IV. *An* ANALYSIS *of the* WATERS *of ſome* HOT SPRINGS *in* ICELAND. *By* JOSEPH BLACK, M. D. *Profeſſor of Medicine and Chemiſtry in the Univerſity of Edinburgh, Firſt Phyſician to his Majeſty for Scotland, Fellow of the Royal College of Phyſicians, and of the Royal Society of Edinburgh; Member of the Academy of Sciences and of the Society of Medicine of Paris, of the Imperial Academy of St Peterſburgh,* &c. &c.

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SIR JOSEPH BANKS, to whoſe indefatigable ardour for the advancement of natural hiſtory, the philoſophical world is ſo much indebted, made a voyage to Iceland in the year 1772, to enquire into the productions of that remote part of the world, and particularly into thoſe of its famous volcano. When he returned, he brought from thence, among many other natural productions, ſome petrified vegetables, and incruſtations, formed by the waters of the boiling ſprings; and he was ſo good as to preſent a part of them to his friends here, who were ſurpriſed to find them compoſed of ſiliceous earth. As this was the firſt example obſerved, of water containing this earth in ſuch quantity as to form ſiliceous petrifactions, it raiſed a ſtrong deſire to have an opportunity of examining the water, and of learning by what means this ſiliceous matter was diſſolved in it; and this opportunity was at laſt given us by JOHN THOMAS STANLEY, Eſq; who, excited by motives ſimilar to thoſe of Sir JOSEPH BANKS, equipped likewiſe a veſſel, and made a voyage to Iceland, during the ſummer 1789. He brought from thence, and from the Faro Iſlands, a number of fine ſpecimens of volcanic and other foſſil productions, and along with them, a quantity of the water of the two moſt remarkable boiling and exploding ſprings of Iceland, called by the natives *Geyzer* and *Rykum;* and having favoured me with a portion of theſe waters, and expreſſed his deſire that I would examine them, I have accordingly made a number of experiments with them, an account of which I ſhall now ſubmit to the Society. If the detail of it ſhould appear tedious; if I ſhall be thought to have given much attention to very ſmall matters; it muſt be conſidered, that the nature of the ſubject requires exactneſs. The quantities of the materials which are to be examined in ſuch experiments, are but ſmall, though it often happens, that theſe ſmall quantities of matter, acting in nature for a great length of time, produce accumulations, and other effects, that appear very ſurpriſing and worthy of attention. I muſt alſo confeſs, that I took pleaſure in promoting, as far as I could, the information concerning Iceland, which the philoſophical zeal and ſpirit of the Gentlemen I mentioned, have procured for us.

BOTH theſe waters had a weak ſmell of the Hepatic Gas, or a ſmall degree of the odour, which is well known in Harrowgate, and other ſulphureous waters. The quantity, however, of this ſulphureous matter in them was ſo very ſmall, that I was not able, by any experiments, to obtain it in a ſeparate ſtate, or bring it into view in any form whatever. I therefore could not make any attempt to eſtimate the quantity of it.

THOSE who are acquainted with ſulphureous waters, know that an incredibly ſmall quantity of their volatile ſulphureous matter is ſufficient to give a perceptible odour; and it is ſo liable to be decompounded and changed, while we attempt to ſeparate it from water, that ſuch an attempt never ſucceeds when the quantity of it is ſmall. There was alſo reaſon to believe, that ſome part of it had already been loſt or changed during the voyage,

voyage, this matter being one of thoſe volatile ingredients of mineral waters, which are the moſt liable to be evaporated or changed by the action of the air and other cauſes. I therefore think it ſufficient to mention, that theſe waters contained a ſmall quantity of this ſubſtance.

I BEGAN by making a few preliminary trials, to acquire ſome notion of the nature of theſe waters.

1. AN equal quantity of lime-water being added to the Iceland waters, there was a little diminution of tranſparency, but only in the ſmalleſt degree, and no ſediment was formed.

2. MILD volatile alkali produced no effect whatever.

3. PAPER ſtained blue with the March violet, being dipped into the water and dried, had its colour changed a little towards a green.

4. CAMBRIC ſtained to a bluiſh purple, with infuſion of litmus, aſſumed a more perfect blue colour, when dipped into the water and dried.

5. ACID of ſugar did not produce a perceptible muddineſs or precipitation.

6. NOR did the ſolution of corroſive ſublimate.

7. THE ſolution of ſal ſaturni (plumbum acetatum) made the water very muddy and white, but a ſmall quantity of diſtilled vinegar rediſſolved nearly the whole of the precipitate, and made the water almoſt perfectly clear again.

8. THE ſolution of barytes in muriatic acid made the water become muddy, and depoſite a ſediment, which was not rediſſolved by adding purified nitrical acid.

9. THE ſolution of ſilver produced a ſtrong muddineſs and conſiderable precipitation, which was not rediſſolved by adding purified nitrical acid.

THE laſt trial ſhewed the preſence of the muriatic acid, and the one preceding it, that of the vitriolic acid in the compoſition of theſe waters; but by the 3d, 4th and 7th, I alſo learned, that there was more than enough of alkaline matter to ſaturate both of them. The 5th trial ſhewed that the alkaline matter was not calcareous earth, but alkaline ſalt ; and the 6th, that this alkaline ſalt was not the volatile, but one of the fixed alkalis. The 1ſt trial ſhewed, that this unſaturated fixed alkali was not combined with air, or that if any was combined with it, the quantity was ſo ſmall as to be ſcarcely perceptible.

NONE of theſe trials gave any indication of the earthy matter contained in theſe waters ; and as my principal object was to inveſtigate the nature of their petrifying power, I now began with the following experiment :

Evaporation of the Water.

I EVAPORATED 10,000 grains weight of each of theſe waters to dryneſs with a gentle heat, in ſeparate glaſſes. The dry extract of the water of Rykum weighed gr. 8.25, and that of Geyzer, gr. 10.

THE evaporation was performed in cylindrical glaſs veſſels, about 3 inches wide and 7½ deep, which received heat from the ſteam of boiling water, not directly, but through the intervention of white-iron caſes, which fitted the glaſſes, and in which they hung. I have often uſed this apparatus in examining and comparing different waters ; and the advantages of it are, that the greater part of the fixed matter is collected on a ſmall ſurface ; that the glaſſes are ſo moderately heated, that they bear water to be added, during the evaporation, without danger of breaking ; and, laſtly, when the whole water is evaporated, the fixed matter, while it is thoroughly dried, by leaving it expoſed ſome hours to the heat, never becomes ſo hot as to ſuffer the loſs of any part of the acid of the ſaline compounds which it may contain, and when it is dry, the quantity of it is accurately determined, by weighing it in the glaſs, the weight of which can be be aſcertained, both before the water is put into it, and after the extract is taken out.

IN the end of theſe evaporations of the Iceland waters, they emitted an odour ſimilar to that of alkaline leys, which contain an alkali not very pure or well calcined, and afterwards, when the evaporation was nearly completed, the reſiduum aſſumed the form of a tranſparent jelly, which had nearly the thickneſs of half a crown. This jelly afterwards became divided by fiſſures, into a great number of ſmall portions, which, in drying, contracted their ſize, and greatly widened the fiſſures, forming at laſt a number of ſmall fragments of white cruſt, unconnected with one another, and not adhering to the bottom of the glaſs. A ſmall quantity only of this matter attached itſelf to the ſides of the glaſs during the evaporation, and formed there circles of an exceeding thin incruſtation, which adhered ſtrongly, and required much patience to ſcrape it off with a knife.

THESE phenomena are exactly ſimilar to thoſe which appear in evaporating water which contains ſiliceous earth, diſſolved in it artificially by means of an alkaline ſalt. The colour of the dry matter obtained from Rykum water, was almoſt a pure white, that of the water of Geyzer was a yellowiſh white.

WHILE theſe dry extracts were kept for ſome time in the glaſſes, placed in a cold room, in the winter ſeaſon, they attracted humidity, and the extract of Geyzer attracted the moſt. Eight grains of the extract of Rykum attracted in one week four grains of humidity; the ſame quantity of the extract of Geyzer attracted in the ſame time ten grains of humidity. My attention, however, was turned for ſome time from theſe experiments; but reſuming them again after ſome months, I found that theſe extracts remaining in the ſame glaſſes, and in the ſame room, had again become dry, and had loſt the greater part of the weight which they had acquired at firſt by attracting humidity. This I imputed partly to the ſtate of the atmoſphere, and partly to their having attracted fixed air, by their union with which they had loſt their ſtrong attraction for water.

The conſtituent parts of theſe extracts were next to be inveſtigated. I ſoon perceived that they contained a portion of alkaline ſalt not ſaturated with acid, which became evident when a ſmall quantity of them was wetted and applied to paper ſtained with the juice of violets, or the colouring matter of the common purple radiſh; the colour in either caſe was changed to a green, I further collected and ſcraped theſe extracts out of the glaſſes, and placing each in a ſmall filtre, I dropped diſtilled water on them repeatedly, until the water came away from them inſipid. The waters which had been thus filtrated through them were put into china cups, and the greater part evaporated with a gentle heat, the reſt was allowed to evaporate ſpontaneouſly in a dry room. Thus, a number of ſmall ſaline cryſtals were formed, which were partly regular cryſtals of common ſalt, and partly cryſtals of an oblong and flatted form, larger than thoſe of the common ſalt. Theſe larger cryſtals were diſtinguiſhable, not only by their form, but by ſome of their properties. They became white, opaque and mealy in dry air, and being taken out, and taſted and tried in different ways, were found to contain ſome of the foſſil alkali in a cryſtalized ſtate.

The undiſſolved matter which had remained on the filtrating paper, appeared by its properties to be totally or principally made up of ſiliceous earth. It was white and exceedingly ſpungy and light. A ſmall portion of it was triturated, and made into a paſte with water; which paſte being laid on a piece of charcoal and dried, was heated intenſely with the blowpipe. No part of it was melted; it was only contracted in its dimenſions, and acquired a weak degree of coheſion. Another ſmall portion was triturated dry, with an equal weight of aerated and exſiccated foſſil alkali; and being put into a ſmall platina ſpoon, againſt the bottom of which the flame of the blowpipe was ſtrongly

ſtrongly directed, the mixture was ſoon melted into a tranſparent colourleſs glaſs, which afterwards, by being digeſted with a ſmall quantity of diſtilled water, was completely diſſolved, and formed a liquor which had all the qualities of the *liquor ſilicum.*

I NEED not take notice here of the quantity of the earth and ſaline matter which were in ſome meaſure ſeparated from one another in this experiment. I had reaſon to ſuſpect, that neither of them were obtained in this way without ſome loſs. The odour emitted by the water in the end of evaporation, gave reaſon to ſuſpect the loſs of ſome part of the ſalts; and it was probable that a part of the earth would remain combined with the alkali, in a ſoluble ſtate, in the dry extract, and would paſs through the filtre, when I diſſolved and waſhed away the ſaline matter.

I THEREFORE planned a ſet of experiments, by which the quantity of each ingredient in theſe waters might be more certainly known; and began with the following

Experiments to inveſtigate the quantity of the un-neutralized alkaline ſalt.

IN making the experiments to decide this queſtion, I made uſe of an acid, which I had often employed before in experiments to learn the quantity of pure or cauſtic alkali, contained in aerated alkalis, and in various barillas, kelps, and other ſuch heterogeneous maſſes. This acid was a quantity of the vitriolic, the power of which, in ſaturating pure alkalis, I had carefully examined, and I was accuſtomed to add it very gradually to filtrated ſolutions of the above ſubſtances, until they were exactly ſaturated; and then, from the quantity of acid required to produce this effect, I learned the quantity of un-neutralized alkali which theſe ſubſtances contained. The ſpecific gravity of this vitriolic acid, compared with that of water, was as 1798 to 1000, in a temperature of heat equal to 60 of FAHRENHEIT. When I had uſed it on former occaſions, I diluted ſome of it, with four times its weight of diſtilled water, and uſed this mixture in place of the pure acid, that I might the more readily portion it into ſmall dozes; but on this occaſion, I made a mixture of it, with about 100 times its weight of diſtilled water; and eſſaying this mixture afterwards, with great attention, I found that 112 grains of it ſaturated one grain of the pure alkaline part of the alkali of tartar, and 171.55 grains were required for the ſaturation of one grain of the pure or cauſtic part of the foſſil alkali.

WITH this largely diluted acid, the ſtrength of which was thus aſcertained, I began to inveſtigate the quantity of alkali in the Iceland waters. I gave a pale purple or blue colour to a portion of the Rykum water, by adding a few drops of an infuſion of litmus, the bluiſh purple of which became more blue when mixed with this alkaline water, and I began to add very gradually ſome of the largely diluted vitriolic acid, expecting to ſee the colour change to a reddiſh purple, when the alkali became completely ſaturated. This method, however, did not ſucceed ſo well as I had ſuppoſed; for although I changed the colour to a reddiſh purple, or even to a pure red, by adding an exceeding ſmall quantity of the diluted acid, the red thus produced was not permanent. Next day, I found it returned again to the blue, and requiring a new addition of acid; and this happened ſo often, after repeated additions of acid, that this proceſs appeared very tedious, and ſcarcely capable of being brought to a preciſe limit; for in proportion as I continued the proceſs the longer time, or had made the more numerous additions of acid, the time neceſſary for the return of the colour from red to blue was always the longer, and at laſt was no leſs than ſeveral weeks.

THESE

These phenomena appear to me to have proceeded from the very weak and ſlow action of the acid and alkali on one another, in conſequence of the exceſſively diluted ſtate in which they were mixed together, the alkali at the ſame time not being pure, but combined with the ſiliceous earth, a ſubſtance for which it has a conſiderable attraction. I therefore ſuppoſed that when I added the ſmall doſes of diluted acid, the acid particles remained for ſome time diſperſed through the liquor, without joining the alkali, and the water contained, at the ſame time, a ſilicated alkali, if I may ſo call it, and an unſaturated acid; but the colour of litmus being much more diſpoſed to be affected and changed by acids than alkalis, it became red, and retained this colour as long as any particles of the acid remained unſaturated. Theſe, however, after ſome time, being all attracted and ſaturated by the alkali, the colour was again changed by the remaining unſaturated alkali.

It may perhaps be ſuſpected, that a ſmall quantity of fixed air, detached from the alkali, might be the cauſe of this temporary red colour, and that the colour returned again to blue, when the fixed air evaporated from the water: And I know that a very ſmall quantity of fixed air, contained in water, is ſufficient to change the colour of litmus, and that a conſiderable time is required for its evaporation from the water, ſo that the litmus may recover its natural tint; but it is equally true, that the fixed air never requires ſo long a time for its evaporation as ſeveral weeks, and that it has not the power to redden litmus, when an alkali is preſent, except when the quantity of the alkali is exceedingly ſmall, and that of the fixed air incomparably more than ſufficient for ſaturating the alkali. In the preſent caſe, the laſt of theſe conditions never could take place, the quantity of acid added at once being far too ſmall to detach enough of air, even although the alkali had been originally ſaturated with air, which it certainly was not; it appeared rather to be in a cauſtic ſtate, or very nearly cauſtic. This reaſoning ſuggeſted

ſuggeſted to me another mode of making the experiment, which ſucceeded perfectly in a moderate time.

The foregoing experiments, and others which I made with ſmall quantities of the water, enabled me to form ſome judgment of the proportion of acid neceſſary to ſaturate the alkali which this water contained. I therefore added to 10,000 grains of the Rykum water, 200 grains, accurately weighed, of the largely diluted vitriolic acid; which quantity I judged to be conſiderably more than ſufficient for ſaturating the alkali of this water; and after the acid was poured in, the ſmall and light glaſs in which it was weighed, was rinſed ſeveral times with diſtilled water, which was added to the Rykum water I alſo gave it a pale tincture with ſome drops of the infuſion of litmus, and then boiled the water gently in a thin bottomed glaſs, until it was reduced to one fourth of its firſt quantity. It ſtill continued of a red colour, without the leaſt tendency to a purpliſh hue, and ſhewed that the acid was more than enough to ſaturate the alkali.

It was neceſſary, in the next place, to learn with certainty how much of the acid had been ſuperfluous. With this intention, I added a largely diluted ſolution of alkali of tartar in diſtilled water. In this ſolution, the pure alkali, conſidered as diſtinct from the air which was joined to it, conſtituted one fortieth part of the weight of the fluid. I weighed 38.6 grains of this ſolution; which quantity I knew, by the previous experiments, was exactly or nearly ſufficient for ſaturating the ſuperfluous acid. I poured it at once into the hot water, and rinſing the ſmall and light glaſs in which it was weighed two or three times with diſtilled water, I poured in this alſo. A little efferveſcence appeared in the hot water. I therefore ſet it again on the furnace to boil, that the fixed air might be expelled, and I added now and then a little diſtilled water, to prevent it from boiling down too much. In leſs than half an hour's boiling, the fixed air being all expelled, the colour changed

changed from red to purple, with a very ſmall tendency towards the red. This ſhewed that the quantity of ſalt of tartar, which had been added, was exactly ſufficient for ſaturating the ſuperfluous acid. Had the ſaturation not been ſufficiently exact, I could have added a little more of the alkali, or a little more of the acid, as I had done in the ſmaller eſſays which were preparatory to this; but the tint of colour which I had here produced, was that which I had found to be the moſt diſcernible and ſatisfactory ſign of exact ſaturation, in former experiments; and it is proper to mention, that one grain more of the largely diluted vitriolic acid changed this purple very remarkably to a more decided red, and that with one grain leſs, the hue of the purple, by being inclined to blue, would have been equally diſtinguiſhable; of which I ſatisfied myſelf, by adding as much of the ſolution of ſalt of tartar as ſaturated one grain weight of the largely diluted acid.

THE quantity of the diluted acid added at firſt was 200 grains. From this was to be ſubſtracted 108.32 grains, the quantity ſaturated by the 38.6 grains of the ſolution of ſalt of tartar; the remainder is gr. 91.68. From this quantity, however, we muſt make another deduction; for, as Profeſſor BERGMAN juſtly obſerved, the infuſion of litmus contains ſomething which is of an alkaline nature, or is capable of ſaturating a certain quantity of acid. To learn how much was to be deducted on this account, I tinged a ſmall quantity of diſtilled water, with the ſame number of drops of the infuſion of litmus that I had uſed in tinging the Iceland water, and then making the diſtilled water boiling hot, I began to add ſome of the largely diluted vitriolic acid, and kept the water boiling all the time. The firſt additions of acid, as I expected, did not produce a change of colour, or, if any change was produced, it ſoon diſappeared again, while the water was boiling; but as ſoon as I had added gr. 3.5, a permanent change was produced to a reddiſh purple: This quantity therefore muſt, in the next place, be deducted from the gr. 91.68, and thus we have gr. 88.18. as the quantity of the diluted vitriolic acid which was employed ſolely in ſaturating the alkali of the water. But from the eſſays I had made of the power of this diluted acid in ſaturating alkalis, it is evident that this quantity of it was ſufficient for ſaturating gr. 0.514 of the pure or cauſtic foſſil alkali, or gr. 0.857 of that which is ſaturated with air and evaporated to dryneſs, or about gr. 2.38 of that which is ſaturated with air and in form of tranſparent cryſtals.

THE next ſtep was to make a ſimilar experiment to determine the proportion of alkali in the Geyzer water; but here I found it neceſſary to change a little the mode of aſcertaining the point of ſaturation.

THE water of Geyzer, by means of the ſulphureous gas, which it contained in greater quantity than the other, and perhaps alſo by means of ſome of the other ingredients which it contained, and which gave it a light yellowiſh colour, produced ſuch a change in the colour of litmus, that it could not be employed, as in the laſt experiment, by mixing it with the acidulated water and boiling them together; the purple of the litmus was changed to an orange, which could not be made to return to blue or purple, although I added a quantity of alkali, which rendered the liquor very evidently alkaline, when it was examined by other trials. I therefore had recourſe to the common method, which I had formerly practiſed in many other experiments of a ſimilar nature, I mean the uſe of linen rags, or bits of cambric, which had been tinged with an infuſion of litmus. A little bit of theſe, when touched with a liquor that is in the ſmalleſt degree acid or alkaline, has its colour changed from the purple to red or blue. This method is, next after the one employed in the laſt experiment, the moſt nice that I know; provided that, in having recourſe to it, we remember what was remarked in the former experiment, that the litmus colour is affected by acids in general much more eaſily than by alkalis; and

and that, though a liquor contain a ſmall quantity of alkali, if this be ſaturated and ſuperſaturated with fixed air, the firſt effect of ſuch liquor upon the ſtained paper will be to change it towards a red. This tint of colour, however, being produced by the ſuperfluous aerial acid, is made to diſappear, by drying the bit of cambric. The colour of it, while drying, will quickly change from the red to purple, and from that to blue, in conſequence of the evaporation of the ſuperſaturating air. Being appriſed of this particular, I firſt made ſome preparatory experiments, with gr. 1000, and alſo with gr. 10,000 of the Geyzer water, and afterwards a more ſatisfactory one with gr. 10,000 of the ſame, in the following manner:

To gr. 10,000 of the Geyzer water, I added gr. 400, accurately weighed, of the largely diluted vitriolic acid, and began ſoon after to evaporate the water, by boiling it gently in a thin bottomed glaſs. The above quantity of acid I knew to be conſiderably more than what was ſufficient for ſaturating the alkali.

THE water was boiled until it was reduced to a quantity little exceeding gr. 3000. I then added gr. 84.5 of the dilute ſolution of ſalt of tartar, and boiled the water again gently until it was reduced to gr. 2000. In weighing ſuch ſmall quantities of acid or alkaline liquors as were added to the water in theſe experiments, it is eaſy to adjuſt the weight with the greateſt preciſion, by dipping the end of a ſlender glaſs rod, or of a pointed ſlip of paper, into the fluid. By theſe means, we can take up a quantity of it, as ſmall as we pleaſe; and this method I likewiſe uſed, when I meant to add theſe fluids gradually, and by very ſmall quantities at a time, to any mixture. The end of a ſlender glaſs rod was dipped into them, and afterwards transferred into the mixture.

WHEN I now examined the above boiled water, by means of the tinctured paper or linen rag, I found it reduced to the exact degree of ſaturation which I deſired; that is to ſay, it ſcarcely

produced

produced a change in the litmus colour, or if any change was produced, it was only a vergency towards the red, which was ſçarcely perceptible; and when the ſtate of ſaturation was varied from this point, by an addition of 3 grains of the largely diluted vitriolic acid, or by an equivalent quantity of the alkaline ſolution, the tint of the colour was remarkably changed towards the red or towards the blue. Suppoſing therefore the above ſtate of ſaturation exact, and I believe it to be the moſt exact that could be depended on, the quantity of largely diluted vitriolic acid, employed in ſaturating the foſſil alkali of the water, was gr. 163.4; for the whole quantity added was gr. 400, and the ſalt of tartar of the gr. 84.5, of the dilute ſolution had required gr. 236.6 for its ſaturation. It follows, therefore, from the eſſays I had made, of the power of this diluted acid in ſaturating the pure or cauſtic foſſil alkali, that the unſaturated quantity of this alkali, contained in the gr. 10,000 of the water, was gr. 0.952, which is equal to gr. 1.587 of the ſame alkali combined with air and evaporated to dryneſs, or gr. 4.409 of the ſame in a cryſtalized ſtate.

THE reaſon for boiling theſe waters, with the quantities of acid which I had added to them, in theſe laſt experiments, is ſufficiently obvious. The abundance of acid was meant to inſure the complete ſaturation of the whole of the alkali, and ſeparation of it from the ſiliceous earth; and the boiling promoted the ſame purpoſe, both by means of the heat which was applied, and alſo by bringing the acid and alkaline particles the nearer to one another, while the water evaporated.

A DOUBT may however poſſibly ariſe in the minds of ſome of my readers, whether this boiling of the water might not be attended with the diſſipation of ſome part of the ſuperfluous acid, which was not neutralized by the alkali of the water; and if any part of the acid was diſſipated, the concluſions concerning the quantity of the alkali would be neceſſarily erroneous.

To remove this ſcruple, I took gr. 10,000 of diſtilled water, and added gr. 112 of the diluted acid. This mixture was then boiled down, in the ſame manner as the Iceland water; that is to ſay, in a glaſs which had an oval or nearly globular body, about 5 inches deep, with a neck as long, and half an inch wide. This glaſs was placed in a ſhallow ſand-heat, the bottom of which was a flat iron plate. The boiling was continued until three fourths of the water were evaporated, and then, removing it from the fire, I added gr. 40 of the dilute ſolution of ſalt of tartar. This neutralized it exactly, and ſhewed that no part of the acid had been diſſipated in boiling; and it continued to ſhew the ſigns of ſufficiently exact ſaturation, after I had evaporated it further to the weight of one ounce, in which ſtate, any ſuperfluous alkali, by being leſs diluted, would have been more eaſily diſcernible.

Experiments to determine the nature and quantity of the earthy matter.

HAVING thus determined the quantity of unſaturated alkali in theſe Iceland waters, my attention was next turned to the earthy matter. A ſmall part of this earthy matter came into view in the boiled and neutralized portions of theſe waters with which I had made the above deſcribed experiments. The neutralized liquors were a little muddy, and depoſited ſlowly a ſmall quantity of ſediment, which collected itſelf cloſely to the bottom of the glaſs, and adhered to it ſlightly. This ſediment, in the *Rykum* water, was deeply tinged with the colouring matter of the litmus; in the *Geyzer* water, it had a brown tinge, and there was a little more of it than in the other. I collected theſe ſediments, by firſt decanting the greater part of the liquor from them, and afterwards filtrating the reſt in a ſmall filtre, in which the ſediment was waſhed, by paſſing diſtilled

water

water through it ſeveral times. Being then dried on the filtrating paper, it contracted greatly, and was divided by fiſſures into a great number of ſmall parts, as would have happened to fine clay, had the ſame quantity of it been dried on paper in a ſimilar manner; and when it was ſeparated from the paper, and further examined, it ſhewed the qualities of an argillaceous earth, combined with a ſmall quantity of colouring matter. This appeared by the following experiments:

1. I PUT ſome of it, which I had procured in different experiments, into a platina ſpoon, and made it red hot. While heating, it firſt became black, then underwent a ſlight inflammation, and afterwards became white, without changing its external form, being only a little contracted in its ſize, and diminiſhed in its weight.

2. To another ſmall maſs of it, laid on a plate of glaſs, I added a drop of aquafortis, which neither effervefced with it, nor diſſolved it, but only changed the colour to a paler red.

3. ANOTHER ſmall portion, which had been gently calcined, was well mixed with an equal weight of the aerated foſſil alkali, and then expoſed to a ſtrong heat in the platina ſpoon. The alkali was quickly melted and became cauſtic; but I could not by its means bring the earth into fuſion, or if any was diſſolved by the melted alkali, it was only a very ſmall portion, not perceptible by the appearances.

4. NOR did I ſucceed much better, when I tried to melt or diſſolve it by means of borax, heated on charcoal with the blowpipe. A little maſs of this earth continued undiſſolved in the melted borax, and without any appearance of effervefcing with it, until I was tired of the experiment.

THIS earth therefore cannot be any other than the argillaceous. Had it been the ſiliceous, it would have been melted with the alkali into a tranſparent glaſs, which happened eaſily with different ſpecimens of pure ſiliceous earth, ſubjected to the ſame trial; and had it been any of the alkaline earths, the borax would

would have diſſolved it quickly with efferveſcence. The quantity of this earthy ſediment, from either of theſe neutralized waters, was very ſmall. From gr. 10,000 of *Rykum* water, I could only collect a quantity, which, after receiving an obſcure red heat, weighed the twentieth part of a grain; from the ſame quantity of the *Geyzer* water, I got about 38 or 39 hundredths of a grain.

In one of my experiments with *Rykum* water, I got this argillaceous earth from it by another proceſs. I had a dry extract, obtained by evaporating gr. 20,000 of this water, and which weighed gr. 16½. Thirty gmins of aquafortis were added to it. This aquafortis was made up of equal parts of the ſtrongeſt nitrous acid and water. The extract was digeſted with it ſix or eight hours, and then diſtilled water being added, the mixture was filtrated in a ſmall filtre, to ſeparate the clear acid liquor from the undiſſolved matter. The filtrated acid liquor was then ſaturated, and a little more than ſaturated, with a pure aerated alkaline ſalt, and the ſaturated mixture was heated to a boiling heat. It became muddy, and depoſited a ſmall quantity of ſediment like mucilage, which being collected by filtration, and dried, and heated to an obſcure red heat, weighed juſt one tenth part of a grain, and had the qualities above enumerated, which ſhewed that it was an argillaceous earth. In another experiment, I digeſted an extract of *Geyzer* water with ſtrong vitriolic acid, and thus got from it a ſimilar earth; but the quantity of it was very little greater than that which I had got by ſubſidence from the neutralized and boiled part of the ſame water, in the experiments above deſcribed.

The greater part, however, of the earthy matter had not yet made its appearance; I mean the ſiliceous earth. It ſtill remained in a ſtate of perfect diſſolution in the neutralized and boiled mixtures above deſcribed, ſome part of which had actually paſſed through filtrating paper; and I learned, by other trials, that the whole of theſe neutralized mixtures might have been

been filtrated, without danger of ſeparating any part of the ſiliceous earth from the water by that operation. This is a conſequence of the ſingular nature of the ſiliceous earth, ſeveral properties of which, hitherto unnoticed, or not exactly deſcribed, I became acquainted with in the courſe of theſe experiments.

We have no experience of the poſſibility of diſſolving this earth in its concrete ſtate by water alone; but if it be diſſolved in water by means of an alkaline ſalt, although we afterwards completely ſaturate the alkali with an acid, the earth thus ſeparated, provided there is enough of water, will not ſubſide; it will remain diſſolved; the mixture will appear perfectly tranſparent, and will paſs through the filtre without the ſmalleſt difficulty. To gr. 1000 of the *Geyzer* water, I added more than enough of acid to ſaturate the alkali. I then boiled the mixture a little while, until a ſmall part of it only was evaporated, and I ſet it aſide in a quiet place. I know it contains a little more than half a grain of ſiliceous earth; but after ſtanding twelve months, there is not the ſmalleſt appearance of ſeparation, the mixture is ſtill perfectly tranſparent and fluid in every part of it, though it be decidedly acid; and I know, that had it been boiled down to a proper degree, a ſeparation of the ſiliceous earth would have happened in a ſhort time. I learned this by another experiment with *Rykum* water. To gr. 1000 of this water, I added a quantity of acid more than ſufficient for ſaturating the alkali. The water was then boiled till it weighed only 138 grains, and it was ſet up in my cloſet to remain undiſturbed. In about eight days, the tranſparency of it was a little diminiſhed, and afterwards there was a very ſlow ſubſidence of the matter which had produced this effect. It formed gradually, at tht bottom, a ſtratum of ſome thickneſs, which was a little leſs tranſparent than the clear water above, and was thereby diſtinguiſhable from it. After a week or two more, I poured off the clear water entirely, without diſturbing the ſediment, which was in fact a tender jelly, adhering to the bottom of of the glaſs, and the upper ſurface of which was level and ſmooth. I knew the quantity of ſiliceous earth contained in it; and comparing this with the weight of the water, when reduced by boiling to gr. 138, I found the proportion of the earth to that quantity of water to be as 2.68 to 1000; and having weighed the jelly by itſelf, the proportion of ſiliceous earth to the water in it, ſuppoſing that it contained the whole of the earth was 10.88 to 1000. In another experiment, in which a ſimilar mixture had been leſs boiled, and in which the ſiliceous earth bore to the water the proportion of 2.1 or 2.2 to 1000, I found a ſoft jelly formed at the end of forty days. And in another, in which the boiling and evaporation was continued until the jelly began to be formed in the upper part of the liquor while it was boiling, I found the proportion of the ſiliceous earth to the remaining water to be nearly as 3.75 to 1000.

After this jelly is once formed, I never could bring it again into a ſtate of diſſolution by water alone, whatever quantity of this laſt was added.

It appears therefore by theſe experiments, that when ſiliceous earth, united with an alkali, is diſſolved in 1000 times, or in more than 500 times its weight of water, it will not ſeparate or ſubſide from that quantity of water, although we ſeparate or diſengage the alkali from it. The particles of it, placed at that diſtance, do not act on one another by their attraction of coheſion or concretion. It is neceſſary, in order to enable them to attract one another, that they be brought nearer, by diminiſhing the quantity of the water, until it be leſs than 500 times the weight of the earth. When this is done, they will enter into a ſtate of coheſion, ſooner or later, according as the water has been more or leſs diminiſhed. But this ſtate of coheſion into which they firſt enter, is alſo remarkable. The force of it is exceedingly weak, and it takes place while the particles of the earth are ſtill at a conſiderable diſtance from one another. They therefore retain and entangle among them a large quantity of water, amounting to about 100 times their own weight, and perhaps more than 200 times their bulk, with which they form a conſiſtent jelly, almoſt perfectly tranſparent.

It may be aſked here, what prevents the particles of this earth from approaching one another more nearly, and entering into a ſtate of ſtronger coheſion? We may, if we pleaſe, imagine that they retain round each of them, by chemical attraction, a quantity of water, which forms a little ſphere or polyhædron, with the particle of earth in its centre. Thus, each particle is prevented from coming within a ſmaller diſtance of the other particles around it, than the diameter of that ſphere; but let the water of theſe ſpherules be diminiſhed in quantity by evaporation, in conſequence of heat, or the attraction of the air, the particles of the earth will immediately enter into a ſtate of cloſer connection and ſtronger coheſion, of which we have examples in the exceſſive contraction of the jelly, while it is dried up into cruſts, and in thoſe circles of thin incruſtation which were formed on the ſides of the glaſs-veſſels, while the waters were evaporated to dryneſs in the firſt experiment, the particles of which were ſo ſtrongly united to one another, and to the ſurface of the glaſs, that they coſt me much trouble and time to ſcrape them off with a knife.

When ſuch a concretion is once formed of this earth, and afterwards receives frequent additions of the ſame matter, which, inſinuating itſelf into the pores of the concretion, is fixed there, and encreaſes its denſity and ſolidity, the maſs may in time acquire a ſurpriſing degree of hardneſs. The petrifactions of *Geyzer* are undoubtedly formed in this manner, and ſome of them are ſo denſe and hard that they are ſcarcely diſtinguiſhable from agate or calcedony.

After making theſe obſervations on the nature of the ſiliceous earth, the proper method for extracting it from the above boiled and neutralized portions of theſe waters, was ſufficiently obvious. I ſeparately evaporated them to dryneſs with a gentle heat

heat in two china cups, carefully waſhing every drop of them from the glaſſes into the cups with diſtilled water, and then taking out the dry extracts out of the cups, I put them ſeparately into ſmall filtrating papers, and paſſed diſtilled water through them repeatedly, until all the ſaline matter was waſhed away. The papers being then carefully dried, I found the earth in them exceedingly ſpungy, fine and tender. The quantity of it, obtained in this ſtate from the gr. 10,000 of *Rykum* water, was gr. 3.8, which were reduced by the action of an obſcure red heat to gr. 3.73 nearly. From the ſame quantity of the *Geyzer* water, I got gr. 6.8 of the dried earth, which, by a ſimilar heat, were reduced to gr. 5.4, and theſe gr. 5.4 being digeſted with aquafortis, and again waſhed with diſtilled water, to extract any argillaceous earth that might remain in them, I obtained only gr. 0.1 of this earth, which added to the quantity obtained before, makes up gr. 0.48 of the argillaceous earth, from the gr. 10,000 of *Geyzer* water, the remaining gr. 5.3 being pure ſiliceous earth. Some of it was melted into a perfect glaſs in the platina ſpoon, with one half of its weight of aerated foſſil alkali evaporated to dryneſs. The diminution of the weight of the dried earth, from gr. 6.8 to gr. 5.4, which happened when it was gently calcined, proceeded from ſome inflammable matter, which adhered to it at firſt, and gave it a yellowiſh colour. This colour changed firſt to black, and afterwards to a pure white, during the calcination. The inflammable colouring matter might have been received in part from the veſſels in which the water was brought, ſome of which were tainted with the odour of ſpirituous liquors, or the water might have got a part of it from ſubterranean ſtrata of clay, or other earths containing inflammable matter.

Experiments to learn the quantity of the neutral ſalts.

THE only ingredients of theſe waters, the quantity of which had not yet been examined, were the neutral ſalts. The preliminary experiments, and the appearances obſerved in the watery ſolutions of the extracts of theſe waters, gave me reaſon to be ſatisfied, that theſe neutral ſalts were partly common ſalt and partly Glauber's ſalt. To aſcertain the quantities of them, I made the following experiments: I had ſome common ſalt, which had been refined by a ſecond cryſtalization, and was in ſolid dry and large cryſtals. Of this I weighed ten grains exactly, which were diſſolved in about half a pound of diſtilled water. I then added a ſolution of ſilver, which contained a little ſuperfluous acid. The ſilver was precipitated in the form of luna cornea or argentum muriatum; and I took care to add rather more than the quantity which the ten grains of common ſalt could precipitate. The luna cornea, after complete ſubſidence, and decantation of the ſaline water from it, was carefully collected on a ſmall filtre, and well waſhed with diſtilled water, and thoroughly dried and weighed. I thus learned, that 100 parts of common ſalt are ſufficient to give 235 of luna cornea. This enabled me to learn, by ſimilar experiments, how much common ſalt is contained in the Iceland waters, and I found that the quantity contained in 10,000 grains of *Rykum* water was gr. 2.90, and in the ſame quantity of the *Geyzer* water, I found there was gr. 2.46 of common ſalt. Some of my readers may perhaps be inclined to ſuſpect, that the Glauber's ſalt contained in the Iceland waters, might, by means of its vitriolic acid, cotribute to the precipitation of a part of the ſilver; but experiments have ſatisfied me, that a ſmall quantity of vitriolic acid, or of any vitriolic ſalt, diſſolved in a large quantity of water,

water, does not precipitate ſilver *; and to prevent any part of the ſilver being precipitated by the alkali of the water, I added of purified aquafortis, more than enough to ſaturate the alkali, before I added the ſolution of ſilver.

ANOTHER ſet of experiments, on the ſame plan, but made with Glauber's ſalt and the ſolution of barytes, in place of common ſalt and ſolution of ſilver, enabled me to aſcertain with equal exactneſs the quantity of Glauber's ſalt contained in theſe waters. I firſt learned that if pure Glauber's ſalt be perfectly exſiccated, by evaporating the water that is in its cryſtals, 10 parts of this exſiccated ſalt are ſufficient to precipitate as much barytes, from its ſolution in muriatic acid, as will form 17 of barytes vitriolica. This fact being aſcertained, I added ſome of the diſſolved barytes, to ſeparate portions of the Iceland waters, ſo long as any muddineſs and precipitation was produced; and I carefully collected, waſhed, dried and weighed the precipitates. I thus learned, that the water of Rykum contains in gr. 10,000 of it, as much Glauber's ſalt as would give gr. 1.28 of exſiccated Glauber's ſalt, and the water of Geyzer as much as would give gr. 1.46 †.

IN making theſe laſt experiments alſo, I added ſome purified nitrical acid to the Iceland waters, to prevent any precipitation of the barytes which might have been occaſioned by the alkali of the water.

IN reviewing the experiments I have now deſcribed, if we neglect the ſmall quantity of ſulphureous gas, the contents of theſe waters will appear as follows:

In

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* See the appendix to this paper.

† The method by which theſe ſmall quantities of ſediments and precipitates were collected and weighed, is explained in the appendix to this paper.

In gr. 10,000 of *Rykum* water there are,

Of cauſtic foſſil alkali,	gr. 0.51
Argillaceous earth,	0.05
Siliceous earth,	3.73
Common ſalt,	2.90
Glauber's ſalt when exſiccated,	1.28
Total,	8.47

In gr. 10,000 of *Geyzer* water,

Cauſtic foſſil alkali,	gr. 0.95
Argillaceous earth,	0.48
Siliceous earth,	5.40
Common ſalt,	2.46
Glauber's ſalt exſiccated,	1.46
Total,	10.75

These quantities of the ingredients, as determined by the above experiments, exceed the quantities of dry extract which I obtained by evaporation. Gr. 10,000 of the *Rykum* water gave by evaporation gr. 8.25 of dry extract, and the ſame quantity of *Geyzer* gave gr. 10 only. This difference, however, can eaſily be accounted for. It is well known that common ſalt, and other ſalts, ſuffer ſome loſs by evaporation, when watery ſolutions of them are evaporated to dryneſs; and the odour which was perceived in the end of the evaporation of theſe waters, made me ſuſpect that a little of the ſalt might have been loſt. There was therefore no reaſon to expect that the reſult of the analytical experiments would tally exactly with the extract by evaporation. I was rather ſurpriſed and pleaſed to find that they

they came ſo near, and am perfectly ſatisfied that this analyſis is as complete and exact as it was in my power to make it, with that quantity of water which I got for this purpoſe.

The proportions of the above enumerated ingredients to the water in which they are contained, ſhew the quantities of them contained in an Engliſh gallon of 231 cubical inches, or 58,484 grains, which are as follows:

In an Engliſh gallon of *Rykum* water:

Cauſtic foſſil alkali,	gr. 3.
Argillaceous earth,	0.29
Siliceous earth,	21.83
Common ſalt,	16.96
Glauber's ſalt exſiccated,	7.53

In an Engliſh gallon of *Geyzer* water:

Cauſtic foſſil alkali,	gr. 5.56
Argillaceous earth,	2.80
Siliceous earth,	31.58
Common ſalt,	14.42
Glauber's ſalt exſiccated,	8.57

Having now ſtated the ſeveral ingredients of theſe hot ſprings, and their proportions, the principal queſtions which remain to be conſidered are, How is the ſiliceous earth diſſolved in them, or combined with the water? Has hot water alone a power to diſſolve this earth, or was it diſſolved by the medium of the alkali only? And how came the ſalts which we find in theſe waters and the ſulphureous gas to be combined with them? As all attempts to anſwer theſe queſtions muſt be conjectural, different opinions will be formed concerning them; and

and I may offer what I have imagined, without its being thought neceſſary to make an apology. Profeſſor BERGMAN conſidered the ſiliceous earth in theſe waters as diſſolved by the power of the hot water alone; and ſuppoſed, that water, aided by exceſſive heat, became a ſolvent of this ſpecies of earth. He formed this opinion, however, under diſadvantageous circumſtances, and from a partial view of the ſubject. He only knew that this earth is actually diſſolved in theſe waters, and depoſited by them, and that they ſpring out of the ground of a full boiling heat, with appearances of their having been hotter below. He did not know what other ingredients they contained along with the earth. As we now know they contain an alkali, which is a powerful medium for combining this earth with water, I do not think that the power of water alone to diſſolve it can be admitted, until it is proved by direct experiments; and I am not of opinion that theſe will ſucceed. I am perſuaded that both the ſiliceous and the argillaceous earth have been diſſolved by the medium of the alkali, but at the ſame time that the violent and long continued heat contributed greatly, and was even neceſſary to this diſſolution. The proportion of the cauſtic alkali to the earthy matter in one of theſe waters, is as 13½ to 100; in the other it is 16 to 100. When we form artificial compounds of ſiliceous earth and alkali in theſe proportions, we find that cold water has no power to diſſolve them, though boiling water, by length of time, would certainly act on them. Even cold water, or the humidity of the earth, is well known to penetrate the hardeſt glaſs that is expoſed to it for years or for ages; and I have had the experience of the power of hot water to act on glaſs, when I have diſtilled water in the ſame glaſs retorts a great number of times, or evaporated water often in other glaſs-veſſels. Their internal ſurface was evidently affected by the continued action of the hot water. Its firſt effect is to ſoften thin laminæ at the ſurface of the glaſs, and to make them ſeparate from that ſurface, in conſequence probably of of their being ſwelled and extended by the water penetrating into them; and by a longer action of the water, there is no doubt that they, or ſome part of them, are completely diſſolved.

THOSE who may have objections againſt admitting, that a boiling heat, and great length of time, are ſufficient aids to enable water to diſſolve a compound of the ſiliceous earth with ſuch a ſmall proportion of alkali, may imagine this earth to have been at firſt combined with a larger proportion of alkali than that we now find combined with it, and that after it was diſſolved in the water, a part of this alkali was neutralized by acid vapours, or acid ſubſtances, which the water found in its way towards the ſurface.

ON the whole, however, the ſuppoſition which appears to me the moſt probable is, that common ſalt and Glauber's ſalt, conveyed by ſea-water, or contained in foſſils formed from ſea-plants, have been applied, under the influence of a violent heat, to ſome of the numerous earthy and ſtony ſtrata which contain mixtures of ſiliceous and argillaceous earth; that thoſe ſalts have been in part decompounded, by the attraction of theſe earths for the alkali of the neutral ſalt, part of the acid has been diſſipated, or changed into ſulphur and ſulphureous gas, by the action on it at the ſame time of inflammable matter, which we know to be preſent in many of the ſtrata; and that the compound of alkali and earthy matter has afterwards been long expoſed, and continues expoſed, to the action of the hot water. By ſuch a ſuppoſition, we can imagine how the ſeveral ingredients of theſe hot ſprings became diſſolved in them; and this ſuppoſition appears the more probable, when we attend to the accurate obſervations of Mr STANLEY, on the nature of the country, and ſtate of the ſoil, in which theſe two hot ſprings are found. The rocks and mountains, which are at a ſmall diſtance, or in the immediate vicinity of each of them, are formed chiefly of different kinds of lava. The lower country and ſoil at the foot of theſe, and in which the ſprings riſe, is compoſed of fragments of theſe lavas; but in digging into this ſoil or rubbiſh to a ſmall depth only, theſe fragments are every where found reſolving, or reſolved, into a matter like clay. At a certain depth, the fragments of ſome ſpecies of lava remain entire and hard, while the reſt are changed. At a greater depth, even theſe more durable kinds are found to have undergone the ſame change with the reſt. As this change is produced by the conſtant action of the hot water, it probably depends on a gradual diſſolution and extraction from theſe lavas of ſome of their ingredients, which are diſſolvable in water ;, and thoſe which we have actually found in the water may have been ſome of theſe. But I offer all this as a conjecture only, which every perſon who does not like it is at liberty to reject.

I SHALL venture further to offer another conjecture, which ſome particulars I learned by Mr STANLEY's voyage to Iceland have ſuggeſted to my mind. It is concerning the origin of the pure ſulphur, which is found at the ſurface of the earth, in the neighbourhood of many volcanos in different parts of the world. In Iceland, there are places in which ſulphur is thus found in very great quantity, covering the ſurface of the ground, and that of the ſtones and rocks, in form of a thick cruſt, and conſtituting what are called ſulphur banks. This was ſeen in Iceland in particular ſpots, in which there were very ſtrong ſulphureous hot ſprings, which emitted ſuch a quantity of ſulphureous or hepatic gas, that the air all around was infected with it to the higheſt degree, and the water itſelf was muddy and black, and conſtantly boiling. Now, as we know, that vital air has the power to decompound this gas, and to make it depoſit the ſulphur which it contains, I am of opinion, that the ſulphur which appeared in ſuch quantity in the vicinity of theſe ſprings, had been depoſited and accumulated in this manner from the hepatic gas, which theſe ſtrongly ſulphureous ſprings have emitted during a great length of time.

APPENDIX.

In order to ſhew, that ſuch a ſmall proportion of a vitriolic ſalt as is contained in the Iceland waters, has not the power to precipitate ſilver, I diſſolved gr. 0.3 of exſiccated Glauber's ſalt, in gr. 2000 of diſtilled water, which thus contained a proportion of Glauber's ſalt rather greater than that contained in the Iceland waters. I then added five drops of purified aquafortis, and five drops of the ſolution of ſilver. The mixture remained tranſparent ſeveral days. I afterwards added gr. 0.7 more of the exſiccated Glauber's ſalt, without diminiſhing in the leaſt the tranſparency of the mixture. After a few days more, I added gr. 9 of the exſiccated Glauber's ſalt. This produced a diminution of tranſparency, and the ſediment ſubſided in a few days more. This ſediment being carefully collected and dried, weighed gr. 0.3; but the clear liquor which had been filtrated from it, ſtill retained the greater part of the ſilver. I therefore added to it ſome pure common ſalt, which precipitated all the reſt of the ſilver, and this laſt precipitate, being alſo collected and dried, weighed juſt one grain.

When I examined theſe two precipitates by means of the blowpipe, their qualities appeared to me ſo much the ſame, that I ſuſpect the firſt was produced by a ſmall quantity of common ſalt, contained imperceptibly in the Glauber's ſalt. If there were 12 or 13 parts of common ſalt in 1000 of the Glauber's ſalt, they were enough to produce the above quantity of the firſt precipitate; and as Glauber's ſalt is prepared from common ſalt, we can eaſily underſtand how a ſmall quantity of the common ſalt may remain in it.

For the ſake of thoſe who may have occaſion to undertake ſuch chemical enquiries as that deſcribed in the above paper, I ſhall here mention the method by which I collected and weighed the ſmall quantities of ſediments or precipitates, which I obtained in ſome of theſe experiments. In moſt caſes, the turbid liquor was left at reſt in a cylindrical glaſs, until the ſediment was ſo well collected at the bottom, that the greateſt part of the liquor was quite clear, and then this clear part was carefully decanted; the reſt, which could not be decanted without diſturbing the ſediment, was ſhaken, and poured gradually into a ſmall filtre, that the ſediment might be collected upon the filtre, and afterwards waſhed on it, by paſſing diſtilled water through it repeatedly. And this part of the proceſs was much facilitated by the preparation of the filtre, and ſome other little manœuvres. When, for example, I uſed for my filtre a piece of paper about four inches in diameter, I began by folding it, and giving it the proper form; then I ſpread it open again, and warming it, I applied melted tallow or bees wax to the margin of it all round, until it was ſoaked therewith to the breadth of a full inch from the margin inwards, the middle part of it being carefully preſerved clean. As ſoon as this was done, and while it was yet a little warm, it was folded again into the proper form of a filtre, and retained in that ſtate until it was cold. On a filtre prepared in this manner, it is much more eaſy to collect a ſediment together, and to waſh it clean, than on an ordinary filtre. In the firſt place, no part of the ſediment adheres to or is depoſited on that part of the paper which was ſoaked with tallow. The whole is collected on the clean part of the paper, and after it is collected there, I condenſe it into the centre as much as poſſible, by dropping the diſtilled water on the margin of that clean part all round, or a little above that margin, by which practice the ſcattered particles of the ſediment are waſhed down into the bottom. Sometimes I apply what may be called a capillary jet of the diſtilled water, directed

rected with force to thoſe parts of the ſcattered ſediment which are more difficultly moved. Having thus condenſed the ſediment as much as poſſible, the filtre is left in a cool place to dry. When it is perfectly or nearly dry, I ſpread it flat on a table, and cut away all that part which was ſoaked with tallow, and and alſo thoſe parts of the clean paper to which the ſediment does not adhere. The reſt, with the ſediment on it, is then well dried before a fire, and weighed, and the weight of it marked down; and, laſtly, in order to know how much of this weight is made up by the paper, I take care, before I prepare the filtre, to chuſe another piece of the filtrating paper, equal in thickneſs to the one of which the filtre is made. This equality of thickneſs is judged of by holding the two pieces between the eye and the light; or, for greater ſecurity, bits of the two pieces may be cut off, exactly ſimilar and equal in form and ſize, and their weight compared, and allowance may afterwards be made for their difference of weight, if there be a difference. After weighing the bit of paper with the ſediment on it, a proper bit of the reſerved paper is laid flat on a ſmooth table or plate of glaſs, and the paper on which the ſediment had been collected is laid over it, with the clean ſide undermoſt; then a bit of card, ſomewhat leſs, but nearly of the ſame form, is preſſed down on both the papers, and, with a pair of ſharp pointed ſciſſars, or a pen-knife, the undermoſt paper is cut exactly to the ſame ſhape and ſize as the uppermoſt, and is afterwards weighed. The weight of it being deducted from that of the former, we thus learn the weight of the ſediment, with a greater degree of exactneſs, and with leſs trouble, than by any other method which I have been able to contrive. To complete this article, I beg leave further to add, that the moſt ready and convenient way to ſoak the margin of the filtrating paper with tallow or wax, is to hold it above a lighted candle, at a proper diſtance for warming it a little, and then melting the end of another candle, apply it immediately to the warmed

warmed paper, and repeat this, until the paper is prepared as above directed. The prominent part of the wick of the candle, which is thus melted, becomes a ſort of pencil, which holds the melted tallow or wax, and facilitates the application of it, and the wick of a tallow candle, on account of its being thicker, is fitter for this purpoſe than the wick of a wax one.

THE laſt remark on theſe experiments I ſhall now make is, that, in the trials with the ſolution of barytes, the barytes vitriolica was formed in particles ſo very minute, that they did not all remain at firſt upon the filtre. Some of them paſſed through it, and made the filtrated liquor a little muddy; but by making this muddy liquor paſs through the filtre a ſecond time, it was made quite clear, the whole of the ſediment being thus collected on the filtre.