrefactive gases which is the source of malaria. However, to free even the worst water from every particle of organic matter that may exist, is a mere question as to the quantity of the purifying material. If for example, it be sought to deprive water of three or four grains of organic matter in a gallon, a thicker layer of carbide is necessary, than for water containing a lesser quantity. At the same time, it is seldom, or never absolutely necessary to destroy the whole of it, for a trace not exceeding half a grain in a gallon does no harm. If the organic matter in water, containing from one to three grains, be reduced by the carbide to about half a grain, every purpose is usually answered, in all but rare cases. It happens too, that the smaller and really harmless portion, is always the most difficult to be got rid of. That is to say, the last isolated half grain requires more magnetic material to reach it, than a grain does, when accompanied by a larger quantity. Nor is this peculiarity without many analogies in chemical operations—the *last* quantity being generally the most difficult to reach. Evidently, the smaller quantity exists under different chemical conditions; or rather, it is held in the water by stronger affinities, as portions so minute are seldom affected by the decomposing influences that generate foul gases in water containing larger quantities.

As a rule, the quantity of carbide put into the domestic filters, which are generally used intermittently, is found sufficient to deprive ordinary water, such as supplied by the Companies, of all

deleterious matter; still extreme cases require correspondingly increased quantities of purifying material, regulated of course by the impurity of the water. As the same applies to the largest filter beds, an extra quantity is always recommended for them in the first instance, to meet the excesses of impurity, which often occur in river or gathered water. At the same time, the quality of even the best spring water is never found to be absolutely constant throughout the year. Though the whole of the organic matter contained in a given water may therefore be converted into carbonic acid, yet, where the quality of purifying medium happens to be inadequate, this matter is only partially destroyed. However, from having made professional analyses of the water supplied to most of the larger towns in this country, I can state with confidence that the worst quality I have met with, can be made perfectly wholesome by means of a moderate quantity of this material. Nor are its powers confined to destroying organic matter in its ordinary state, for it exercises corresponding effects on gaseous impurity. In short, by its singular property of forcing organic exhalations into combination with oxygen, even the worst are rendered harmless. I may mention that it exercises an equally salutary effect on the volatile, but highly deleterious compounds of raw grain spirit, and is now used by some rectifiers for this purpose. Moreover, its magnetic power has the effect of depriving chalybeate waters of their iron, the oxide resulting from the operation being subsequently got rid of by a simple arrangement of flushing upwards, which in no way disturbs the magnetic material in the filter bed.

For the first few years I avoided publishing statements of its durable qualities until more fully warranted by longer experience. Not that I entertained any doubts on this score, but to prevent the appearance of rash, or hasty conclusion. Its claims were not therefore as much before the public as they, perhaps, might. It is true, that some years ago, I proffered its use in a letter to the Chairman of the Metropolitan Board of Works, as a means of getting rid of the sewage nuisance, without affecting the volume of the river. After providing for every probable contingency, of flood and rainfall, the object would have been attained at less than a sixth of the cost of the plan now being carried out, whilst the navigable depth of the Thames would have remained unaffected; I having always considered the inevitable diminution of the river's volume, to be the great objection to this plan. However,

when my proffer was made, I had hardly a hope of its being entertained—knowing how such matters are treated at public Boards. Generally they are referred to the engineer or his consulting adviser, but, as a rule, no salaried officer recommends anything of importance decidedly new, if not his own—especially if its author happens to be a contemporary. Hence, public bodies are seldom induced to apply a novelty, unless forced into doing so by the public. Though on terms of intimacy with several members of this Board at the time, I took no pains to enlist them in support of my plan, assuming that when an offer is made to a public body to solve an important difficulty, by one not altogether unknown, it becomes its duty to institute, at least some inquiry. Though, as I expected, no notice was taken of my proffer, I still consider it was my duty to make it—but not to repeat it. Be this as it may—I have since thoroughly purified worse water than that complained of in the Thames at the time.

This process has now been in operation above seven years, during which period it has been tested on most qualities of impure water, and in no instance have I known it to fail. The first town supply for which it was tried on a large scale, was Southport, in Lancashire. As it happened, the works were constructed before the water to supply them was obtained. That is, the Act was passed, land purchased, an engine-house built with a pair of engines, whilst the mains and supply pipes were laid down, all before sinking the well from which the water to fill them was hoped to be obtained. However, the well was sunk, and plenty of water was found, but of a quality that it was hardly possible to supply. In short, in addition to its organic matter, derived from the adjoining peaty district, it was found to be permanently chalybeate, and hence totally unfitted for domestic or culinary purposes. Though sufficiently bright, it turned tea and brandy ink-coloured, and on slight heating, left a copious brown sediment. In fact, most of the inhabitants refused to have it laid on to their dwellings. Under these circumstances, I need not say the pecuniary affairs of the Company were at the lowest ebb. I was called in by the shareholders—and shortly after the purifying arrangements were in operation, a salutary change began to take place. House after house was added to the Company's rent-roll; and in spite of a few grumblers, who at first did not seem to relish the idea of what they called "Doctored water," the inhabitants soon became loud in their praise of its purity. More need not be stated than,

that at present, the Company divides 10 per cent. among its shareholders, and, as it happens, while I write, a bill is pending in Parliament, promoted by the municipal authorities of the town, seeking powers to purchase the works from the Company.

The same system of purification has been in operation in Spalding (Lincolnshire) some four or five years, with equal success. Previously, there was no public supply at all. People obtained water where they could. Some from impure shallow wells; others, from the rain caught on roofs; whilst the bulk, who had not wells, in dry weather obtained it from those who had. No supply of even tolerably pure water was to be had at any reasonable distance from the town, so as to induce a Company to embark capital with any chance of its return. At the same time, the great ditches in the neighbourhood contained an abundance of highly coloured drainage water from the adjoining fens, but no one dreamed of recommending a supply from these sources for the town. The ague, which the inhabitants of these districts are so much liable to, has always been attributed to the use of this water.

Soon after my system of purification became known to Messrs. Easton and Amos, the eminent engineers, they at once saw it would be the very thing for Spalding. Experiments were accordingly made to purify the coloured fen water, which were highly successful. This led to the formation of a Company to supply the town from this source, when purified. Already the Company pays a handsome dividend; and above all, the inhabitants are perfectly satisfied with the purified drainage water. In proof of which, I may mention that, on the formation of the Company, many urged the promoters to go to springs at a considerable distance for the supply. However, the distance was too great; consequently, the drain water, after purification, much to the annoyance and fear of many, became the recognised source of supply to the town. Last year, during the extreme drought, the surface drain water began to fail, when the supply became dependent on a few springs that still held out, at its bottom. The alteration that consequently took place in the quality of the water was soon discovered by the inhabitants, who began to call out for a return to the purified soft water of the formerly much abused drains,—on which they had been thriving healthily for the previous four years.

A considerable time after the completion of the works, Messrs. Easton sent me the following letter:—

Grove, Southwark, S.E.
24th October, 1862.

Dear Sir,

We have great pleasure in bearing testimony to the value of your Patent Magnetic Carbide for the purposes of filtration. We have applied it on a considerable scale, at the works recently constructed by us for the water supply of Spalding, in Lincolnshire. The supply is derived from streams, flowing through the fen lands, which are tainted, to a very considerable extent with organic matter; so much so, that the water before passing through the carbide, has a very perceptible taste. No other water could be obtained without an outlay which would have rendered the carrying out of works for Spalding commercially impossible; but by the use of your Carbide in the filter beds, the water previously described is rendered perfectly pure, tasteless, free from organic matter, and in every respect suitable for the supply of the town.

We have also had considerable experience of the domestic filters, and have always found them successful in removing organic impurity.

We are, dear Sir,
Very faithfully yours,
EASTON AMOS & SONS.

Thomas Spencer, Esq., F.C.S.. &c.

The Spalding works are in constant operation, and the water continues to give, at the time I write, as much satisfaction as it did at first, whilst no change, or renovation of the material has been found necessary. It is most important to state, in regard to this process, that the first is its only cost. In no instance have I been subsequently called to supply material to supplement that at first laid down. That some waste may occur is true, but not necessarily so, for being made to rest on a layer of fine gravel, and surmounted by another of fine sand to intercept suspended matters, it is thus placed beyond the reach of ordinary casualty. Nor does it depreciate in quality or activity, by the longest usage. That supplied to Southport over six years ago—where the water has to be freed from a large quantity of iron as well—acts now as vigorously as it did at first. So in every case within my experience. Moreover, no physical reason can be assigned why this state of things should ever alter, the magnetic substance being insusceptible of change by moisture, or atmosphere, whilst it contributes no part of itself to the process, beyond what Professor Clark has well designated, its "moral influence," which, in short, is Catalytic action.

I have already referred to the purification of the Wakefield supply, and the quality of the water of the Calder, from which it is taken. The volume of this river, near the Company's works,

does not very much exceed that of a canal. Before being pumped into the subsiding reservoirs, the river receives the whole of the sewage water of the town from the main drain, as well as the refuse of the public works on its banks. But, when I say that the *outfall* of the town sewage is at a short distance *above* the *inlet* to the Company's works, the quality of the water may be inferred, without saying more.

It is but right to state, however, that the river water is itself relatively soft, and but for its excess of organic matter, arising from the sewage, &c., is, in other respects, a most desirable water for a town supply. But had softer water been procurable in the surrounding country, the river supply would have been abandoned long ago. All long however, I assured the Company of the certainty of its purification in the event of no other source of supply being found. Long previous to determining to have recourse to my process, the directors had repeated analyses made not only of the Calder water, but of every water deemed at all available in the district, both before and after purification through a experimental carbide filter; but the results were in no case, nearly so satisfactory, as those with the river water. At length I suggested that a purified sample should be given for analysis to an entirely independent chemist, but without communicating the source, or any of the circumstances connected with it. Accordingly, a sample purified by Mr. Sykes, the Company's Manager, was sent to Mr. D. Campbell, of London, for analysis, the results of which are given below.* Though originally more

* Mr. Campbell's Analysis in 1863.

Silica	0·60
Iron	0·25
Carbonate of Lime	1·49
Sulphate of Lime	4·34
Sulphate of Magnesia	3·54
Chloride of Potassium	1·11
Chloride of Sodium	1·34
Carbonate of Soda	1·65
Total mineral matter in a gallon	14·32 gr.
Organic matter.	0·48—*or less than half a grain.*

I believe Mr. Campbell stated in his report, that if this was a sample of river water, it was freer from organic matter than any he had analysed. My own analysis of the same purified water, at the time, made the organic matter even less thana quarter of a grain in a gallon—a bare trace, in fact,—whilst that of the river whence it was taken, contained 3 grains.

impure than any water I ever found in the Thames, even near the bridges, yet after its passage through the carbide, it was found to contain less than half a grain of organic matter; showing a degree of purity, in this respect, beyond that of any town supply I am acquainted with in this country.

On its becoming known that the Company had at length determined not to abandon the Calder supply, but to erect works for its purification, a violent outcry arose in the town as to its utter impossibility, but which, although I was the chief object of animadversion, was perhaps, not unnatural. Last summer and autumn, whilst the works were in progress, the continued drought rendered the river water worse than it had ever been remembered. To meet these untoward circumstances in the best way possible, the works were hurried forward, and two out of the three intended filter beds were hastily opened for public use. Notwithstanding the incomplete state of all our arrangements, and the unprecedented impurity of the water, the change in its quality was at once most satisfactory. This however, did not silence the opponents, for although they were unable to deny that improvement was effected, they contended it was altogether delusive, on the old ground, of the impossibility of removing organic matter—*in solution*. About the same time the evidence and report of Lord Montagu's Sewage Committee was published, repeating, but with more force, the same, now easily, demonstrable fallacy.

To satisfy the town that it was not deluded by the appearance of the purified water, it was determined to have it analysed by several metropolitan chemists. Four samples were taken at the reservoir, simultaneously, by a well known medical gentleman, who, if anything, held opinions adverse to the possibility of purification. The samples were conveyed to as many chemists here, but, with the exception of myself, none were made acquainted with any circumstance connected with them, even the town from which they were sent. This was deemed advisable, as one of the gentlemen selected had already given evidence before Lord Montagu's Committee, that such water could not be purified.

The reports received in due course, were highly satisfactory; and though independently made, they concurred in showing the water to be, in all respects, purer and softer than the best filtered water supplied to the metropolis. They were published *in extenso*, in the local papers, accompanied by the following letter from Mr. Sykes, the Company's manager:—

To the Editor of the "Wakefield Examiner."

Sir,

Public attention having been called to the water supply of towns from rivers, the Directors of the Wakefield Water Works have thought it desirable to place before the public recent analyses of the present supply of water to this town, made by four of the most eminent analytical chemists in London, after the new works for the filtration and purification of the water were got into partial operation.

For their own satisfaction, the Directors had the samples taken at the works, and immediately sealed by a well-known gentleman residing in Wakefield, but who has no connection whatever with the Company.

With the exception of Mr. Spencer, none of the chemists were made acquainted, in any way, with the locality or circumstances connected with the samples; and their first intimation on these heads will be through the medium of your columns.

Let me add, the works are not yet completed, but when so, the Directors believe that the water will be still better than at present. Even now, according to the analyses appended, the Wakefield water supply is, to use Dr. A. S. Taylor's words:—"A good and wholesome water, fit and proper for the supply of a town population. Compared with the metropolitan supply, it is quite equal to the best samples, and superior to some which I have examined. The amount of organic matter, which appears to be of vegetable nature is small; it does not exceed the average of that which is found in good river or spring water."

The moot point as to whether sewage matters can be removed from river water appears to be satisfactorily disposed of at Wakefield by these analyses.

Yours faithfully,

JOS. SYKES,

Secretary and Manager to the Wakefield Waterworks Company.

Wakefield, *Dec. 8th*, 1864.

Though the reports referred to by Mr. Sykes are too long to be given here, I must refer to one from Mr. Way, who had stated that water containing sewage matters cannot be purified. He was therefore selected by the Company to analyse a sample of the purified water; but had other chemists given similar evidence on the inquiry, samples would also have been sent to them, as no doubt their reports would have concurred with the others. After stating its hardness, and the quantity of organic and mineral matter, Mr. Way says:—"; but judging from the results obtained, I should consider it a water well adapted for ordinary domestic use. It contains less total solid matter thanthe average of waters supplied to the metropolis. The organic matter is not in excess of such average, and in smaller proportion than is found in several of them—especially in times of drought—whilst its hardness is from one-half to two-thirds of that of the London supply."—(*Way's Report.*)

This passage is not cited with a view to exhibit want of consistency in this chemist, but simply to show, that when giving his evidence a month or two previously, a means of purification existed with which he was practically unacquainted. Be this as it may, the people of Wakefield continue to be satisfied with the result of the purified water; and when the works are completed, which will be in the course of the present year, no doubt their satisfaction will be increased.

In regard to the general application of this principle to the purification of ordinary town supplies, I have no more doubt of its ultimate adoption in all water works, than I had on first seeing the Liverpool and Manchester Railway, that such would supersede all other known modes of communication. The two strong points in its favour are, first, that the mode of purification is identical with that employed by nature; second, that the raw material abounds, cheaply and plentifully everywhere; and that the protoxide of iron being the most magnetic of all oxides, there is as little probability of its being superseded, as there is that coal will be superseded by some other fuel. A third might be added, that town populations will not long be contented to use impure water, when the means of obtaining it perfectly pure are known to be so easily at command. Companies, if left to themselves, seldom take the initative in adopting improvements that involve cost, unless with an absolute certainty of increasing their revenue.

I speak advisedly when I say, that by means of this process, not a grain in the gallon of organic impurity, or even a tinge of colour, need be found in the water supply of any town in this country, no matter from what source it may be originally derived. Were it necessary to take the whole metropolitan supply from the Thames, between London and Westminster Bridges, it could be delivered free from organic impurity—even were its water to maintain the whole of its former quantity of sewage. At the same time, absolute freedom from gaseous impurity is also insured, as a result of the instantaneous oxidation of all such products of putrefaction.

In regard to Water Works, where sand filter beds are already in operation, the expense of its application to them is comparatively trifling—considering that pure water is the result.

Where, on the other hand, filter beds have to be made, the new system effects a considerable saving of material and construction, as compared with those erected according to the former engineering plans. The usual thickness of filtering material

for instance, put into filter beds, is 6 feet 6 inches, which is made up of graduated layers of well selected sand and gravel. By the mechanical arrangement I have introduced at Wakefield and elsewhere, I find 3 feet 6 inches of material, including the purifying layer, to be ample, and were it necessary, 2 feet could be made sufficient. I need hardly say, therefore, that along with this saving of material in the beds themselves, is included a saving to the extent of more than a yard in the height of their walls. Filter beds may therefore be erected *over* covered reservoirs—as at Southport—the weight to be upheld by the arches beneath being thus much diminished, to say nothing on the score of saving in respect to the area of the land occupied by the works.

As yet I have cited no example of the power of this new material to purify sewage water *before* entering streams, and consequently in a very little diluted state. Nearly five years ago the residents on the banks of a stream in Essex obtained an injunction against the authorities of the Military Depôt at Warley, to restrain them from polluting the same, by reason of a most offensive water-closet influx from the barracks. In this dilemma, the engineer of the department, Mr. Digby Wyatt, applied to me. I confess that on seeing a sample of the fluid with which the material was required to deal, I was not sanguine as to the permanency of the result.

Previously, it is true, I had thoroughly purified river water containing sewage, but this was something more than mere foul water. However, the successful nature of the result is better told in the following statement of Mr. Wyatt himself; and to which I may add my belief, that nothing since has been heard of the injunction.

India Office, *Nov.* 1, 1862.

I have much pleasure in stating that my experience of the operation of the Magnetic Carbide applied by Mr. Spencer to the *purification* of water is of the most favourable description.

Some years ago it was my duty to remedy, if possible, the deleterious effects alleged to be produced from an overflow of sewage water from manure tanks constructed at the Honourable the East India Company's Depôt at Warley. I took great pains, and made various experiments towards attaining the desired end; visiting Aldershott and other localities in which attempts had been made, with more or less success, and almost invariably with great cost, to attain the same purpose.

From reading a communication in the Reports of the British Association I was induced to put myself in communication with Mr. Spencer, and procured from that gentleman sufficient Magnetic Carbide to experiment upon. I had some of the sewage water sent up in sealed bottles, and can only describe the action which took place on passing some of this water through

a small glass filter which I caused to be made—as *magical*. Not only had the foul water become perfectly pellucid, but it had entirely lost every offensive property, so far at least as I can judge.

The satisfactory nature of repeated experiments led me to design and construct extensive tanks, which I caused to be filled in layers with various filtering media, including strata of the Magnetic Carbide. Through these tanks for many months I passed the overflow drainage from a barrack establishment at that time of upwards of 1,200 men, the overflow passing off in the state of clear and inodorous water. With the exception, that under the action of thunderstorm, my filtering surface proved of insufficient extent to filter as fast as the head water accumulated, and that an occasional disturbance of the strata took place at the times of such floods, I am aware of no drawback of importance to the use, under similar circumstances, of tanks such as those I constructed at Warley.

M. DIGBY WYATT,
Architect, Assoc. C.E., &c.

It may be well to say here, that though this purifying process does not in itself profess to economise sewage in regard to its application to land, yet it certainly claims to free river water from this impurity, where its influx is inevitable—and which is undoubtedly so, throughout a large extent of this country, especially wherever the water-closet system has been introduced.

Notwithstanding all that has been said, both in and out of Parliament, regarding legislative enactments to interdict the natural flow of such matters into streams, on the ground that their waters thereby suffer "irreclaimable pollution," I submit that such would not only be based on an erroneous estimation of existing facts, but—if not inoperative—the machinery for the proposed measures would give rise to a more vexatious state of things than can be easily imagined by those who have not thought over the matter deeply.

Nor should it be forgotten, that all the newspaper outcry as to the great benefits that are to result from turning the sewage on the land, has arisen from some, confessedly erroneous, estimates of its value, which were made by Mr. Way about 1850. Over and over again has this chemist repudiated his analyses of this period, but still this has not prevented their being cited on every occasion:—"*These analyses of mine*" (says this gentleman, in his examination on the Sewage Committee of 1864) "*have been continually used in this discussion, in spite of the fact that I have over and over again repudiated them as indicating at all the value of the London sewage.*" The truth is, the samples were furnished to him from exceptional places, and not as they ought to have been, from the outfall of the

sewers. About the period in question, I also had to make analyses of the metropolitan sewage—but as my samples were taken at a sewer's mouth, their results differ much from those of Mr. Way. Had mine, however, given the larger and more flattering estimates of its value, they would no doubt have been cited by vestry authorities everywhere; on the other hand, if Mr. Way's samples had happened to have been taken from where mine were, his results would have been consigned to the same popular oblivion that mine are.

Few would more gladly see our rivers and streams freed from pollution than myself; but I greatly doubt whether it can be effected by Parliamentary enactment. Still, my own experience goes greatly to show, that the refuse water from manufacturing processes acts far more injuriously,—especially in regard to fish, —than what I may be allowed to term—legitimate sewage. Few that have not had opportunities of investigating this subject experimentally would believe how soon sewage loses its injurious character *in a flowing stream* of even moderate volume. For a Parliamentary inquiry in the summer of 1862, I had to investigate this question in relation to the streams included in the watersheds of the Mersey and the Irwell. The numerous tributaries to these rivers extend over a considerable portion of three counties, and on their banks is, perhaps, the largest manufacturing population in England. Although all the streams flowing through these Lancashire and Derbyshire valleys serve in reality as common sewers for this large population, by nothing was I, and the eminent chemist by whom I was accompanied, so much struck as by the rapidity with which a comparatively short flow deprives foul water of its offensive character; and we were well provided with the means of putting our observations to the test of chemical experiment, on the spot. I am far from asserting, that water thus deprived of offensiveness is fit for domestic use, but I do say that it soon becomes harmless, and, where required, can be easily rendered as pure and wholesome as spring water.

I have now only to add a few words in conclusion. To meet an emergency, this pamphlet was thrown together without much consideration. Though my object throughout was to be brief, I find it has outgrown my original intentions. I find also, a topic or two touched on, perhaps unnecessarily, whilst there is a predominance of egoism, which, without an appearance of affectation, I hardly saw how to avoid, when speaking of the progress of the invention in my hands from its birth to its present degree of

maturity. In the early days of geology, a celebrated geologist and highly literary man, Dr. Maculloch, was discovered to have written a not unfavourable review of his own work—the *Geology of the Western Islands of Scotland.* On being accused, the Doctor very frankly admitted the fact, and at the same time asked his accusers, "who would have done it if he had not?" The book (an excellent one, by the way) had lain for a long time unnoticed on the shelves of the publishers, who could find no one who felt himself competent to the task of reviewing it—so the Doctor did it himself, and very well and fairly it was done. So in my case; if I had not given some account of this mode of purification, who else is there that would have done so? At all events, it has been now some seven years before the public, and as yet, no one else has—until at length its very existence—as we have seen—was becoming ignored, even in the highest places. That I have performed my hasty task "well and fairly" I can hardly hope—though it may possibly meet the occasion which has called it forth, and afford, in addition, some information on a subject which is every day becoming of more and more interest to all.

Euston Square, London, *May*, 1864.

ADDENDUM.

I subjoin the following letter from a Wakefield paper, as showing the sort of evidence on which that part of the Sewage Report is founded which relates to the non-purification of water.

To the Editor.

Sir,

As the adviser of the Wakefield Water Company, in the operations now in progress to purify the town supply, you will probably allow me a few words in reference to a lecture on water, delivered in the Church Institution last evening.

The lecturer, in remarking on the impurity of river water, arising from the influx of sewage, more particularly in regard to the Calder—after stating his own belief in the impossibility of freeing water from these impurities, cited a corroborative passage from the Report of the Select Committee of the House of Commons, which sat during the greater part of last Session. My object in writing, is more to show what value ought to be placed on the Report he quoted from, than to find fault with the lecturer himself. This Report states very positively, that:—there is no known method by which water, once contaminated with sewage, can be purified. I need hardly say, that opinions given in Parliamentary Reports, founded on evidence of witnesses supposed to be well versed on the subject to which they bear testimony, always carry with them considerable weight. In the present instance, the statement

above, is made to rest, by the framers of the Report, almost entirely on the evidence of Professor Acland, of Oxford, and Mr. J. T. Way, the chemist, In giving evidence on a subject of so much importance, on which a legal measure is intended to be founded, one would naturally suppose that the witnesses would have based their testimony on some personal experience, or knowledge of the recent discoveries of science, or, that the Committee would at least have required some such proof. Nothing however of the kind was required. You will see from what follows, that the evidence is a mere reiteration of popular belief as to a state of things which no longer exists.

To prove this, it will only be necessary to quote the following portion of the evidence of Professor Acland:— (The questions are put by Mr. Caird, a member of the committee.)

"3516. Is it not possible (asks Mr. Caird) that the volume of water might be so great as to entirely do away with all injurious and noxious effects to health, with regard to such 'excreta' going into the river?—Theoretically that may be so.

"3517. You specially refer in this memorial to the basin of the Thames; have you yourself observed the condition of the river above Staines, or between Walton and Staines, which is below every one of the towns mentioned in this memorial except Richmond?—I have seen it, but I have made no scientific observations upon the condition of the water.

"3518. You have not examined it chemically, have you?—No.

"3519. Have you examined it superficially and looked at it and seen the condition of it?—I could make no scientific report upon that; I have pulled down there on purpose to see it.

"3520. Have you not found it looked pretty pure to the eye?—It is three or four years since I was at that part of the river, and I could give no answer to that which would be of any scientific value." (*Blue Book*, p. 152.)

On reading this, one may fairly ask, what value ought to attach to evidence in which the witness thus admits his inability to arrive at a reliable conclusion. It is but right to say, however, that elsewhere he does not pretend to higher information on the point than is implied in these answers to Mr. Caird, but then, such evidence ought not to have been cited in the Committee's Report.

A few words now as to that of Mr. Way. Without unduly disparaging the other witnesses, Mr. Way, being an analytical chemist, is the only one who appeared on the inquiry, professionally competent to give a scientific opinion of any value. On turning to this gentleman's evidence, I find it commences thus—the question being put by the noble chairman:—

"4702. A case was mentioned to this Committee of a trial at Uxbridge, with regard to allowing the sewage to run into the river; and from the Report of Vice-Chancellor Wood, it appeared that he rested chiefly on your evidence; for you stated that science had discovered no means of perfectly purifying water which had once been polluted by sewage; is that statement correct?—At this moment (says the witness, in reply) I cannot recal the circumstances exactly. I recollect something about the case, but it has never been brought before my attention since that time, and I was not aware that the decision of the vice-chancellor was founded on my evidence. Of course it must be so if the noble chairman states it; but if I stated that the sewage could not be perfectly purified by any known chemical means, I should repeat that statement."

On reading this, I leave it to any acquainted with evidence to say, whether a question to a witness, so put, and so answered, would be accepted as evidence in a court of law.

Thus, we have a species of loose testimony which would certainly not be permitted to weigh in a case between two private individuals, deemed sufficient on which to found a law to bind a whole kingdom. Farther on, it is true, Mr. Way repeats the statement he was thus *led* to make at the commencement, but it is clear that in so doing, he intended to speak of the inefficiency of ordinary sand filtration, including deodorisation with lime, as well as chloride of iron—all of which, I am equally aware, are delusive as purifying agents.

On reading this chemist's evidence *in extenso*, I wrote to him to inquire, whether, previously to giving it, he had made any experimental trials of my mode of purifying water, which he must have known I had already applied—and was applying, in several instances?

Though I am not on unfriendly terms with Mr. Way, and, as a member of my own profession, have had occasion to correspond with him before now—and although my letter is several months old, he has not as yet ventured on a reply. Nor can this be set down to his not having heard of the mode I referred to, as I have more than once had casual conversations with him on the subject. Meanwhile, I much doubt whether he has had any opportunity of making himself acquainted with it in practice, and therefore, in his evidence, could hardly speak of what was so little known to him. But what I object to is, that he should be quoted as an authority in condemning, by implication, a mode of purification with which he has had no practical acquaintance; and so also with regard to Professor Acland and the other witnesses. At the same time, the Committee, or its Chairman, shut out evidence of a mode of purification calculated to obviate the evil for which a main object of its appointment was, to suggest a remedy.

Yours very respectfully,

THOS. SPENCER, F.C.S.,

LONDON, *Dec. 2nd*, 1864. *Chemical Engineer.*

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