

XXVIII. *Experiments and Observations on various Phænomena attending the Solution of Salts: By R. Watfon, A. M. F. R. S: Fellow of Trinity College, and Professor of Chemistry, in the University of Cambridge.*

Read May 24, 31,
1770. **H**AVING lately had occasion, in some chemical enquiries, to make various solutions of salts, I met with some phænomena, which did not appear to me either to have been sufficiently attended to, or consistently explained by writers upon that subject. The suspension of salts in water, of metals in acids, of sulphur in oils, and of other bodies in menstrooms specifically lighter than the bodies themselves, hath ever been considered in chemistry, as a problem of difficult solution. Those philosophers who acquiesce, upon the whole, in the cause which hath been assigned for this phænomenon by Sir Isaac Newton, in his optical Questions, have taken great pains to illustrate the manner how it is effected, by supposing that the bodies are received into the pores of their respective menstrooms, and there kept suspended by the attraction or, as Bernouilli and Freind would have it, by the resistance arising from the tenacity of the fluid. Hence it happens, say these philosophers, that
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after water is saturated with one salt, it is still capable of dissolving somewhat of a second kind, and being saturated with that, of a third, and so on; just as a vessel filled as full as possible with spheres or cylinders of one magnitude hath a capability of receiving similar bodies of an inferior size, or bodies of a different figure. The opinion of Gassendus seems to have been generally adopted; he endeavours to prove, from the experiment which hath been mentioned, not only the porosity of water, but a diversity in the figures of the pores: *Affero & aliud experimentum singulare, quo visus sum mihi deprehendere interspersa hujusmodi spatiola inania intra aquam dari.—Aiebam, cum sint salis corpuscula cubica, poterunt ea quidem replere spatiola, quæ & ipsa cubica fuerint; at cum non modo commune sal, sed alumen etiam, quod est octahedricum, halinitrum item, & sal ammoniacum saccharumque & alia quæ aliarum sunt figurarum eâdem aquâ exsolvi possunt; erunt ergo etiam in aqua spatiola octahedrica atque id genus alia; adeo ut aqua, tametsi sale saturata fuerit, nihilominus & alumen et cetera omnia exsolvere possit ac in sese transfundere.* Gaf. Phys. l. i. sect. i. cap. iii. The reason why warm water dissolves in general more salt than cold water, seems as if it might be derived from the same principle, was it true; the interstices between the elementary particles of water are enlarged by the expansion of the fluid, and might therefore be supposed capable of admitting into them a larger quantity of salt. This doctrine hath been embraced by most philosophers, especially by the late Abbé Nollet, in the 4th volume of his *Leçons de Physique*; and I do not know that it hath been opposed by any body. The late

late Mr. Eller, of Berlin, hath carried this speculation so far, as to publish a Table in the Berlin Memoirs for 1750, exhibiting the several quantities of above twenty different kinds of salt, which a given quantity of water will absorb into its pores, without being in the least augmented in bulk. It is not therefore without some uneasiness that I find myself constrained to dissent from the general opinion, and particularly to differ from Mr. Eller, who hath treated this subject *ex professo*; who made his experiments, as he himself assures us, with the greatest exactness; and who was led by them to the discovery of what he is pleased to call, *une verité incontestable, savoir, que les plus petites parties constituantes de l'eau sont douées de pores ou d'interstices dans lesquels les atomes de sel peuvent nicher, sans augmenter leur volume.* I do not at present see any very probable method of reconciling the different results of our enquiries; I will therefore content myself with giving a plain relation of the experiments which I have made upon this subject.

EXPERIMENT I.

I took a large mattrais, containing, when filled to the middle of its neck, 132 ounces of water, Troy weight; the diameter of the cavity of the neck was six lines: having with a diamond marked the place where the water stood in the neck of the mattrais, I dropped into it a single piece of purified nitre, the weight of which was a 2600th part of the weight of the water, and immediately observed that the water was considerably elevated in the tube: during the
 solution

solution of the salt, the water sunk near one third of its whole elevation; but when the solution was entirely finished, it remained very sensibly raised above the mark: so that, even from the experiment with this instrument, we may be assured that water cannot absorb $\frac{1}{3000}$ th part of its weight of nitre, without being augmented in bulk. Mr. Eller, from his experiments, concludes, that eight ounces of water will absorb one drachm and a half, or above a 42d part of its weight of nitre; and hence I supposed the quantity of water which I used would have absorbed above sixteen times as much, or above 3 ounces; whereas the event shewed that it could not absorb $\frac{1}{20}$ of an ounce. From the sinking of the water during the solution, I was at first inclined to believe that some part at least of the nitre was taken into the pores of the water: in order to see whether this conjecture could be verified by fact, I made the following experiment.

EXPERIMENT II.

I chose two mattraffes of unequal sizes, containing quantities of water in the proportion of 12 to 1, the diameters of the necks being equal: into the largest I put $\frac{1}{3000}$ th part of the water's weight of nitre, and an equal quantity into the smaller; and I observed that the water, as well before as after the solution, was equally elevated in them both: this experiment was repeated. Now, if a given quantity of water can absorb into its pores, without being increased in magnitude, any quantity of salt however small, it seems reasonable to suppose that a quantity containing

containing twelve times as many pores should absorb twelve times as much, (since it is an allowed fact that the minutest portion of a salt is uniformly diffused through the largest quantity of water) and it might consequently be expected, that the water should rise higher in the neck of the smaller matras than in that of the larger, which is contrary to the experiment.

EXPERIMENT III.

Apprehending that common pump water, with which I had made the preceding experiments, might have its interstices preoccupied by selenites and other heterogeneous matters, and be thereby rendered incapable of admitting into them any additional substance; and observing that Mr. Eller had used in all his experiments 8 ounces of distilled water, I had hopes to have reconciled my experiments to his by that means: but upon trial, with distilled water, I found the elevation precisely the same as before. Nor do the conclusions depend upon the kind of salt; they hold true *mutatis mutandis* of any other salt as well as nitre. During the solution the water is refrigerated and thereby contracted in magnitude, and the smaller the quantity the greater will be the cold and consequent contraction produced by the addition of small portions of salt; but I cannot suppose that this circumstance could be overlooked by Mr. Eller, though it induced me to use a much larger quantity, or that he attributed the sinking of the water during the solution, to an imbibition of the particles of the several salts into the pores of the

water, and thence by calculation constructed his table.

EXPERIMENT IV.

Having always remarked that the water in the neck of the matrafs was elevated higher upon the first immerfion of the falt, than after it was wholly diffolved, I endeavoured to ascertain the difference in feveral kinds of falt. To do this with the greater exactnefs, I pitched upon a matrafs which had a neck as far as I wanted it accurately cylindrical, as I found by obferving the elevations occafioned by the additions of equal portions of water; the matrafs held about 67 ounces of water. The falts I ufed were all dry, and in as large pieces as the neck of the matrafs would admit; the water was heated to the forty fecond degree of Fahrenheit's thermometer, and kept as nearly as could be in that temperature. I changed the water for each experiment, and ufed in each 24 penny weights of falt; the heights to which the water rofe, as meafured from a mark in the middle of the tube, before and after the folution of each falt are expreffed in the following table: the firft column denotes the height to which the water was elevated by 24 penny weights of falt before its folution, the fecond after its folution, the third the difference in fractional parts of the elevation before folution

Elevation by 24 penny weights of fimple water	0	58	
24 penny weights of genuine Glaubers falt	42	36	$\frac{1}{7}$ Vol.

Vol. falt of fal. ammon.	40	33	$\frac{7}{48}$
Sal ammon.	40	39	$\frac{1}{40}$
Refined white fugar	39	36	$\frac{1}{13}$
Coarse brown fugar	39	36	$\frac{1}{13}$
White fugar candy	37	36	$\frac{1}{37}$
Glaubers falt from Lymington	35	29	$\frac{6}{33}$
Terra foliata tar.	37	30	$\frac{7}{37}$
Rochelle falt	33	28	$\frac{5}{33}$
Alum not quite diffolved	33	28	$\frac{5}{33}$
Borax not half diffolved in 2 days	33	31	$\frac{2}{33}$
Green vitriol	32	26	$\frac{3}{16}$
White vitriol	30	24	$\frac{1}{5}$
Nitre	30	21	$\frac{3}{10}$
Sal gem. from Northwich	27	17	$\frac{10}{27}$
Blue vitriol	26	20	$\frac{3}{13}$
Pearl afh	25	10	$\frac{3}{5}$
Vitriolated tartar	22	11	$\frac{1}{2}$
Green vitriol calcined to whitenefs	22	11	$\frac{1}{2}$
Dry falt of tartar	21	13	$\frac{8}{13}$
Basket fea falt	19	15	$\frac{4}{19}$
Corrofive sublimate	14	10	$\frac{2}{7}$
Turbith mineral	9	0	

Had I not been in fome meafure perfuaded, from the refult of the preceding experiments, that no portion of any falt could be abforbed into the pores of water, I fhould have readily concluded that the third column of this table denoted fuch parts of 24 penny weights of the feveral falts as might be lodged in the interfices of 67 ounces of water without increafing its magnitude: the quantities indeed which might have been thus afcertained would have but ill agreed with thofe which are determined by Mr. Eller; and

that diversity of quantity may suggest a doubt concerning the validity of his principle. The sinking of the water in the neck of the matrafs seems to be a general phænomenon attending the solution of all salts; the quantity of the descent is various from $\frac{1}{4}$ to $\frac{1}{2}$ of the whole elevation in those salts which I have tried. In forming the table, I repeated many of the experiments, but found no variation which could affect the general conclusion; with particular attention I repeated the solution of vitriolated tartar, for I thought it a very remarkable circumstance that one of the hardest salts should be more diminished in proportion to its whole bulk than any other, but the numbers in the table 22 and 11 accurately expressed the height before and after solution upon the repetition of the experiment, so that it may be relied upon as a certain fact that a cubic inch of vitriolated tartar is by solution in water reduced to half a cubic inch, though the water cannot, as appeared from an experiment I made, absorb $\frac{1}{1000}$ th part, nor, as I believe, any part, of that salt without being augmented in magnitude. It is evident from the table that sal gemmæ, blue vitriol, corrosive sublimate, calcined vitriol, and in general those salts which retain the least water in their composition and constitute the hardest masses, sink more in proportion to their respective bulks than any other. I own myself at a loss for a general principle to explain this general phænomenon, unless the air contained in the several salts may be esteemed sufficient for the purpose; a very copious separation of air from the salts during the whole time of their solution may be readily observed in all of them, and a small portion of it, combined with
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the particles of a salt, may augment its bulk, without sensibly increasing its weight. Yet the two following experiments rather tend to diminish the probability of this opinion.

EXPERIMENT V.

I took water which had been well purged from its air by long boiling, and which had been corked up whilst it was warm; when it had acquired a proper temperature, I filled a matrafs with it, as before, and putting into it *sal gemmæ*, &c. I observed that the elevation before solution was the same as when common water was used, and that it sunk equally in the neck during the solution; but then the separation of air seemed greatly less in all the trials I made. This phenomenon is easily explained: common water is always saturated with air; upon the addition of any salt, the particles of water begin to attract and dissolve the salt, and let go the air with which they are united; this air, added to the air contained in the salt, renders the whole much more visible in common than in boiled water. Musschenbrook and others are of opinion, that air only fills the interstices of water, without augmenting its bulk; they ground their opinion upon observing that the specific gravities of common water and of water purged from its air are equal; the fact, taking it for granted, will scarcely authorize the conclusion: for, supposing that a cubic inch of common water contains even a cubic inch of air, the difference of the weight of the water when saturated with air, and when freed as much as possible from it (though probably it can never be wholly

wholly freed from it), will not equal $\frac{1}{4}$ of a grain: how imperceptible then must the difference be, if water, instead of an equal bulk, doth not contain $\frac{1}{100}$ th part of its bulk of air, which is a supposition much nearer to the truth: the air is separated from the water during the solution of the salt, and the particles of the salt probably occupy its place as happens in other chemical precipitations; but we cannot thence infer that they are received into the interstices of the water, unless we had more conclusive arguments, to prove that the air itself was lodged in them. I varied the preceding experiment by putting two equal and transparent pieces of sal gemmæ into two tall drinking glasses, filled one with common, the other with boiled water; from the first there continually ascended a very visible stream of air, and the salt and the bottom of the glass were covered with bubbles, it seeming as if the water quitted its air to dissolve the salt; in the other, though some air was seen breaking out from the salt whilst it was dissolving, there did not seem to be any precipitated, as it were, from the water. In most of the experiments which I made, the boiled water dissolved a given quantity of salt sooner than the common water, when they had the same degree of heat; but the difference in time might be owing to the different magnitude of the surfaces of the salt, though from the generality of the event, I should rather attribute it to the different dissolving powers of water, when replete with, and when deprived of air.

EXPERIMENT VI.

Thinking that the difference in the bulks of the water before and after solution might be owing to the separation and escape of some volatile principle; I took care to balance as accurately as I could, water and sal gemmæ, water and salt of tartar, water and vitriolated tartar, &c. and then putting the several salts into the water, I observed when the solution was accomplished, whether the equilibrium of the scales was affected, but I could not distinguish any change. Dr. Hales and others have spoken of the existence of air in salts, and have in two or three instances investigated the quantity, but after a very different manner from that I have used; nor can I think myself at liberty to esteem this air which is separated by solution, of the same nature with that which is called by him and others fixed air, inasmuch as fixed air makes a considerable part of the weight of the bodies from which it is extracted, precipitates lime water, and is seldom discharged (or perhaps produced from some of the minute parts of the body being converted by the violence of the fire, &c. into an elastic fluid), except when the body is decomposed; whereas this makes only a considerable part of the bulk of bodies, and thus diminishes their specific gravity without sensibly increasing their absolute weight; does not, as I collected from some rough trials, render lime water turbid; and is set at liberty, though not by a mechanical division, yet by an operation somewhat different from chemical decomposition. It hath been remarked by some, that
saline

saline solutions will not crystallize without much difficulty in an exhausted receiver; perhaps because the particles of salt cannot attract that principle which should cement them together, which at least may be seen escaping from them when they begin to be separated. Mr. Boyle observed, that aquafortis, poured upon a strong vegetable alkali, did not crystallize till it had been long exposed to the air (though I should rather attribute this failure to the weakness of his aquafortis than to the want of air, since I have frequently, by using the fuming spirit of nitre, obtained crystals of an inch in length almost instantaneously); and several other phenomena might be adduced respecting the crystallization of salts, which seem to indicate the necessity of admitting air as a very efficacious instrument in producing that effect: but future experience may tend to elucidate this matter. Having used great attention in making the experiments from which the preceding table was composed; I thought I had a good opportunity of deriving from it the specific gravities of the salts which are there mentioned. I accordingly calculated the following table; in the first column of which are expressed the specific gravities as calculated from the increase of bulk before solution; in the second, after the solution.

Genuine Glauber's salt	1,380	1,611
Crystals of kelp	1,414	1,467
Volat. salt of sal ammoniac	1,450	1,787
Sal ammoniac	1,450	1,487
Sugar refined, brown, barley	1,487	1,611
White sugar candy	1,567	1,611
		Terra

Terra foliata tartari	1,567	1,933
Glauber's salt from Lymington	1,657	2,000
Rochelle salt	1,757	2,071
Alum	1,757	2,071
Borax	1,757	
Green vitriol	1,812	2,230
White vitriol	1,933	2,416
Nitre	1,933	2,766
Very transparent sal gem. from Nortwich	2,143	3,411
Blue vitriol purified	2,230	2,900
Pearl ash	2,320	5,800
Vitriolated tartar	2,636	5,272
Green vitriol calcined to whiteness	2,636	5,272
Dry salt of tartar	2,761	4,461
Basket sea salt	3,052	3,866
Corrosive sublimate	4,142	5,800
Mercury distilled with acid of vitriol, and freed from its acid by a strong fire		6,444

The numbers in the first column correspond very well, upon the whole, with the specific gravities which have been determined by others hydrostatically; thus the specific gravities of nitre, alum, white and green vitriol, sal ammoniac, sal gemmæ, &c. are greater than what are assigned to these bodies by some authors, and less than what have been determined by others; it seems as if the specific gravities of saline bodies might, in a proper vessel, be more accurately ascertained from the observed increase of the water's bulk than any other way. Upon the supposition that the escape of the air is the reason of

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the water's sinking during the solution, and that this air contributes little to the weight of the salts, though it may be absolutely necessary to the exhibiting the saline molecularæ under a visible crystalline appearance; the second column will denote the real specific gravities of the salts as freed from air. That this air is combined with the salts, and doth not simply adhere to their surfaces, may appear from hence, that the specific gravities, as calculated from the increase of bulk observed in the water before solution, sufficiently correspond with those which philosophers have determined hydrostatically: nor indeed, upon exhausting the air from the salts, by an air pump, could I observe that it was separated, in less quantity during solution.

EXPERIMENT VII.

Since equal quantities of salt must contain equal quantities of air, it might be expected *a priori*, if the escape of the air was the occasion of the water's sinking, that equal weights of salt would produce equal augmentations of bulk, and unequal weights augmentations proportionable to their weights; but, to be assured of this, I took a matrafs containing about 30 ounces of water, the tube being cylindrical for about 7 inches in length. When the matrafs was filled to a proper mark, I put into it 7 pennyweights of powdered sal gem.: the water after the solution had risen through 17 tenths of an inch; by the addition of 14 pennyweights more, the water was raised through 51 divisions from the first mark, or twice 17 from where it stood after the solution of
7 penny-

7 pennyweights. In the same matrafs I tried a similar experiment with nitre; the water was raised through 10 divisions, by 3 pennyweights of powdered nitre; and by 18 more, it stood after the solution at the 70th division from the first mark, and consequently rose through six times the space, through which it had been raised by 3 pennyweights. From these, and other experiments of the same kind, I am disposed to believe that equal portions of salt produce equal augmentations in the bulk of the water wherein they are dissolved; at least, this holds true when the salt dissolved bears but a small proportion to what would be requisite to saturate the water. But, in making this experiment, great care must be taken to keep the salts of the same dryness; I had once tried it with three equal quantities of sea salt, and arrived at a quite different conclusion; the increases of bulk occasioned by the solution of the several salts being separately taken, as 15, 16, 17, but the salt being much drier than the air in the laboratory, had undoubtedly attracted the humidity, and that portion had attracted the most which had been the longest in it, and which was last dissolved. Nor should the temperature of the water be neglected; a sensible error may proceed from a minute change in that. This experiment confirms the first, for, was any part of salt absorbed into the pores of the water, it certainly ought to be expected that the elevation occasioned by the solution of 3 pennyweights of nitre should be less than $\frac{1}{6}$ th of that occasioned by 18 pennyweights, and yet I found it to be accurately $\frac{1}{6}$ th upon repeating the experiment with distilled water. It confirms it too in another view, 3 penny-

weights or $\frac{1}{200}$ th part of the weight of the water, raised it through one inch; hence $\frac{1}{2000}$ th part would have raised it through one tenth of an inch, which any eye may distinguish.

Dr. Lewis, for whose great abilities in chemistry I have a very high respect, in his little treatise upon American potashes, is of opinion, that the augmentation of the bulk of water doth not proceed uniformly, according to the quantity of salt added; and he forms his conclusion from observing, that the losses of weight sustained by the same body in different solutions, were not uniform, but continually diminished; the losses corresponding to seven successive equal quantities being as $24\frac{1}{2}$. 24. $23\frac{1}{2}$. 22. 22. 21. 20. Upon considering this matter in a mathematical light, I am inclined to draw a quite different conclusion; but I will first mention some experiments which I had formerly made with a different view, and which agree very well with Dr. Lewis's.

EXPERIMENT VIII.

I had conceived that if, in a given quantity of water, several quantities of salt, increasing in any arithmetical or geometrical progression, were dissolved; that the increments of specific gravity would increase in the same progression. In order to see whether this conjecture could be established by experiment, I dissolved in a given quantity of water, different portions of sea salt, increasing in the progressions expressed in the annexed tables, where the first column of each denotes the proportional quantities of salt in pennyweights, the second, the loss of weight of a given body

body in quarter grains; the third the excess of the specific gravity of each solution, above the specific gravity of water.

TAB. I.			TAB. II.			TAB. III.		
	263	0		263	0		883	0
9	273	10	5	269	6	4	899	16
18	282	19	10	274	11	8	915	32
27	292	29	15	280	17	12	930	47
36	301	38	20	285	22	16	945	62
45	309	46	25	289	26	20	959	76
			30	294	31	24	971	88
			35	300	37	28	985	102
			40	304	41	32	996	113
			45	309	46	36	1009	126
			50	312	49	40	1020	137
			55	316	53			

The difference of the numbers in the third column of each table from arithmetical progressions, is obvious at first view, the difference of the two last numbers of each being considerably less than the difference between the two first: and the numbers 6. 11. 22. 41. corresponding to the geometrical progression 5. 10. 20. 40. in the second table as well as the numbers 16. 32. 62. 113 corresponding to the geometrical progression 4. 8. 16. 32, in the third, differ considerably from geometrical progressions, whose common ratio is $\frac{1}{2}$.

In making these experiments there are three obvious sources of error: the heat may not remain constant; the additional weights of salt may not be accurately equal; and the weight of the given body may be more or less than what is expressed by any quantity

quantity less than $\frac{1}{4}$ of a grain; yet the differences of the preceding numbers, from arithmetical or geometrical progressions, are too great to be explained from any or all of these sources taken together. We may observe that the losses of weight, corresponding to equal portions of salt, are, upon the whole, diminished; but it will not follow from thence that the bulks are not equally augmented. For, since the specific gravity of every body is properly denoted by a fraction, whose numerator expresses the absolute weight, and denominator the magnitude of the body; let $\frac{w}{m}, \frac{w+x}{m+y}, \frac{w+2x}{m+z}, \frac{w+3x}{m+s},$ &c. be a series of fractions, whose several numerators express the weights of a given quantity of water, as increased by the addition of equal portions of any salt denoted by x , and whose denominators express the bulks of the water after the solution of each portion of salt, the increments of bulk being denoted by y, z, s ; now let us suppose that the losses of weight sustained by the same body, that is, the specific gravities, increase uniformly, then will the above series of fractions increase uniformly, let $\frac{w}{m} = a; \frac{w+x}{m+y} = a + b; \frac{w+2x}{m+z} = a + 2b; \frac{w+3x}{m+s} = a + 3b,$ from these equations investigating the proportion between y, z, s , which represent the augmentations of bulk, it will appear that $y : z :: a + 2b : 2a + 2b$, or in a greater ratio than that of $1 : 2$ and that $z : s :: 2a + 6b : 3a + 6b$ or in a greater ratio than that of $2 : 3$, in which ratios they ought respectively to have been, had the denominators or the bulks of the fluid increased

creased uniformly, when the specific gravities or absolute weights increased uniformly. We see from this, what conclusion should have been formed, had the increments of specific gravity from equal portions

of salt been equal. Again, suppose that $\frac{w}{m}, \frac{w+p}{m+q}, \frac{w+2p}{m+2q}, \frac{w+3p}{m+3q},$ &c. denote a series of fractions, whose

numerators, expressing the weights of a given quantity of water as increased by the addition of salt, and whose denominators, expressing the bulks, both increase uniformly, then will the several differences between the 2d and 1st, between the 3d and 2d,

and so on, be as $\frac{1}{m \times m + q}, \frac{1}{m + q \times m + 2q}, \frac{1}{m + 2q \times m + 3q},$
 $\frac{1}{m + 3q \times m + 4q},$ &c. which fractions being inversely

as their denominators constitute a decreasing series; but the increments of specific gravity from the addition of equal portions of salt, are proportionable to these fractions, and therefore ought perpetually to decrease, though we allowed the bulk of the compound to be precisely equal to the bulk of the water and salt taken together, that is, though we allowed the bulk of the water to increase uniformly according to the quantity of salt added: now as it is evident from Dr. Lewis's experiments, and from each of the preceding tables, that the increments of specific gravity do decrease upon the whole, when the absolute weights increase uniformly, we may venture to conclude that the bulks increase uniformly also. I thought proper to explain the foregoing principle and to determine the ratio, because the

matter

matter seems to have been mistaken by many; however, it may be easily apprehended that the increments of specific gravity, from the addition of equal quantities of salt to a given weight of water, ought perpetually to decrease; because the difference between the specific gravities of the water and of the salt perpetually decreases, as the water approaches to perfect saturation. In like manner, if to a given quantity of water we add any number of equal quantities of oil of vitriol, or any fluid miscible with and heavier than water; the increments of specific gravity will perpetually decrease, though they will never entirely vanish, because there is a perpetual approximation to the specific gravity of the acid, which yet the mixture can never acquire; and, *vice versa*, if to water we add a lighter fluid, as spirits of wine by equal portions, the specific gravity of the mixture will constantly decrease by unequal decrements; but the decrements will never vanish, because the mixture must ever remain specifically heavier than spirit of wine.

EXPERIMENT IX.

The quantities of various salts, which may be dissolved in a given quantity of water, have been ascertained by Boerhaave, Eller, Spielman, and others; their accounts differ somewhat from one another, as might be expected from the different temperatures of the air, the different state of their salts; the different times (a circumstance of no small consideration in this matter) which they allowed the water to act upon the salts before they concluded it
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to be fully saturated, and from some other circumstances which might perhaps with advantage be taken into the account, and a more accurate table composed than hath hitherto been published ; but as the differences would be small, and might not tend to any new discoveries, I could not persuade myself to be at the trouble of making the requisite experiments. I thought it would be a more useful undertaking to determine the specific gravities of saturated solutions of various salts. In composing the following table, I used every possible precaution ; the solutions were fully saturated, by permitting the water to rest upon the salts for some weeks, and frequently shaking the solutions during the interval : I had some reasons for chusing this method rather than the much shorter one of dissolving the salts in hot water, and letting the solutions cool, though the event will be much the same in both ways ; my balance was extremely sensible, though I did not use any weight less than a quarter of a grain ; the water in which the salts were dissolved was not $\frac{1}{4}$ of a grain in 890 heavier than distilled water ; the solutions were all of the same temperature, Fahrenheit's thermometer standing between 41 and 42° during the whole time of taking the specific gravities.

A Table exhibiting the specific gravities of water saturated with various salts. • Thermometer 41—42°, barometer 30 inches.

Water in which the		Crytals of tar	1,001
salts were diff.	1,000	Arsenic	1,005
Saturated with		Borax	1,010
quicklime	1,001	Corros. sublim.	1,037
VOL. LX.		Y y	Alum

Alum	1,033	Nitre purified	1,095
Genuine Glau. falt	1,052	Rochelle falt	1,114
Vitriolated tart.	1,054	Blue vitriol	1,150
Common falt	1,108	Green vitriol	1,157
Arsen. nitre	1,184	Sal gemmæ	1,170
Glau. falt Lyming.	1,232	Epſom falt Lym.	1,218
Sa! ammon.	1,072	White vitriol	1,386
Vol. falt of fal. am.	1,077	Pearl aſh	1,534
Cryſtals of kelp	1,087		

By making other tables ſimilar to the preceding, when the thermometer ſtands at 62°, 82°, 102°, &c. or when the heat increaſes or decreaſes in any known ratio; it is extremely probable that the law, according to which the diſſolving power of water varies with the variation of its heat, might be investigated. I have ſome reaſons for thinking that though it increaſes with the increaſe of heat, yet it doth not increaſe in the direct ſimple ratio of the heat; but what the law is, or whether all ſalts follow the ſame law, I cannot, from any experiments I have already made, determine; and I have no leiſure at preſent to proſecute the enquiry. The concluſion will be unavoidably liable to a ſmall inaccuracy; for whether the ſpecific gravities be investigated by weighing the ſeveral fluids in a given veſſel filled to a given mark, or by weighing a given ſolid in each of them, we ſhall not thence obtain the weights of equal bulks, ſince the containing veſſel or the ſolid, from the difference of the heats, have a different capacity or a different bulk. However, it is not apprehended that this circumſtance would ſenſibly affect the concluſion, eſpecially as it is ſubject to calculation and might

might be allowed for. It ought, at the same time, to be observed, that a given bulk of the water with which the specific gravities are composed, will have different weights when the heats are different; and these differences ought first to be ascertained.

EXPERIMENT X.

Having thus determined the specific gravities of saturated solutions of several salts, in a given degree of heat; my next enquiry was to find the specific gravities of water impregnated with a given quantity of the several salts: I accordingly dissolved in 168 pennyweights of water, 14 pennyweights, or $\frac{1}{12}$ of the weight of the water of the eight following salts. The thermometer was at 40° and barometer at $29\frac{1}{2}$.

A Table of the specific gravities of water impregnated with $\frac{1}{12}$ of its weight of

Water	1,000
Sea salt	1,059
Blue vitriol	1,052
Nitre	1,050
White vitriol	1,045
Green vitriol	1,043
Glau. salts Lym.	1,039
Glau. salts Genu.	1,029
Sal ammon.	1,026

I could not have made this table much more extensive, since in the 40th degree of the thermom.

water will not dissolve $\frac{1}{12}$ of its weight of alum, borax, vitriolated tartar, corrosive sublimate and a great many other salts; however, as such a table cannot fail of being useful in chemical, and perhaps medical researches, it would be worth while to make it more general, either by dissolving a less portion of salt, or making use of a greater degree of heat.

EXPERIMENT XI.

To these tables I have subjoined another of a different nature, wherein the specific gravities of water impregnated with different quantities of the same salt from $\frac{1}{3}$ down to the 1024th part of the weight of the water, are determined. I cannot accuse myself of carelessness in making any of the experiments from which the table is formed; but part of it being made in a room where the heat was about 55°, and the other in my laboratory, when it did not exceed 46°, a certain inaccuracy, though it will be a very small one and scarce sensible in the weight of the small body which I used, will attend it upon that account. The salt was sea salt of the finest kind, and extremely dry; many of the experiments were repeated.

A Table of the specific gravity of water impregnated with different quantities of sea salt. Thermometer between 46 and 55°.

Water	1,000
Salt $\frac{1}{3}$	1,206

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$\frac{1}{4}$	1,160
$\frac{1}{5}$	1,121
$\frac{1}{6}$	1,107
$\frac{1}{7}$	1,096
$\frac{1}{8}$	1,087
$\frac{1}{9}$	1,074
$\frac{1}{12}$	1,059
$\frac{1}{14}$	1,050
$\frac{1}{15}$	1,048
$\frac{1}{16}$	1,045
$\frac{1}{18}$	1,040
$\frac{1}{21}$	1,032
$\frac{1}{24}$	1,029
$\frac{1}{27}$	1,027
$\frac{1}{28}$	1,025
$\frac{1}{30}$	1,024
$\frac{1}{32}$	1,023
$\frac{1}{36}$	1,020
$\frac{1}{39}$	1,019
$\frac{1}{42}$	1,015
$\frac{1}{48}$	1,014
$\frac{1}{54}$,013
$\frac{1}{36}$	1,012
$\frac{1}{72}$	1,009
$\frac{1}{84}$	1,007
$\frac{1}{108}$	1,006
$\frac{1}{126}$	1,005
$\frac{1}{144}$	1,004
$\frac{1}{162}$	1,003
$\frac{1}{189}$	1,0029
$\frac{1}{252}$	1,0023
$\frac{1}{320}$	1,0018

$\frac{1}{4 \cdot 4 \cdot 8}$	1,0017
$\frac{1}{3 \cdot 1 \cdot 2}$	1,0014
$\frac{1}{8 \cdot 4 \cdot 8}$	1,0008
$\frac{1}{1 \cdot 6 \cdot 2 \cdot 4}$	1,0006

From this table it will be easy to determine how much the specific gravity of water is increased by the solution of a given quantity of salt, and, *vice versa*, if we know the specific gravity of any solution of salt, we may form a good conjecture of the quantity of salt contained in it, which observation may be of ready use in estimating the strength of brine springs, and of sea water, taken up in different climates, or upon different coasts in the same climate. Thus, if a salt spring, or sea water, should weigh $\frac{1}{3 \cdot 6}$ more, bulk for bulk, than common water; we may conclude that it contains $\frac{1}{3 \cdot 6}$ of its weight of salt; if $\frac{1}{4 \cdot 8}$, it hath nearly $\frac{1}{2 \cdot 8}$; if $\frac{1}{2 \cdot 15}$, $\frac{1}{1 \cdot 8}$; if $\frac{1}{2 \cdot 6}$, $\frac{1}{1 \cdot 4}$; and so on: we may always find limits near enough to form a conclusion from, though the exact number denoting the weight in any particular case should not be met with in the table.

After I had drawn up the preceding account of the experiments which I had made, I received the Berlin Memoirs for 1762, published last year, in which there is a memoire entitled—*Experiences sur le poids du sel et la gravité spécifique des saumures faites et analysées, par M. Lambert*. In this memoire, the very ingenious author hath made much use of the principle, which I have endeavoured to call in question in the beginning of this paper; and hath calculated the different quantities of sea salt, which are absorbed into

into the pores of water, when a given quantity is dissolved in different quantities of water. The admission of this principle hath drawn him into some conclusions which seem not quite consonant to true philosophy; as when he asserts that the quantity which is absorbed into the pores, is not proportional to the number of the pores or the quantity of water: for, if a given quantity of water, suppose A , will absorb a given quantity of any salt, suppose a , I can see no possible reason why $m A$ should not absorb ma : for imagining $m A$ to be divided into portions respectively equal to A , and equal quantities of salt to be dissolved in each of them; then, from the supposition, each of them will absorb a ; and when they are all mixed together, as no precipitation will ensue, the sum, or $m A$, must have absorbed ma . But I have no inclination to animadvert upon what seems to be a small mistake of an author, whose various writings do much honour to philosophy in general, nor to involve myself in a dispute with any one. The following experiment may perhaps be thought conclusive against the doctrine of salts being absorbed into the pores of water: I took a large glass receiver, containing near six gallons; into its neck, by means of a hole bored through a cork, I cemented a small glass tube; and having filled the whole up to the middle of the tube with water, I dropped in a piece of sea salt, weighing less than one forty thousandth part the weight of the water: the water instantly rose in the tube, continued sinking during the solution, but at last remained as much elevated as it would have been had there been no

more water than what would have been sufficient to dissolve it. In making this experiment, the receiver should not be touched by the hand, for its parts suddenly expanding themselves occasion an instantaneous sinking of the water in the tube, as I have frequently experienced, and might thus induce a suspicion of the water's not being elevated by the addition of salt. I would not be understood from these experiments to deny the porosity of water, since philosophers have thought that the passage of light through it, and other phenomena indicate the existence of vacuities in it; but I cannot believe, however solution be carried on, that the smallest quantity of salt can be dissolved in the largest quantity of water, without increasing its magnitude. The cause of the water's sinking during solution doth not appear to be so certain; the escape of air, to which all the appearances induced me to refer it, and to which it may perhaps still be owing, seems to be liable to some objections, not only from the experiments I have before mentioned, but from the following.

EXPERIMENT XII.

I took two matraffes of equal dimensions, one filled with common water, the other with boiled water. I poured into them equal quantities of oil of vitriol; in the first there seemed to be an universal precipitation of air, as it were, from every particle of the fluid, which, by little and little, formed itself into larger bubbles, and ascending through
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the neck, escaped; in the other, hardly any air could be observed, the water sunk during the solution of the acid very apparently, yet $\frac{1}{1000}$ th part of the water's weight of acid caused a sensible elevation: so that, whatever may be thought of the cause of the water's sinking during the solution of a salt, the principle of its being to a certain degree imbibed into the pores of water seems in no case to be true, whether the salt be in a concrete or fluid form. This subject may receive some illustration from what is observed in the freezing of water; ice from common water is always specifically lighter than water, from its retaining in its concrete form several air-bubbles, which enlarge its bulk without adding to its weight; this ice, when put into a matrafs, after the manner in which all the preceding experiments with salts were made, would elevate the water most upon the first immersion: the water would sink as the ice melted; equal portions of ice would produce equal elevations both before and after solution; the air would be separated in a form more or less visible, according to the circumstances in which the experiment should be tried; and not the smallest portion of ice could be dissolved without increasing the bulk of the whole. Salts do not seem to differ much from ice in the manner of their formation, and as similar phænomena attend their solution in water, why may we not explain them from the same cause? But if any one should think differently, notwithstanding the experiments which have been produced, I profess myself extremely ready to listen to any reasoning founded upon experiment which

may tend to prove my opinion to be erroneous; having no partiality for any thing but truth, nor being ashamed of ignorance or mistake in any matter, respecting the comprehension or explication of even the minutest operation of nature: *ego quidem hoc sum contentus, quod licet quo quidque fiat ignorem, quid fiat intelligo.*