AN

INVESTIGATION

OF

THE PROPERTIES

OF

THE THAMES WATER.

BY WILLIAM LAMBE, M.D.,

FELLOW OF THE ROYAL COLLEGE OF PHYSICIANS.

DEDICATED, BY PERMISSION, TO

THE RIGHT HON. ROBERT PEEL,

HIS MAJESTY'S PRINCIPAL SECRETARY OF STATE FOR THE HOME DEPARTMENT.

" For not to know at large of things remote
" From use, obscure and subtile, but to know
" That which before us lies in daily life,
" Is the prime wisdom."

MILTON.

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RIGHT HON. ROBERT PEEL,

HIS MAJESTY'S PRINCIPAL SECRETARY OF STATE FOR THE HOME DEPARTMENT.

SIR;

In presenting to you (which I do with feelings of unfeigned respect) these inquiries into the properties of the Thames water, I shall take the liberty to offer to your consideration a very few general reflections connected with the subject. I hope it will appear that in the course of this inquiry something has been gained for the advance of general science, and towards the acquisition of correct notions concerning the real constitution of the impurities of water. But it must be confessed that our knowledge of this subject, considered even as a chemical question, is still miserably defective, and that to supply the deficiency, the labour and science of the most intelligent chemists that the country can supply are necessary.

I consider it, however, to be a certain and almost a demonstrated fact, that all the impurities usually found in common water are the effect of a common cause; namely, the putrefaction of matter either diffused over the whole surface of the soil, or accumulated by adventitious and local circumstances. If this be true, it is impossible that the noxious matters complained of can be confined to the water of the Thames, but it must be participated by the whole of the supply of the metropolis, unless it can be shewn that any particular portion of this supply is exempted, by peculiar circumstances, from the causes of contamination.

I must take leave, therefore, humbly, but earnestly, to suggest to you the necessity of pursuing and extending the inquiries, instituted almost wholly by the zeal, spirit, and activity of one meritorious individual—Mr. John Wright. The consequences of such an inquiry will prove, in my opinion, that the quality of the whole of the supply, from whatever source originally taken, is very nearly the same. If, therefore, the evil be proved to be of such magnitude as to require an efficacious remedy, the remedy must be general, and not partial.

In the estimation of its magnitude, there seem to me to be two parties among those who interest themselves on this question. One side conceives that they have discovered a new and unheard-of mischief, threatening us with a pestilence or other great calamity, and are urgent that not a day should be lost in taking measures to rescue us from the impending danger. Others, again, can see no danger at all, and believe the alarm that has been excited to be a mere creature of the imagination. I can coincide with neither of these parties. I believe the evil to be deep and serious; not merely injurious to cleanliness and comfort, but that it is a mischief which saps the foundations of life, and brings multitudes to a premature grave. But I believe that it is no new evil, but one that has existed and afflicted the inhabitants of London as long as London has been a great city, unknown, and therefore unheeded. I do not think it to be caused by the sewers, but that it would exist though not a single sewer had been formed. The sewers, by concentrating and bringing immediately under the eye the whole source of the pollution, have merely rendered it obvious to the most cursory observation.

If I appear to press, more than is needful, these considerations upon your attention, my apology is, that probably you will not have the same views of the subject presented to you from any other quarter; and I am therefore naturally anxious that they should receive a dispassionate and candid examination. An abundance of projects will, no doubt, be offered for the correction of the evil complained of; and to your judgment, more than to that of any other man, will the public look with respect and confidence.

But no due estimate, it is obvious, can be made of the value of any proposal, if the source and origin of the contamination are not developed and clearly understood.

I shall not, Sir, intrude further on your valuable time; nor shall I presume to take the privilege of a dedication by offering any eulogy on a statesman, whom men of all parties, even political enemies, concur in eulogizing; but conclude with requesting a favourable reception of this trifling homage of respect from,

Sir,

Your most obedient servant,

WILLIAM LAMBE.

2, King's Road, Bedford Row,
26th April 1828.

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PREFACE.

THE writer of the following sheets, some time about the beginning of the present century, discovered that common water, kept in contact with lead, had the power of dissolving some of the metal; a fact which had been misconceived by the most respectable physicians of the last century, particularly by Sir George Baker, and the very learned Dr. Heberden, sen^{r}. He published his observations in the year 1802, in a little work entitled "Researches into the Properties of Spring Water." This book has been long out of print, but it is the intention of the author, at no distant period, to give a new and improved edition of it.

This observation it was that fixed his attention intensely on the salubrity of water, and its effects on the human system. About the year 1803 he observed, for the first time, that the precipitates formed by salts of lead from water were revived by heat without any addition; he observed, by the distillation of a particular water, a large quantity of fetid gas extricated, and an oily matter produced, with the smell of what has been called Dippel's animal oil. Hence he acquired the idea that common water was tainted with the exuviæ of animal bodies left by putrefaction. These, and some other corresponding observations, produced in his mind a perfect revolution on the subject of common water. From supposing it (in common with all the world) the source of health, he began to suspect it to be the prolific source of disease, and was incited to institute experiments on the subject, both personal, and upon those over whom he had any influence.

In the year 1805 he published his opinions on the subject of water, in his "Inquiry into the Origin of Constitutional Diseases," recommending the distillation of water. In 1809 he published his "Reports on Cancer," enforcing his recommendation by observations on that disease, but uniting with it a vegetable regimen. Finally, in 1815, he published his "Additional Reports on Regimen," confirming his opinions by such arguments as he thought appropriate, and producing examples of the utility of the practice he recommended.

That his opinions have met with an exceedingly cold reception from the mass of mankind; that they have been deemed erroneous, extravagant, or enthusiastic, is to their author a subject of regret and disappointment more than of surprise. He was an individual obscure and unknown. New opinions, though supported by powerful names or unanswerable arguments, have ever been of slow reception. The passions of mankind have been armed too against them. But justice to himself requires him now to assert, that whatever change of sentiment with regard to the salubrity of water has taken place in the mind either of the profession or of a few individuals out of the profession, this change is principally, if not solely due to the observations and experiments originating from himself.

But on this subject a perfect apathy pervaded the public mind. He considers it, therefore, to be a most fortunate event for the public that an individual, making no pretensions to depth of science, but guided only by common feeling and common sense, was informed of, and examined the great sources of pollution affecting the Thames; and denounced the nuisance to the public. He means, of course, Mr. John Wright, the author of the Dolphin. To his zeal, activity, and indefatigable exertions, are solely due the excitation of the public mind on a subject closely connected with the public health, and the appointment of the Government Commission for the investigation of the subject.

The writer's own medical observations on the effect of water on the body have now been pursued with uniform attention for five and twenty years. But his chemical researches had been long suspended, excepting as directed to one or two particular objects. The inquiry instituted by authority induced him to resume them; and he now offers to the public the result of his recent labours in this almost uncultivated field of science. The greater part of the experiments on the Thames water have been presented to the Commission of Inquiry. But some observations have been made which he considers to be very interesting, since he presented his memoir to the Commission; and both facts and reflections are here introduced, with which he did not think it becoming him that he should occupy the time of a public Commission. He hopes, however, that they may be of use to the public, and with this view he has committed them to the press.

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INTRODUCTORY OBSERVATIONS.

No part of chemistry has been cultivated with greater assiduity than that which regards the analysis of waters; nor does the success of these investigations appear to be disproportionate to the attention and labour which has been bestowed upon them. But this assertion must be limited to those waters which are termed *mineral;* which being used as medicines, have acquired a certain degree of celebrity, or which are objects of interested speculation. With regard to our domestic waters, those which are in daily use by the whole population of crowded cities, or extensive tracts of countries; those which, if they contain any noxious principles, must convey disease and death into the bosom of every family of all degrees, affecting equally the prince and the peasant; they have hardly been deemed worthy of the attention of the chemist, and what is called the analysis of these waters, by chemical writers of the first celebrity, presents nothing but the result of the most superficial examination. By consequence, the information we can gain on the subject from our elementary books is of that meagre and imperfect kind as to be destitute of all real instruction. I do not, I think, deviate from the truth when I assert, that they tend more to mislead than to instruct.

Bergman may with justice be called the father of the modern art of analyzing waters; since the methods at present in use, though improved and extended since his time, are founded essentially on the rules laid down in his excellent treatise on this subject. Yet from this estimable writer we learn no more than that domestic waters contain very minute quantities of saline matter; so minute, indeed, that it would be absurd to attribute to them any serious injury to the animal economy. Spring water, river water, and well water, are those in common use for drinking, and the preparation of food. Spring-water, he tells us, contains most frequently carbonate of lime, muriate of lime, muriate of soda, or carbonate of soda. River-water has the same principles, but in less abundance. Well-water contains, besides the above-named salts, sulphate of lime or nitrate of potash. Such is the sum of what we learn from this eminent chemist, which is cited and copied by Fourcroy, without addition or comment.*

In judging of the fitness of water for human use, we are referred to the tests employed for the detection of these saline bodies, or to the vague and wholly unscientific criterions handed down to us from the rude ages of antiquity. Waters, they tell us, may be considered to be good, which are fresh, limpid, and without smell. Legumes should readily boil in them, and soap should dissolve without curdling. They should only be slightly affected by the tests of sulphuric, muriatic, and calcareous salts, and by evaporation should leave only a trifling residuum. Such, and such alone, are the rules laid down for our guidance in what is perhaps the most important concern of life, by one of the most distinguished of the French chemists.† We may therefore venture to assert that, in this branch of analysis, modern chemistry has hitherto contributed nothing but some trifling improvements in the use of re-agents; and that it is by no means certain that even these have not led to erroneous inferences.

In proof of this assertion I will take two examples from modern elementary works of established reputation. The first shall be from Dr. Thompson's System of Chemistry, (vol. iii. p. 193, 6th edit.) In this place the author informs us, that the water of the Clyde contains, in 10,000 grains, 1·3 grains of foreign matter. This matter is composed of

* Fourcroy, Connaissances Chimiques, vol. iv. p. 302.

† Traité de Chimie, par L. G. Thenard, vol. ii. p. 29, éd. 5me.

Saline bodies...............	0·788	grs.
Carbonate of lime.........	0·394	
Silica.........................	0·118	
Total......	1·300	

The salts are,

Muriate of soda	0·369
Muriate of magnesia ...	0 305
Sulphate of soda	0·114
Total......	0·788

No authority being given for this statement, the author himself, it is to be presumed, was the analyst, and the analysis, therefore, may be considered as correct,* as far as it goes. Conceding it then to be the truth, if it is also the whole truth, it would be perfect childishness to suspect any noxious influence from the use of these waters. But before forming this conclusion, we ought to inquire, does the water of the Clyde preserve, when kept, its sweetness and freshness? Is it as palatable after the lapse of a certain time as when taken immediately from the river? Does it never become offensive to the smell and taste during the heat of summer? Are there no fish in the Clyde which perish and rot, or which contaminate the stream with excrementitious matter? Are not the streams which, finally collecting, swell its ample bed, originally the washings of tens of thousands of acres, over which are strewed, and in the substance of which are imbedded, the decayed and decaying remains of myriads of animals and vegetables, in every stage of decomposition and putrefaction? If these questions must, most of them, at least, be answered affirmatively, the next object of inquiry would be the nature of the principles imparted to the water from the circumstances in which it is placed. If any soluble

* Perhaps this is conceding too much: for the deliquescent matter in this water is said to be muriate of magnesia. Now I find the deliquescent substance in the Thames water not to be an earthy salt, but a peculiar and very compound body; and I believe it to be universally diffused in common water. I therefore think it much more probable that the deliquescent matter was this body, than that which it is said to be.

matters are received from these sources, how happens it that they have escaped observation? Are these matters fixed or volatile? If volatile, may they not have eluded all chemical tests? If fixed, may they not possibly be in combination with the saline and earthy substances, which are, in respect of magnitude, the chief ingredients? In either case, these matters being constantly applied to human bodies, the whole species is concerned to know, whether they are useful to the animal frame, whether they are noxious, or whether they are inert, and therefore insignificant. Such are the questions which have now, for a series of years, appeared to me of the first importance, in the discussion of a subject regarding the health of human beings; and a suspicion can hardly fail to be excited in a reflecting mind, that an exact and complete investigation of the facts may lead to conclusions, the opposite of those which we may be inclined to deduce from a hasty and superficial survey.

M. Thenard has given, at the place above quoted of his Treatise on Chemistry, a table of the matters contained in all the waters brought, or which it was intended to bring to Paris, the analysis having been made by M. Colin, professor of natural philosophy and chemistry at the Ecole de St. Cyr. This table includes the gaseous, earthy, and saline substances contained in these waters. They are thirteen in number, including two analyses of the Seine at two different points; and the matters ascertained are the relative quantities of carbonic acid and common air, the weight of the residuum of each water after evaporation, and the relative quantities of sulphate of lime, carbonate of lime, common salt, and deliquescent salts. These deliquescent salts are, without doubt, presumed to be the earthy muriates.

From an inspection of this table, it appears that the water of the Seine leaves the smallest residuum, by evaporation, of any of these twelve waters; that six of them contain more sulphate of lime than the Seine, and the others very nearly the same proportion; that seven of them contain more deliquescent matter than the Seine water, and in the others the quantity was very nearly the same. The fact most worthy of observation (I mean, merely, as a chemical fact) in the table is, that after the passage of the stream of the Seine through Paris, and its having received, as it may be supposed, much contamination from the impurities of this large city, there is found a considerable increase of carbonate of lime and of deliquescent salts, and a small diminution of sulphate of lime.

If now we were to form our judgment of the salubrity of this water from what is delivered to us as criterions of salubrity, that is to say, from its levity and the small quantity of heterogeneous ingredients, we should affirm that the water of the Seine is the most salubrious nearly of all those destined to the supply of the French metropolis. But this inference is at variance with common experience, and the testimony of all observers. Strangers, arriving at Paris, are usually affected in the stomach, bowels, and head, after the residence of a few days, and this disease, however slight in degree, has, from its frequent occurrence, obtained its place in the classification of nosologists. *Mal de Paris* is its trivial name. Sauvages calls it *Dysenteria Parisiaca ;* and the use of the waters of the Seine has been deemed its principal antecedent cause; in conformity with which opinion, *abstinendum ab aquâ sequanâ* is placed among the rules of treatment.* Certainly this account of the origin of this malady is as probable as any that has been advanced. If it be the true account, it is equally clear that the chemical analysis of this water, as given in the above-mentioned table, does not throw the smallest glimmering of light on the subject.

I conclude therefore that chemistry, in the present state of our knowledge, either is inadequate to the detection of those matters in our domestic waters which have the greatest influence on the body, or that the methods hitherto employed are imperfect and defective. I believe that both of these causes have contributed to the present obscurity of the subject. To the first of them I shall have occasion to revert. On the second a few words may not be here misplaced.

I apprehend then, that one great source of our imperfect knowledge of this subject is the confining of our analytic processes to those employed is the analysis of mineral waters. Mineral waters may be considered as solutions of saline matters

* Sauvages, Nosologia Methodica, vol. ii. p. 326.

in simple water; if any organic matters are mixed with the saline ingredients, these bear a very small proportion to the whole of their constituents, and therefore induce very little error in the results of experiments. But if I mistake not, nearly the whole of the impregnations of domestic water are the direct product of organic matter in a state of decomposition; derived either from the soil through which it has passed, or from the animal and vegetable substances, derived from the beings living in them, or from matter introduced by adventitious circumstances. They are, therefore, a mixture or combination of matters, some of which have gone through every stage of decomposition, and have become permanent saline, earthy, or metallic matter, and of organic matter in an imperfect and progressing state of decomposition. In consequence of this, the fixed and perfect matters (the metals, salts, and earths) either do not conform to the laws of affinity, which are observed in their isolated state; or the result of these affinities are a combination of the perfect matter (the earth, the metal, or the salt) and the undecompounded organic matter. We shall, therefore, form erroneous conclusions, if by the use of precipitants, or of crystallization, or other processes used in the examination of mineral waters, we calculate upon obtaining similar results when applied to domestic waters, either with respect to quantity or quality. One or two examples will illustrate my meaning better than any superfluity of words.

Nothing is more easy than to shew the existence of iron in the water of the Thames, taken up at London. But the common test (prussiate of potash) added to the water indicates nothing. Tincture of galls likewise produces no discolouration. There must, therefore, be matters in the water, having a stronger affinity to the iron than those agents, and which counteract their influence.

If having added tincture of galls to the Thames water, I acidify it by muriatic acid, I obtain a cloud of a light colour. Here then the affinity of the gallic acid for the iron preponderates: but it does not combine with pure iron, but with some other matter in union with the iron; otherwise we should have a violet or black precipitate.

Many years ago I precipitated by an alkaline sub-carbonate, the chalk from New River water, and exposed the matter collected to a decomposing heat. The gas obtained was not above half of it carbonic acid. I concluded at the time (I believe correctly) that the remainder was pure azote, and that therefore there was a matter of animal origin precipitated in combination with the carbonate of lime. In addition to this proof I may add, that this precipitate being kept moist in hot weather, quickly emits an intolerably offensive odour.

I shall shortly prove that the matter separated from the Thames water by boiling, is by no means a simple carbonate of lime, but, on the contrary, is a very compounded substance.

When, therefore, we are informed, in the tables given us by M. Thenard, for example, that the waters of Paris contain so many grains of carbonate of lime, so many of sulphate of lime, so many of common salt, and so forth, we are not to conceive these to be the pure substances of fixed proportions, the composition of which is ascertained in our chemical books; but that the carbonate, the sulphate of lime, and the common salt, are the preponderating substances of the matter so designated; but that to arrive at a perfect knowledge of the true constituent principles, each of these matters ought to be submitted to a second analysis, and their composition to be fully developed.

For myself, I am far from pretending to have made an approach towards an analysis of the Thames water, prosecuted according to this method; but the observations which this investigation has presented, have shewn the correctness of the view which I have taken; and I here lay it before the public, in the hope that other persons better qualified for the undertaking, may at some time prosecute it with a diligence and accuracy proportionate to the importance of the subject.

The principal object of the following experiments is to prove that the water of the Thames is, in addition to saline ingredients, impregnated with matter immediately derived from the decomposition of organic matter. This matter is of two kinds, as derived from the decay either of animal or of vegetable bodies; but as the products of this decomposition are completely blended together, so that it is impossible to distinguish precisely what product originates from one source and what from the other, I shall use the general term, organic matter, to comprehend them both. It seems now to be generally agreed, that whatever may be the noxious influence affecting the human frame from water, confessedly insalubrious, it proceeds from matter of this sort. To determine therefore the existence of such matter in any particular subject of inquiry, ought to be the first object of the art of analysis: to this end therefore has my labour been chiefly directed. I trust that I have put this point beyond the reach of contradiction; a point of the more importance, as no regular method of determining it had been known, and in the particular case of the Thames water, doubts had been thrown out as to the reality of this impregnation, from a quarter deserving much consideration on any question of chemical science.

One single property of a domestic water is sufficient to demonstrate the existence of this impregnation of decaying organic matter to common sense, and is intelligible to every understanding. If it changes by spontaneous decomposition, and becomes foul or fetid, this change can be ascribed to no other cause. In this case, the common feeling of disgust induces men to reject it as unfit for human use, and every scrutiny but the evidence of the senses would be deemed to be an unprofitable misapplication of labour.

But it may happen, and such is commonly the case, that the subject of examination is perfectly pellucid, void of odour, and agreeable to the palate. Here then arises the important question, whether these are sufficient tests of salubrity. Does not such water contain the same principles as the offensive compound which is instinctively rejected, but in a form not immediately distinguishable by the senses? As to the pleasantness of flavour, which many call delightful, be it remembered, that our likings are formed by habit; and that what gives delight to a diseased palate may excite disgust in one that is unvitiated. Here then analysis must come to our aid, and having shown that the methods hitherto employed in this particular branch of analysis lead to no satisfactory results, I shall now, without further exordium, set down briefly the processes I have employed in this attempt to investigate the properties of the water of the Thames.

First, then, I have used precipitants, and examined the properties of the precipitated matters. I have confined myself to two only, which I have found sufficient for the object I have had in view. One of them has been salts of lead; and, in the choice of the salt, I have preferred the muriate (or chloride) of lead, and have done this on several accounts. Thames water contains principally common salt, as its saline ingredient, which would be decomposed by nitrate and acetate of lead, but cannot by the muriate. Nitrate of lead may contain an excess of acid, which would dissolve some of the matter which ought to be precipitated. Acetate of lead has the same defect, and is liable to a still stronger objection. It is made from a coarse and impure vinegar, containing mucilaginous or other organic matter. In an attempt therefore to discover organic matter in the precipitate formed, it would be doubtful whether this matter might not have been furnished by the precipitant. These reasons have induced me to use the muriate of lead, which I formed by preparing nitrate of lead with litharge and nitric acid, and precipitating this salt by muriate of lime.

The other precipitant used is the nitrate of mercury, formed by dissolving the metal in diluted nitric acid without heat, and called the *proto-nitrate* of mercury. Each of these precipitants may be made to indicate the presence of organic matter in the water; a purpose to which, as far as I know, they have been first applied by myself.

Secondly, I have evaporated considerable masses of the water; exposed the products, either in the whole, or in separate forms, to a decomposing heat; and collected and examined the gaseous products of this decomposition. The analysis of the gasses has not been as perfect as I wished; but enough has been done to demonstrate that they must be a product of the decomposition of organic matter.

Lastly, I have analyzed, though not perfectly, the matters remaining after the action of heat; and have thus arrived at the same conclusion. Some results have been obtained in the course of the investigation which have been wholly unexpected, and which it was very desirable that they might have been repeated and confirmed before they had been communicated to the public. But circumstances, beyond the control of a humble individual, or even of a parliamentary commission, have rendered this impossible. The principal obstacle has been the urgency of powerful Members of Parliament to receive the report of the Commissioners. But I trust that, now that the proper path of investigation has been discovered, that the most curious and most important of all the facts which have occurred, will be verified before the close of the present Parliamentary session.

The general result of the experiments has been the production of bodies, some of which can be traced to the decomposition of organic matter alone; others, which are known to exist abundantly in this species of matter, though not confined to it. They are charcoal (or carbone), carbonic acid, a gas convertible into carbonic acid and azote, nitrous acid, hydrocyanic (or prussic) acid, sulphuretted hydrogen gas, phosphoric acid, oily matter. I omit arsenic and arsenical acid till the reality of their existence is proved or disproved by farther experiments, contenting myself with saying that there are appearances which strongly indicate its presence.

I have examined the Thames water at three separate and distant points, with the object, of course, of assisting in the inquiry, how far the contaminations, which are suspected to be received from the vicinity of the metropolis, may be avoided by drawing the supply from a distant point of the river. It will be more convenient to reserve this question till the result of the examinations has appeared. The inquiry was begun with the water supplied by the Grand Junction Water Company; but finding the quality of this to be the same, or very nearly, as that of the river at London, I include it under the title of "Examination of the Thames Water at London." The two other points selected were a spot a little above Twickenham, and Windsor. I shall therefore now detail the observations made at these several points, acknowledging them to be imperfect, from the want of opportunities of varying and repeating the processes employed; but trusting that, as far as they go, they will be found to be essentially correct.

I. EXAMINATION OF THE THAMES WATER AT WINDSOR.

This water was taken out of the river beside the church of Old Windsor, in the month of January, the river being muddy and swollen by preceding rains, but not overflowing its banks. It was filtered through clean sand previous to examination.

A. Effects of Re-agents.

1. Muriate of lead produced a copious white precipitate, which is immediately re-dissolved by adding nitric acid. This distinguishes it from the cloud formed by salts containing sulphuric acid, which is insoluble in nitric acid.

2. Having observed that carbonate of lime, dissolved by carbonic acid in water, decomposes salts of lead, I boiled the water first to precipitate the carbonate of lime. But the muriate of lead still caused a copious white precipitate.

3. Nitrate of mercury (in solution) causes also a copious white precipitate.

4. Muriate of lead was added to some water which had been previously precipitated by nitrate of mercury, and filtered; a second precipitate was obtained by this addition.

The use of salts of lead, as a precipitant of organic matter, has been received by some chemical writers for some years, and I see the acetate of lead mentioned as a test of animal matter by M. Clement, in the Annales de Chimie for 1817.* The fact of the union of the oxide of lead with matter of this sort was first observed by me about the year 1803 or 1804, and it was this observation which gave me the first insight into the impregnation of common water with organic matter.

But waters in common use are observed which afford no

* Mémoire sur la Distillation de l'Eau de Mer, par M. L. de Freycinet, capitaine de frégate, et M. Clement; Annales de Chimie et de Physique, tom. iv. p. 232, New Series. L'acetate de plomb et le nitrate d'argent n'y produisient pas la plus legère apparence de précipité, ce qui annonce *l'absence complète de matière animale* et de muriates.

precipitate, or hardly any, with salts of lead; and I have seen it concluded that these waters are free from animal impregnations. But it appears from the last experiment, that this is an erroneous inference. The nitrate of mercury, besides precipitating the muriatic salts, precipitates likewise organic matter, as will subsequently be proved. It appears, therefore, that there are two species, at least, of organic matter in this water; one, which may be be precipitated by salts of lead, and a second by the nitrate of mercury.

B. Evaporation.

1. Six gallons of this water were evaporated to dryness. The residuum consisted, in part, of insoluble matter, and partly of saline and soluble matter. The insoluble residuum weighed fifty-four grains; the soluble matter was not weighed, but I judge it to have been between fifteen and twenty grains.

2. If in evaporating Thames water, taken up (I doubt not) at any point, the precipitate formed by boiling (carbonate of lime principally) be removed by filtration, the fluid continues transparent to the end of the evaporation. The deposit found upon re-dissolving the soluble matter is formed by a chemical change in the whole mass, upon the loss of the water evaporated. Were it otherwise, the matter would of course, like a saline substance, be re-dissolved by restoring the water which had previously dissolved it. The soluble portion will form a fresh deposit by a second exsiccation, and some more matter is deposited without evaporation.

3. If New River water be evaporated, and the same treatment be used, similar appearances are observed. Hence in those waters the insoluble residuum is formed out of, and as it were, at the expense of the soluble matter, which appears to be convertible, finally, into pure saline matter, and insoluble residuum.

C. The Insoluble Residuum.

1. The sixty-four grains of insoluble matter (B. 1.) were introduced into a small coated glass retort, with a tube attached to it, which was heated to redness, and the gas which escaped was received over water.

Carbonic Acid.

2. This gas measured thirty cubical inches, and (allowance being made for that remaining in the apparatus) may be considered to be thirty-six or thirty-eight cubical inches. The matter in the retort weighed, after the process, forty-two grains. Loss, twenty-two grains. But this matter appearing to be partly lime, it was exposed to the air for a fortnight, by which it gained an increase of four grains. Eight cubic inches is therefore a large allowance for the gas produced from the decomposition of carbonate of lime, which gives nearly thirty inches from the remaining portion of the residuum. The greater portion of this gas was carbonic acid. The portion which was not soluble in water was not duly examined, from circumstances which it is not necessary to relate.

Hydro-cyanic (or Prussic) Acid.

3. In experimenting upon residuary matter taken up at other places, I have suspected, and even observed, that some other gas, which is soluble in water, is evolved from it by the heat; and I have suspected some modification of hydro-cyanic acid, but have hitherto failed to obtain satisfactory evidence on the subject. I mention it to explain my motive in trying to detect this matter, in which object, in this instance, and in this alone, I have hitherto succeeded.

The method I employed was that suggested by Scheele, in his Essay on Prussian Blue. I dipped a slip of paper in a solution of proto-sulphate of iron (green vitriol), and touched it over with a solution of subcarbonate of potash. This slip of paper was introduced into the extremity of the retort, and consequently exposed to the gas generated by the heat. After the process, it was moistened with muriatic acid, which produced upon it a stain of a blue colour.

Sulphuretted Hydrogen Gas, Appearance of Arsenic, Phosphate of Iron.

4. The residuary matter, weighing now forty-six grains, was treated in the following manner. It was put into diluted muriatic acid, which dissolved very nearly the whole with effervescence, and a strong smell of sulphuretted hydrogen gas.

The solution was filtered, and evaporated to free it from superfluous acid. The matter was re-dissolved, and again filtered. The solution was precipitated first by pure ammonia; and, this precipitate being removed, a second time by subcarbonate of potash. These processes gave (*a*) a small quantity of black insoluble matter, hardly exceeding the half of a grain; (*b*) a matter of a yellowish red colour, approaching to that of brick-dust; its weight was about 2·5 grains; (*c*) a very minute precipitate of a light yellow colour, not amounting to the half of a grain; (*d*) carbonate of lime, weighing twenty-seven grains.

(*a*) The small quantity of black insoluble matter was mixed with a little peroxide of copper. No attention was paid to weight, the experiment being thought of little moment; but Mr. Pereira, who made it, judged the weight of the oxide to be three or four grains: I believe that it was considerably less. The mixture was introduced into a bent tube, and heated by a spirit lamp in the mercurial bath. It was done, as I have said, by Mr. Pereira, in the lecture-room at the General Dispensary, Aldersgate Street, in the presence of Mr. Pollock, chemist and druggist, of Fenchurch Street, and Mr. Pereira, Jun. We obtained, of carbonic acid, just enough to convince us that it was produced. But what most attracted our notice, was a sublimate formed in the first bend of the tube, dark, having a metallic lustre, with some white matter at its superior edge. Its resemblance in appearance to arsenic has been acknowledged by all who have seen it. I have not thought it right to submit this matter to further experiment, but the tube is hermetically sealed, to preserve it for inspection.

I trust that I may be excused for here adding, that this observation is defence enough for the assertion that I have made, and maintained singly now for almost five and twenty years, that there is a true arsenical matter universally diffused through the organic as well as through the mineral kingdom.

(*b*) This matter was mixed with its weight of subcarbonate of soda, and exposed to a low red heat in a silver crucible. The alkali became neutralized, the salt formed was dissolved, a little muriate of lime added; the mixture evaporated, and re-dissolved. This left a residuum soluble in an acid without effervescence.—2. Another portion of the salt was allowed to crystallize by spontaneous evaporation; and among the crystals were discernible rhomboids with angles of 60° and 120°, and prismatic crystals, with similar angles. These are, I presume, sufficient proofs that the acid of this salt was phosphoric acid. I may add to them, that I could obtain no crystals when I used potash instead of soda, in a former experiment on the water taken up at a different point.

In the table of salts given in Dr. Ure's Chemical Dictionary, super-phosphate of iron is said to be a brown-red powder; this matter would appear, therefore, to be of this kind. I have observed also a white powder formed by the same process that gave this matter, which is the colour of phosphate of per-oxide of iron.

This phosphate of iron is not absolutely pure; for a small quantity of the residuum left by evaporation remains undissolved by the muriatic acid.

(*c*). Of this precipitate I can only say, that it is only partially soluble in muriatic acid, and appears to contain iron. I consider it to be merely a little of the matter last described, which has remained in solution, from the superfluous acid not having been wholly expelled.

(*d*). Carbonate of lime, twenty-seven grains. The total, therefore, of the matter obtained by this analysis is not more than thirty-one grains, leaving a deficit of fifteen grains. Now this is much more than can be attributed either to the sulphuretted hydrogen, which is not taken into the account, to inaccuracy, or to the loss in the processes. I have observed too the same deficit in treating the matter obtained from other samples in a similar manner, at least three or four times. I am therefore persuaded that there is still some other matter which escapes during the action of the muriatic acid: of what nature I shall offer no conjecture, but leave it for future investigation.

D. The Soluble Residuum.

1. This matter hardly yielded crystals, owing to a deliquescent substance which impeded crystallization; but there can be no doubt, both from the appearance and taste, that common salt is the principal, if not the only soluble saline ingredient.

Charcoal.

2. After evaporation of the water, the soluble residuum was re-dissolved, and precipitated by nitrate of mercury. This precipitate was exposed to the heat of a spirit-lamp in the bulb of a glass tube. A white sublimate, mixed with revived mercury, was formed, and some pure charcoal remained in the bulb; an acid matter was also produced, which was evident to the smell, and by the common test of acidity.

Charcoal, I need hardly observe, is the common residuum of both animal and vegetable substances decomposed by heat; it is clear, therefore, that organic matter exists in this soluble residuum, and is precipitated by the mercurial salt. From the decomposition of this matter by the heat must come the charcoally residuum.

What portion of the soluble residuum is it then, which yields by its decomposition this charcoally residuum? I could only conjecture it to be the deliquescent matter; and to verify my suspicion, have submitted this substance to a particular examination. The matter examined was not taken from this water from Windsor; but that is obviously unimportant, and it will be convenient to introduce some obervations made upon it in this place.

E. Some Properties of the Deliquescent Substance found in the Water of the Thames, and in other common Waters.

1. Subcarbonate of potash added to the saline residuum of Thames water, from whatever point taken, occasions no precipitate. It is evident, therefore, that this substance is not a deliquescent salt with an earthy base.

But there can be no doubt that, from a hasty examination, it has been considered to be such by chemical writers. In the table of the Paris waters above referred to, the deliquescent matter is called *sels deliquescens*, obviously meaning earthy muriates or nitrates. Dr. Thomson has called the deliquescent matter of the water of the Clyde muriate of magnesia. This point ought to be re-examined. I first conceived it to be muriate of lime (though the taste did not correspond with it), finding sulphuric acid to cause a small precipitate of sulphate of lime.

2. It is not easy to obtain this matter separated from common salt, with which it is combined. I first tried to procure it by digesting the soluble residuum in rectified spirit, but I obtained a mixture abounding in common salt, and the mass remaining was still deliquescent.

3. I next dissolved the soluble matter of the whole residuum of the water in a small quantity of water, and boiled and digested the insoluble residuum in a considerable quantity of water; which was filtered and evaporated to dryness. By this I separated the sulphate of lime, which was however of a yellow colour, and obviously united with deliquescent matter. The whole was now digested in rectified spirit, which extracted a small portion of the deliquescent substance, in a state as pure, I suppose, as it can be obtained in.

4. The solution in the rectified spirit is of a deep yellow colour, with which colour it tinges the saline matters united to it. The sulphate of lime was not deprived of this colour by the spirit. Evaporated it leaves a mass, partly yellow, partly white, on the glass, with the edges glossy and shining. It is highly deliquescent, and leaves when re-dissolved a yellow stain on portions of the glass. Its taste, when concentrated, is sharp, but not acrid; diluted it is bitter with some degree of nauseousness.

5. A very small portion of this matter was heated to redness at the bottom of a silver crucible. It seemed to be wholly decomposed by the heat. A little common salt and oxide of iron, with a trace of insoluble matter, was all that could be observed. I suspect that both of the former matters were accidental.

6. This matter has continued deliquescent, when exposed to the air, for many weeks. But in one example recently observed, and which was obtained as above described, it lost this property in a few days, as did the residuum of the water which had contained it. There must then be a variety in its composition.

Nitrous Acid.

7. The small quantity, that I could procure by the treatment

above-mentioned, was precipitated by the nitrate of mercury, and, the precipitate being put into a tube, was heated by a spirit lamp, in the mercurial trough. There was a white sublimate mixed with revived mercury, and a residuum of charcoal in the bulb of the tube. The origin therefore of the charcoal in the residuum is evident. But besides these products the tube was filled with red vapours, like those of nitrous acid, and water being admitted to the gas collected (only, I believe, the air of the tube), gave indications that some mercury had been dissolved by this acid.

As far therefore as the present indications go, this deliquescent substance is very complicated, its precipitate affording chlorine, charcoal, and azote, in the form of nitrous acid.

A proof will be given, in the sequel, that this matter is not peculiar to the water of the Thames, but is very generally diffused, and I think it probable that it may be extracted from the soil, and originates there. I shall call it here merely the deliquescent substance. But should chemists think fit to assign to it a generic name, its properties having been first investigated from the waters of our majestic Thames, the name of *Tamesin*, (from Tamesis, the Thames) might be sufficiently appropriate.

It is worthy of remark, that in the table given by Mr. Thenard, to which I have referred, the proportion of the deliquescent salts in the water of the Seine, above and below Paris, is stated to be as 171 to 378; that is to say, the deliquescent matter is nearly tripled by the river receiving the impurities of the city; whilst, on the other hand, the sulphate of lime is much diminished, and the carbonate of lime only a little increased. Such, with a small proportion of common salt, are according to this analysis, the only matters found in the Seine water. But after what has been demonstrated of the very complicated composition of the water of the Thames at Windsor, what confidence can be placed in such results, presuming that by deliquescent salts, nothing but earthy muriates or nitrates is understood?

II. EXAMINATION OF THE THAMES WATER AT LONDON.

This water, previous to examination, was filtered either through clean sand or doubled folds of paper. When sand was used, it soon acquired a degree of glutinous tenacity, which made the continuation of the process very tedious, and the sand contracted an offensive and fish-like odour. The water itself has constantly a smell, which is not agreeable, nor is it sweet and pleasant to the palate. It was taken up (if not otherwise expressed) at a short distance below Blackfriars Bridge.

A. Effects of Re-agents.

1. The muriate of lead and nitrate of mercury produce precisely the same appearances in the water taken up at London, as those described. (1. A. 1, 2, 3, 4.)

2. In one trial, two gallons of water yielded by muriate of lead a precipitate weighing twenty-five grains. In a second, four gallons yielded forty-five grains.

3. These forty-five grains of precipitate were introduced into a tube, properly prepared, and exposed to heat in a charcoal fire. By this process a stream of several cubical inches of carbonic acid gas was formed; and in the bulb of the tube, when cooled, lead was found reduced, in the form of minute globules, disseminated through a charcoally matter.

Here then we have a demonstration that this precipitate is composed of an oxide of lead combined with some matter, which gives a residuum of charcoal by a decomposing heat; that is to say, with some organic matter: or it may be that it consists of metallic lead in union with an organic oxide. It matters not which supposition we make. In the first hypothesis we have more charcoal than is sufficient for the reduction of the lead. In the latter, we must suppose that the lead fixes the organic matter, and so occasions its decomposition by heat.*

* In making this experiment I have commonly mixed the precipitate with subcarbonate of potash, and from the reduction of the metal inferred the ex-

4. The New River water shews precisely the same appearances with the salts of lead and mercury as the Thames water. The precipitate by salts of lead is, like the latter, soluble in nitric acid.

5. The precipitate made by muriate of lead with New River water was collected, and heated in a tube, as described at 3. Carbonic acid was produced, and the lead was reduced in two distinct globules; nor was any other matter found in the bulb of the tube.

6. When the nitrate of mercury occurred to me as a proper agent for the discovery of organic matter, it was first applied to the water supplying my own house, at Kentish Town; that of the Hampstead water-works, coming from the Hampstead ponds. Two or three gallons of this water were precipitated, and the precipitate, heated in a tube, yielded a residuum of charcoal. But the process was troublesome, much nitrate of mercury was necessary, and the precipitate subsided slowly and imperfectly. Expecting the same difficulty with the water of the Thames, it was for this reason that I used the very large quantity of water related in the following experiment; a trouble, however, that was really needless.

7. Thirty gallons, then, of this water were precipitated by nitrate of mercury, and the precipitate heated to redness in a small retort, with a tube attached to it. Little or no gas was collected. A white sublimate, mixed with particles of revived mercury, rose into the neck of the retort. A charcoally matter was found in its body, the weight of which was twenty-eight grains.

istence of charcoal. Having related this proof in a public journal, I was told, and with great asperity, that it was a gross error, for the reduction was caused by the flux. Now I do really find that a small globule of metallic lead is always formed when sulphate, muriate, or carbonate of lead is fused with carbonate of potash; and, farther, I have no scruple in acknowledging that I was not aware of the fact, till it was pointed out to me by my very good-natured critic. But, in fact, I have known for these five and twenty years, that the precipitate in question is reducible without the use of any flux, and that the objection of my censor was therefore unfounded.

An Oily or Waxy Matter.

8. The following appearance being quite unexpected, the account I can give of it is imperfect; but it appears to be very deserving of further investigation.

The sublimate of the last experiment, consisting of the white matter and revived mercury, was shaken with water, the solid matter allowed to subside; and the water having been, as I believe,* first filtered, was placed in an oven to dry. Upon taking the saucer from the oven, a matter which seemed moist remained in it. It was therefore kept for some time in a moderate heat, but still the matter remained apparently moist. On cooling, however, the appearance of moisture ceased, and by a moderate heat it was renewed. The matter then appeared to be tasteless, unctuous to the feel, and melting like wax or tallow by a gentle heat, and it must therefore be placed among this class of bodies.

9. The only experiment I have made with it was attempting to dissolve in it rectified spirit, thinking it might be *adipocire;* but it was totally different from that body. The spirit did not dissolve it, but extracted from it a small portion of matter which was black and solid, and not oily, after the evaporation of the spirit. The whole quantity taken up was very minute, and after the separation of this matter, the matter left looked more fluid, like common oil.

B. Evaporation.

Very great variety has been observed in the quantity of residuum after evaporation, when the water is taken up at the same spot at different times. The extremes of different evaporations of six gallons of this water, performed in the autumn and early part of the winter of last year, were for the *maximum* 108 grains of insoluble and fifty-four grains of soluble residuum; and for the *minimum* fifty-eight grains of insoluble and thirty-two of soluble matter. From comparing the

* I say "*as I believe,*" because it is not easy to conceive how a matter, such as was left after evaporation, could have been miscible with water, and passed through a filter; unless, indeed, it was a kind of soap. But I relate the facts as suggested by my memory.

result of evaporation on the water taken up at London and at Chelsea, I have always found the former more loaded than the latter, and am inclined to believe that the water taken up at high tide will be more loaded than at the low tide. At the former, the water must consist of the lower parts of the river carried upwards, whilst circumstances will be reversed at the low tide.

But the result of an evaporation performed in the month of February, gave me products remarkably less in quantity than those afforded in the preceding autumn. The following was at this time the product of the evaporation of six gallons:

	Grs.
Carbonate of lime	20
Residuary matter......	14
Sulphate of lime	3
Soluble residuum	6·5
	43·5

which is only 37 of what I call insoluble residuum, and 6·5 of soluble.

To account for this variation we must have recourse, I apprehend, to a different principle than that just advanced, though the experimental proof of its correctness must depend on future observations, if any such should be made. I think it perfectly demonstrable, that the contents of all our common waters, even the perfect saline and earthy matters, are the immediate product of recent putrefaction, or, if the term be less offensive, of recent decomposition of animal and vegetable matter. This may, I apprehend, be demonstrated, *a priori*, independent of experiment, and follows also directly from experiment on this very water, which will be related in a short time; and I postpone therefore, for the present, this proof. Granting, however, the truth of this position, it will follow that when putrefaction or decomposition is most rapid, its product will be most abundant, and *vice versâ*. I presume, therefore, that the smallness of the quantity of foreign matter contained in the river at this time was caused by the season and the cold weather, and suppose that such a variation will be found to be uniform. Some water taken up at a much higher point of the river three or four weeks previously, shewed in a manner equally remarkable an extraordinary smallness of foreign matter, or apparent purity, which will be presently noticed.

C. The Insoluble Residuum.

1. The matter operated upon in the experiments to be related was 109 grains of the insoluble residuum, of which ninety was the product of five gallons of water, and nineteen some obtained by a former process. They were in appearance perfectly similar.

2. The matter was of an ochry yellow colour, and dissolved with effervescence in muriatic acid, except a small residuum, which appeared to be silica and sulphate of lime. When treated in this manner by simple solution no iron could be detected, notwithstanding the ochry appearance.

3. The 109 grains of this matter were exposed to heat in a proper apparatus to obtain its gaseous product. A copious current of gas was procured, which was collected in different bottles. The quantity obtained was 75·5 cubical inches; therefore we may estimate it to be 85·5 inches at the least, allowing for what remained in the apparatus, and some waste.

4. The whole appeared to be of uniform quality, and to have the same proportion of gas soluble in water and insoluble. The soluble portion, which I consider to be carbonic acid, amounted to about seven-eighths of the whole; that is, it was very nearly seventy-five cubic inches.

5. The matter, after the exposure to heat, weighed sixty-five grains. As it contained lime, it was exposed to the air for a fortnight, after which it weighed eighty-five grains;* therefore forty cubical inches is a large allowance for the carbonic acid proceeding from the decomposition of carbonate of lime. It follows then that in this residuum there were thirty-five

* This is much more in proportion than the weight acquired in the former experiment on the Windsor water: in that, forty-two grains of matter acquired four only from the atmosphere; but in the last experiment the heat was stronger and continued longer.

cubical inches of carbonic acid furnished by another species of matter, which must have been of *organic* origin.

Inflammable Gas producing Azote and Carbonic Acid.

6. The first portion of the gas not soluble in water was mixed with the common air of the apparatus, and it inflamed by a candle. Another portion being mixed with less than its own bulk of common air, exploded with a force almost equal to the detonation of hydrogen gas; and every portion examined proved inflammable. The colour of the flame was of a deep blue, and was said by Mr. Pereira to have a perfect resemblance to the flame of cyanogen.

7. The very last portion of the gas received was examined, by the aid of Mr. Pereira, with as much accuracy as the quantity we had to operate upon would admit. We first inflamed a portion in the mercurial trough with double its volume of oxygen gas; carbonic acid was produced, which was removed by potash, and then the oxygen not consumed was absorbed by a solution of the proto-sulphate of iron. There still was a considerable residuum, which we considered to be azote; and we concluded that the gas required only half its volume of oxygen gas for saturation; and that more carbonic acid, in volume, was produced than the volume of oxygen gas which had disappeared.

8. We therefore now mixed two volumes of the gas with one volume of oxygen gas, and were enabled to make three examinations of this mixture. After the explosion there was a dilation of about one-fifth of the mixture; that is to say, five parts became six, or a little more. The carbonic acid produced was more in volume than the oxygen consumed; the gas therefore contains some oxygen as one of its constituent principles. Finally, the residuary gas would not inflame by an additional quantity of oxygen, and must be regarded therefore to be azote.

The last trial gave the following result:

	Cubical Inches.	
Unknown gas ..	0·333	} =5.
Oxygen	0·166	

After explosion,

	Cubical Inches.
The measure was....	0·600
Potash absorbed	0·250
Azote	0·350

One defect in this experiment we were unable to correct; namely, whether the half measure of oxygen was not more than was consumed. But it is sufficiently evident that this gas contains apparently no hydrogen; that it is a compound of carbone, azote and oxygen, in which the azote is in the largest proportion, and that it must be deemed a product of the decomposition of animal matter.

9. We consider the matter separated from this or other common waters by simple boiling to be carbonate of lime; but nothing is further from the truth than that it is a pure carbonate of lime; on the contrary, it seems to be a substance of very complicated composition, as is evinced by the following experiment:

(a.) By boiling six gallons of this water at separate intervals, and filtration, I collected twenty grains of this substance; from which I attempted to procure gas exactly as in the last experiment. I procured only, by the unassisted heat of the fire, eight cubical inches of gas, of which a small portion only was absorbed by water; the remainder, being mixed probably with too much of the air of the apparatus, afforded no observation that could be depended upon. I now substituted a very small phial for the bottle which had received the first product; and raising the heat by blowing, and continuing it for some time, I contrived to fill completely the small phial. This gas, then, may be considered to be that afforded by the matter exposed to the heat, and to be pure, or very nearly so. One-half of it, very exactly, was soluble in water, and was therefore carbonic acid; the remaining half, both from the manner of its burning and from analysing it by inflammation with oxygen gas, seemed to be a gas of exactly the same nature and composition as that described in the last paragraph.

(b.) The weight lost by the matter in the retort was eight grains; but in diluted muriatic acid it effervesced, and gave out sulphuretted hydrogen gas (mixed undoubtedly with carbonic acid). The acid took up, besides lime, some iron, and left a small quantity of black residuum, which, had it not been for the result of a former experiment (1. C. 4. a.) I should have called pure charcoal. It was, however, too minute for accurate examination.

It appears, therefore, to be certain that the precipitate gained by boiling and spontaneous separation is not pure carbonate of lime, but contains in addition most of the principles of the insoluble residuum, iron, sulphur, charcoal, and perhaps a trace of arsenic. This observation is much in favour of the common practice of boiling water, and permitting the sediment to subside, to obtain a partial purification.

We proceed to examine the residuum after torrefaction.

10. The 85 grains of residuum (11. C. 5.) were dissolved in diluted muriatic acid; a slip of paper, dipped in a solution of nitrate of lead, was exposed to the gas evolved, and it was immediately blackened; the smell too of sulphuretted hydrogen gas was emitted.

11. The processes used in the examination of the water, from Windsor were applied to this residuum (See 1. C. 4) and they afforded,

	Grs.
a. A light-coloured insoluble matter, weighing	7
b. A yellow powder	15
c. A small yellowish precipitate, too minute to estimate	—
d. Carbonate of lime..........................	59
Total	81

(a.) This matter, of little solubility it may easily be supposed from the manner in which it was procured, must have been principally sulphate of lime, with a little silica. But after the remarkable observation before related (1. C. 4. a), I thought it right to use this matter in attempting to detect arsenical matter in its composition. As I obtained nothing in a distinct form, I shall not relate the observations made with it, saying only that it afforded me what I consider to be proofs of an arsenical body, in a state of combination not to be separated by common processes. The proofs were such, principally, as I laid before the public more than twenty years ago, but which have met with no attention from the chemical public. I will only add, in addition to them, that I extracted from this matter a combination which, by a red heat, impressed on a slip of platinum a distinct white stain, precisely such as would have been formed by arsenical matter.

(b.) The yellow matter left after the evaporation of the muriatic acid was proved to be, in part at least, phosphate of iron, as in the former trial (1. C. 4. b). Mixed with its weight of subcarbonate of soda, and heated to redness, the soda was not neutralized. Saturation was effected, therefore, by adding muriatic acid, and evaporation. No precipitate appeared by adding muriate of lime: but after the lapse of twenty-four hours a jelly-like matter had subsided, with white spots interspersed. By evaporation and re-solution a precipitate was procured, which was totally soluble in muriatic acid. This is a sufficient proof that this matter contained phosphoric acid, but the oxide of iron appeared to be in excess in the composition.

(c.) See (1. C. 4. c.)

(d.) Carbonate of lime 59 grains. We have here only a deficit of four grains, of which some is to be referred to the sulphuretted hydrogen. I must therefore repeat, that in the former example (1. C. 4. d.) and in several others, in which a similarly large deficit was observed, some matter must have escaped which was neither carbonic acid nor sulphuretted hydrogen, and which requires further examination.

D. The Soluble Residuum.

1. The saline and soluble matter formed a solution of a deep yellow colour, and the colour became darker by concentrating the liquid, approaching to that of port wine. The insoluble residuum is, in part, a deposit from this, effected by evaporation; and when the soluble matter is re-dissolved a fresh deposit takes place, probably of the same nature. I divided this matter (originally 45 grains) into two parts, very nearly equal.

2. One portion was precipitated by nitrate of mercury, and the precipitate exposed to a low red heat in a tube. The result was what has been so often mentioned, a white sublimate mixed with revived mercury, a disengagement of an acid, and a residuum of charcoal.

Production of Nitrous Acid.

3. The other portion was evaporated to dryness, and introduced into a wide glass tube, sealed and coated at one extremity. It was then exposed to a red heat; an effervescence seemed to take place, and the tube quickly became filled with red vapours, which were evidently, from their appearance and smell, those of nitrous acid.

4. The matter at the bottom of the tube was removed by solution in water. During the solution a white powder was separated; its weight hardly exceeded a grain, and it appeared to be a phosphate by the same proofs, which, having been already more than once delivered, it is not necessary to repeat. The base to which the acid had been united appeared to be siliceous; at least a white powder, insoluble in muriatic acid, was the only matter discovered.

5. The saline matter after the process was colourless, and still deliquescent. An alkali caused no precipitation.

An Oily Substance.

6. I added to this matter a solution of nitrate of mercury, with the view of ascertaining whether any charcoal could be procured from it under these circumstances. I obtained only a very small precipitate, not certainly amounting to a grain, of a yellow colour. This matter was heated in a tube which accidentally, and I may say very fortunately, was of considerable length. When the matter became hot, liquid yellow drops appeared at its further extremity, several of which ran together, and had all the appearance of an oil; its smell was like that of soot. The oil preserved its fluidity for several days, but the air having access to it, it gradually concreted. Globules of revived mercury were mixed with this oily substance.

The lower extremity of the tube was broken in removing the coating. A few shining black particles appeared, however, adhering to this part, and a few similar particles were sublimed at a short distance from this extremity.

Thus was I so fortunate as to obtain a second time an oily substance from the Thames water taken up at London, and to show at least how an oily substance may have become mixed with the large quantity of sublimate from which it was obtained in the experiment related (11. A. 8.). But I do not regard these two oils as identical. I conjecture rather, that the last obtained is the effect of the decomposition of the former by heat.

7. (a.) I procured three or four gallons of New River water to be evaporated, and separated from it the soluble matter, evaporated its solution to dryness, and heated it to redness in a tube duly prepared. I should observe, that this soluble residuum is like that of the Thames water, composed of saline matter and a deliquescent substance. Upon the application of heat, the heated matter appeared, from the noise, to effervesce, and the tube became filled with the red vapours of nitrous acid.

(b.) It appeared distinctly, on examination of the lower extremity of the tube, that the glass was corroded wherever the matter had come in contact with it. I had constantly suspected this to be the case, whenever the soluble residuum of the Thames water was heated in contact with glass.

(c.) There was no deposit of phosphoric or other matter observed, when the matter was detached from the glass by maceration in water.

(d) The matter after the process was not deliquescent, but appeared to be pure saline matter ; and, from the mode of crystallization, I apprehend the salts left after the process to be a mixture of common salt and nitre. It should be observed, however, that the heat in this experiment was greater, and continued longer, than when the soluble residuum of Thames water was treated in the same manner, which may account for the more complete destruction of the deliquescent substance.

(e.) Another portion of New River water was precipitated by nitrate of mercury; the precipitate was a yellow powder. Heated to redness in a tube a residuum was found, which, however, was not charcoal, but a very peculiar substance, requiring, to determine its nature, further examination. There was a small quantity only of a chrystalline sublimate; but the tube was coated internally with revived mercury. The matter precipitated, then, must have contained an inflammable, and it may with confidence be presumed, an organic body.

III. EXAMINATION OF THE WATER OF THE THAMES ABOVE TWICKENHAM.

In speaking of this water I shall call it, for the sake of brevity, the Teddington water, that being the point from which it has been proposed to supply the metropolis, and from which I intended to take that submitted to examination. I found, however, that first brought to me to have been really taken from a point by the side of the turnpike road, a little above Twickenham, at a short distance from the eleventh milestone, which point is in the parish of Twickenham; and on this water have all the experiments been made. The distance from Teddington is so little, that the water may be considered to be the same as if taken from that point.

A. Effects of Re-agents.

1. The muriate of lead and nitrate of mercury produce the same appearances as those already described (1. A. 1, 2, 8, 4.)

2. Four gallons of this water yielded, by muriate of lead, a precipitate weighing 38 grains.

3. This precipitate was heated in a tube, and a stream of carbonic acid obtained from it. The lead was found reduced; the globules were distinct, embedded in a black substance, and covered superficially with a yellow shining crust. We conclude, therefore, here, as before, that the precipitate is composed of lead in union with organic matter.

B. Evaporation.

I have shewn the great variety in the product of evaporation in the water taken up at London; that taken up at this point has presented a variety equally striking.

Having observed that the water taken up at Chelsea afforded a smaller residuum than that taken up at London, I formed the conclusion that the river became more and more loaded with foreign matter in its progress downwards, and expected to demonstrate this easily by actual trial. The last evaporation of six gallons of the London water which I had made had yielded a residuum of 93 grains, 83 of which were insoluble, and 10 soluble matter. The same quantity of the Chelsea (or Grand Junction) water had given 76 grains only, of which 50·5 was insoluble, and 25·5 soluble matter: therefore,

1. Six gallons of Teddington water were evaporated. The residuum amounted to 122 grains; of this, 92 grains was insoluble matter, 30 soluble. According to this experiment, then, the Teddington water was more loaded with foreign matter than even the water taken up below Blackfriars Bridge.

I could account for this fact upon one hypothesis only, and I see no reason for thinking that the explanation is not in the main correct. I presume, that the trifling difference observed between the London and the Chelsea water depended principally on the state of the tide. The latter was calculated from thirty gallons reduced to six, and taken probably at various times of the tide. The former, I know, was taken only when the river was full, at high water; the situation being such that it could not be procured when the river was low.

As to the amount of the residuum obtained from this particular sample of Teddington water, it is to be considered, that though it was greater than that of the London water examined a little before, yet a residuum considerably larger has been at another time procured from the latter; therefore many trials are requisite, and the average ought to be taken from the sum total of these trials, before a safe conclusion can be formed, even on so simple a point as the proportional quantity of residuum gained from the water at various points.

I can, however, assert with perfect confidence, that in its chemical properties this sample of Teddington water agreed completely with that taken up at London, and it did not agree with that taken up at Windsor; from which circumstance I draw the important practical conclusion, that the bed of the river from London upwards, as far as the tide and the influence of the tide extends (for the tide may have influence much higher than the visible elevation and depression of the river) may be considered to be a large lake of nearly stagnant water, but subject to constant internal agitation, by means of which the impurities received at London and elsewhere are carried not only downwards, but upwards to its very superior extremity. At the same time it is obviously probable, and experiment confirms the conjecture, that the nearer we approach to the original source of contamination, the more impure will the water prove to be.

Volatility of the Insoluble Residuum.

4. But there may be a fallacy even in the estimation of the quantity of residuum afforded by any portion of the water; for it is important to observe, that a portion of theinsoluble residuum is not a fixed substance, but a matter of which a large proportion may be dissipated by a boiling heat. I observed this fact in obtaining the sulphate of lime, with the view of separating from it the deliquescent substance. (1. E. 3.) I used for this purpose more than an ounce (troy) of residuum, which I boiled, macerated and evaporated in about half a gallon of water, and I found that the loss of weight of the residuum far exceeded the sulphate of lime obtained. Accident prevented an exact estimate, but I concluded the residuum to have lost upwards of two drachms, whilst the sulphate did not amount to half a drachm.

5. I used the 92 grains of the residuum of Teddington water partly to determine this point more precisely. It was boiled in a large quantity of water, which was evaporated. The matter obtained (chiefly sulphate of lime) did not amount to ten grains; but the weight of the residuum, after the boiling and digestion, was only 55 grains. More, therefore, than 27 grains had been taken up by the water, in addition to the sulphate of lime, and had been dissipated by the boiling.

There may, therefore, be differences in the result, even of so simple a process as the ascertaining of the product of evaporation. Though we may operate upon specimens of the very same water, the mode of evaporation, the degree of heat employed, it may be the surface exposed to the air, and other minute differences, may produce variations in the result. I need not suggest to the chemical reader, how strongly these circumstances prove that the matter operated upon must be of organic origin.

Water raised by the Heat of the Steam Engine.

5. I am informed that my respectable friend and colleague, Dr. Clutterbuck, wished to make use of the water from the waste pipe of a steam-engine, as a substitute for distilled water, expecting it to possess equal purity; but, upon trial, he found it to be quite as much loaded with foreign matter as the water which had supplied it, and even apparently more so. This observation is in conformity with what I have observed on the volatility of the insoluble residuum.

The Matter precipitating Salts of Lead is volatile.

6. I have added a solution of muriate of lead to the soluble residuum left by evaporation, and re-dissolved. If this be done as soon as the solution is made, not the smallest cloud is formed; but if the residuum be kept for a time in contact with the water, a little cloud, soluble in nitric acid, is formed. The matter, therefore, occasioning this cloud, is, for the most part, but not totally, dissipated by the evaporation.

7. Six gallons of this Teddington water evaporated gave the following product:

	Grs.
Insoluble residuum	23
Soluble matter	30
Total	53

Here, then, I was as much surprised at the smallness as I had before been at the magnitude of the residuum afforded by this water. The most probable explanation of the cause of this great variety is that it depends on the season, as I have recently suggested. This water was taken up towards the latter end of the month of January.*

C. The Insoluble Residuum.

One object which I had in view was thwarted by the smallness of this residuum; which was, to examine more perfectly the gaseous products; however,

1. The 25 grains were exposed to a red heat, in order to obtain the gaseous matter, as on the former occasions; but enough for examination could not be procured. The small quantity collected inflamed in a manner perfectly similar to that obtained from the London water.

2. After the action of the heat, the matter weighed only nine grains. This, by muriatic acid, lost three grains; the remaining six grains had the appearance of a charcoally substance.

Under these circumstances, I employed these six grains in an attempt to obtain that appearance of arsenic before related (1. C. 4. a). To supply the defect, therefore, of an investigation of the properties of this water by the mode of analysis employed in that of Windsor and of London, I must here relate the observations made upon it by a different manner of operating, and explain why this method was abandoned.

3. Last autumn, I evaporated at various times considerable quantities (thirty gallons for each process) of Thames water taken up both at London and at Chelsea; using for the purpose a tin vessel, at the bottom of which the tinning was not quite perfect, and by this use of it the tinning was soon wholly destroyed. Separating the soluble residuum, and dissolving what was soluble in muriatic acid of the insoluble residuum, I obtained very considerable quantities of a black substance, having the appearance of charcoal. By heating this substance with an oxide of mercury, I ascertained that it is for the most part a real charcoal, mixed with a few siliceous crystals, and a little sulphate of lime; a little iron also probably enters into it, so that I consider it to be analogous to plumbago, or black-lead.

* Some may suspect that I was deceived by the man employed to procure this water. But, in fact, I was present myself when both the specimens which were evaporated were taken out of the river.

I next treated the muriatic solution exactly as in the experiments related, and gained from it phosphate of iron, oxide of iron, and carbonate of lime; and it was in these experiments that I met with the deficit to which I have alluded more than once, and which has made me assert that some other matter, requiring further examination, is separated from the insoluble residuum by the action of muriatic acid, besides carbonic acid and sulphuretted hydrogen. (See I. C. 4. d. and II. C. 9. d.)

These experiments were intended to be the basis of a Memorial to be presented to the Commissioners of Inquiry into the Water of the Metropolis, and the memorial was to have been prepared nearly upon the opening of the Commission.

However, it appeared that when the same water was evaporated in a glass or earthen vessel, the result was materially different. No charcoally matter was then discoverable by solution in muriatic acid; and ammonia, added after the expulsion of the superfluous acid, threw down no oxide of iron. It was clear, then, that the iron obtained in these experiments was not an original ingredient of the water, but it had been introduced from the exposed iron bottom of my tinned vessel; and that the charcoally substance, or plumbago, had been formed by the combination of this oxide of iron with the charcoal existing in the water.

I therefore found myself compelled to renew my examination, to begin almost *de novo*, and to operate on smaller quantities of water, on account of the time consumed in the process of evaporation. The results of this renewed examination is what is here offered to the public, which has been wholly executed during this present year.

But I am far from thinking of my former experiments that (to use the words of Virgil) " *ibi omnis effusus labor*." On the contrary, I look with the highest satisfaction on the discovery of so simple a method, by which any one may convince himself of the existence of charcoally matter in the Thames water, both at London, and even as high up the river as Teddington.

But the Windsor water, though evaporated in the very same tin vessel, yielded by the same processes neither charcoal nor oxide of iron. It is therefore evident that this power of dissolving so copiously the oxide of iron is dependent on the impurities received in the metropolis and its vicinage; and this remark is the foundation of the assertion I have made, that the water at Teddington has the same essential properties as that at London; but that the water at Windsor is essentially different.

4. It would be tedious and useless to enter into the detail of these experiments. It is enough to give the general result, which was, that six gallons of the London water yielded a black residuum, almost wholly charcoal, in one trial of eight grains, in a second of twelve. The same quantity of Teddington water gave a similar residuum of 6·5 grains. It will be recollected, however, that of 92 grains of the original residuum, 37 had been lost by boiling and evaporation; and we may therefore conjecture, that the original 92 grains would have yielded 10·8 of this matter, or as much, on an average, as the London water. The London water yielded in one experiment 4·5 grains of phosphate of iron; in a second, 4 grains. But the Teddington water gave 1·5 grains only of this matter.

Appearance of Arsenious and Arsenical Acid.

5. The six grains of black matter left after the action of the muriatic acid (III. C. 2.) were mixed with 24 grains of peroxide of copper, and introduced into a curved tube. The tube was of considerable capacity, so that it contained much air. Heat being applied, no metalliform matter arose, but a white sublimate was formed, which by the heat of a spirit lamp was readily volatilized. So far the appearance was similar to the arsenious acid. Unfortunately the tube gave way, when this sublimate was too small for further examination.

6. The matter was now removed to a coated Florence flask, and exposed to a red heat for some hours. The flask was found to be coated internally with a white substance adhering so firmly, that a quantity for distinct examination could hardly be collected. But in two places nitro-muriatic acid was placed on it, and evaporated. The coating became dissolved, and a deliquescent substance remained. It did not prove to be an acid, as it would have been had the matter operated on been

pure arsenious acid; but a salt with an astringent cupreous taste.

Without further examination, I shall not venture to say that we had here arsenious and arsenical acid united to an oxide of copper; but the appearances suggest that conclusion, and, as far as I am informed, that alone.

The time and trouble necessary to obtain the matter fit for examination, have hitherto been an obstacle to making further progress in this most interesting part of the inquiry; but preparations have been made for its prosecution with as little delay as possible.

I have noted (III. C. 2.) a muriatic solution weighing three grains. This solution, by evaporation, left a small quantity of matter having the appearance of phosphate of iron. There was hardly a trace of lime, so that this specimen of Thames water was void of a sensible portion of carbonate of lime.

D. The Soluble Residuum.

1. The examination of this matter amounting to 30 grains, (III A. 7.) gave precisely the same result as that both of the Windsor and London waters; that is to say, (a.) introduced into a tube, and exposed to a red heat, it effervesced, and the tube became filled with nitrous vapours; (b.) the glass was observed to be corroded, where it had been in contact with the matter (c.); a colourless solution, but still deliquescent, was formed, and (d.) a small quantity of white matter was deposited; (e.) precipitated with nitrate of silver, and the precipitate exposed to heat, there was merely a white sublimate, no appearance of charcoal, nor any of oil.

In this last property we seem to have a marked distinction between the water taken up at London and at Teddington. But it is to be considered that the specimen of water which afforded these observations was, probably, peculiar, differing greatly from the portion first examined; and further inquiry is necessary before this opinion can be considered to be established.

Formation of an Alkaline Matter.

2. But a new property of the solution now offered itself. Finding it to be deliquescent, and observing that in this as in former cases, potash caused no precipitation, I suspected that potash itself might have been produced, and that the deliquescence was owing to this cause. I examined it then with paper tinged by turmeric, which immediately became of a bright red. But further examination has convinced me that this is not caused by potash; and my present opinion is, that the deliquescence is owing merely to the heat not having been continued long enough wholly to decompose the deliquescent substance. This property, however, induced me to make a further investigation of the properties of the deliquescent substance, which I think deserving of relation.

E. A further Examination of the Deliquescent Substance, both singly and in composition.

1. I procured a very small quantity of this matter, in a separate form, from the three grains of sulphate of lime noted at (II. B.), and placed it in a dry form on a slip of platinum. On the application of heat it first blackened, then fused and boiled like a salt in its water of crystallization, and finally having been urged by the heat of the blow-pipe, left a brown solid matter on the slip. This matter, being moistened, instantly tinged of a deep red the turmeric paper.

2. I may say, in general, without entering into details, that every portion of the residuum of the water to which this matter adheres, and which is sufficiently obvious by the yellow colour it imparts, treated in the same manner, produces the same effect; that it immediately tinges the turmeric paper.

3. This appearance is easily explained by the production of lime out of the deliquescent substance, of which I became convinced by the following process.

(a.) I possessed some of the saline and soluble matter of the London water. A quantity of deliquescent matter had been separated from it by draining, so that this salt was not deliquescent, but had moist matter adherent to it, and was of a deep yellow tinge.

(b.) Some of this substance having been first dried, was exposed for a considerable time to a strong red heat in a crucible of platinum. The whole of the matter was removed by water out of the crucible, and it separated into a soluble and insoluble matter. At first it tinged turmeric paper, and, after a few hours, a film formed on the surface of the water, as would have happened with a solution of lime.

(c.) I analysed with care the products of this process. The saline soluble parts were merely common salt and sulphate of lime. The solid matter was principally carbonate of lime and a little silica. A trace of iron was also detected.

Now these are the matters which are found in the water itself, and which appear to be the ultimate products of the decomposition of the substances which it contains. Am I not justified then in saying that this deliquescent matter is the matrix of the saline and earthy bodies which are found in the water; that this deliquescent substance contains all the ingredients out of which the common salt, the sulphate of lime, the carbonate of lime, are formed by its ultimate decomposition; and that the heat only effects suddenly the separation of the volatile substances, that which takes place slowly in the natural progress of decomposition? Not, however, to deviate further into subjects of mere speculation, I shall mention only one beautiful phenomenon which appeared, and which I conceive strongly confirms this view of the subject.

4. I had heated some of this same saline residuum on a slip of platinum, and a portion of it had run down from the heated point to the edge of the slip. On the surface of this substance, which was for the most part a brown shining substance, appeared a beautiful brilliant plate of the lustre of steel; a similar matter was observed in its body, and one of its edges was composed of partly the brown matter, partly the shining metalliform matter, both being perfectly uniform in shape, and continuous with each other. Hence it clearly appears, that it is the property of this matter to assume a metallic appearance before its conversion into its ultimate forms. I shall not pur-

sue the subject further, as not being suitable to this place, but shall now take a cursory retrospect of the matters which have been discovered in the course of these observations.

RECAPITULATION.

We have, 1. *Carbonic Acid*, developed from the insoluble residuum of the London water, of the Windsor water, and no doubt it might have been from the Teddington water first collected, had it been treated by the same process. This acid is common to the mineral and the organic kingdom, being constantly produced in organic processes and decompositions: and, considering the manner in which it was produced, it proves that that residuum must be regarded to be the product of organic matter in each of these waters.

2. *The Inflammable Gas*, which was duly observed in the London water only, but which would probably have been observed in both the others by correct observation. It was convertible into oxygen, carbone, and azote, and affords the same conclusion as the carbonic acid; the abundance of azote denoting a product of animal matter.

3. *Sulphuretted Hydrogen*. This substance was not distinctly separated in any of the processes employed, but its presence was indicated by its odour in all the waters, and it was more than once demonstrated by its regular test. There is no doubt, then, that it may be produced from them all. It belongs to each of the three kingdoms of nature, and may be abundantly produced from animal matter.

4. *Phosphoric Acid* we have proved to be found in the London and in the Windsor water in these experiments; and in the Teddington water in the experiments which have not been given in detail, and in which no heat was employed. These last observations render it probable that this acid is not formed by the action of the fire, but that it exists fully formed in the water itself. Indeed I think it was extracted more fully and distinctly where the acid alone was employed than where heat had been used. It must, without doubt, have been derived from animal matter.

5. *Charçoal* has been obtained by the new and simple process first made known in this Memoir, from the Thames water, wherever taken up. Nor do I entertain a doubt that, by the application of the same method, it may be separated from many of those waters, which are now considered to be perfectly pure. Its production can be ascribed to the decomposition of organic matter alone; and it is the most appropriate test of the presence of such matter in the subject operated upon.

6. *Nitrous Acid* has appeared in like manner produced from a particular part of the residuum of the Thames water, wherever taken up; from the New River water treated in like manner; and from that peculiar deliquescent substance, which, I apprehend, will be found to contaminate every common water applied to domestic uses. Its constituent principles are azote and oxygen, both of which must therefore enter into the composition of this matter; and the azote indicates that it originates from animal matter.

I conceive that it is owing to the production of the nitrous acid that we have found no trace of ammonia in the course of these experiments; which is a matter containing azote as one of its elements, and is furnished abundantly by animal matter. But the azote was employed in the production of nitrous acid, and hence no ammonia was produced.

These were the matters which were produced from the Thames water, wherever taken up. There were some peculiar to the particular points that were examined.

The Windsor water afforded *hydrocyanic*, or *prussic acid*, a matter which, in a state of concentration, is the most active and sudden of all poisons. But by this description I wish to excite no absurd and ill-grounded alarms. The matter may have been a product of the heat employed, and the elements only of this substance have existed in the water; those elements are hydrogen, carbone, and azote. The production, therefore, of this substance is an additional proof that the Thames at Windsor is contaminated by animal organic matter.

Of the body having the appearance of *arsenic*, I wish to say nothing in the present unfinished state of the inquiry.

The water taken up at London gave the remarkable appearance of the *oily matter* described in the experiments; and from an oil being produced a second time from the same water and not from the others, I incline to the belief that it is peculiar to the river at this place. I certainly cannot but regard it as a proof of the abundance of animal matter which is undergoing decomposition (putrefying, if you please to say so,) in the body of the river. We learn, from writers on the subject of putrefaction, that an oily matter is one of the last portions of the animal body which resists decomposition. The last product of this process of destruction is a small quantity of a kind of unctuous earth, called animal earth, which remains after the disengagement of the volatile substances, which form the great mass of the putrifying body. It amounts only to some hundredth parts of the original matters, and contains, besides other principles, a carbonaceous fatty substance, which by distillation gives out a portion of oil. This fixed residuum of the animal body retains for a long time a portion of oily bodies, more or less concrete, which is destroyed by very slow degrees, and for complete destruction almost always requires a long course of years.* Add to this, that animal bodies putrifying under water are more disposed than in any other situation to be converted into fatty or oily matter. These circumstances create a strong suspicion that the oily matter which I detected is a real residuum of decayed animal bodies, and that it will be found constantly, if looked for in the manner I have described. This alone would form an insuperable objection to the use of this water in any article of diet.

But though other domestic waters may not be tainted by this particular product, it does not follow that they are entirely free from oily matter, derived originally from the same source of animal bodies. The matter precipitated by salts of lead appears to me from its obvious characters to be an union of the oxide of lead with an oil. It is tenacious, with an unctuous feel, and I suspect that a skilful analysis would extract an oil from it.

* I have taken these facts from Fourcroy, who appears to have derived his information from authentic sources. See his Connoissances Chimiques, om. ix. pp. 96, 97, &c.

Even the empyreuma, which arises in the distillation of common water, and which gives the offensive smell when the distillation has been carried too far, proceeds from an oily body of great volatility. This oil becomes concrete by exposure to air, and is precipitated; upon which the empyreumatic smell ceases.

THE CONTAMINATION OF WATER IS FROM RECENT PUTREFACTION.

These facts lead me to state in a few words, why the impregnations of water, both the saline and earthy matters, and whatever substances are capable of conversion into saline or earthy matters, are of necessity a product of recent putrefaction. Of course I exclude matters obviously proceeding from permanent sources, as salt springs, mines, and so forth. I think a very slight consideration must convince any one that these impregnatious can have no other source than what I have stated. They must all be produced either from bodies subsisting and perishing in the water itself, or be brought from the soil over which the water passes, or through which it filtrates. Of the matter originating from the first source, there can be no question. Let us now take any spot of earth, which has preserved its present site and configuration beyond the existence of human records. When this spot first obtained this site and configuration, whatever matter it contained that was liable to be changed, or to be dissolved by the agents with which it was brought into contact, must have undergone this change and the effects have been wholly completed in ages long past, and antecedent perhaps even to the existence of the human race. If bodies of any kind still retain what seems their primæval form, they must either consist of matter on which air and water have no effect, or have been placed in situations entirely removed from their influence.

These agents of never-ceasing change have destroyed not merely the perishable works of human art, so that immense cities have absolutely disappeared, and left not a vestige of their existence, but solid rocks have been converted into fleeting sands, and mountains have been levelled, and disappeared from the face of the earth.

It must follow then, that whatever there is of what is soluble and perishable in the spot which we have imagined, must be of a formation posterior to its primæval structure, and whatever matters are found disseminated through the soil of such a spot which are destructible, can exist there only because they are constantly renewed.

To come more closely to the point in question.—In the soil of our country, examined at this day, are found substances soluble by air and water, as carbonate of lime, sulphate of lime, and magnesia. Of these one is soluble by water alone, the others by carbonic acid, though in small proportions; but however small this proportion is, some must be carried off daily. Of necessity, then, some must be renewed daily; and there must exist in the soil itself a matrix of these bodies, or a matter which, by the aid of the atmosphere, is converted into them.

If any saline matter, as common salt, or any other salt, is constantly found in the soil, the same reasoning must still more evidently apply to it. There must be a matrix, or a body convertible into these saline bodies.

What now do we find in nature subject to this eternal mutation, but animal and vegetable matter? And this change, what is it but putrefaction? It is indeed void of the offensiveness of putrefaction when taking place in large masses of matter; but still the process must be the same, only diffused over an extended surface.

Thus then these substances, the common salt, the carbonate of lime, the sulphate of lime, will, by solution, necessarily find their way into our brooks, into our rivers, into our wells. But will it be so of the matrix from which these substances are generated? It must depend upon whether this be matter soluble or insoluble in water, and to determine this point seems a proper object of experiment, and one that is absolutely necessary to an elucidation of the very important point regarding the origin of the contamination of common water.

What I have done myself on this subject is very trifling indeed; but, trifling as it is, it has led to a very satisfactory conclusion, and I think it therefore deserving of insertion in this place.

COMMON SOIL EXAMINED.

I steeped in pure water, for about forty-eight hours, a small quantity of mould taken from the side of a bank, stirring the matter frequently; filtered the water, and evaporated it to dryness. When concentrated the water had a yellow tinge, and it yielded a yellow deliquescent residuum.

A little of this matter heated on a slip of platinum blackened on the first application of the heat, and left a perfectly white matter on the platinum, which, with a drop of moisture, reddened turmeric paper, like an alkali.

So far then this matter was similar to the deliquescent substance of the water. But its other properties were different. (a.) An alkali caused an abundant precipitate. (b.) Some of it was kept in a red heat in a platina crucible. The residuum was deliquescent, and contained muriate of lime; it likewise smelt strongly of sulphuretted hydrogen. Sulphate of lime seemed also to be produced. (c.) Some of this matter was precipitated by nitrate of mercury. The precipitate was yellow, and heated in a tube, it left a residuum of charcoal. Nothing like nitrous acid rose into the tube, but a matter which showed acid properties by reddening paper tinged with turnsole. It appeared to be an ammoniacal salt with an excess of acid.(d.) Another portion of this matter was heated in a tube *par se*. It left a large residuum of charcoal, mixed with some white particles. A large quantity of white smoke was driven out of the tube, and a dark matter was sublimed, which was neither acid nor alkaline, but which was readily proved to contain ammonia. The smell of the tube was precisely that of soot.

That this matter is not precisely the same as the deliquescent substance of the water can occasion no surprise. We have it here at its origin. It must be subject to constant change from the action of air, water, and whatever other agents come in contact with it. It must probably generate ammonia, which would precipitate a carbonate of lime, and give a ready solution of the constant renewal of this substance in the soil. We see then from this examination, however hasty and superficial, that there exists in the soil a deliquescent *magma* of very complicated constitution; and from the products which it yields it must have originated from putrefaction, or from the decomposition, in part at least, of animal matter: that this substance, though from its constitution in a constant state of change, must be at all times moist, except during the rigour of frost, or, perhaps, superficially in the burning heats of summer, and it must, of necessity, be contained in every drop, rill, or current, which uniting form our pools, our lakes, our brooks, our rivers: that neither fountain, spring, nor well, can be free from it: and I have long entertained the opinion, that the very purest of our springs, all of which leave some residuary matter, however slight, might be proved to have received some contamination from this never-ceasing work of putrefaction, though from defective chemical skill, and being unable to gain any light from published works to guide me, I have been hitherto unable to give an adequate proof of it, except in the cases where the matter is precipitated by salts of lead.

The foundation of all reasoning on the properties of domestic water, ought to be laid on a knowledge of the properties of rain water. The accounts given of it by chemical writers, who assimilate it to distilled water, are entirely erroneous, which, when a fit opportunity offers, I am prepared to prove.

This, however, I may consider as a demonstrated fact, that the contamination of domestic waters proceeds from putrefactive processes; that these processes being a part of the course of nature, there is a real impossibility of obtaining a water of absolute purity, except by an artificial process. This must be true in all countries and in every situation. But in a country like England, where the arts of industry and of cultivation are carried to excess, and particularly in the vicinity of the metropolis of England, it appears to be a real impossibility.

But I shall abstain from pursuing this subject further, till I see what are the measures proposed either by the Commission of Inquiry or by any parliamentary committee for attaining the object which all have in view, intending to return to it on another occasion, if necessary. I will now, therefore, proceed to state the inferences which follow from the facts and observations made in the course of these experiments.

GENERAL CONCLUSION.

The result of the examination of the Thames water at London, has proved that every product of it which can be obtained and exhibited in a distinct form, is tainted, or to speak more truly, is wholly composed of the *exuviæ* of animal and vegetable matter. We have extracted bodies which can be traced to no other origin out of the soluble saline matter; out of the insoluble residuum; out of the hardly soluble sulphate of lime; finally, out of that insoluble matter separated by simple boiling, and improperly called carbonate of lime. We have shown that the insoluble residuum itself is partly at least, if not totally, volatile at the low degree of heat of boiling water, a property which distinguishes it completely from all common earthy matter, and entitles it to be ranked among organic bodies. We have extracted from it oily substances, which can be traced to no other source than to the accumulation of organic remains. Finally, we have extracted abundantly the constant residuum of organic bodies, a charcoally matter, by the simple process of solution in acid (under peculiar circumstances) which I conceive could never happen but with a water deeply and incurably tainted with heterogeneous substances, deposited from the filth with which it is incessantly mingled. No man, therefore, I should think, who calls himself a chemist, or who aspires to the more exalted title of a philosopher, can have the hardihood to deny that this water is loaded with animal and vegetable putridity, in as high a degree as it is possible, in any which the human organs can sustain. And all those who, thinking with me, consider such matters to be deleterious and destructive of animal life, must join in condemning it as unfit to be applied to any dietetic purpose whatever.

If we trace the river higher up to the point where the tide nearly ceases to be observed, strong presumption has been afforded that the same mischief predominates, but in a somewhat mitigated form. Circumstances which have been already detailed have prevented my acquiring the evidence on this point which it was intended and desired. But I entertain no doubt that it will be established, that the pollutions of the river received at the metropolis are carried upwards as far as the influence of the tide extends. The public will be enabled, probably, to form a proper judgment on this part of the question from sources to which I have no access. Analyses of these waters, undertaken by authority, but conducted, doubtless, on principles wholly at variance with those that I have adopted, will be published. If from them it appears that the same saline substances, the same or nearly the same quantity of insoluble matter, with the same general properties, are obtained both at this point and at those in the proximity of London, the conclusion that there can be little difference in salubrity between the water taken from the one and the other points will be sufficiently established, and the condemnation passed upon the one must be extended to the other.

If then the opinion I have given on this point is correct, the drawing of the supply from any spot where the influence of the tide reaches is a proposal not to be thought of. Whether taking it from a still greater distance (let us say from Windsor) would be an effectual remedy for the evil, is a question deserving of consideration.

Now this is really not a particular, but a general question; which is, whether river water, in general, can be considered to be perfectly salubrious? There is no reason for thinking that the quality of the Thames water at Windsor differs essentially from that of any other river of the same magnitude; nor do I doubt, that whatever matters have been proved to exist in this water, may be detected in all others, in this country at least. These matters are really nothing more than the washings of the soil, in conjunction with the *exuviæ* of animals and vegetables nourished in the water itself; principally, I suppose, the former. If the sources of the river are derived from barren or mountainous districts, or from large tracts of a meagre and sterile country, the waters will, of course, be less loaded with foreign matters; but if the streamlets, whose accumulated waters become a great river, pass through a rich and fertile country, where there is a dense population; where cultivation has arrived at its highest perfection; where large capitals enable the farmer to cover the whole ground with rich manures, and where large cities supply inexhaustible stores of recrementitious matter, the waters must then partake of the properties of the soil, and must become loaded with whatever soluble matter an abundant putrefaction can supply. But both in the one and in the other case the contaminating matter must be the same in kind, being drawn from similar sources, and differing only in quantity.

The substances, then, which we have found in this water of Windsor, are what will, I doubt not, be found in every river, and perhaps in every streamlet of Europe. The very same substances are in the water at London, but in increased quantity, and joined to other substances from which the Windsor water is exempt. I will notice only the charcoally and phosphoric matters, those being the most universal products of organic decomposition.

From thirty gallons of London water we obtained 28 grains of charcoal by precipitation. That obtained from six gallons of the Windsor water was not weighed; in fact it appeared too small to be weighed, and I am certain it could not have amounted to a single grain. We may say, perhaps without a gross error, that these products were in the ratio of 28 to 4, or 7 to 1. But there was a large quantity of charcoal procured from the London water by a different process, of which not a particle appeared in the Windsor water.

Of the phosphate of iron, the Windsor water gave 1·5 grains only; the London water (on an average) 4·25;* therefore the London water contained three times as much of the animalized substance affording this product as the Windsor water. Add to this, the latter was wholly free from those oily substances which were extracted from the latter. These facts united impress upon us the conclusion, that the contamination of the water at Windsor is incomparably less that at London, and that the substitution of it would be of great advantage to the general health.

But if I am asked, is this water absolutely salubrious, that is to say, does its continual use produce no disease in those who use it? Here I am compelled to say, that I think it the very opposite of salubrious. Whatever are the ultimate mischiefs which follow from the use of unwholesome water, must

* I here calculate from the relative quantities obtained by the action of muriatic acid without heat; the matter separated after heat had been applied was 15 grains, but in this there was a great excess of oxide of iron.

attend the use of this. It would be tedious and useless to labour on this point by a string of arguments. Let the products which have been extracted from it speak for themselves. An arsenical taint has been almost proved against it; this fact alone is surely enough to rouse the suspicion of any inquiring mind. Supposing that, however, to be an idle fable, putrescent animal and vegetable matter it has been proved to contain in abundance. If this, then, be the evil now complained of, the drawing of the supply from Windsor could be nothing but a partial and imperfect remedy.

But let us suppose this water to be perfectly salubrious, I ask, how is it to be brought to the metropolis, so as to retain its quality? I presume an open canal, like that of the New River, is the only device which has entered the heads of surveyors and projectors of new water companies. How then is it to be kept free from the incursion of the land-springs, with which it would come in contact during the whole of its course? The sides of this canal may be puddled, as is done in navigable canals; but what is to be done with the bottom of our canal? Here, of course, the land-springs would be constantly coming in, the quality of the water would be constantly changing, and when our water arrived at the metropolis it would be any thing but the pure streams of Windsor. These land-springs, it is to be observed, are the drainings of a country in the highest cultivation, the most covered with manures, and the most densely peopled of any part of England; and it is not improbable that they are contaminated from these causes, almost as much as the Thames itself is from the sewers of the metropolis.

The nature of the country through which our projected canal is to pass confirms greatly the force of this objection. The whole of the northern or Middlesex side of the Thames is for miles a dead flat, and must have been, previous to cultivation, a complete morass; and its properties as a morass are not eradicated at this day. Agues are still common in these districts. Now, in such a soil, water is always found within a foot of the surface, even in the heat of summer, and water of the worst description, marshy water, condemned by every writer as a prolific cause of disease. Cultivation and drainage have no doubt mitigated this evil, but it has not cured it; and cultivation, by an accumulation of recrementitious matter on the surface of the soil, has added, probably, as much to the mischief as it has taken away. But our canal passing, as it must, through the centre of this soil, would act as the real drain to it, into which would be constantly flowing a stream of this noxious fluid.

Can it be believed that the New River water at London and the New River water at Ware are really of the same quality? I conceive it to be impossible. I think that if the snows of Snowdon were conveyed across the kingdom in an open canal, hardly a particle of them would reach the metropolis, but we should obtain only that furnished by the soil through which this canal passed.

If then a really pure and wholesome water can be found within thirty miles of the metropolis (which I hold to be impossible), it would be absolutely necessary, in order to preserve its purity, that it should be conveyed either by pipes, or by an aqueduct; both of them impracticable projects by any private association, if a due return for the capital expended be one of the objects in view.

I throw out these general reflections as the mere suggestions of my own mind, with a view to check the evils of ill-founded and unsuccessful speculation. If my ideas are wrong, they will be corrected by those who have thought more justly and more profoundly on the subject. But, for the most part, those to whom inducements will be held out to embark their property in projects of this kind, have certainly not thought at all upon the difficulties with which they are environed, and the disappointments to which they may be subjected. My own end will be answered, if such persons shall be induced to pause, and consult the best sources of information, before they hazard that, the loss of which may make the remainder of their lives uncomfortable.

I shall offer two more reflections only, before saying a few words on what I conceive to be the only practicable remedy for the correction of what I believe to be an intolerable nuisance.

My first observation is, that the art of analysis, as at present applied to the analysis of water, is unable to detect the greater part of the matters which escape from decomposing organic bodies. We by it determine little more than the fixed parts of these bodies, which remain after the dissipation of the far greater part of their substance. These fixed portions amount probably only to some thousandth parts of their mass, as it existed when perfect; the remainder has vanished in the form of vapours or gas, of an infinite variety of composition. Let it not then be thought, because we extract only a few grains of an organic body from several gallons of fluid, that it must be inert by reason of its minuteness. These few grains are the index of some thousand grains of vaporous substances which taint the whole body of the fluid, and of whose influence on the body we are for the most part ignorant. But of these vapours we have a very limited knowledge, and, except of the carbonic acid, no means of detecting and discriminating them. It is in vain therefore to say, that where nothing is discovered there is nothing wrong. The defect is in the imperfection of the art.

What, for example, is the matter which gives a certain offensive odour to the Thames water at London? Here I am in absolute ignorance. I hoped to gain some insight into it, by examining the very first product of the distillation of this water. But I found that neither the nitrates of silver, of mercury, nor of lead, produced in it the smallest cloud. We should consider this water, therefore, to be perfectly pure, as judged by these tests; but in this judgment we should probably err.

My second observation is, that the question of the salubrity of water is not one that can be solved by chemistry alone, independent of experience. Suppose I gain from water prussic (hydrocyanic) acid, or a real arsenic, why, I can gain from sugar oxalic acid; and though this acid is a strong poison, the sugar is not the less wholesome. If those substances can be extracted from water, it is only because such substances or their elements are the constituent principles of organic matter, the introduction of which into the animal frame, in some form or other, is necessary to life. Nothing then regarding salubrity can be determined by chemistry. But chemistry renders an important service, by directing the mind to the proper objects of suspicion and research. It gives a clue to the elucidation of phenomena, which are, without its aid, involved in the most profound obscurity. It teaches us how the matters, which in one form are agreeable and nutritious, may become offensive and noxious by a slight variation in the proportions of their elementary principles; and above all, it affords a prospect of rendering the most essential service to humanity, by tracing effects to their true causes, and by thus teaching men either the prevention or the remedy of evils otherwise unavoidable.

THE REMEDIES CONSIDERED.

What then is to be done to correct the present evils, which I have denominated, I think with perfect propriety, a most intolerable nuisance. I have proved, I hope, that bringing a supply from any point of the Thames would be a very imperfect remedy, were it practicable, and that it hardly is practicable, except at an expense which would never remunerate any private association; and I fear greatly that the very same objections will be valid against procuring a supply from any new and distant sources.

If then it is vain to look at a distance, let us turn our attion to the means within our power, and ask ourselves whether it is not possible so to correct that which at present supplies in abundance this immense metropolis, as to render it completely and perfectly innoxious. And I now re-assert what I asserted and proposed upwards of twenty-three years ago, that this can be done easily, completely, and cheaply, by the practice of distillation, and that it can be effected by no other means whatever.

We can conceive only four modes of purification of water. 1. Chemical agents; 2. Boiling; 3. Filtration, and these two may be united; and, lastly, Distillation.

1. For the first, a little alum clears the foulness of water very readily; the salt is decomposed by the carbonate of

lime, and the alumina carries down all sensible impurities; but the acid retains the lime and other matters united with it, so that probably little or no difference in point of salubrity is effected. Experience has not shown the utility of any other mode of chemical purification.

2. Boiling and suffering the sediment to subside I consider to be a decided improvement, and as such is practised by many.

3. Filtration without boiling must be less perfect, removing only sensible foulnesses; but boiling the water first, and then filtering, are practices that ought to be employed by those who have no means of using a more efficacious remedy.

4. Filtration through charcoal frees water from putrid and perhaps from putrescent vapour, and so far must be preferable to every other mode of filtration. In other respects it cannot change the chemical composition of the water. It is obvious therefore that some real advantage is gained by these practices, and that they ought to be generally employed, if other methods of purification are impracticable.

DISTILLATION PROPOSED.

But I have, as I trust, demonstrated in this particular case of the Thames water, that every one of those substances which are held in solution by it are tainted with the very same contamination; that the carbonate of lime, the sulphate of lime, the common salt, are united to a quantity of heterogeneous matter of organic origin; that the solid residuum is of similar origin; and the deliquescent body is composed wholly of elementary matter derived from organic bodies. Now, with the exception of the carbonate of lime, boiling and filtration cannot deprive the water of any of these ingredients; and distillation appears therefore to be the only resource we have left.

A popular and estimable writer, whilst in words, speaking unfavourably of this practice, has given, I think, the best possible reason for its adoption; and I will quote his words that the weight of his reasons may counteract the weight of his authority. " How far," says Dr. Paris, " the existence of fo-
" reign matter may injure its salubrity, has been a subject of

" much controversy. The truth, perhaps, lies between the ex-
" tremes; those who insist on the necessity of distillation for " its purification, and those who consider every species of " water as alike salubrious, are in my opinion equally remote " from truth; that the presence of minute quantities of " earthy matter can become a source of disease appears ab- " surd; while it would be highly dangerous to deny the mor- " bid tendency of water that holds putrescent animal or vege- " table matter in solution, or which abounds in mineral impreg- " nation."* Here I must take the liberty to claim the doctrine of this last sentence (as far as regards the putrescent matter) as my own, advanced by me three and twenty years ago, and made the foundation of the recommendation then given by me of distillation, to free the water from this putrescent matter.† And as it is acknowledged by Dr. Paris, that it would be highly dangerous to suppose this putrescent matter innocent, it would have been an acceptable service to his readers to have been informed how to detect this putrescent matter, and how to avoid it when detected. But instead of this we find distillation spoken slightingly of, and a filtering machine alone recommended, which will leave the putrescent matter just where it found it. But as in principle there can be no disagreement between me and Dr. Paris, I shall say no more, than that I recommend this point to his more mature consideration.

The salubrity of distilled water has been put beyond a question long ago, and repeatedly by the experience of navigators, whose names are celebrated throughout the civilized world: Cook, Bougainville, Phipps, Hamelin, and many others. And so much is the use of it established in the naval service, that a large manufactory has been established in London now many years, for the supply of vessels with an apparatus adapted both for cookery and the distillation of water.

I know very few recorded observations on the effects of

* Paris on Diet, p. 120, 1st edition.

† See my " Inquiry into the Origin, Symptoms, and Cure of Constitutional Diseases, *passim*; published in 1805.

distilled water on the human economy, as distinguished from common water. The late highly respectable Dr. Heberden was a strenuous advocate for the use of perfectly pure water, recommending it by precept, and as it is said, by example. It is much to be lamented, therefore, that he did not leave behind him some memorials of its effects, either on healthy or diseased subjects. A slight remark that distilled water relieves pains of the stomach is all, as far as my recollection serves me, that can be collected from his writings.

EXPERIMENTS WITH DISTILLED WATER MADE BY THE FRENCH GOVERNMENT.

The result of some experiments made with distilled water by the French Government is given in the *Annales de Chimie*, which I think sufficiently distinct and interesting to deserve insertion. The experiments were made at Brest, Rochefort, and Toulon, at the same time, and an account of them is extracted from the *Annales Maritimes et Coloniales* of January, 1818.

At Brest, eight galley slaves were confined for a month to the use of distilled water. They used the ordinary diet, and were placed together in a chamber of the Marine Hospital. During the first days of the trial, these men complained of a sense of weight at the stomach. This was attributed to the confinement in a room of men accustomed to constant labour in the open air. They were ordered to exercise for two hours in the air daily, and the uneasiness diminished; and it completely disappeared by the addition of an ounce of biscuit to their ordinary ration. One of these men, on the twenty-ninth day, had symptoms which he himself attributed to an indigestion from lard, and which was removed in five days after the conclusion of the trial. With this trifling exception, the health of these eight men continued perfect.

At Toulon the event was nearly similar; the men (six in number) after a few days complained of insufficiency of food, and soup was added to their ordinary ration. The Commission remarked, that all the men acquired *enbonpoint;* that the complexion became more clear and coloured; in a word, that the health was ameliorated. One of them suffered an indisposition of constipation, colicy pains, and diarrhœa; but these quickly disappeared without discontinuing the water, which therefore could not have occasioned these symptoms, which may be attributed with more reason to the dry and bilious temperament of the subject, exalted by heat and dryness, which prevailed at Toulon in the month of August.

At Rochefort there were two parties, one of fifteen galley slaves, who were in the hospital; men whose digestive functions were perfect, but who were suffering from trifling affections, such as simple ulcers, rheumatic pains, &c. These men were kept in ignorance of the quality of the water given to them. Twelve other men, on the *Ile d'Hénet* (a fortified rock in the middle of the road) underwent the same course.

Of the fifteen galley slaves, who were placed together in one chamber, two after a few days complained of colic and diarrhœa; but they were practising an imposition, the latter complaint being disproved by inspection of their evacuations. Many of these men were cured of their wounds and their rheumatic pains by this course of distilled water; and it was remarked that one of them, suffering from jaundice, received a sensible amelioration of his complaint.

Of the twelve men on the Ile d'Hénet, one had a diarrhœa, which quickly disappeared, though the distilled water was continued. This indisposition was followed by a slight ophthalmy, which, under the same regimen, yielded to the most simple application. A boil appeared on a second subject; and a third, subject to annual access of fever of the season, suffered some paroxysms, and was completely cured, without changing the regimen. All the others remained in perfect health.*

We find, then, from this account, that the substitution of distilled for common water, tried upon forty-one individuals, produced almost uniformly an improvement on the health. In the experiments instituted at Brest and Toulon, the appetite improved; for in both it was necessary to increase the allowance of food. At Rochefort, the diseases which the men were

* Annales de Chimie et de Physique, tom. vii. p. 220 (New Series).

suffering were relieved. These authentic facts, then, completely confirm the statements made by myself (some of them about twelve or thirteen years previously) on the beneficial consequences to the health, of the use of water purified by the only method of complete purification, by DISTILLATION. These facts have not been wholly lost to society; as is proved by the slow but never-ceasing increase and diffusion of the practice recommended. But to the mass of the community they have continued to be wholly and completely unknown; and if not unknown to the medical profession in general, they are at least unheeded, or affected to be despised. I hope, therefore, that I may be excused, or even that I may lay claim to the performance of some service to humanity, if I here bring again before the public a very short abstract of some of the facts contained in the earliest of my publications, confining myself to the cases in which no change of regimen was employed, except the use of distilled instead of common water.

ORIGINAL EXPERIMENTS WITH DISTILLED WATER.

1. The Rev. Mr. M. had been subject for sixteen years to the most severe and distressing sick head-aches, attended with the derangement of the digestive organs common to this complaint. This disease, with its concomitant affection, was palliated in the most striking manner by the use of the distilled water. The paroxysms came on much more rarely, and with so little severity, that whereas they had confined him before sometimes for twenty-four hours, sometimes for forty-eight hours, he was now enabled to follow his ordinary occupations.

Having used distilled water about nine months during the year 1804, towards the close of the year it was discontinued for a time; in consequence, the disease began to return with its former frequency and severity, and it was again relieved by the use of the distilled water.

2. A little boy, afflicted with a paralytic disorder of the lower limbs, acquired a considerable increase of power over them in

the course of three or four months, and the contracted muscles became more full and strong. So much, however, was gained at the end of about nine months, that he was enabled to get up stairs without aid, though the legs had been at first quite motionless.

3. A clergyman, in the most deplorate state of universal paralytic weakness, received a remarkable accession of strength, so as to be enabled, at the end of four months, to take long walks; ulcers on the legs first inflamed and became painful, and afterwards perfectly healed. The lower limbs regained the flexibility which had been nearly destroyed.

4. A lady had been afflicted some months with a hot and red efflorescence on the face, forehead, and eyelids. By using distilled water the efflorescence faded greatly, but the change produced great flatulence, head-ache, and constipation. She renounced the water after a short trial, and the efflorescence returned. In a fortnight she resumed the distilled water, and now it produced no disagreeable effects.

5. Mr. Goring, a gentleman who had filled a high situation in the service of the East-India Company, gave me the most satisfactory evidence of the beneficial effects of distilled water, in two letters dated September 30, 1804, and March 18, 1805. He escaped from gout, to which he had been subject for a series of years, and though he had reason to expect a paroxysm according to the experience of former years; and with this the general health improved, particularly by the removal of flatulence, acidity, and distention of the stomach The lower limbs had become so stiff as to be an impediment to exercise. This stiffness, to which he had been subject for a series of years, was totally removed, and he was enabled to take long walks without inconvenience.

The effect this change had on the tongue I shall relate in his own words. " I feared (he said) myself to be liable to be
" attacked by another and more serious calamity, which I felt
" yearly increasing upon me; I mean a diseased condition of
" the tongue. This uneasiness several years ago affected me
" by an extraordinary dryness, tingling, and aching, particu-
" larly in the morning. On first waking, it appeared to me as
" something not belonging to myself, but as if it were a dried

"tongue put into my mouth, whose flexibility was impaired "and almost destroyed. It has now in a great measure re-"gained its moisture and flexibility. It has sometimes a "tingling in the morning, but the aching pain is gone," &c.

He goes on to say, that in the year 1779 he returned from India in a French ship; the passage was of eight months, and the ship wretchedly provided with fresh provisions and water. Immediately after his arrival in England, he was attacked with a very violent fever, which fell (as he says) on his lower gums, and in the end deprived him of the teeth of the lower jaw. "Soon after," he adds, "I felt a circle rising, and gradually "extending towards the root of my tongue, occupying inter-"nally both the upper and lower jaw, including the tongue. "This I occasionally washed with warm water and spirits of "wine, which caused it to smart exceedingly, whilst the sound "parts bore it without pain. This circle has been gradually "decreasing since I have drank the distilled water. It is now "confined to the lips and end of the tongue, whereas it was "before getting gradually more and more towards the root of "the tongue."

6. Mr. Bodenham, a country gentleman residing near Hereford, gave me an equally distinct evidence on the beneficial effects of distilled water in the case of his wife, who was an habitual sufferer from a high degree of what may be called a bilious disease, with great muscular debility. He informed me that the strength increased, the digestion improved, the skin became free from an habitual yellow tinge. "When she "awoke in the morning," he said, "she had no longer that "wretched lowness and depression of spirits (which she used "to call the horrors), accompanied with a little low fever, "which she could never get rid of but by quitting her bed. "Her sleep is more comfortable and refreshing; her spirits are "improved, and she has certainly, in some degree, recovered "her *embonpoint*."

7. In a case of cancer of the breast in a woman of middle age, it was evident to the eye that the progressive increase of the disease, and in particular the spreading of the ulceration, was completely stopped by no other change than the use of distilled water. The whole breast was scirrhous but not

ulcerated. Round the tumour the skin was elevated into spots or empty vesicles. Two of these vesicles had ulcerated. One of the ulcers had gradually increased to the size of half-a-crown; the other was only of the bigness of a pea, and it had formed very shortly after the adoption of the distilled water; the first had appeared in the year 1804, which was six months previously, and from being one of these little vesicles had been slowly and gradually increasing to the above-mentioned magnitude. When the distilled water had been used about a month, it was observed that each of these ulcers became perfectly stationary, and they continued in this condition many months, the one retaining its acquired magnitude, the second never enlarging, but continuing of the size of a pea. These two ulcers lost their thickened edges, and acquired a flat, superficial, granulating surface, and no more ulcerations formed duriug the remainder of the life. In one word, as a local disease, it was rendered perfectly stationary, and what changes took place in the ulcerated surface, were processes of healing and restoration.

I had the opportunity of shewing this case to Mr. Abernethy in the year 1805, and endeavoured to impress upon him the changes that had been effected by the distilled water. Mr. Abernethy acknowledged the disease to be cancer.

8. A woman about sixty-four years of age, afflicted with a cancer of the breast, had for three years suffered pains of the limbs like rheumatism, and the right leg and thigh were so much contracted that she was a perfect cripple, unable to move or to stir from her chair. Among other remarks made on the effects of the distilled water, it appeared that the contracted limb at the expiration of about five months became less benumbed, and some power of motion was restored. This was in the winter of 1805. In the spring of the year following she had regained so much power as to be enabled to go down stairs and take short walks, with the aid of crutches; and this benefit she enjoyed during the whole of the year 1806, and the following winter.

I do not find this fact mentioned in my printed account of her case. It is, however, strictly true.

9. It may be thought that I have viewed appearances with

an eye prepossessed by a favourite hypothesis; I must therefore now appeal to the testimony of an unprejudiced, and certainly not a partial witness.

Mr. Abernethy, in the year 1805, permitted me to attend and treat the case of a lady suffering under a large ulcerated cancer of the breast. What the symptoms were, and what were the changes introduced by the use of the distilled water, I need not here relate. The case is the sixth recorded in my "Reports on Cancer," to which I refer my reader. After the process had been continued between two and three months, Mr. Abernethy gave me his opinion on the subject in the following explicit and decisive language:—"I cannot be insen-"sible to the effect of this treatment. Whether it will cure "the disease or not I cannot tell; but I have no doubt that it "would prevent it."

Certainly at the time such was my own opinion likewise. It is as certain that this opinion was in part erroneous, and that other measures are necessary for a successful treatment of this deplorable disease. But the effect of the distilled water cannot be invalidated, because it will fail without the aid of other measures; and these effects are such as to prove that its use ought to form an essential part of any plan of regular and proper treatment.

I expect from Mr. Abernethy's candour an acknowledgment of the truth of this statement; and that whatever error of judgment I committed at this period, he participated in it. He had at this time made much more numerous trials of distilled water than I had, from his great opportunities, of which I was totally destitute. But he has observed a profound silence on the subject in his published works. Some twelve or fourteen years after this period, he put forward some theory about drinking and the action of liquids, the honour of which (it being to me unintelligible) I shall not contest with him. But I must take leave to claim the merit of having shown the facts, or given the information, which has formed the ground-work of his present opinions, be they what they may. Mr. Abernethy is too rich in original stores of science, to need to withhold from the poor and the humble what is his just and most valuable property.

10. A widow woman, aged about sixty, had a hard indolent tumour of the right breast. It was of a granulated texture, as if composed of a number of small tumours compacted together. The nipple was retracted and the skin puckered. Blood had sometimes come out of the nipple, and there had been a serous discharge from a crevice that had formed in the skin contiguous. It had existed in this state many years. By the use of distilled water alone the diseased parts immediately appeared to have less tension and uneasiness. In two months the nipple became more elevated, and the wrinkle of the skin in a considerable degree unfolded. In five or six months, the little tumours of which the whole was apparently composed, were much more distinct, so that the mass seemed splitting into different parts. In a short time after this, a large quantity of serous discharge took place from the same point as it formerly done; which seemed to be a cicatrix of a small superficial ulcer, which opened afresh. At the end of a twelvemonth, the whole tumour was either wholly or very nearly absorbed, and the skin of the breast was brought almost into close contact with the pectoral muscle.

I do not cite this as being a case of cancer (though at the time I might have considered it such), but as a proof of the effect of distilled water, which was evident to the eye, and in which a fallacy was impossible.

CONCLUSION.

Why, now, have I drawn these facts at this time from the repose in which they have been slumbering, some of them three and twenty, others nineteen years on the booksellers' shelves ?* It is to re-assert, after these long intervals, the perfect truth and fidelity of the representations there made ; and to show our legislators, if the subject is brought before them, and if they will vouchsafe to me one moment of their attention, that they are not deciding on a question of little moment, but on one in which the life, the health, and the comfort of the community is

* These facts are extracted from two publications of mine ; one entitled, " An Inquiry into the Origin, &c. of Constitutional Diseases," published in 1805 ; the other, " Reports on Cancer," published in 1809.

essentially involved. These facts it was which have laid the foundation of a practice, which, though neglected by the profession, makes its way daily among the people, and which, I will venture to prognosticate, will ultimately triumph over every opposition of prejudice, of malevolence, or of a mistaken self-interest.

This method of purifying water may be applied at a trifling expense; not trifling, indeed, to those who think water to be an article that ought to cost nothing, but so trifling as to be within the reach of any individual, whose means are equal to his necessary expenditure. It is sold at present by several chemists at the price of four-pence per gallon, or even less; which may be on an average one shilling per week for a full-grown person. I can hardly doubt, then, that it may prepared on a large scale at the prime cost of a penny per gallon.

A great advantage of instituting experiments on a large scale, on the effects of perfectly pure water on the system, whether in health or under disease, is that it might be done immediately, before even estimates for any great and expensive undertaking could be made, and at a most trifling cost. But if it should be thought that diseased subjects would afford the most satisfactory evidence, I must protest against inferences from any trials, unless the whole of the regimen advised and introduced by myself in the treatment of chronic disease be adopted. Experience has proved to me, upwards of two and twenty years ago, that the use of distilled water alone, as a remedy for chronic diseases, would prove futile. The same experience has shown me, that, in union with a strict vegetable regimen,* its powers are far greater than I had myself hoped or imagined.

I do not think that public hospitals are the places fitted for

* I doubt not, that the disease which afflicted for such a length of time the inmates of the Penitentiary at Milbank, and which was attributed in a great measure to vegetable diet, has strengthened much the prejudice against its use. But, besides that the physicians very nearly renounced their first opinion on this subject, I am prepared to prove, from documents furnished me by the officers of this establishment, that the Penitentiary dysentery was, not only during its course, but at its very origin, a contagious disease, introduced by some unknown subjects or cause, and quite disconnected with the new diet, which had been introduced some months previously.

experiments of the kind I would wish to see instituted. The time necessary to be employed, in order to obtain satisfactory results, would cause too great a pressure on the funds of a hospital. But one of the warmest hopes that I have formed from the introduction of a regular and successful treatment of chronic diseases, has been, that eventually the public hospitals of great cities might be relieved from the necessity of receiving cases which really cannot be benefited by them, and be thereby rendered more serviceable to cases adapted to hospital treatment.

The diseased subjects in workhouses, penitentiaries, even in gaols or houses of correction, of any institution, in short, where the inmates are constant residents, and at the same time under a degree of superintendence and control, are the situations in which the treatment I have proposed might be easily adopted.

From the low price at which distilled water may be prepared, its employment by persons in moderate and even in very humble circumstances, may be left, with regard to such persons, to the progress of knowledge, to parental affection, to that regard to present ease or future comfort, which influences the conduct of every prudent individual. But there is an immense mass of population, who struggling hard to earn their daily subsistence, either must, or who undoubtedly will never use any thing in the form of water, but what costs them absolutely nothing. This class of the community, the instruments of all the comforts and enjoyments of the others, have the strongest claim upon a paternal government to be protected in the most efficient manner from the evil which it has been our object to expose; and indeed from all others which result from the frame of society itself, and which are not the indispensable inflictions of Providence.

THE END.

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