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Contributors

Marcet, Alexander Gaspard, 1770-1822.
Saunders, William, 1743-1817.
Royal College of Surgeons of England

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CHEMICAL ACCOUNT
OF THE
BRIGHTON CHALYBEATE.

By ALEXANDER MARCET, M.D.

ONE OF THE PHYSICIANS
TO
GUY'S HOSPITAL.

FROM THE NEW EDITION OF
DR. SAUNDERS'S "TREATISE ON MINERAL WATERS."

London: Printed by Phillips & Fardon, George Yard, Lombard Street.

1805.

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A CHEMICAL ACCOUNT OF THE CHALYBEATE SPRING, NEAR BRIGHTON. *

THE Chalybeate Spring, near Brighton, commonly called *The Wick*, has long been noticed as a ferruginous water. But as far as I can learn, no regular account of its chemical or medicinal properties has ever been published. The only notice which I can trace respecting this spring, previous to the mention which Dr. Saunders has made of it in his work on Mineral Waters, is contained in the *Brighton Guide*, where a very cursory and imperfect account of the most obvious properties of this water is given, first on the

* The Chemical inquiry which is the subject of this essay, was suggested by Dr. Saunders, and undertaken in consequence of his having expressed a wish that an opportunity might be afforded him to introduce, in the second edition of his "*Treatise on Mineral Waters*," now in the press, a more particular account of the Brighton Chalybeate, which he had merely noticed in his first edition. This paper has swelled to a much greater size than was at first expected; but as it is not intended to be published in any other shape, Dr. Saunders, instead of giving only an abstract of it in the above treatise, has done me the honour to insert it at full length, allowing me to procure a few detached copies of it for my own use.

authority of Dr. Relhan, whose observations are stated to have been made a considerable time since, and afterwards on that of Dr. Henderson of Brighton. But these accounts, in the present state of chemistry, can scarcely be of any other use, than that of enabling us to observe, that in earlier periods, this spring had not remained unnoticed.

§ I. *Situation of the Spring.*

The spring issues from the declivity of a small eminence, situated about the distance of half a mile to the westward of Brighton, and a quarter of a mile from the sea. The ascent from the sea shore to the spring, is very gentle and inconsiderable; but the eminence from which it issues, is commanded on every other side, by a succession of small hills, which gradually rise round it.

The hill which extends immediately to the westward of Brighton, and over which the Horsham road passes, consists chiefly of limestone. Extensive lime quarries are opened on the borders of this hill, and the soil for some distance continues calcareous. But on approaching towards the chalybeate spring, the

soil gradually becomes argillaceous, and the particular spot on which it is situated, appears to consist almost entirely of clay. Besides clay, however, it was found, in clearing away the rubbish to form the reservoir, that the soil in that spot, was intermixed with veins of a black oily combustible substance, some specimens of which I have seen, which evidently contained a quantity of coaly and pyritic matter. * In the immediate vicinity of the spring, some fir trees and shrubs have been planted, which seem to thrive; but except the turf, and some scanty heath, no spontaneous vegetation is to be seen for a considerable distance. On the top of the eminence, and at a very little distance from the well, there is a pretty large pond, apparently supplied by a spring, but which has no particular taste, or other striking properties.

For the accommodation of those who drink the water, a convenient building has been erected immediately over the spot from which

* For these specimens I was indebted to Dr. Tierney of Brighton. Mr. Tennant, to whom I shewed them, told me, that he had repeatedly found on the sea shore, at Brighton, similar fragments of coaly pyritic matter; a circumstance which he thought then rather singular, but which seems now to be accounted for.

the spring issues, where the water is received a few feet under ground, into a basin of Portland stone. This reservoir contains only a few gallons of water, but it fills again as soon as it is emptied, and is prevented from overflowing by a drain, which conducts the superfluous water into a contiguous pond. This basin, I observed, has its internal surface deeply corroded by the water, and its bottom is covered with a thick yellowish sediment, which shews itself abundantly wherever the water is allowed to stagnate, and particularly in the small crevices formed by the decayed brick-work which surrounds the well.

§ II. *More obvious qualities of the water.*

(A)—On first inspecting the basin, early in the morning, and before the water has been stirred, its surface is commonly found covered, sometimes entirely, sometimes only partially, with a very thin iridescent pellicle; and besides this, when the water has not been disturbed for some hours, there is also often a kind of yellowish scum, floating in irregular patches on its surface. The spring is at all

seasons very plentiful, and does not appear to vary sensibly in its qualities. Yet after heavy rains the water is sometimes slightly turbid. The temperature of the well was found to be 54° , when the thermometer stood in the air at 68° . I have been told by people who live on the spot, that the spring has never been known to freeze.

(B)—The water, after the small quantity of scum just mentioned has been removed, is quite clear and transparent, and no gas is seen to escape from it, although, if poured high from one vessel into another, there is a sort of sparkling appearance, which, I believe, would be found to be common to all waters, and ought not to be mistaken for a disengagement of air bubbles.

(C)—The Wick water, when quite fresh, has a peculiar faint smell, not uncommon in ferruginous waters, and a strong, though not unpleasing, chalybeate taste. Its specific gravity is 1001.08.* It instantly curdles

* The specific gravity was taken twice with sufficient care and accuracy, and with similar results. But in both cases the water was three or four days old, before I could have an opportunity of ascertaining its density.

soap. Some of the water which had been allowed to stand a whole night in a glass tumbler by the side of the well, with some of the scum above described floating on its surface, was found the next morning still perfectly clear; but the greatest part of the scum had precipitated to the bottom of the glass, in the form of a yellowish ochry sediment, in which I could only perceive a kind of faint earthy taste.

(D)—Having taken to my lodgings, in a glass bottle, a gallon of this water, free from any scum or sediment, it continued perfectly clear the next day, and exhibited no appearance of precipitation or any change whatever, except perhaps that the peculiar smell, which I have mentioned, was less perceptible. On the third day, some small air bubbles appeared on the surface of the water, but I could perceive no other change. The day after however, on removing the stopper, I remarked a slight hissing noise, as if a quantity of air was making its escape. The bottle had been kept well stopped, but as it was not full, a quantity of atmospheric air had remained in contact

with the water. At the expiration of five or six days, the water was found somewhat turbid, and small yellowish particles were seen floating in it. It had lost entirely its peculiar smell, and the chalybeate inky taste was less perceptible. After a few weeks, a considerable yellow sediment had subsided, the water had become more turbid, and the chalybeate taste was farther diminished. Some of the same water, which had been carefully corked up in a transparent glass vessel (no air being left in contact with it, and the vessel remaining at complete rest), preserved its transparency **much** longer. Yet after a few weeks, this water also was found to have deposited a yellow substance all over the inside of the vessel. But the water itself, on being poured out, after standing for near two months, and when the chalybeate taste was considerably diminished, appeared quite clear, leaving behind the yellow sediment, which adhered strongly to the sides of the bottle.

§ III. *General effects produced on the water by chemical tests.*

Having now stated the spontaneous changes which this water undergoes, and its most obvious properties, I proceed to relate the general results obtained by the application of tests, or chemical re-agents.

EXPERIMENT I.—Some of the water, just brought from the well, being heated in an open vessel, and kept boiling for a few minutes, no precipitation took place, and no other observable change was produced in the water, except a slight diminution of its transparency.

EXPER. II.—The same experiment being repeated in a tubulated receiver, the neck of which was immersed in mercury, in order to prevent the access of air, the water, after undergoing ebullition for a few minutes, appeared as transparent as before.

EXPER. III.—A small quantity of the water being quickly reduced by boiling to about

one-third of its volume, continued free from precipitate, although it did not appear quite so transparent. On attempting to push the concentration farther, a yellowish substance began to collect at the bottom of the vessel.

EXPER. IV.—Some of the water being slowly heated in an open vessel, and kept for about an hour over a lamp, without being ever allowed to boil, a yellow precipitate began to take place, before the water had undergone any sensible concentration.

EXPER. V.—The foregoing experiments being repeated on water that had been kept for some time, the separation of yellow matter took place more quickly; and in general, there appeared to be some kind of proportion, between the readiness with which the heated water yielded this precipitate, and the time which had elapsed after it was taken from the well.

EXPER. VI.—Water, quite fresh from the well, altered paper stained with litmus, to a reddish purple colour.

EXPER. VII.—The same water, after being boiled, altered litmus exactly in the same manner; and the change of colour seemed even to take place somewhat more readily in the boiled than in the unboiled water.

EXPER. VIII.—Paper stained with turmeric had not its colour any way altered by the water, whether previously boiled or not.

EXPER. IX.—Paper stained with the red infusion of Brazil wood, being moistened with the water, turned to a dark brown colour, with a faint purplish hue; and this effect took place, whether the water had been previously boiled or not.

EXPER. X.—Prussiat of potash instantly produced a blue cloudiness, and after the separation of the yellow particles mentioned in Exper. 3, 4, and 5, whether by boiling, or by keeping, still the clear water continued to strike blue with prussiat of potash, though evidently in a fainter way than before this operation.

EXPER. XI.—Tincture of galls being drop-

ped into the water just brought from the well, produced, at first, neither cloud, nor change of colour. But on being allowed to stand, the mixture gradually became cloudy, and the next day it was found quite black and turbid.

EXPER. XII.—Tincture of galls being mixed with some of the water, which had been kept for a week or two, the black precipitate took place immediately.

EXPER. XIII.—The sulphuric, nitric, and muriatic acids, produced no effervescence, precipitation, or hepatic smell whatever. But on the contrary, if the water had begun to undergo the spontaneous precipitation before mentioned, any of these acids restored its pellucidity instantly.

EXPER. XIV.—Oxalic acid produced no immediate precipitate; but a cloud appeared in the water after standing for some time.

EXPER. XV.—Both oxalat of ammonia and oxalat of potash, produced an immediate white precipitate.

EXPER. XVI.—Caustic alkalis occasioned a precipitation of yellowish flakes, and the same effect took place in water which had been boiled. Lime water produced similar effects.

EXPER. XVII.—Barytic water instantly produced a copious white precipitate, whether the water had been boiled or not; and this precipitate was not redissolved by adding muriatic acid.

EXPER. XVIII.—Both muriat and nitrat of barytes, threw down a copious white precipitate.

EXPER. XIX.—Nitrat of silver produced a whey-coloured cloud, which, on standing, subsided, and passed to a grey colour.

EXPER. XX.—Having previously added a few drops of nitric acid, and of nitrat of barytes, till no further precipitation took place, still the water gave a copious precipitation with nitrat of silver.

EXPER. XXI.—Nitrat of mercury occa-

sioned a white precipitate, which continued white after standing for some days.

EXPER. XXII.—Nitrat of strontites produced no precipitate at first; but after standing for a day or two, a white incrustation was formed on the sides of the glass.

EXPER. XXIII.—Acetite of lead only produced at first a slight cloudiness; but after standing for a day or two, a pretty copious purplish powder subsided.

§ IV. *Inferences drawn from the preceding experiments.*

From the preceding experiments some inferences were deduced, respecting the particular ingredients contained in this mineral water, which prepared the way for a more minute investigation. It was soon perceived, that in several of the above experiments, useless redundancies had occurred; and if I have not suppressed them here, it is because I wished to relate faithfully the gradual progress of this

inquiry, and the various methods which I have used in prosecuting it.

1st, From experiment 1, 2, and 3, it appeared obvious, that no part of the solid ingredients of this water was kept in solution by means of a gaseous acid; since boiling, even to a considerable extent, did not produce any precipitation.

2dly, From experiments 2, 3, 4, 5, it was inferred, that the access of air promoted in this water, the precipitation of some metallic substance.

3dly, From experiment 6, it was suspected that the water was slightly acid; but, from experiment 7, it appeared evident that this circumstance did not depend upon the presence of an aerial acid.

4thly, It was inferred from experiment 8, that neither pure nor carbonated alkali, nor pure earth were present in the water. From experiment 9, a small quantity of carbonated earth might have been suspected, but such an inference being incompatible with experiment 13, the slight change of colour alluded to, in the infusion of turmeric, was supposed to depend on some other cause.

5thly, The presence of iron was clearly indicated by experiment 10; and it was conjectured from experiment 12, that the flowiness with which tincture of galls had produced its effect in experiment 11, was owing to a want of sufficient oxygenation. The circumstance of a quantity of oxyd of iron being precipitated from the water by a gentle heat (exp. 4) whilst on the contrary, no precipitation took place (exper. 3), when the water was boiled briskly, could only be accounted for by the action of the atmosphere, which, in the first instance, oxygenated the iron, whilst in the latter, its access was prevented by the rapid emission of steam.

6thly, Both experiments 14 and 15, but more especially the latter, indicated the presence of lime; but from some of the results above mentioned, it was obvious, that the lime, in this instance, was combined with a mineral acid.

7thly, The experiment 16, confirmed the former conclusions with regard to the existence of iron in the water; but did not afford satisfactory information as to the existence of earths, as these might easily be confounded with the oxyd of iron.

8thly, The experiments 17 and 18, indicated the presence of sulphuric acid, and it became probable that the alteration of litmus, observed in experiment 8, depended either upon this acid, in a separate state, or upon some of its compounds, which have the same property with regard to blue vegetable colours.

9thly, From experiments 19 and 20, it appeared extremely probable, that the water contained muriatic acid.

10thly, From the colour of the precipitate obtained in experiment 21; from the effect of the mineral acids (experiment 13); and the total absence of hepatic smell, it appeared sufficiently obvious, that the water was free from hepatic gas, or hepatic compounds.

§ V. *Plan of Analysis.*

On collecting the information derived from these general results, it appeared that the only substances which I could positively expect to find in the water, by a more particular investigation, were iron, combined with the sulphuric, or perhaps with the muriatic acid,

and lime, in the state of sulphat, or possibly in that of muriat. I suspected the presence of alum, Epsom, and common salt ; but this I did from conjecture alone, and upon no other grounds, than a comparison of this water with other mineral springs of an analogous composition. With regard to the gaseous contents, the only substances of this class, which I could reasonably expect to find, were atmospheric air, and carbonic acid gas ; but neither the one nor the other had yet been positively shewn, and on the contrary, some circumstances had occurred, which rendered the presence of the latter rather improbable.

These general notions however, enabled me to form the following plan of analysis :

1st, To examine the *gaseous contents* of the water.

2dly, To obtain by evaporation, the *fixed ingredients* of the water in a solid form, as a previous step to their chemical examination. For the state of great dilution in which these ingredients appeared to exist in the water, and the great tendency shewn by some of

them to be precipitated by concentration, would have made it exceedingly inexpedient to operate on the water itself.

3dly, To redissolve a known quantity of the residue obtained by this evaporation, and precipitate the iron from it, by prussiat of potash, or succinat of ammonia.

4thly, To separate the lime from the same solution by oxalat of ammonia.

5thly, To add caustic potash to the same solution, with a view to precipitate both the *magnesia* and *alumine*, if these earths should exist in the water.

6thly, To boil this last precipitate (if any was obtained), in pure potash, in order to dissolve the alumine, and thus obtain the *magnesia* in its separate state.

7thly, In case the potash should appear to have taken up any *alumine*, to precipitate the latter by boiling the alkaline solution with muriat of ammonia.

8thly, To dissolve another portion of the residue in nitric acid, and to add nitrat of barytes, with a view to ascertain the quantity of *sulphuric acid*.

9thly, To add nitrat of silver to the same

solution, in order to precipitate the *muriatic acid*.

10thly, Lastly, to try, by the agency of alcohol, which has the power of dissolving certain salts, and of precipitating others; and by slow evaporation and crystallization, whether some light might not be thrown on other parts of the analysis, and other substances discovered, which were not unlikely to exist in the water, but might have escaped notice, had the former method been exclusively employed.

§ VI. *Examination of the gaseous contents.*

EXPER. I.—A quantity of the water just brought from the well, and measuring exactly $5\frac{1}{4}$ cubic inches, was put into a phial, or small receiver, the neck of which terminated in a bent tube, which had its extremity immersed in a mercurial bath. The phial was not quite filled with the water, but the quantity of atmospheric air, contained both in the tube and the upper part of the phial, was accu-

rately ascertained. The heat of a lamp being now applied, an elastic fluid soon began to come over, which was collected in a graduated jar. After allowing the water to boil for about a quarter of an hour, no more gas was given out; and as the water itself was now beginning to boil over, the process was stopped, and the result examined. For this purpose, the quantity of air collected in the jar, was carefully noted, and a solution of caustic alkali being introduced into it, a quantity of gas equal to $\frac{4}{10}$ ths of an inch, was immediately absorbed, which, of course, was carbonic acid.

EXPER. II.—With regard to the portion of air left in the jar unabsorbed, it was found, (every allowance being made for pressure and temperature), that its bulk was perfectly similar to that of the air contained in the apparatus: and this air being examined by the nitrous test, gave precisely the same result as atmospheric air. These two experiments were tried a second time with results exactly similar.

EXPER. III.—Some of the water which had been sent from Brighton to London, and kept for five days, being tried in the same manner, yielded only the $\frac{1}{100}$ th part of an inch, which is a little less than half the quantity obtained in the former experiment.

From these experiments it may be inferred that the Brighton chalybeate yields, when recently taken from the spring, about $\frac{1}{13}$ th part of its bulk of carbonic acid gas, and that it does not appear to contain any other elastic fluid. From the readiness with which this gas is given out; from the obvious diminution which it suffers by keeping; and from other circumstances of the analysis, it appears extremely probable that the gas exists in the water free from any combination.

§ VII. *Evaporation of the water.*

(A)—In order to prevent such chemical changes as are liable to happen in operations of this kind, from too great a heat being applied, I was desirous of carrying on this evaporation on a pretty large mass of water at a

very gentle heat. I had brought no apparatus with me for that purpose, but Mr. Glasfer, a very intelligent and well-informed chemist and druggist, of Brighton, obligingly offered his assistance for this tedious operation. A whole gallon of water was carefully evaporated over a water bath, and the residue, which appeared in the form of a greenish mass, was allowed to dry at the same temperature. This residue weighed exactly 55 grains, which makes for each pint $8\frac{1}{8}$ th grains of solid contents, dried at 212° . It soon gave signs of deliquescence, and increased in weight by exposure to the atmosphere.

(B).—After this first trial, having often had occasion, in London, during the course of this analysis, to evaporate new quantities of water, which were sent to me from Brighton, I found it much easier and more expeditious to begin by boiling the water down briskly to a small quantity over an Argand's lamp, in a glass vessel containing only about one pint, and then to finish the evaporation and desiccation over a water bath, at any desired temperature. The heat which I have gene-

rally used for these deficcations is that of 160° , and I have uniformly found that the Brighton chalybeate, whether it was taken from the well in dry or rainy weather, in summer or winter, constantly yielded *eight grains and a half* of solid residue, dried at 160° , for each pint of the water, and the accidental deviations from this proportion, which have occasionally occurred, have never exceeded a quarter of a grain.

(C)—The process being carried on in the manner just related, the whole mass of solid matter presented itself in the form of a greenish incrustation, thick at the bottom, and gradually thinner towards the edges. But on closer examination, a quantity of yellow powder, (which afterwards proved to consist chiefly of iron), appeared collected in the centre, and there was seen also a kind of snow-like substance slightly spread over the whole surface. Viewed through a magnifying glass, the whole mass offered the appearance of a confused crystallization; and soon afterwards signs of deliquescence manifested themselves by the appearance of small drops of water on diffe-

rent parts of the surface. These various appearances deserved the more notice, as they pointed out the necessity, before proceeding any farther, of rendering the residue homogeneous, by careful mixture and trituration, in order to obtain corresponding results from different portions of the same residue.

§ VIII. *On the modes of filtration and desiccation adopted in this analysis.*

This part of chemical manipulation is of such material importance in an inquiry of this kind, and is so intimately connected with the results, that although I wish to avoid enlarging upon practical details, yet I can hardly proceed farther, without giving once for all, a few explanations which the subject seems to require.

As the difficulty of getting, in London, large supplies of the chalybeate, in its recent state, made it necessary to operate upon a small scale, it became the more necessary to guard against the various sources of inaccuracy to which chemical manipulations are liable. And as it is principally to the diffe-

rent modes of *filtration* and *desiccation*, that the striking discordances which frequently occur amongst experiments, in other respects similarly conducted, are to be ascribed, it is only on this part of the subject that I shall offer a few observations.

(A)—With regard to filtration, I have, after many comparative trials, adopted the following method. Two round pieces of filtering paper, of between three and four inches in diameter, and exactly similar in weight, are cut from the same sheet, and applied the one over the other, in order to filter the solution through both. The residue is then, after due desiccation, weighed, by putting the paper containing it in one side of the balance, and the plain piece of paper, similarly dried, in the other. The difference gives the weight desired. In some cases it may be expedient to separate the residue, in order to weigh it by itself; but in general the former method is less liable to error. A little tediousness in this mode of proceeding no doubt arises from filtering through a double paper; but it is sometimes the only way of

avoiding differences, which, however trifling, will, in a complicated analysis, arise from the wetted paper remaining impregnated with various soluble ingredients, after the moisture has been evaporated from it. I attempted also to use Dr. Black's ingenious method,* which consists in covering every part of the filter with wax, except a small spot in the centre, into which the whole of the residue subsides. But this mode, owing to a variety of little inconveniences, did not appear to me to answer the purpose so well as the method just described.

(B)—With regard to desiccation, I have used an apparatus, which I saw first in nurseries, applied to the purpose of keeping the food of children of an uniform temperature, and which, with some trifling improvements, I have found extremely well adapted to chemical purposes. It consists simply in a tin pan, about four inches in diameter, and three in depth, in which is placed another similar vessel, which fits the former pretty exactly,

* See the Analysis of some mineral springs in Iceland, in the 3d Volume of the Transactions of the Royal Society of Edinburgh.

except that it is about half an inch shallower, so as to form a small space between the two. This space is filled with water, and the substance to be dried being placed in the upper or smaller pan, the heat of a lamp is applied; and in order to prevent oscillations in the flame, as well as to keep the vessels suspended over it, the lamp is inclosed in a cylindrical tin case, in the sides of which there are apertures to answer the purpose of chimnies.

In placing in the apparatus, the substance to be dried, care must be taken, in order to prevent inaccuracies of temperature, to insulate that substance from the apparatus itself. For being made of tin,* which is a good conductor of heat, the bottom and sides of the vessel are much more quickly heated, than the air or internal space in which the thermometer is immersed. This is easily done by placing the substance in a thin glass capsule, and laying this on a small stand consisting of three legs of glass or fine wire, fixed on a flat piece of cork. Things being thus disposed, and a thermometer being suspended immediately

* Or *tinned iron*, as it might more properly be called.

over the capsule, a pretty accurate notion of the temperature to which the substance is exposed, will be obtained; and this indication will be still more correct, if the apparatus be covered: but this can only be done towards the end of the process, when most of the moisture is already volatilized.

(C)—I have only further to observe that the temperature which I have generally used in desiccations, is that of 160° ; and whenever a different degree of heat has been used, it has been expressly mentioned. It must be confessed however, that as it is only by the trimming of the lamp, that the temperature can be regulated, or by putting on, or taking off the cover, occasional deviations of five or even ten degrees, are scarcely avoidable; and the heat, in this apparatus, can hardly be raised above 180° . It is in every instance to be understood, that the substance has been left exposed to the stated degree of heat, until it has reached its maximum of dryness, under that temperature, which will sometimes take several hours.

§ IX. *Circumstances attending the solution of the solid ingredients of the water.*

Ten grains of residue, obtained in the manner described § VII. 2, being boiled with distilled water, the fluid became muddy, and a considerable portion of the residue remained undissolved. A few drops of muriatic acid being then added, and heat applied, the whole was immediately dissolved, with the exception of a small quantity of sediment, which, on boiling briskly, seemed for a moment to disperse, but soon reappeared and subsided in the form of a whitish powder. I tried to redissolve this substance by means of concentrated acids, and by long boiling and digesting, but in vain. I therefore began to suspect that the water contained some siliceous earth, an uncommon occurrence, which, till then, no circumstance had led me to suspect.

§ X. *Silica.*

(A)—This insoluble substance, after being repeatedly washed with distilled water, and

heated to redness in a small platina crucible, weighed $\frac{1}{4}$ th of a grain. This being mixed with a minute portion of alkali, and tried with the blow pipe, readily melted into a transparent glass. The experiment being repeated with a different portion of the same residue, a similar result was obtained; only the quantity of silica yielded in the latter instance, was $\frac{1}{5}$ th instead of $\frac{1}{4}$ th of a grain, a small difference, which gives an average of $\frac{11}{60}$ ths of a grain of ignited silica, for 10 grains of the solid residue; or a quantity corresponding to 17 parts of silica for 1000 of the solid ingredients of the water, dried at 160° . *

(B)—Some suspicion having arisen that the siliceous matter might have been yielded by the glass vessels, and in the processes of solution and filtration, in order to ascertain

* I shall observe, once for all, that in giving the results of my experiments, most of which have been repeated several times, I have generally, for the sake of brevity, stated only one result. Whenever I have been able to trace any particular source of error, in any individual experiment, I have repeated it, and have only stated the result of that which has clearly appeared to be the most accurate. But when experiments, conducted exactly in the same way, have presented but very slight variations in the results, I have, in this case, given the average.

this point, a quantity of water, was evaporated in a tin vessel, and the residue of this was treated as in a former experiment. But the same results were obtained, and no difference could be perceived either in the quantity or nature of this insoluble vitrifiable matter.

§ XI. *Sulphat of iron.*

Having been led to conclude from the circumstances before mentioned (§ IV. & V), that a quantity of iron, most probably in the state of sulphat, was contained in the Brighton chalybeate, my next object was to determine in what proportion this salt existed in the water.

EXPER. I.—Twenty grains of the residue* were dissolved in about four ounces of water, by means of a few drops of muriatic acid, and the solution was gently heated, a previous step which I have found effectual in facilitating the precipitation of iron by prussi-

* When I speak simply of *residue*, I always mean the residue obtained from the Brighton chalybeate, in the manner described in § VIII. B.

ated alkali; probably in consequence of its bringing the metal to that state of uniform oxygenation which is most favourable to its union with the prussic acid. Having then added a solution of prussiat of potash, measuring one cubic inch and a half, (a quantity known by previous trials to be sufficient for the precipitation of the whole iron), the fluid instantly passed to a blue colour, and a blue precipitate, darker than the solution, gradually subsided, leaving the supernatant fluid transparent and nearly colourless. After a few hours, the clear fluid was separated by means of a syphon, and the remaining muddy fluid, containing the Prussian blue, was thrown into a filter. This prussiat of iron, after being carefully dried, at the temperature of 160° , weighed exactly 8 grains. I repeated this experiment three times, without any sensible variation in the result.

Before I could draw any positive conclusion from the last experiment, with regard to the real quantity of sulphat of iron, it remained to be determined by comparative trials, what quantity of prussiat of iron my solution of prussiated potash, would precipitate from a

known quantity of sulphat of iron. This led me to various inquiries respecting the agency of the prussic test, which are too much connected with the subject to be passed over in silence.

EXPER. II.—Ten grains of green sulphat of iron, recently prepared and regularly crystallized, were dissolved in water, and after adding a few drops of muriatic acid, the solution was gently heated, in order to render this experiment perfectly parallel to the former. One cubic inch and a half of the above mentioned solution of prussiat of potash, being then added, a quantity of Prussian blue was instantly precipitated, which dried at 160° , weighed 11.3 grains. But in the former experiment 8 grains of Prussian blue were obtained from 20 grs. of residue. Therefore according to the present comparative result, it appears that 20 grains of residue from the Brighton chalybeate, dried at 160° , contain a quantity of sulphat of iron equal to 7.079 ($11.3:10=8:7.079$) of this salt in its crystallized state.

EXPER. III.—I have hitherto considered as a standard of comparison sulphat of iron in its *crystallized state*, which is not that in which it actually exists in the residue under examination. In order to ascertain what allowance ought to be made for this circumstance, or in other words, how much a given quantity of green sulphat of iron, in its crystallized state, lost in weight by being desiccated, 20 grains of the crystallized salt, reduced to a powder, were exposed to a heat of 160° , and weighed at different periods of the desiccation. In a quarter of an hour, the 20 grains were reduced to 17. In about half an hour longer, they were reduced to 15; and after an interval of two hours, which seemed to bring the salt to its maximum of desiccation, under that temperature, the 20 grains were reduced to 14; and the salt, in this state, was changed to a whitish powder.

EXPER. IV.—The same experiment being repeated, with this difference, that the 20 grains of crystallized sulphat were previously dissolved in water, and then evaporated to

dryness at the same temperature of 160° , the 20 grains were reduced to 12, which is 2 grains less than in the preceding experiment.

Taking the last of these experiments (which seems the most applicable to the present case) as a standard of reduction, it will be found that 20 grains of the residue contain in fact only 4.24 grains of *dried* sulphat of iron, a quantity corresponding to the 7.079 grains of *crystallized* sulphat, which were deduced from the former estimation. It is obvious therefore, that the mode of calculation just proposed, would be more strictly correct; but the state of crystallization being a much more uniform standard of the quantity of moisture, than any artificial process of desiccation, I should, on that account, prefer the former mode of computation.

EXPER. V. — The solution (Exper. 1) from which the iron had been precipitated by prussiat of potash, although quite clear at first; and having only a greenish cast scarcely perceptible, was found, after standing for a few days, to have deposited an additional quantity of Prussian blue, which however

was too small to be weighed. Suspecting from this circumstance, that some minute portion of iron had escaped the action of the prussiated potash, I heated the solution with a view to complete the precipitation. But instead of the very small additional quantity of precipitate which I expected, the fluid having previously passed to a muddy green, and then to a blue colour, soon deposited another copious blue precipitate, which, after the usual filtration and desiccation, weighed no less than 2.8 grains.

EXPER. VI.—A suspicion naturally arose, that this new prussiat, proceeded, at least principally, from the test itself. In order to ascertain this point, one cubic inch and a half of the above mentioned solution of prussiat of potash (a quantity equal to that used in ex. 1) was boiled, first by itself, which produced no change or precipitation, and afterwards, with the addition of a few drops of muriatic acid,*

* Immediately on adding concentrated muriatic acid to the cold solution of prussiat of potash, a dense white precipitate appeared, which was instantly redissolved without any application of heat; but if a considerable proportion of acid was added, a permanent white precipitate subsided.

which, in a few minutes, occasioned a copious blue precipitate, weighing, after the usual filtration and desiccation, 2.7 grains. It appears therefore that each cubic inch of the test, yielded, on being boiled with muriatic acid, 1.8 grains of prussiat of iron; and consequently, that the additional precipitation of 2.8, in the experiment above mentioned, proceeded (with the exception of only $\frac{1}{10}$ th of a grain) from the iron contained in the prussiat of potash, and not from the solution under examination.

As however the quantity of iron thus precipitated from prussiat of potash, by boiling with muriatic acid, was not the whole of the iron contained in that test, I do not entirely depend on the accuracy of this last conclusion. For, unless the quantities of acid used, and the degree of heat applied, be exactly the same in both experiments, corresponding results cannot be expected. In the present instance therefore, where these circumstances were not attended to, the coincidence obtained may have been accidental. But it may be remembered, that my estimate of the quantity of iron in the chalybeate rested upon a direct

comparative experiment (exper. 2) on artificial sulphat of iron, the result of which was afterwards confirmed by the application of fuccinat of ammonia (exper. 7 & 8), a test totally different from the former. This estimate therefore is quite independent of the peculiarities of the prussic test above mentioned (exper. 5 & 6). But I have thought it right to state them, as they point out the necessity, whenever there is an excess of acid, of boiling the solutions to which the prussic test has been applied, as a previous step to any other part of the process in which heat may be required; since, otherwise, a precipitate arising from the test itself, would interfere with the subsequent results. And it may be also observed, that, boiling with muriatic acid, provided it be carried to a sufficient extent, will be, on many occasions, a convenient mode of separating entirely the iron from solutions in which the prussic test has been concerned; since, as Scheele has first observed, the mineral acids possess that power.

It may not be useless to observe, that the solution of prussiated alkali, which I used in all these experiments, consisted of one part of

the prussiat in crystals, to sixteen parts of distilled water; and that a specimen of these crystals, burnt with a little wax, in a silver crucible, yielded 0.225 of brown oxyd of iron. This prussiat of potash was so prepared, as not to be tinged blue by the mineral acids, unless heat was applied.

Having thus obtained by means of the prussic test, results which appeared sufficiently accurate, with regard to the quantity of sulphat of iron contained in the water, I was desirous to ascertain, by some other process totally unconnected with this, the real proportion of metallic iron, or rather of oxyd of iron, actually contained in a given quantity of this chalybeate.

For this purpose, and in order to obtain a solution of this question, perfectly independent of my own experiments, I requested of my friend Mr. Allen, one of the lecturers of chemistry in the medical school of Guy's Hospital, to examine, by any method he might think proper, a portion of residue procured in the same manner as that which had been the object of the preceding experiments.

He very obligingly complied with my request, and soon afterwards favoured me with the following account, which I shall give in his own words :

EXPER. VII.—“ Ten grains of the precipitate from the chalybeate spring at Brighton, dried at the temperature of 160° , were dissolved in distilled water, by the assistance of a little muriatic acid, with the exception of a minute fraction of a grain, which appears by Dr. Marcet's experiments, to be filix. This solution was exactly neutralized by ammonia. A solution of succinat of ammonia being added, and the whole boiled, a brown precipitate was obtained. This roasted with wax, in a silver crucible, gave 1.3 grains of oxyd of iron.

“ Dr. Marcet having found that the oxyd of iron in this mineral water, is combined with sulphuric acid, the following comparative experiment was made with the green sulphat of iron, to determine the quantity of oxyd of iron contained in it, by the test of succinat of ammonia.

EXPER. VIII.—“Five grains of crystallized green sulphat of iron, were dissolved in distilled water; and the iron precipitated by fuccinat of ammonia in a boiling heat. The precipitate being treated with wax in a red heat, gave 1.8 oxyd of iron, which was of a reddish colour, resembling cinnamon, but rather darker.

“Then $1.8:5=1.3:3.61$ grains of green sulphat of iron, in 10 grains of the precipitate procured by boiling the water down to dryness, in a heat not exceeding 160° .

“In employing the fuccinat of ammonia as a test, care must be taken to saturate the solution to which it is applied, very accurately.” *

On comparing these results with my own, I had the satisfaction to observe that the quan-

* In addition to this, I would observe, that long and repeated boiling, is also necessary to promote the action of this test on sulphat of iron, upon which it acts but very slowly and imperfectly without it. It is evident that the effect of boiling, in this instance, depends merely upon a further oxydation of the iron; since by allowing the solution to stand for a sufficient length of time, or by adding nitric acid, the iron becomes readily and entirely precipitable.

tity of sulphat of iron, which I concluded to be $\frac{7.079}{20}$ appeared from Mr. Allen's more direct estimate, to be $\frac{7.20}{20}$, a degree of coincidence which it can hardly be expected to surpass in researches of this nature.

In order to form an estimate of the actual quantity of metal or metallic iron, contained in the chalybeate, I tried the following experiment :

EXPER. IX.—Five grains of iron, filed from the purest specimen of malleable iron which I could procure, were dissolved in diluted sulphuric acid, by long digestion in a gentle heat. This solution being previously neutralized, was repeatedly boiled and filtered with succinat of ammonia, and afterwards treated with pure ammonia, to precipitate a small remaining portion of iron, which had escaped the action of the succinic test. The whole of this precipitate being exposed to a red heat, in an open silver crucible, and treated with wax, in the same way as in exper. 7 & 8, gave 7.4 grains of a dark red brown oxyd of iron, which was attracted by the magnet. It appears therefore that 100

grains of this oxyd, consisted of 67.6 of metal, and 32.4 of oxygen.

By combining these results with those of the two former experiments, it will be found that the 10 grains of residue (which, in exper. 7, yielded 1.3 grains of oxyd of iron), contained really no more than 0.87 grains of metallic iron, or 8.7 in 100 grains. And the actual proportion of metal in 5 grains of crystallized sulphat (which, in exper. 8, yielded 1.8 grs. of oxyd), would be 1.22 grs. or 24.4 in 100; a quantity corresponding to 0.36 of the oxyd.* As however the proportion of oxyd in the salt, must depend upon its degree of oxydation, these results, even allowing them to be perfectly accurate, can only be applied to experiments made exactly under the same circumstances as those just related. But I should not omit to mention, that my brown oxyd of iron, had a more distinct reddish hue than that obtained by Mr. Allen, in the experiment to which this is compared;

* Chemical writers agree in reckoning 0.27 of oxygen in oxyd of iron at its minimum of oxydation, such as it is obtained from pure green sulphat; and 0.48 in the red oxyd, such as it exists in the red sulphat of iron. In my experiment, the oxyd appeared to contain 0.324, which is a kind of intermediate proportion between those just mentioned.

although the process of precipitation and calcination, was, as much as possible, carried on in a similar manner. It must be observed, also, that my solution of 5 grains of iron, having not been brought to the state of crystallized sulphat, some doubts may arise as to the degree of oxydation which the metal underwent in that solution.

Before I conclude my observations on this part of the subject, I should not omit to remark, that wherever in the course of this inquiry a quantity of iron has been precipitated from the water, whether merely in consequence of its being kept for some time (as in exper. 2. D), or by means of concentration, it has never presented itself in the state of ochre, or simple oxyd of iron; but on the contrary, has always, upon careful examination, appeared to be combined with a portion of sulphuric acid, forming what has been called a sub-sulphat of iron.

§ XII. *Red sulphat of iron.*

EXPER. I.—Twenty grains of residue were put in a phial, with about 150 grains of alco-

hol, of the specific gravity of 8.10, and the mixture, after being often shaken and kept well corked for a few hours, was thrown into a filter. This filtered spirituous solution was of a reddish yellow colour. Being evaporated to dryness, it deposited a deliquescent brownish residue, which being redissolved in water, imparted to it a deep yellow colour. But a small portion of this residue (which proved to be a sub-sulphat of iron, formed by the action of the atmosphere, during the process of evaporation) remained undissolved, till a little muriatic acid was added. The residue which had been deposited in the filter by a spirituous solution, was become of a paler colour, in consequence of its being treated with alcohol, and had lost its deliquescent quality. Its weight was reduced to 14.1 grains.*

EXPER. II.—As it is well known that alcohol has the property of dissolving red sulphat of iron, whilst on the contrary it

* Yet the residue of the solution in alcohol dried at 160° , weighed only 3.1, instead of 6.9 grains, which would have been the complement of the 20 grains of residuous matter. This must be owing to a loss of moisture, in consequence of the action of alcohol.

precipitates the green fulphat, there could be no doubt but that the deliquescent residue, obtained in the former experiment, contained a quantity of red fulphat of iron. In order to learn in what proportion this salt was contained in a given quantity of the residue, the yellow watery solution above mentioned, proceeding (exper. 1) from 20 grains of residue, was treated with prussiat of potash, which occasioned a precipitation of Prussian blue, weighing 1.45; which denotes a quantity of red fulphat of iron, equivalent to 1.3 grains of the green fulphat.

(A)—But although the presence of red fulphat in the residuary matter, is manifestly shewn by these experiments, yet they do not by any means prove, that the salt actually existed in the recent chalybeate. On the contrary it appears probable, that it is the product* of the several operations to which

* Mr. Kirwan, in his "Essay on the Analysis of Mineral Waters," in which he has so much contributed to the advancement of that part of chemical science, expresses a belief that the fulphat of iron, which has occasionally been discovered in mineral waters, did not exist in those waters, previous to the analytic processes to which they were subjected.

the water has been submitted. This I think may be inferred, not only from the tincture of galls not tinging the recent chalybeate black, but also from the constant formation and precipitation of sub-sulphat, which happens whenever the water is exposed to a process of oxygenation. For it is reasonable to suppose, that, whilst on one hand, a process of oxygenation, and consequent separation of oxyd of iron from the acid which held it in solution, takes place, the portion of acid thus liberated, will unite with as much of the superoxygenated oxyd as it will be capable of combining with, and thus a quantity of the red or superoxygenated sulphat will be generated. This question however, is, in the present instance, a mere matter of curiosity, since it cannot affect in any material manner the former results obtained, either as to the absolute quantity of iron in the chalybeate, or as to the nature of its combination.

§ XIII. *Muriat of iron.*

I have suggested in a former part of this paper, the possibility of some muriat of iron being contained in the water. This conjecture, the experiments above related, do not precisely contradict, but they render it extremely improbable.

In the first place it must be remembered, that if any muriated iron did exist in the water, it could only be in a very minute quantity, since it must have made part of the small portion of red sulphat of iron which was estimated from the preceding experiments, both these salts being equally soluble in alcohol.

Had the spirituous solution contained no other salts but the red sulphat or muriat of iron, the presence or absence of the latter would have been easily demonstrated by nitrat of silver, or any other test of muriatic acid. But in this case, as it will be proved hereafter, other muriats were discovered, which prevented my obtaining an absolute proof of the water being perfectly free from the presence of muriated iron.

§ XIV. *Sulphat of lime.*

EXPER. I.—Oxalat of ammonia being added to a solution of 10 grains of residue, (the iron of which had been previously separated by succinat of ammonia), a considerable turbidness instantly took place, and a white precipitate readily subsided, which, dried as usual, weighed 4.1 grains.

Having had every reason to conclude from some of the preliminary experiments, mentioned in a former part of this paper, that the lime existed in the water in the state of sulphat, it remained to be determined, what quantity of selenite corresponded to the 4.1 grains of oxalat of lime, obtained from 10 grains of residue.

Not finding in chemical works any facts from which I could immediately deduce the solution of this question, I made the following comparative experiment:

EXPER. II.—A specimen of the purest native selenite that I could procure, was pulverized and boiled to saturation in distilled water. This solution was decanted, and the clear fluid evaporated to dryness in the tem-

perature of 160° . Five grains of the felenite thus obtained and dried, being redissolved in water, and afterwards precipitated by oxalat of ammonia, and dried at 160° , weighed 4.25 grains; which is equivalent to 117 parts of felenite, for 100 parts of oxalat of ammonia. Consequently, 4.82 grains ($4.25 : 5 = 4.1 : 4.82$) will be the quantity of felenite contained in 10 grains of residue, dried at 160° *

§ XV. *Alum and Epsom salt.*

The presence of these two earthy sulphats in the water, being by no means improbable, I tried to discover them in the following manner.

* This estimate of course supposes that felenite is the only calcareous salt present in the water. Muriat of lime, which is often found in mineral waters, could not be expected in this, since its existence, except in extremely minute quantities, is incompatible with sulphat of iron. In confirmation of this, I tried the muriatic salts obtained from the solution in alcohol (§ XIII. exp. 1), with oxalat of ammonia, which occasioned only a very slight cloud, without any ponderable precipitate.

EXPER. I.—The portion of residue which had been left in the filter (§ XII. exper. 1), by the solution in alcohol, was dissolved in a quantity of distilled water, not sufficient to take up any considerable quantity of selenite, and the filtered solution was evaporated in a temperature not exceeding 80° . As the evaporation advanced, regular cubic crystals deposited themselves on the bottom of the vessel; and when it was completed, there appeared also, besides the crystals (which formed the greatest part of this residue), a minute quantity of a yellowish powder, collected in the centre of the cup, and a kind of snow-like efflorescence slightly spread over it. But no other crystals but those just mentioned could be discovered. The whole of this residuary mass, dried as usual, weighed precisely 5 grains. But the heat applied in the process of drying, was not sufficient to deprive the crystals of their water of crystallization. From the form of these crystals, from their decrepitation on being heated, and their well known taste, there could be no doubt but that they were muriat of soda.

EXPER. II.—In order to obtain more positive information in this respect, and also to ascertain the nature of the small portion of uncrytallized matter mixed with the crytals, the residue of 5 grains above mentioned, was mixed with about 2 ounces of distilled water, which redissolved the whole of it, with the exception of a very minute quantity of a yellowish powder. The following tests were then tried :

(a) Prussiat of iron produced a pale green colour, and a precipitate weighing $\frac{6}{10}$ ths of a grain.

(b) Oxalat of ammonia occasioned a precipitate weighing 1.6 grains.

(c) Nitrat of silver, and muriat of barytes, both produced copious precipitates.

(d) A solution of platina in nitro-muriatic acid, produced no precipitate whatever.*

(e) The small quantity of undissolved

* This test is exceedingly convenient and decisive with regard to the presence of potash, as it forms a distinct and immediate precipitate with the smallest portion of this alkali, or any of its compounds, whilst it is not at all affected by the mineral alkali.

yellow powder, which weighed at most $\frac{1}{10}$ th of a grain, proved to be subfulphat of iron. *

(f) After the complete removal of the iron and lime, the remaining solution was tried both with pure ammonia and pure potash, which occasioned no cloud or precipitate whatever; whilst the least quantity of any earthy salt, added to this solution, was readily discovered by the alkali.

From these experiments I inferred, that the 5 grains soluble in water consisted of about 2 grains of fulphat of iron and selenite, and 3 grains of muriat of soda: and from the last result in particular, I concluded, that neither alum, nor fulphat of magnesia, were contained in the water.

* It may be observed that the quantity of fulphat of iron yet discovered, either in the spirituous or the watery solution, does not amount to more than 2 grains, instead of the 7 grains which the 20 grains of residue under examination should contain, according to my former statements. But the remaining 5 grains were found in the state of subfulphat, in the residue insoluble both in alcohol and water, which consisted entirely of selenite and subfulphat of iron.

§ XVI. *Muriats of Alumine and Magnesia ;
and an inquiry into the properties of succinat
of ammonia as a test of alumine.*

EXPER. I.—Twenty grains of residue were redissolved in dilute muriatic acid, and boiled with prussiat of potash, till the iron was completely removed. The solution was afterwards neutralized, and the lime separated by oxalat of ammonia. A solution of pure potash being then added, in order to precipitate the alumine and magnesia, whitish flakes gradually subsided, which being collected on a filter, and dried at 160° , weighed 2.1 grains.

EXPER. II.—This precipitate was boiled in a solution of pure potash, with a view to dissolve the alumine, and thus separate it from the magnesia, which is not soluble in alkali. This solution being allowed to cool, soon deposited a whitish powder, and the fluid, which continued somewhat turbid, being decanted off, the white powder, dried as usual, was found to have lost about half of its weight.

From these first results I naturally conjectured, that the 2.1 grains consisted of nearly

equal parts of alumine and magnesia. This however, as it will soon appear, was contradicted by subsequent experiments.

EXPER. III.—The whitish powder insoluble in potash being examined, proved to be magnesia, mixed with a small quantity of lime, which was readily discovered by the oxalic test. But the supposed solution of alumine in potash, being allowed to stand for some time, deposited a further quantity of a whitish powder, and the clear alkaline liquor being boiled with muriat of ammonia, produced no precipitation whatever, which led me to suspect that the whole of the original precipitate (exper. I) was magnesia, with the exception of the minute quantity of lime just mentioned.

EXPER. IV.—In hopes to obtain more decisive results on this subject, and in order to vary the last experiments, I made a new solution of 10 grains of residue, from which the iron was precipitated by succinat of ammonia. But from this solution, treated like the former with caustic alkali, I could obtain no earthy precipitate.

EXPER. V.—This circumstance, as it appeared afterwards, was simply owing to the state of very great dilution in which the earthy salt existed in this solution; but before I was aware of this, an idea occurred to me, that the non-appearance of a precipitate in this case, might possibly arise from the previous application of succinat of ammonia. In order to ascertain this point, I prepared artificial solutions of muriat of alumine, and muriat of magnesia, and observed, that on pouring a few drops of succinat of ammonia into the former, a copious precipitate subsided.

It appeared therefore useless to prosecute any farther my inquiry on the above solution. But this property of succinat of ammonia, to precipitate alumine, struck me as being new and curious, and attracted for some time my attention.

(A)—I shall not enter here into all the particulars of the inquiry and various experiments respecting alumine and its relations with the succinic test, which arose from the circumstance just related; but as the subject is by no means unconnected with the object

of this essay, and as some explanation may be of use to facilitate the application of this test to the examination of solutions of alumine, I shall mention the following general results :

1st, That succinat of ammonia precipitates alumine from its combinations, readily and entirely, provided there be no considerable excess of acid, in which case the solution must be previously neutralized.

2dly, That this substance does not appear to have any action on magnesia or its compounds, and will therefore in many cases afford a new and convenient discriminating test between this earth and alumine, the method of boiling the alumine in potash being in some instances inapplicable. *

3dly, That if a solution of 100 parts of

* Since this was written, I have seen in the *Journal des Mines*, N° 70, a paper on Yttria, by Mr. Ekeberg, in which the author notices the property of succinat of ammonia to precipitate glucine, which is another common feature between this earth and alumine. But I do not find that either Mr. Ekeberg or any other writer has noticed the property of this test with regard to alumine. On the contrary I observe, that the celebrated chemist Klaproth (*Analytical Essays*, vol. ii), in his analysis of the Gadolinite, a mineral which contains a small quantity of alumine, precipitated the iron by succinat of ammonia, without remarking the property alluded to, although it could not fail to have some influence on the result.

octahedral crystals of alum be decomposed by succinat of ammonia, the precipitate, calcined in a red heat, weighs exactly 12. This result I offer with the more confidence, as it agrees perfectly with Mr. Kirwan's statement, a coincidence which I did not notice until I had completed my own experiments.

4thly, That if a similar solution be decomposed by pure ammonia, the precipitate, calcined in the same manner, gives precisely the same weight.

5thly, That however there is this difference between the precipitates produced from alum by succinat of ammonia, and those obtained from pure ammonia, that in the first instance, the precipitate, dried at 160° , is white like starch, and weighs 35; whilst in the latter, the precipitate, dried in the same manner, shrinks to a brownish powder, somewhat resembling glue coarsely pulverized, and weighs only 15, instead of 35. *

* It has been shewn by my ingenious friend Mr. Theodore De Saussure, in his valuable paper on alumine (published in the *Journal de Physique*, vol. 52), that this peculiar colour and shrinking of alumine, depends on the great proportion of water in the solution from which it is precipitated; and that this appearance is entirely prevented, by using a concentrated solution of alum.

6thly, That if 100 parts of pure alumine prepared from alum, and brought to the consistence of a paste,* be dissolved in sulphuric acid, and afterwards precipitated by succinat of ammonia, and calcined in a red heat, the residue will be exactly equal in weight, to that procured by calcining in the same manner, without any previous solution and precipitation, a similar quantity of pure alumine taken from the same mass.

EXPER. VI.—Being now possessed of a direct mode of precipitating alumine, I made a solution of 10 grains of residue, and after precipitating the iron from it by prussiat of potash, and the lime by oxalat of ammonia, I added succinat of ammonia. But not the

* I procured this paste by precipitating a solution of alum by pure ammonia, and afterwards washing the precipitate repeatedly in great quantities of distilled water, and heating it gently once or twice with ammonia. The water at last came off perfectly free from sulphuric acid; and a portion of the paste thus prepared, being dissolved in muriatic acid, and a strong solution of muriated barytes added, a slight cloud only was produced, without any distinct precipitate. And as nitrat of barytes produced no cloud whatever, I suspected that the slight effect of muriated barytes might possibly be one of those anomalies, such as Mr. Kirwan has observed with regard to muriat of magnesia, quite independent of the presence of any foreign substance.

least effect was produced, although caustic alkali occasioned a precipitate, and although the least quantity of muriat of alumine produced an immediate cloudiness. This experiment, which I repeated several times with the same result, appearing sufficiently decisive, with regard to the absence of alumine in the Brighton chalybeate,* my attention was now exclusively directed to the magnesia, the presence of which had been proved by former experiments.

EXPER. VII.—Magnesia, in this instance, as it has been shewn before (§ XV. exper. 2. f), could only be supposed to exist in the state

* Whilst this sheet was printing, I accidentally observed, that muriat of alumine was decomposed by the prussiat of potash which I used in my experiments, and which, I have every reason to suppose, had been prepared with sufficient care and accuracy. I have since found upon inquiry, that this property, though not generally known, has been noticed by some chemists, and probably belongs to all prussiated alkalies, in whatever manner they have been prepared. No conclusion therefore, respecting the non-existence of muriat of alumine, can be deduced from the above experiment, nor from any other process in which the prussic test has previously been used. But from many other circumstances, and particularly from the results obtained in the analysis by alcohol, it appears sufficiently obvious, that muriat of alumine (unless it be in extremely small quantity), cannot exist in the water.

of muriat. In attempting to estimate the quantity of this salt, I had again recourse to alcohol. 20 grains of residue were put into a phial, with about 120 grains of alcohol of the specific gravity of 810. The undissolved part was allowed to subside, after shaking the solution repeatedly, and letting it stand for several hours. The clear fluid being then decanted off, and a solution of carbonat of ammonia *fully saturated* with carbonic acid, being added, the mixture became thick and turbid, and passed to a dirty brown colour. This muddy fluid being filtered, and phosphat of soda added to the clear solution, a cloudiness immediately appeared, and in a few minutes a white powder subsided, which, when dried at a temperature not exceeding 100° , weighed 2.8 grains.

The precipitate obtained by this method*, being a triple salt, composed of phosphoric

* This very easy and valuable method of precipitating magnesia, was first suggested by Dr. Wollaston. It is obviously founded upon the property which fully neutralised carbonat of ammonia possesses, first to dissolve the carbonat of magnesia which is formed in consequence of a double elective attraction, and afterwards to yield the earth to the phosphoric acid, with which, and the ammonia, it forms a triple salt. With

acid, magnesia and volatile alkali, (the same combination which Mr. Fourcroy has discovered in the bladder of a horse, and Dr. Wollaston has shewn to compose one of the concretions which are formed in the human bladder), my next object was to determine, by a comparative experiment, what quantity of muriat of magnesia was required to form, by the process above mentioned, a known quantity of this triple salt.

EXPER. VIII.—For this purpose, I evaporated to dryness a solution of muriat of magnesia, in a heat of 160° . 5 grains of the residue thus obtained were dissolved in water, and both

regard to the preparation of the carbonat of ammonia, Dr. Wollaston's method consists simply in pulverising a quantity of the common carbonat, and exposing it for a few hours to the action of the atmosphere, thinly spread on a piece of paper. I found that 100 grains of common carbonat of ammonia recently sublimed, being treated in this manner, were reduced to 57 grains: and a solution of salt thus prepared had no smell whatever. But no certain inference can be drawn from this experiment, respecting the proportions of carbonic acid and volatile alkali in the common carbonat, since (as Mr. Davy has shewn in his excellent treatise on the nitrous oxyd), those proportions vary considerably according to the degree of heat with which this salt is sublimed.

carbonat of ammonia, and phosphat of soda were successively added, as in the former experiment. The precipitate, dried in a heat not exceeding 100° , weighed 7.8 grains; and the same was reduced to 7 grains, by raising the heat to 120° . It appeared in the form of an impalpable, and nearly tasteless white powder.

This result therefore, combined with that of the former experiment, gives 1.79 grains ($78:5 = 28:179$), as the quantity of muriat of magnesia contained in the 20 grains of residue.

EXPER. IX.—In order to know what quantity of pure magnesia the above 2.8 grains of triple salt contained, I made the following comparative experiment: *

Five grains of magnesia, prepared by ex-

* It may be observed, that in the comparative experiments, I have generally used but small quantities of the substance to be examined, though it would have been very easy to employ larger quantities of materials. But it appears to me, that generally speaking, there is seldom any thing to be gained by making experiments of inquiry upon a large scale; whilst on the contrary, small quantities are, with proper management, more susceptible of giving expeditious as well as accurate results, particularly when the processes of filtering and drying are concerned.

posing for about a quarter of an hour, common calcined magnesia to a red heat, so as to be certain that all the moisture and the carbonic acid had been expelled, were dissolved in muriatic acid, and precipitated as in exper. 7. The triple salt being collected on a filter, and thoroughly dried in a heat not exceeding 100° , weighed 26.3 grains. Consequently 2.8 grains of this salt, or 1.79 grains of muriat of magnesia, contained only 0.53 grains of pure magnesia; a proportion which is equivalent to 100 grains of triple salt dried at 100° , for 19 of magnesia prepared in the manner just described. I dried the 26.3 grains of triple salt at a temperature not exceeding 100° , fearing that it might be decomposed by a greater heat; but I was rather surprised to find, that being afterwards exposed to a heat of 160° , it lost only 2 grains.

§ XVII. *Sulphuric acid.*

In order to ascertain the quantity of sulphuric acid in the chalybeate, a solution of nitrat of barytes was added to a pint of the

water (equivalent to $8\frac{1}{2}$ grains of solid residue), which precipitated from it 6.8 grains of sulphat of barytes* ; a quantity which, supposing this salt to contain 0.33 of acid, according to the assertion of Fourcroy and Kirwan, would correspond to 2.24 grains of sulphuric acid in a pint, or 2.64 in 10 grains of the residue.

§ XVIII. *Muriatic acid and muriat of soda.*

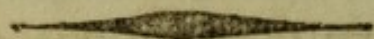
EXPER. I.—Ten grains of residue were dissolved in very dilute nitric acid, and nitrat of silver was added, in order to separate the muriatic acid. The precipitate dried as usual, weighed precisely 6 grains ; a quantity which, (if according to Mr. Kirwan,† 100 parts of muriated silver contain 16 parts of acid) would make 0.96 grains of muriatic acid in 10 grains of residue.

(A) With regard to the quantity of muriat

* Ten grains of residue would therefore have yielded 8 grains of sulphated barytes dried at 160°.

† See Kirwan on Mineral Waters, table IV. Mr. Kirwan's precise estimate is $\frac{16.54}{100}$ of acid, the salt being dried at 130°.

of soda, it will be found that if according to Dr. Black's statement*, 235 grains of luna cornea are equivalent to 100 grains of common salt, the above 6 grains of muriated silver will represent 2.5 grains of muriat of soda: and by deducting from these 2.5 grains, 0.7 grains (which may be considered as an adequate allowance for the 0.89 grains of muriat of magnesia discovered in XVI. exper. 7 & 8), the quantity of muriat of soda will be reduced to 1.8 grains, in 10 grains of residue.



§ XIX. *Examination of the sediment deposited by the chalybeate, and of the changes which the water undergoes by long exposure to the atmosphere.*

(A) I have mentioned at the beginning of this paper a copious earthy or ochry sediment which is formed wherever the water has been suffered to stagnate. Some of this substance

* See Transactions of the Royal Society of Edinburgh III. 116.

which I had collected and brought with me in a muddy state, became in a few weeks hard and friable like ochre, of which it had exactly the colour and appearance. Being slightly examined, it appeared to contain iron, in the state (partly at least) of subfulphat; lime, chiefly in the state of carbonat; and a small portion of argillaceous earth. The two last ingredients, there is reason to suppose, are merely conveyed and deposited by the water, without having ever been chemically combined with it.

(B) With regard to the pond before mentioned, formed by the superfluous water from the Wick chalybeate, I found on examination, that a pint of it contained only $3\frac{1}{2}$ grains of solid matter, in which I discovered nearly the same ingredients as in the fresh chalybeate, except that the whole of the iron had disappeared. The nature of this water, however, must vary exceedingly according to the weather and the season; since heavy rains must always dilute its contents, and sometimes occasion the introduction of various other ingredients.

§ XX. *Recapitulation, and conclusion.*

On recapitulating the various results of this analysis, in order to present them in one view, it appears, that 100 parts of the solid residue obtained by evaporation from the Brighton chalybeate, and dried at the temperature of 160° , have given :

GRAINS.		
Sulphat of iron -	21.2	} ... { equal to 35.3 grs. of crystallized green fulphat.
Sulphat of lime -	48.2	
Muriat of soda -	18.	} Dried at 160°
Muriat of Magnesia	8.9	
Siliceous earth -	1.7	
Loss - - - -	2.0	
	<hr/>	
	100	

And a pint of the water (which is equal to 8.5 grains of the solid residue) contains :

Carbonic acid gas, about $2\frac{1}{2}$ cubic inches, or $\frac{1}{13}$ th part of its volume :

	GRAINS	
Sulphat of iron -	1.80	} ... { equal to 3 grains of crys- tallized green sulphat.
Sulphat of lime -	4.09	
Muriat of soda -	1.53	} Dried at 160°
Muriat of magnesia	75	
Siliceous earth -	- 14	
Loss - - - -	- 19	
	<hr/>	
	8.50.	}

I shall not prolong this paper by remarks on the application of the Brighton chalybeate to the cure of diseases, as I have no personal experience of its medicinal properties. But I may be allowed to point out the considerable proportion of active ingredients, and more particularly of iron, which it contains, compared to other springs of an analogous composition. The chalybeate of Tunbridge, for instance, which has long been justly celebrated for its medicinal virtues, contains, according to Dr. Babington's analysis, no more than 1 grain of oxyd of iron in a gallon; whilst the Brighton spring holds in one pint, more than an equal quantity of the same oxyd. It is true that the chemical composition of the two springs is very different, since in the one, the iron is

suspended by a fixed acid, whilst in the other, it is held in solution by a gaseous menstruum. But experience has shewn that the specific effects of iron on the human frame, do not materially differ, whether it be taken in the state of carbonat, or of sulphat, or of simple oxyd, or in any other form, provided the metal be nearly in the same state of oxydation ; although it must be allowed that there may be differences in the degree of that effect, as well as in the manner in which the acid, or vehicle of the metal, affects the organs of digestion, in particular individuals. It may be observed also, that this spring contains a quantity of neutral salts, which may probably be of use in affording a gentle stimulus to the stomach and intestines, and thus contributing to insure the beneficial effects of its ferruginous ingredients. It is therefore very reasonable to suppose, that the Brighton chalybeate is likely to be of considerable use in diseases which require the tonic powers of this kind of medicines ; and from the peculiarly advantageous situation of that spring, there is every reason to expect, that at no distant period,

its medicinal qualities will be more generally acknowledged. *

Before I conclude, I must beg leave to mention a circumstance respecting the medicinal effect of this chalybeate, which I have learnt from Dr. Tierney. This gentleman has observed, that in common with most mineral waters of this kind, it is apt to occasion in some individuals a degree of nausea and a sense of weight in the stomach, when taken cold; but he has found that those effects generally disappear, if the water be drank moderately warm. This remark appears to me important, and is particularly applicable to the Brighton chalybeate, as its analysis shews, that no iron is precipitated, nor any other material change produced, by applying heat to it, provided it be done quickly, and in vessels which, from their shape, expose but a small surface of the water to the action of the atmosphere. In

* Several practitioners at Brighton, have been for some years in the habit of recommending the Wick water in a variety of complaints. Both Dr. Tierney and Mr. Hall have told me, that they have had frequent opportunities of ascertaining its beneficial effects. That spring, however, is yet comparatively speaking, but little known, and seldom resorted to.

the Tunbridge chalybeate, on the contrary, such a method would be quite impracticable, since that water cannot be heated, without the oxyd being immediately precipitated.



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