XXIII. *A Dissertation on the Nature of Evaporation and several Phænomena of Air, Water, and boiling Liquors: In a Letter to the Rev.* Charles Dodgson, *D. D. F. R. S. from the Rev.* Hugh Hamilton, *D. D. F. R. S. Professor of Natural Philosophy in the University of* Dublin.

Dear Sir;

Read May 16, 1765.

I Here send you, according to promise, my thoughts on the nature of evaporation, the ascent of watery vapours, and some other phænomena of the atmosphere, in explaining which I have employed a principle that, as far as I can find, is different from what has been hitherto used on this occasion, hoping thereby to avoid those objections, which some late writers have made to the former accounts, that have been given us of these phænomena. For in all the accounts I have met with, fire or heat, and rarefaction, by which watery vapours are supposed to become specifically lighter than air, are made to be the principal, if not, the only causes of their ascent into the atmosphere. Doctor Nieuwentyt, and some others, supposed that the particles of fire, by adhering to those of water, make up moleculæ, or small bodies, specifically lighter than air. And Dr. Halley thought, that by the action of heat, the particles of water are formed into hollow spherules filled with a finer air highly rarefied, so as to become specifically lighter than the external air. This last was

was the opinion moſt commonly received, as Doctor Deſaguliers tells us in his diſſertation on this ſubject, publiſhed, in the Philoſophical Tranſactions, in the year 1729, in which he examines and refutes theſe two former opinions, and endeavours to eſtabliſh his own. He aſcribes the aſcent of aqueous vapours to their being turned into an elaſtic ſteam, and always rarefied more than the air is, by the degrees of heat, to which bodies are uſually ſubject in the different ſeaſons of the year.

This opinion, I find, has been as ill received by ſubſequent writers as the former ones. Mr. Clare, in his Treatiſe on the Motion of Fluids, has brought many objections againſt it; as Mr. Rowning has alſo done in his Syſtem of Natural Philoſophy not long ſince publiſhed; who ſays, that the cauſe of the aſcent of vapours has been much diſputed, but not yet ſettled, by philoſophers, and owns that he cannot think of any true principle of philoſophy, upon which it may be accounted for.

I ſhall not now repeat the objections made by thoſe gentlemen; but muſt beg leave to add only the two following; which, among many others that might ſtill be urged, they have not taken notice of.

Firſt; If heat was the only cauſe of evaporation, water in a cloſe warm room would evaporate faſter than when expoſed in a colder place where there is a conſtant current of air; which is contrary to experience.

Secondly; The evaporation of water is ſo far from depending upon its being rarefied by heat, that it is carried on even whilſt water is condenſed by the coldneſs of the air; for water is gradually condenſed by cold till the moment it freezes, and ſince it evaporates even when frozen into hard ice, it muſt alſo evaporate in all the leſſer degrees of cold. Now Mr. Boyle having counterpoiſed a piece of ice in a ſcale, hung it out in a froſty night, and found next morning that it had loſt conſiderably of its weight by evaporation. "Who (ſays he) would have thought that ſo extremely hard and cold a body would evaporate ſo faſt in the clear air of a freezing night?" And ſince that time others have obſerved the ſame thing; which fact ſeems to be an unanſwerable objection to all the accounts in which rarefaction by heat is made to be the chief, if not the only cauſe of evaporation; and, therefore, we muſt have recourſe to ſome other principle to aſſiſt us in accounting for this phænomenon.

As the author of nature does not employ in his works a greater variety of cauſes than is abſolutely neceſſary; it is the buſineſs of natural philoſophy to reduce as many phænomena as may be to ſome general well known cauſe; and this is to be done by comparing the phænomena together in their ſeveral circumſtances, in which, if they are found to agree, they are then to be conſidered as effects of the ſame kind, and aſcribed to the ſame cauſe. By which means the cauſes, whoſe exiſtence is already proved, will be rendered more general, and our knowledge more extenſive. Now as the ſuſpenſion of the particles of water in air, of ſalt in the waters of the ocean, and of other heavy bodies in the fluids that diſſolve them, ſeem to be phænomena of the ſame kind, we might reaſonably ſuppoſe that they ariſe from the

the ſame cauſe; and that what we call evaporation is no more than a gradual ſolution of water in air. But that I may not propoſe this meerly as an hypotheſis, I ſhall endeavour to prove the truth of it, by conſidering the nature of ſolution in general, and comparing its properties and effects with thoſe of evaporation. By ſolution we underſtand, *The uniting ſo intimately the particles of a body with thoſe of a fluid, that the whole ſhall appear an homogeneous maſs, as tranſparent as the fluid was before ſuch union, and ſhall ſo continue till ſome external cauſe produces a change.* The nature of ſolution has been explained by the writers on chymiſtry in this manner. When the particles of any body ſurrounded by a fluid are leſs ſtrongly attracted by each other than by the fluid, they ſeparate from each other, and join themſelves to thoſe of the fluid, and remain ſuſpended therein.

Thus various ſalts are diſſolved in water, eſſential oyls are diſſolved in ſpirits of wine, gold in aqua regia, mercury, ſilver, and other metals, in other acid ſpirits.

And indeed it ſeems to be with great appearance of reaſon, that the attraction between the minute particles of bodies (of which we have ſo many other inſtances) is aſſigned as the cauſe of that union between them, which we experience in ſolutions. The chief properties of which I ſhall now mention, ſo far as may be neceſſary for the purpoſe to which I mean to apply them.

In moſt caſes a diſſolving fluid, or menſtruum, as the chymiſts call it, will diſſolve or take up only a certain proportion of the body immerſed; and if then any more of the ſame body be added, it will

precipitate

precipitate or fall to the bottom, and then the fluid is ſaid to be ſaturated with the body it has diſſolved; yet a fluid, which is ſaturated with one bódy, may afterwards diſſolve others of different kinds, and keep all their particles ſuſpended together.

When any menſtruum has entirely diſſolved a body, it will continue as tranſparent as it was before. The cauſe of which may be aſſigned from what Sir Iſaac Newton diſcovered by experiments; that the particles of bodies muſt be of a certain ſize, or bigneſs, to cauſe any reflection or refraction of the rays of light at their ſurfaces. From whence he gives the reaſon why ſome bodies are opake, and others tranſparent; and he alſo obſerves, that the moſt opake bodies, ſuch as metals, being diſſolved in an acid menſtruum, and thereby reduced to their ultimate and ſmalleſt particles, do not take away the tranſparency of the menſtruum.

Hence we may always know how to diſtinguiſh a ſolution from a mixture; for if a body be reduced to powder, and thrown into a fluid that will diſſolve it, and they are then ſhaken ſuddenly together, the fluid will continue ſomewhat opake till the ſolution be effected, or till what remains undiſſolved falls to the bottom. For in this caſe the particles are not at firſt reduced to their ſmalleſt ſize, as they are always in a ſolution. I think, therefore, we may conſider the tranſparency of an heterogeneous fluid (or one that contains in it particles of another body) as the criterion of a true ſolution, and where that is wanting, it is only a mixture, as when water and air appear together in froth, or in a cloud, or a thick miſt, it is only a mixture of thoſe bodies, and not a ſolution of either.

Thus much being premiſed of the nature of ſolutions in general; I proceed to the proof of what I propoſed; and in order to this, I ſhall firſt ſhew, that there is a mutual attraction between water and air, the ſame that we obſerve between the particles of any two bodies, one of which diſſolves the other. I ſhall then compare, in ſeveral inſtances, the properties and effects of common ſolutions, with thoſe of evaporation, that, from the exact reſemblance between theſe two phænomena, it may appear that they are natural operations, or effects of a like kind, and therefore to be explained upon the ſame principle, or aſcribed to the ſame cauſe. From thence I ſhall ſhew how the aſcent of vapours, and ſeveral other phænomena of the atmoſphere, may be accounted for. And laſtly, I ſhall add ſomething on the riſing of ſteam from boiling liquors, and ſhew wherein it differs from common evaporation.

I am firſt to prove, that there is really an attractive force between the particles of air and water. It is well known that all waters contain a conſiderable quantity of air that retains its elaſticity, by means of which it may be ſeparated from the water by boiling, and including it in an exhauſted receiver. And Dr. Boerhaave, in his Elements of Chymiſtry, has ſhewn, by an elegant experiment, that air extricated from water by boiling, and reſtored to its common ſtate, will occupy a ſpace greater than that poſſeſſed by the water in which it was contained. Now ſince it is allowed, that the particles of ſo heavy a body as gold are ſuſpended in aqua regia by their attraction towards the particles of that fluid, it ſeems reaſonable to ſuppoſe, that ſo light and elaſtic a body as air muſt be retained under water by a like force, without which it would always aſcend to the ſurface, and eſcape. But that there is really ſuch an attractive force between air and water, the learned and ingenious gentleman laſt mentioned has fully proved by the following experiment.

Let an oil flaſk be filled almoſt full with water, deprived of its air as much as may be, let the mouth of it be then ſtopped until the neck be immerſed in a veſſel of water, a bubble of air will then aſcend into the upper part of the flaſk. When things have ſtood in this way for ſome days, the water will be found to have abſorbed the whole bubble of air (if it was not too large) and intirely filled the flaſk; but if the bubble was too large, part of it will be left, for the water after ſome time will abſorb no more air, being then ſufficiently ſaturated with it. It is obſervable that a part of the included air enters pretty quickly into the water at firſt, but what remains afterwards makes its way in but very ſlowly. This experiment ſhews that water, when deprived of its air, will again draw the air gradually into its pores, juſt in the ſame manner that a lump of dry ſugar will draw up water into its pores, which will aſcend pretty quickly at firſt, but very ſlowly after ſome time. We have reaſon, therefore, to conclude that there is the ſame kind of attraction between air and water, that there is between water and any dry porous body that will imbibe it.

As water contains a conſiderable quantity of air, ſo does air contain a good deal of water, even when we think it quite pure and dry, as appears from the moiſture drawn from it by dry ſalt of tartar, in ſuch quantity as to make the ſalt become intirely fluid.

Now

Now ſince the air is an heterogeneous fluid, containing in it particles of another body, and yet retaining a perfect tranſparency, which is the criterion of a true ſolution in other caſes, why ſhould we not infer from analogy, that in this caſe alſo it indicates a true ſolution of water in air? eſpecially when we conſider that there are hardly any two fluids that may not, by themſelves, or by the means of ſome third body, be ſo thoroughly incorporated, that one of them may be properly ſaid to be diſſolved in the other. But the truth of this will be further confirmed, by comparing the properties of common ſolutions with thoſe of evaporation; which I ſhall now do in ſeveral inſtances.

Firſt; when a body is immerſed in a fluid that diſſolves it; for inſtance, a lump of ſalt in water; we ſee the ſalt ſoon begin to diſſolve and impregnate with its particles the water that ſurrounds it, which will then appear thick and loaded; and if the water be at reſt, the ſolution will proceed very ſlowly; but if it be ſtirred about, the ſalt will ſoon be intirely diſſolved. How exactly does this correſpond with what Dr. Halley remarked in an experiment made on the evaporation of water in a cloſe room? Phil. Tranſ. N° 192. " The ſame obſervations (ſays he) do " likewiſe ſhew an odd quality in the vapours of wa- " ter, which is that of adhering to the ſurface that " exhales them, which they clothe, as it were, " with a fleece of vapourous air, which once in- " veſting it, the vapour riſes afterwards in much leſs " quantity." Here we ſee, that the air, which lay at reſt over the water, appeared thick, and loaded with aqueous particles; and then the evaporation

 proceeded

proceeded very ſlowly, juſt as the water that lies about the ſalt appears thick and loaded, and while it continues at reſt, the ſalt is diſſolved very ſlowly. He alſo obſerves on the ſame occaſion that evaporation is vaſtly promoted by a current of freſh air paſſing over the exhaling ſurface; and this I have no doubt happens for the ſame reaſon that ſolution is greatly promoted by agitation, which continually brings freſh particles of the fluid into contact with the body, it diſſolves, in the place of thoſe that have been already ſaturated.

Secondly; into a glaſs of clear cold water throw a lump of any kind of ſalt which is ſoluble in it, and when it has ſtood a little time, ſhake the glaſs, or ſtir the water gently with a wire, and the water which is ſaturated with the ſalt will riſe up among the reſt of the water in curled wreaths, or long ſtriæ, which will render the water ſomewhat opake, cauſing it to refract in different directions the light of an object ſeen through it, and it will make the object appear to have a tremulous motion, and this will continue until all parts of the water are equally impregnated with the ſalt, and then its tranſparency will be reſtored. As the parts of the water, which are impregnated with the ſalt, are of different denſities from the reſt, while they are mixing together they muſt occaſion thoſe refractions, and this apparent tremulous motion, which will ceaſe as ſoon as all the water becomes of the ſame denſity. The very ſame appearances will attend the mixing together of any two fluids of different denſities, and wich will thoroughly incorporate with each other.

In

In like manner when ſmoak or ſteam iſſuing from the pipe of a boiling veſſel firſt riſes into the air, it appears in curled wreaths, and renders the air opake, but as ſoon as it is intirely diſperſed, the tranſparency is reſtored. Thus alſo in a calm hot ſun-ſhine day, when we look along a moiſt piece of ground, the air, and any object ſeen through it, appears to have a tremulous motion like that which we obſerve in an object ſeen through any two fluids that are mixing together.

Now as the vapours riſe here in great abundance, and the air has but little motion, thoſe parts of it that are much impregnated with the aqueous particles are mixed gradually with the air that is drier and of a different denſity, which will occaſion refractions of the light, and that apparent tremulous motion juſt now mentioned. And in this caſe the ſolution of water in air (if I may yet call it ſo) is carried on in a manner viſible to the eye, as it is in other fluids. The ſame tremulous undulating motion is more obſervable when we look in warm weather through a teleſcope, which magnifies the vapours floating in the air; and from this kind of refraction the twinkling of the ſtars ſeems to ariſe, with this difference only, that the watery refracting particles in the day time are paſſing into a ſtate of ſolution, whereas the vapours already diſſolved are, by the cold of the night, beginning to precipitate and return into particles large enough to cauſe refractions in the light of the ſtars.

Thirdly; Heat promotes, and cold in ſome meaſure ſtops, or checks, both ſolution and evaporation; very hot water will diſſolve ſalt ſooner, and in a greater

 quantity

quantity than cold water; and if a ſtrong ſolution of ſalt be made in hot water, the water, when cold, will let go ſome of the ſalt before diſſolved, which will fall to the bottom in ſmall particles, or ſhoot into cryſtals. Juſt ſo will water evaporate faſter in warm than in cold air; and the aqueous vapours, ſuſpended in the air during the heat of the day, fall down at night, and form themſelves into drops of *dew*; or, if the night be very cold, appear next morning in a *hoar-froſt*. And thus, if in a hot day, a bottle be filled with any very cold liquor, and expoſed to the air, which to us ſeems very dry, a dew will be ſoon formed on the outſide of the bottle; for the air about it, becoming cold, will let go part of its moiſture, which will be attracted to the ſurface of the glaſs. And, for the ſame reaſon, a dew is formed on the inſide of the windows of a warm room, which on their outſide are expoſed to the cold air. And hence we may obſerve, that, as there cannot be ſo continual and copious an evaporation in cold weather, the air will then be generally clearer than it is in hot weather.

Heat ſeems to promote ſolution, becauſe it expands bodies, and thereby enlarges their pores, and leſſens the coheſive attraction of their particles, ſo that a body when hot will more eaſily admit a diſſolving fluid into its pores, and its particles not cohering together ſo ſtrongly, as when cold, will more readily quit each other, and unite themſelves to the particles of the fluid by which they are attracted; and for the ſame reaſon heat will alſo promote the evaporation of fluids.

But

But fourthly; The quantity of a body diſſolved, and of a fluid evaporated, in a given time, depends (ceteris paribus) on its quantity of ſurface. Thus a body reduced to powder is ſooner diſſolved than when it is in a ſolid form. And thus ſmoak, or ſteam, which is water, reduced to very ſmall particles by heat, is much ſooner diſperſed, and incorporated with air, than water in its uſual form.

Fifthly; Chymiſts obſerve, that when ſea ſalt, ſal ammoniac, or nitre, is diſſolved in water, or eſſential oyls in ſpirit of wine, ſome degree of cold is produced in the immediate act of ſolution; and the quicker the ſolution, the greater is the cold. And by diſſolving pounded ice, or rather ſnow, (whoſe particles have a greater ſurface) in ſpirit of nitre, a degree of cold has been produced ſo great as to freeze quick-ſilver. Cold is likewiſe produced in the act of evaporation; for if ſpirit of wine, or æther, having the ſame temperature with the air, be rubbed lightly with a feather over the ball of a thermometer, it will ſink as the ſpirits evaporate, and the quicker they evaporate, the faſter will the thermometer ſink. And the ſame thing will happen if water be uſed inſtead of ſpirits, provided its evaporation be promoted by a ſtrong current of air. And thus have I ſeen ice produced meerly by repeated evaporations of æther.

This laſt obſervation ſhews a very remarkable agreement between the natures of ſolution and evaporation. How the cold is produced in either caſe I cannot pretend to ſay; but I muſt beg leave juſt to apply this fact to account for a thing which I believe moſt people have taken notice of.

If

If we rub Hungary water, or any other volatile ſpirit, over our hand, it will feel much colder than water, though they be both of the ſame temperature, and will both feel equally cold if we dip our finger into each. The reaſon of which is, that the ſpirit evaporating much quicker than the water, produces thereby a greater degree of cold; and ſo æther, if it be applied in the ſame way, will feel colder than any other ſpirit on account of its more ſudden evaporation.

Sixthly; It is known that rectified ſpirit of wine, when well purged of air, will imbibe a large bubble of air, in a much ſhorter time, than water will do; and I have myſelf experienced the truth of this, which ſhews that there is a ſtronger attraction or affinity, as the chymiſts call it, between ſpirit of wine and air, than between water and air; and ſince the ſpirit evaporates much faſter than the water, I think we may conclude from hence, that the evaporation of fluids ariſes from an attractive force between their particles and thoſe of air *.

* As water and ſpirit of wine, are in no degree viſcid, they may evaporate in proportion to the attraction between them and air. But the caſe is very different in ſuch fluids as are viſcid; for though I found that oil of olives, when purged of air, will imbibe a bubble of air almoſt as ſoon as water does, yet the evaporation of the oil is hardly (if at all) ſenſible in a very long time. The reaſon of which muſt be, that the attraction between air and the oil, is not able to overcome the tenacity of its particles, and ſeparate them from each other, though it is ſufficient to draw into the oil particles of air, which have no attraction to each other, juſt as a ſponge draws in water, without having its particles ſeparated by the attraction of the water.

Seventhly;

Seventhly; If into any menſtruum we throw a body, which it diſſolves, and afterwards add another, to which the menſtruum has a greater affinity than it has to the firſt, it will diſſolve the ſecond body, and let go the firſt, which will be precipitated and fall to the bottom. In like manner, if to well rectified ſpirit of wine, we add an equal quantity of clear rain, or river water, theſe fluids (which incorporate ſo readily) having a greater affinity to each other than to the air they contain, will let go a great part of the air, which will riſe to the top, or ſtick in ſmall bubbles to the bottom and ſides of the veſſel; from whence I infer, that air is contained in theſe fluids in the ſame manner that the particles of a body are contained in a menſtruum that diſſolves it; and, therefore, that the air imbibed by theſe fluids, is properly ſpeaking, diſſolved in them, and conſequently that any fluid which evaporates, or is imbibed by the air, is alſo, properly ſpeaking, diſſolved in air. And upon this principle we may ſay, that water is drawn out of the air, by dry ſalt of tartar, from its having a greater affinity to that ſalt than to the air.

I ſhould not have been ſo tedious in comparing together the natures of ſolution and evaporation, in ſo many inſtances, but that it gave me an opportunity, at the ſame time, of explaining ſome of the phænomena that I at firſt intended to conſider; which explanations, I believe, will be admitted, if I am right in the mean point, I have endeavoured to prove. And really when we conſider how exactly ſolution and evaporation agree in their ſeveral appearances, properties, and effects, I think we may be convinced that they are natural operations of the

the ſame kind, and that what we call evaporation is nothing more than a gradual ſolution of water in air, produced and promoted by the ſame means (to wit) attraction, heat, and motion, by which other ſolutions are effected.

I ſhall now endeavour to account for ſeveral phænomena of the atmoſphere upon this principle, which will be ſtill further confirmed, if it be found to anſwer the purpoſe to which it is applied.

The loweſt part of the air being preſſed by the weight of the atmoſphere againſt the ſurface of the water, and continually rubbing upon it by its motion, has thereby an opportunity of attracting and diſſolving thoſe particles with which it is in contact, and ſeparating them from the reſt of the water. And ſince the cauſe of ſolution, in this caſe, is the ſtronger attraction of the particles of water towards air, than towards each other, thoſe that are already diſſolved, and taken up, will be ſtill further raiſed by the attraction of the dry air that lies over them, and thus will diffuſe themſelves, riſing gradually higher and higher, and thereby leave the loweſt air not ſo much ſaturated, but that it will ſtill be able to diſſolve, and take up freſh particles of water. And thus ice, or ſnow, will evaporate as well as water, its particles being attracted and diſſolved by the air, which is ſtrongly preſſed againſt its ſurface, for though heat promotes both ſolution and evaporation, yet we do not find that in either caſe any ſenſible degree of it is abſolutely neceſſary *.

* Water, by freezing, is deprived of its air, which we ſee gathered into bubbles through the ice, therefore the ſubſtance of the ice, being deprived of air, will attract the external air

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In this manner will aqueous vapours aſcend ſlowly into the atmoſphere, even when we ſuppoſe the air almoſt at reſt, for I believe it is never perfectly ſo: but the ſolution of water in air, and the aſcent of vapours, is greatly promoted by the motion of the winds, which bring freſh and drier air into the place of that which may be already ſaturated and loaded with moiſture, carrying it together with its moiſture into the higher parts of the atmoſphere, and diſperſing it into all quarters. If we ſhould now ſuppoſe the atmoſphere to remain always of the ſame temperature as to heat and cold, and to have always the ſame denſity; when it was once ſaturated with water, all evaporation would ceaſe, and the vapours already raiſed would always remain ſuſpended; for a fluid, while it continues of the ſame temperature and denſity, will never let go the particles of a body that it has diſſolved. We muſt, therefore, conſider what are the cauſes which occaſion the air ſometimes to part with the water it has diſſolved, and which thereby keep up a continual circulation of vapours. And theſe I ſhall ſhew to be the frequent viciſſitudes of heat and cold, condenſation, and rarefaction, to which the atmoſphere is ſubject.

As to the effects of heat and cold, I have already ſhewn that the former promotes, and the latter checks, or in ſome meaſure hinders evaporation, as well as

more ſtrongly than common water does, which is ſaturated with air. And, on this account, I ſhould think it probable that ice, notwithſtanding its hardneſs, will evaporate as faſt as common water.

other ſolutions; of which I gave an inſtance in the vapours that are ſuſpended in the heat of the day, and by the cold of the night are precipitated, and ſuffered to coaleſce into drops of dew. From the ſnow that lies ſo long on the tops of mountains, and from the experience of thoſe who have paſſed over them, we find that the higher parts of the atmoſphere are much colder than the lower. Now, though vapours are firſt raiſed, and abound moſt in the lower parts of the atmoſphere, yet they cannot there form themſelves into clouds, becauſe the heat that helped to diſſolve them helps alſo to keep them diſſolved. But when they are carried by the winds into the higher parts, where the ſame heat is wanting, the cold air will not be able to keep diſſolved all that are carried up, but muſt ſuffer ſome of them to coaleſce into ſmall particles, which ſlightly attracting each other, and being intermixed with air, will form *clouds*, having the very ſame appearance with ſteam, or ſmoak, which alſo conſiſts of ſmall particles of water, mixed with air, and not yet diſſolved in it. Theſe clouds, when firſt formed, will remain ſuſpended, though they conſiſt of water as well as air, becauſe the weight of their particles will not be able to overcome the reſiſtance they muſt meet with in deſcending through the air. For when bodies are diminiſhed, their quantities of matter, to which their weights are proportional, decreaſe faſter, or in a greater ratio, than their ſurfaces, to which the reſiſtance they meet with is proportional; and, therefore, in very ſmall particles, this reſiſtance may become greater than their weight. The different heights at which clouds are formed, depend on the quantity

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tity of vapours carried up, and the degrees of heat in the upper parts of the atmoſphere; for the vapours will always aſcend, till they meet with air ſo cold, or ſo thin, that it is not able to keep diſſolved all that comes up; hence clouds are generally higher in ſummer than in winter. When clouds are much increaſed by a continued addition of vapours, and their particles are driven cloſe together by the force of the winds, they will run into drops heavy enough to fall down in *rain*; ſometimes the clouds are frozen before their particles are gathered into drops, and then ſmall pieces of them, being condenſed and made heavier by the cold, fall down in thin flakes of *ſnow*, which appear to be fragments of a frozen cloud. But if the particles be formed into drops before they are frozen, they fall down in *hail-ſtones*.

When the air is replete with vapours, and a cold breeze ſprings up, which it often does from the ſea, the ſolution of theſe vapours is checked, and clouds are formed in the lower parts of the atmoſphere, and compoſe what we call a *miſt* or *fog*. This generally happens in a cold morning; but when the ſun has been up for ſome time, the warm air again diſſolves thoſe watery particles, and it frequently clears up.

In a hot ſummer's day, the air lying over wet marſhy ground, is copiouſly ſaturated with aqueous vapours; but the air growing cooler after ſun-ſet, will not be able to keep all thoſe vapours diſſolved, but muſt let ſome part of them coaleſce into very ſmall viſible particles, that form thoſe *miſts*, which appear to riſe from marſhy grounds in a ſummer's evening. The vapours near the ground, being more denſe and copious, will be firſt affected by the cold,

 and

and afterwards thoſe that are thinner and higher up, ſo that the miſt will be low at firſt, but will increaſe in height afterwards; but beſides, theſe grounds, and the water they contain, will acquire ſuch a heat from the ſun, that they may retain it for ſome time, and communicate it to the contiguous air, ſo that the vapours may continue to riſe for ſome time after ſunſet, and will become viſible when they get up a little way in the cooler air. Thoſe cold thick morning fogs, I mentioned juſt now, are often attended with a very light ſmall rain; for we then ſee the drops at their firſt formation, and they are ſuch as are generally met with in paſſing over high mountains; ſo that it ſeems the drops of rain are very ſmall when firſt formed in the clouds; but being driven about by the motion of the air, in their deſcent, ſome of them will probably touch each other, and run into a drop of a larger ſize, and the farther they have to fall, the more will their ſize be increaſed before they come to the ground. And, for this reaſon, the drops, which fall from the higher clouds in ſummer, are found to be generally larger than they are in winter, when the clouds are low. It has been likewiſe obſerved, that the drops of rain are remarkably large that fall in thunder ſhowers; of which the reaſon may be, that the lightning burſting from a cloud, and expanding itſelf greatly, will ſuddenly remove the air from its place, which air, therefore, muſt return to its place with great violence, and thereby the watery particles in the clouds will be ſtrongly agitated and daſhed againſt each other, by which means they will form themſelves into larger drops than at other times; or, perhaps it may be ſaid, that when

a cloud

a cloud is filled with lightning, which is the ſame as the electric matter, the watery particles, like other electrified bodies, will repel each other, but being ſuddenly deprived of this repelling matter, will by their mutual attraction come together again with ſome velocity, and, therefore, will run into drops larger than uſual.

When the wind blows from the ſouth, it is generally warm, and comes replete with aqueous vapours, which it has diſſolved; but coming into a colder climate, it cannot there keep the ſame quantity of vapours diſſolved that it did before, and conſequently muſt part with ſome of them, and let them precipitate; and, therefore, ſoutherly winds generally bring us rain. On the other hand, when the wind blows from the north, or any point near it, as it is very cold, it cannot have diſſolved a great deal of aqueous vapours where it came from; and, therefore, coming into a warmer climate, it is ready to diſſolve more; and, on this account, theſe winds, if they continue long, are found to be very dry and parching, and are generally attended with fair weather.

Theſe ſeem to be the principal effects of heat and cold in cauſing the air to diſſolve, aud take up, or let go, and precipitate the aqueous vapours, and in conſequence of which we ſometimes perceive changes of the weather, even when there is no change in the height of the barometer.

But condenſation and rarefaction will alſo have the like effects in promoting the ſolution of water in air, or in cauſing ſome part of what has been diſſolved to return again into water and precipitate. It ſeems reaſonable to ſuppoſe, that denſe air, in which the particles

particles lie very near each other, will be better able to diſſolve and keep ſuſpended a greater quantity of water, than the ſame air when diffuſed thro' a greater ſpace. But that this is really ſo we have an experimental proof. For when a receiver is partly exhauſted, we ſee the rarefied air begin to let go the water it contained, which gathering into ſmall particles appears like ſteam or ſmoak falling to the bottom. In order to prove the ſame thing by another experiment, I took from the air pump a large exhauſted receiver twenty inches long, having at the bottom a braſs plate, with a ſtop-cock in the middle of it; when the ſtop-cock was opened, the external air, ruſhing in violently, and being much rarefied, let go the water it contained, and threw it againſt the other end of the receiver, where it ſtuck on the glaſs, and covered it with a thin dew, which I found to increaſe until the receiver was almoſt full of air.

Theſe experiments prove that air, when rarefied, cannot keep as much water diſſolved as it does in a more condenſed ſtate. Hence we muſt conclude, that when the atmoſphere is much ſaturated with water, and changes from a denſer to a rarer ſtate, the higher and colder parts of it eſpecially, will begin to let go ſome part of the water diſſolved; which will form new clouds, or add to the ſize and number of the particles before formed, and thereby render them more apt to fall down in rain. On the contrary, when the atmoſphere changes from a rarer to a denſer ſtate, it will then be able to ſtop the precipitation of the water, and again diſſolve in the whole, or in part, ſome of thoſe clouds that were formed before, and conſequently will render their

particles

particles leſs apt to run into drops, and fall down in rain. And thus we generally find, by experience, that the rarefied and condenſed ſtates of the atmoſphere are reſpectively attended with rain and fair weather; though this does not happen at all times, for the air, though rarefied, may not then abound much with aqueous vapours, having already parted with a good deal of them; ſo likewiſe, when the air is denſe and heavy, it may then be much loaded with vapours, which will increaſe its weight; and indeed it muſt be ſo after a long continuance of fair weather, ſo that we may then have rain even before the atmoſphere changes to a rarer ſtate.

Upon this principle I think we may account for the changes of the weather, which uſually attend the riſing and falling of the mercury in the barometer, better than by ſaying, that when the air grows rarer and lighter, it cannot by the laws of hydroſtaticks ſo well ſupport the clouds and vapours, and therefore muſt permit them to fall down in drops of rain: for when the air grows rarer, although the clouds will deſcend into a lower and denſer part of it, yet they will be there ſupported; and I do not ſee why their particles ſhould be more apt to run into drops there, than when they were higher up, unleſs they received ſome addition from the water depoſited among them, by the rarefied air, in the manner I have juſt now mentioned. For ſince the air is rarefied gradually, the clouds can deſcend but very ſlowly; and, therefore, their particles will not be ſo much preſſed together by the reſiſtance they meet with in their deſcent, as they generally are by the winds which blow upon them.

When the atmoſphere is much ſaturated with water, and grows colder or rarer than it was before, we ſhall then perceive the lower air begin to part with ſome of the water it contains, which will fall inſenſibly to the ground, or adhere to the walls of houſes, or other bodies expoſed to it, and make them become damp and wet. And if the moiſture ſettles on the ſmooth ſurfaces of cold bodies, ſuch as marble or other ſtones, whoſe pores cannot imbibe it, it will cover them with a kind of dew, and then thoſe bodies are vulgarly ſaid to *ſweat*.

At this time the hygrometer being affected by the moiſture will point to *wet*, and, as we perceive from thence, that the air is diſpoſed to part with the water it contains, we may generally expect rain. But when the air again grows warm or denſe, it will be able again to diſſolve, and take up the water it before depoſited, and the moiſture on the bodies expoſed to it will diſappear, the hygrometer will point to *dry*, and we may then promiſe ourſelves fair weather.

I obſerved before, that, if a bottle be filled with a very cold liquor, and expoſed to the warm air, a dew will ſoon be formed on its ſurface by the moiſture which the cold air depoſites. Now if we ſuppoſe this body ſtill to retain the ſame degree of cold, whilſt the air paſſes over it, the dew on its ſurface will continually increaſe, and run down its ſides in ſmall ſtreams of water. This ſeems to be exactly the caſe of mountains, whoſe tops reach into the colder parts of the atmoſphere; and which, therefore, are themſelves colder than the air is in general. For when the wind blows the lower parts of the atmoſphere (which are the warmeſt and moſt replete with vapours)

pours) againſt the ſides of the mountains, being there ſtopped in its courſe, it muſt neceſſarily aſcend and paſs over their tops. This air, therefore, will be conſiderably cooled in its progreſs up the ſides and over the tops of the mountains, and conſequently muſt let go a great part of the watery vapours it contains; which will be precipitated in dew and moiſture upon the ſurface of the mountain, where it will ſoak into the earthy parts, or inſinuate itſelf into the chinks and crevices of the rocks, where being collected, it will afterwards break out in ſprings and fountains, and become the ſource of rivers, which are known always to take their riſe in mountainous countries; and, on this account, we might have ſmall ſprings and rivers near mountains, although there were neither clouds nor rain. But the moiſture, which the air uſually depoſites on the mountains, muſt be conſiderably increaſed by the clouds, which are driven againſt them, and accumulated by the winds, for their particles being then preſſed together will run into ſmall drops of rain. Beſides, it is well known, that mountains do gather and retain the clouds about them by their attractive force, in conſequence of which we often ſee ſome clouds continue at reſt on the mountains, whilſt the others are carried on gently by the wind; hence it is, that countries, in the neighbourhood of high mountains, are the moſt ſubject to frequent rains.

Thus I have ſhewn how the aſcent of aqueous vapours, and their conſtant circulation, by precipitating again in moiſture, or drops of rain, will ariſe from the diſſolving power of the air, influenced by the viciſſitude of heat and cold, rarefaction and condenſation. For we

 find,

find, by experience, that the heat and cold, which we feel, does not either ſo much rarefy, or ſo much condenſe the air, but that the former may ſtill increaſe, and the latter leſſen the evaporation of fluids. Nor, indeed, does the ſtate of the atmoſphere in general, as to rarity and denſity, depend upon the heat or cold we experience here below. Theſe cauſes, therefore, according as they take place, in different degrees, will occaſion thoſe various ſtates of the atmoſphere, in reſpect to dryneſs or moiſture, which we experience in the ſeveral changes of the weather. To which the winds contribute very much by heating or cooling, condenſing or rarefying, the different parts of the atmoſphere; and, by promoting the ſolution of water in air, as they mix thoſe two fluids together, or when the air is already ſaturated with aqueous vapours, by preſſing together the particles in the clouds, and thereby cauſing them to run into drops. And thus, from the knownproperties of ſolution, we may account in a ſatisfactory manner for the aſcent and circulation of aqueous vapours, and the ſeveral phænomena of the atmoſphere ariſing from thence, which is a great confirmation of the argument brought to prove that evaporation is only a particular ſpecies of ſolution *;

* Some time after this eſſay had been read at a meeting of the Royal Society, the author was informed that the Abbé Nollet (to whoſe works he was then an entire ſtranger) had conſidered evaporation as a kind of ſolution; and, having lately looked into his lectures, he finds the Abbé offers it as a conjecture, that the air may perform the office of a ſolvent and a ſpunge in regard to the bodies it touches; but this he does not prove, nor does he afterwards apply this principle of ſolution, in accounting for ſeveral phænomena that depend on the nature of

and, therefore, that they both proceed from the ſame cauſe, to wit, the attraction that obtains between the minute particles of different bodies, and which is the means of carrying on ſo many other operations of nature. And, indeed, upon this principle, air ſeems better fitted to be a general *ſolvent* than any fluid we know of, becauſe its particles, not attracting each other, are more at liberty to unite themſelves to the minute particles of other bodies, which they do attract, and accordingly we find the atmoſphere contains in it bodies of all kinds.

The particles of volatile ſpirits, which are ſo eaſily, and ſo much rarefied by heat, ſeem to cohere very ſlightly together, and, therefore, may be more readily attracted by the air, and evaporate more quickly than other fluids. The particles of odoriferous bodies ſeem to be ſtrongly attracted by the air, as they are ſo readily diſperſed through it, and *camphor*, which is a light volatile body, may be intirely diſſolved in the air, without leaving any remainder. The air abounds with vitriolic and other acids, as is plain from the ruſting of iron expoſed to it. It abounds alſo with ſulphurous, nitrous, and other inflammable particles, as appears by the frequent meteors kindled in it. In ſhort, the atmoſphere, as

evaporation, but has recourſe to others of a different kind. Now as the Abbé's works have been ſome years publiſhed, it might be ſuſpected that the author of this eſſay had borrowed a hint from thence without acknowledging it, which would have been diſingenuous: he, therefore, thinks himſelf obliged to declare, what is certainly true, that he has not here repreſented any thing as new, which he was conſcious had ever been propoſed by any one before him, even as a conjecture.

Dr. Boerhaave obſerves, may be conſidered as a chaos containing particles of all kinds of matter. And, here it may be obſerved, that the ſeveral fluids that are diſſolved in the air, will probably aſſiſt in diſſolving and taking up the ſubtile effluvia, which are carried off from volatile bodies. The air, we find, is neceſſary for the preſervation of animal life; but when it has paſſed two or three times through the lungs of an animal, it becomes unfit for reſpiration, and an animal incloſed in ſuch air will ſoon expire; whether the air we breathe depoſites in our lungs any kind of matter neceſſary to the ſupport of life, I cannot pretend to judge. But I think we may be ſure that one purpoſe for which air was deſigned, is the carrying off that moiſture, and other perſpirable matter, which conſtantly exhales from the lungs. Now as air loſes nothing of its elaſticity, by paſſing through the lungs, it will ſtill continue fit for ſuch purpoſes, in the animal oeconomy, as may be anſwered by the alternate expanſion and contraction of the lungs in reſpiration. And, therefore, I believe that air is rendered unfit for reſpiration, chiefly by being ſaturated with that moiſture, and other perſpirable matter, which it meets with in the lungs, and thereby loſing its power of diſſolving, and carrying off any more of that kind of matter, which will then continually increaſe and clog the lungs, ſo that an animal incloſed in ſuch air will die, perhaps ſomewhat in the ſame manner, though not ſo quickly as if it had been drowned.

Does it not ſeem probable that, in the conſtant and quick evaporation of moiſture from the lungs, ſome degree of cold may be produced, as it is in other

other evaporations, which, together with the fresh air taken in, may serve to cool the lungs, and the blood passing through them * ?

* As air, even when incorporated with water, does not lose its elasticity, I took it for granted, that it would not become less elastic by passing through the lungs of an animal. But being told, that the contrary opinion was held by some, who supposed that air, having passed through the lungs of an animal, became unfit for respiration by losing its elasticity, I resolved to try how the fact was, by the following experiment. In a receiver eight inches diameter, and twelve inches high, having under it a soft piece of oyled leather, I included a pretty large chicken, and tied the receiver close down to the table; through a hole in the top of the receiver went a glass tube, open at both ends, cemented round the hole with wax, the lower end was immersed in water (tinged blue) which stood in a glass under the receiver. In about an hour after the chicken was included, it grew very much distressed, gaped wide, and breathed with great difficulty, and in half an hour more it seemed almost ready to expire; the inside of the receiver was then covered with moisture, which in some places ran down in drops. Now if the included air had lost any of its elasticity by passing through the lungs of this animal, it could not have pressed so strongly on the water in the glass as it did at first, and then the external air would have pressed through the tube, and appeared coming up through the water in bubbles; but no such thing happened, for as soon as the receiver was tied down, the water in the tube rose about one fifth of an inch above the water in the glass, and so continued during the whole time of the experiment, except that it rose and fell near one tenth of an inch every time the chicken breathed; and these vibrations of the water in the tube, I observed grew slower, and moved through a greater space towards the latter end of the time; which shewed that the chicken then took in more air every time it breathed than it did at first. After things had stood thus above an hour and a half, and those who saw the experiment were convinced that the included air had not lost any of its elasticity, though grown quite unfit for respiration, the animal being ready to expire in it, I thought it unnecessary to confine the chicken any longer, and it soon recovered.

Air is not leſs neceſſary for the ſupport of fire than of animal life, for fire will not long continue to burn without a circulation of freſh air, which I ſuppoſe happens not from its adding any thing to the pabulum of fire (for that ſeems unneceſſary) but rather on this account. The air immediately about a body on fire, is heated and made ſpecifically lighter than the air at ſome diſtance from it. This hot air muſt, therefore, aſcend and carry with it all thoſe minute particles of different kinds, which are thrown off from the burning body, and which otherwiſe would reſt upon its ſurface, and thereby clog and ſtop the ſubtile vibrations of the burning matter, in which the nature of fire partly conſiſts. If, therefore, fire be confined in a cloſe place, where there can be no circulation of freſh air, the air about it being ſoon ſaturated, with the particles ariſing from the burning matter, will not be able to take up any more of them, and, therefore, the fire muſt go out ſmothered as it were in its own aſhes. And hence it is, that fire burns faſter when air is ſtrongly blown upon it; for this air carries off the aſhes as faſt as they are formed on the ſurface of the burning body, and thereby keeps thoſe particles that have juſt taken fire quite free from any thing that can impede or clog their vibratory motion. This air will alſo ſpread the fire quickly through the fuel, by blowing the particles that are already kindled amongſt them that are not; and, perhaps, the motion of the air may promote theſe ſubtile vibrations in the burning matter, by which the fire is propagated through its parts.

Though the particles of fluids, in common evaporation, are raiſed into the atmoſphere, by the attracting and diſſolving power of the air, yet in ſome particular caſes vapours will riſe into the air on another account; for in ſome places the earth often ſends forth hot elaſtic vapours that riſe into the air by means of their elaſticity, and carry with them mineral particles of different kinds. Fermentation generates elaſtic vapours, which expand themſelves into the air. And the particles of water, when ſufficiently heated, acquire a repelling force, which ſeparates them from the ſurface of the water, and throws them upwards into the air. But all theſe vapours ſoon loſe that elaſticity by which they were firſt raiſed, and they are then retained, and kept ſuſpended in the air, by the ſame power that keep ſuſpended all theſe vapours that riſe without any elaſticity in common evaporation. That the particles of ſteam, which riſes from hot water, are endued with a repelling force, appears plainly when water is boiled in a cloſe veſſel; for then the ſteam becomes ſo exceedingly elaſtic, that, unleſs proper caution be uſed, it will burſt the ſtrongeſt veſſel. In this caſe the boiling water, being ſtrongly preſſed by the force of the included ſteam, conceives a much greater heat, than it will do in an open veſſel; for even when water is boiled in the open air, it is ſomewhat hotter when the atmoſphere is heavy, than when it is light, which ſhews that preſſure, upon boiling water, increaſes its heat; the reaſon of which we ſhall ſee preſently.

But the moſt remarkable phænomenon that attends the boiling of water, is thoſe large bubbles which

which continue to riſe from the bottom whilſt even the water boils, and long after all the air is driven out of it. Dr. Boerhaave, in his Elements of Chemiſtry, part II. has proved, by ſeveral arguments, that theſe bubbles do not ariſe from air; and with regard to their production, he ſeems to be of the ſame opinion with Stairs (to whoſe work he refers) that they ariſe from ſome active fires reſiding in the water. Mariotte, whom he alſo mentions on this occaſion, calls theſe bubbles fulminations, and ſuppoſes that they may ariſe from ſome ſaline particles contained in the water, which being heated act in the ſame manner with the aurum fulminans. I find it is a received opinion (but how generally I cannot tell) that theſe bubbles are occaſioned by ſome ſubtile elaſtic fluid, tranſmitted from the fire through the bottom of the veſſel. However, I conceive that the fluid ſo ſubtile as to paſs readily through the bottom of the veſſel, would paſs alſo through the water ſo eaſily as not diſturb it; and, therefore, I have for ſome time ſuſpected that theſe bubbles are formed only by an *elaſtic ſteam*, in the manner I ſhall now deſcribe. The particles on the ſurface of the water long before it boils, will, by means of the repelling force, which the heat introduces amongſt them, riſe in ſteam, and will inſinuate themſelves into the air, which yields eaſily to them; but thoſe particles that are preſſed againſt the bottom, by the weight of the atmoſphere, and of the incumbent water, will require a great degree of heat to render them ſo elaſtic, that they ſhall be able to overcome this reſiſtance, and expand themſelves into a greater ſpace. Now ſince heat expands water, and makes its

its particles repel each other according to its different degrees, we may reaſonably ſuppoſe that theſe particles, from their being in contact with the bottom of the veſſel, may, at length, acquire ſuch a degree of heat as will give them a repelling force ſufficient to overcome the preſſure they ſuſtain, and expand them ſuddenly into thoſe large bubbles that aſcend through the water when it boils violently. I have lately made ſome obſervations and experiments, which ſeem very much to favour this opinion. Theſe bubbles, which aſcend from the bottom, I obſerved always grow leſs before they reach the ſurface, and thoſe ſmall bubbles, which adhere to the bottom, for ſome time before they aſcend, often diſappear intirely before they reach the ſurface; which ſhews, that when the matter they contain, or any part of it, loſes the heat it had at firſt, it is turned into water. When water, that has juſt boiled, is poured into a glaſs, and ſet under the receiver of an air pump, and the air is almoſt drawn out, the water appears to boil more violently than it does on the fire; and the bubbles do not all riſe from the bottom, but break out from other parts of the water, eſpecially towards the middle, where we may ſuppoſe it hotteſt. In this caſe no ſubtile fluid can be ſuppoſed to riſe through the bottom of the veſſel; but the heat, which the water retains, will give its particles an elaſtic force ſufficient to overcome the preſſure of what little air remains in the receiver, and expand them into bubbles; and that theſe bubbles are compoſed of ſteam appears plainly from this experiment, for as ſoon as they begin to aſcend, the receiver is filled with ſteam, which being condenſed

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by the cold, runs plentifully down its ſides in water. After a veſſel of water had boiled till all the air bubbles were driven out of it, I turned upon its mouth a large glaſs that lay in the water; the bubbles that aſcended under the glaſs remained in the upper part of it, and forced out the water it before contained, and then the elaſtic matter in the glaſs overturned it, againſt the ſide of the veſſel, and burſting out aſcended to the top in one large bubble, upon which the ſteam on the ſurface appeared to be much increaſed. Now this ſhews that the matter contained in thoſe bubbles, which is quite tranſparent, being a very rare and homogeneous fluid, appears afterwards like ſteam when it is mixed with the air. But I thought I ſhould make a more deciſive experiment, if I could obſerve the effects of very hot ſteam conveyed under boiling water. Therefore when an æolipile had boiled till the air was all driven out of the water it contained, without taking it off the fire, I immerſed its pipe into a veſſel of water which had juſt been boiled, and immediately the ſteam, which iſſued from the pipe, roſe up in very large bubbles through the water, and made it ſeem to boil violently. Then I held a large glaſs of cold water, ſo that the pipe of the boiling æolipile was immerſed into it. At firſt none of theſe large bubbles appeared; for the ſteam being then condenſed by the cold water, was mixed with it, making a very uncommon noiſe; but as ſoon as the water in the glaſs grew very hot this noiſe ceaſed, and the ſteam being no longer condenſed roſe in large bubbles, and made the water appear to boil with great violence.

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These obſervations and experiments ſeem to diſcover to us fully the nature of thoſe bubbles that aſcend through boiling water. And from hence I think we may learn the reaſon why any fluid, in an open veſſel, will acquire only a certain degree of heat when it boils, and will not grow hotter afterwards; and why different fluids will acquire different degrees of heat in boiling? The parts of the fluid neareſt the bottom grow hot at firſt, and being then expanded and made lighter, they aſcend and change place with the colder and heavier parts *(which occaſions that inteſtine motion we perceive in liquors while they are growing hot)*; and thus the heat of the whole will increaſe, until thoſe particles, that are in contact with the bottom of the veſſel, acquire ſuch a degree of heat as will give them a repelling force able to overcome the weight of the atmoſphere, the weight of the incumbent fluid, and the tenacity of its particles; and then they will be ſuddenly expanded into bubbles of ſteam, and aſcend quickly to the top, without communicating this heat to the ſurrounding fluid: for as theſe bubbles have a degree of heat but little ſuperior to that of the fluid, and juſt ſufficient to keep them expanded, if they were to loſe this heat, by communicating it to the fluid in their aſcent, they would all diſappear before they got to the ſurface; or if the whole fluid was to grow as hot as the bubbles, it would, like them, be all turned into elaſtic ſteam; and, therefore, the fluid itſelf cannot grow hotter than when theſe bubbles began to aſcend.

That theſe bubbles are really hotter than the other parts of the fluid I found by the following experiment.

ment. A tin veſſel, about nine inches in diameter, was ſet on the fire, ſo that the water on one ſide only boiled violently, but the motion ariſing from thence made all the water circulate through the veſſel; and, therefore, all the water (after the veſſel had boiled for ſome time) muſt have acquired the ſame degree of heat. I then held a mercurial thermometer, with Fahrenheit's ſcale, under the water; where it was juſt out of the reach of the bubbles, it roſe there no higher than to 211; but when it was held among the bubbles, where they aſcended thickeſt, it roſe to 212, which is uſually reckoned the mean heat of boiling water, though it ſeems rather to be the heat of the ſteam contained in the bubbles; and, therefore it is ſomewhat greater than the heat which water will bear without being turned into ſteam. The heat of the water was not then ſo great as it ſome times is, for the atmoſphere was then very light, the barometer ſtanding at 29.

From what has been ſaid, it follows, that the degrees of heat neceſſary to raiſe theſe bubbles in any fluid, and make it boil, will be greater as the fluid is more ſtrongly preſſed, and as its particles are more tenacious or viſcid. And this we find is exactly agreeable to experience. For ſpirit of wine, which is a fluid very light, eaſily rarefied, and in no degree viſcid, will boil with a leſs heat than water does. But mercury, whoſe particles are heavier, and oil or pitch, whoſe particles are more viſcid, than thoſe of water, will require a much greater degree of heat to make it boil than water does. And it is known, that water boiled in a cloſe veſſel, where it is ſtrongly preſſed by the confined elaſtic ſteam, will become much

much hotter than when boiled in the open air; ſo that if the cloſe veſſel ſhould burſt, or be opened ſuddenly, I doubt not but the whole body of the water would immediately expand itſelf into ſteam, and fly out of the veſſel with great violence.

I know not whether theſe experiments, and the obſervations founded upon them, will appear new to you as they did to me; ſuch as they are, I ſubmit them to your conſideration; and am,

S I R,

With great reſpect,

Your moſt obedient

Humble ſervant,

Trinity College, Dublin,
March 5, 1765.

Hugh Hamilton.