that of the particles of liquid water with which it is in contact, (the warmer particles of this Fluid, in consequence of their greater specific gravity, taking their places below,) the communication of Heat between the water and the ice is necessarily very slow on that account.

As soon as the upper surface of the ice is covered with snow, (which commonly happens soon after the ice is formed) this is an additional and very powerful obstacle to prevent the escape of the Heat out of the water; and though the most intense cold may reign in the atmosphere, the increase of the thickness of the ice will be very slow.

During this time, the mass of water which remains unfrozen will lose *no part of its Heat;* on the contrary, it will continually be receiving Heat from the ground. This Heat, which is accumulated in the earth during the summer, will not only serve, in some measure, to replace that which is communicated to the atmosphere through the ice, and prevent its being furnished at the expence of the latent Heat of the water in contact with its surface, but when the temperature of the air is not much below that of freezing, this supply of Heat from below will be quite sufficient to replace that which the air carries off; and the thickness of the ice will not increase.

Whenever the temperature of the air is not actually *colder* than freezing water, the Heat which rises from the bottom of the lake will be all employed in melting the ice at its under surface, and diminishing its thickness.

It will indeed frequently happen, when the ice is very thick, and eſpecially when its upper ſurface is covered with deep ſnow, that the melting of the ice at its under ſurface will be going on, when the temperature of the atmoſphere is conſiderably below the freezing point.

As the particles of water which, receiving Heat from the ground at the bottom of the lake, acquire a higher temperature than that of 40°, and being *expanded*, and becoming ſpecifically lighter by this additional Heat, riſe up to the upper ſurface of the fluid water, and give off their ſenſible Heat to the under ſurface of the ice, never return to the bottom, this communication of the Heat which exhales from the earth produces very little motion in the maſs of the water; and this circumſtance is, no doubt, very favourable to the preſervation of the Heat of the water.

When a ſtrong wind prevails, and the ſurface of the water is much agitated, ice is not formed, even though the whole maſs of water ſhould, by a long continuance of cold weather, have been previouſly cooled down to that point to which it is neceſſary that it ſhould be brought, in order that its internal motions may ceaſe, and it may be diſpoſed to congeal; for though the particles at and near the ſurface may no longer have any tendency to deſcend, on being farther cooled, yet, as they have ſo conſiderable a quantity of ſenſible Heat (eight or ten degrees) to diſpoſe of, after their condenſation with cold ceaſes, and as the agitation into which the water is thrown by the wind does not

not permit any particle to remain long enough in contact with the cold air to give off all its Heat at once, there is a continual succession of fresh particles at the surface, all of which give off Heat to the air; but none of them have time to be cooled sufficiently to be disposed to form ice. The water will lose a vast quantity of Heat, and as soon as the wind ceases, if the cold should continue, ice will be formed very rapidly.

But it is not merely the agitation of the water which renders the communication of the Heat very rapid, the agitation of the wind also tends to produce the same effect.

On the return of spring, the snow melting before the sun as he advances and his rays become more powerful, all the Heat which the earth exhales is employed in dissolving the ice at its under surface, while the sun on the other side acts still more powerfully to produce the same effect.

Though ice is transparent, yet it is not perfectly so; and as the light which is stopped in its passage through it cannot fail to generate Heat *when* and *where* it is stopped, or absorbed, it is by no means surprising that snow should be found to melt when exposed in the sun's rays, even when the temperature of the air in the shade is considerably below the point of freezing. Snow exposed to the sun melts long before the even surface of ice begins to be sensibly softened by its beams, and it is not till fome time after all the hills are bare that the ice on the lakes and rivers breaks up.

The rays which penetrate a bank of fnow being often reflected and refracted, defcend deep into it, and the Heat is depofited in a place where it is not expofed to be carried off by the cold air of the atmofphere; but the rays which fall upon the horizontal and fmooth furface of ice, are mostly reflected upwards into the atmofphere; and if any part of them are ftopped at the furface of the ice, the Heat generated by them *there* is inftantaneoufly carried off by the cold air, and a particle of water is no fooner made fluid than it is again frozen.

Hence we fee that the fnow which in cold countries covers the ice that is formed on the furface of frefh water, not only prevents the Heat of the water from being carried off by the air, during the winter, but alfo affifts very powerfully in thawing the ice early in the fpring.

Should the waters of a lake be fo deep, or fo imperfectly tranfparent as to intercept a great proportion of rays of the fun before they reach the bottom, in that cafe, the temperature of the water at the bottom of the lake will be *nearly the fame all the year round*; and in countries where there is *any* froft in winter, and particularly in thofe lakes which lie near high mountains, and are fed by torrents which proceed from *Glaciers*, and melting fnow, this *conftant temperature* at the bottom can never

never be much above or below 41° F. whatever may be the heat to which the *surface* of the lake is exposed in summer, or however long and intensely hot the summer may be

Let us now see what the consequences would have been, had the condensation of water with cold followed the law which obtains in regard to all other Fluids.

As the internal motion of the water could not have failed to continue as long as its specific gravity continued to be increased by parting with Heat, ice would not have begun to be formed till the whole mass of water had arrived at the temperature of 32° of Fahrenheit's thermometer.

To

* In a letter from Professor PICTET of Geneva to the Author, of the 7th July 1797, accompanying the 36th number of the BIBLIOTHEQUE BRITANNIQUE, (in which an account, or rather translation, of the first Edition of this Essay is published in the French language,) there is the following paragraph.

" I took the liberty to throw in, as usual," (in the translation) " some occasional notes; one of which will, I hope, deserve your at-" tention. It points out the near coincidence of the mean tempera-" ture of the bottom, observed in ten different lakes, by M. de Saussure " and myself, viz. $4\frac{1}{3}$° R."—(equal to $41\frac{3}{4}$° F.) " with the tempera-" ture where the *minimum* of volume, or *maximum* of density of water " takes place. We vainly strove to this day to explain the uni-" formity we observed in that particular in several lakes very differ-" ently situated, in many respects, but your reflections seem to me " fully to resolve the problem."

The following is the note in the *Bibliothèque Britannique*, alluded to by Professor PICTET, in the foregoing paragraph of his letter.

" Ce n'est pas seulement dans le lac de Genève que M. de Saussure, " notre savant ami, a fait les expériences curieuses qui sont ici rap-" pellées, et à quelques-unes des quelles nous avons eu le plaisir " d'assister: il les a répétées dans la Méditerranée, et dans dix " lacs qui bordent de part et d'autre la chaine des Alpes. Nous

 " tirons

To see what an enormous quantity of Heat would be lost when the water is deep in consequence of its whole mass being cooled in this manner, we have only to compute how much ice this Heat would melt, or how much water it would heat from the point of freezing to that of boiling.

It has been shown by experiment, that any given quantity of ice requires as much Heat to melt it as an equal quantity of fluid water loses in cooling

" tirons de son grand ouvrage sur les montagnes les températures " observées au fond de ces lacs comme suit ;

" Noms des Lacs.	Profondeurs en pieds de France.	Températures du fond Degrés de Reaumur.
" Lac de Genève -	950 -	4.3
" de Neuchâtel -	325 -	4.1
" de Bienne - -	217 -	5.5
" du Bourget -	240 -	4.5
" d'Annecy -	163 -	4.5.
" de Thun - -	350 -	4.0
" de Brientz -	500 -	3.8
" de Lucerne -	600 -	3.9
" de Constance -	370 -	3.4
" Lac Majeur - -	335 -	5.4

" Température moyenne du fond de dix lacs 4.34, ou $4\frac{1}{3}$° R."

" Il n'est peutêtre aucun de nos lecteurs qui, plein des idées que " notre auteur vient de discuter, ne soit frappé de la coincidence entre " cette température du fond des lacs dans nos latitudes moyennes et " celle à laquelle l'eau atteint son *minimum* de volume ou *maximum* de " densité ! La permanence de cette température, et son identité dans " des lacs d'ailleurs très-diversement situés, paroissent intimément " liées avec cette circonstance du *minimum* de volume. Mais ce " n'est pas ici le lieu de donner cours aux idées que peut suggerer ce " rapprochement ; nous l'indiquons à l'auteur comme un objet digne " de ses méditations."

The Author of this Essay feels himself very much obliged to his ingenious and respectable friend, Professor PICTET, for these interesting observations.

140 degrees; confequently the quantity of ice which might be melted by the Heat given off by any given quantity of water in cooling any given number of degrees, is to the given quantity of water, as the number of degrees which it is cooled, to 140 degrees.

Hence it follows that when the temperature of the water is 8 degrees above the freezing point, it gives off in cooling down to that temperature as much Heat as would melt $\frac{8}{140}$ or $\frac{2}{35}$ of its weight of ice; the water, therefore, which is cooled from the temperature of 40° to that of 32°, if it be 35 feet deep, will give off as much Heat in being fo cooled as would melt a covering of ice 2 feet thick.

But this even is not all; for as the particles of water on being cooled at the furface would, in confequence of the increafe of their fpecific gravity on parting with a portion of their Heat, immediately defcend to the bottom, the greateft part of the Heat accumulated during the fummer in the earth on which the water repofes would be carried off and loft, before the water began to freeze; and when ice was once formed, its thicknefs would increafe with great rapidity, and would continue increafing during the whole winter; and it feems very probable that, in climates which are now temperate, the water in the large lakes would be frozen to fuch a depth in the courfe of a fevere winter that the Heat of the enfuing fummer would not be fufficient to thaw them; and fhould this once happen, the following winter could hardly fail to change the whole mafs of its waters to one folid body of ice, which never more could recover its liquid form, but muft remain immoveable till the end of time.

In the month of February, after a froft which had lafted a month, the temperature of the air being 38°, M. DE SAUSSURE found the temperature of the water of the Lake of Geneva, at the furface, at 41°,—and at the depth of 1000 feet, at 40°. Had the froft continued but a little longer, ice would have been formed; but had the conftitution of water been fuch that the whole mafs of that Fluid in the Lake muft have been cooled down to the temperature of 32° before ice could have been formed, this event could not have happened till the water had given off as much Heat as would be fufficient to melt a covering of ice above 57 feet thick!—

This quantity of Heat would be fufficient to heat, to the point of boiling, a quantity of ice-cold water as large as the Lake, and 49 feet deep.

We cannot fufficiently admire the fimplicity of the contrivance by which all this Heat is faved. It well deferves to be compared with that by which the feafons are produced; and I muft think that every candid enquirer, who will begin by divefting himfelf of all unreafonable prejudices, will agree with me in attributing them both TO THE SAME AUTHOR.

When we trace ftill farther the aftonifhing effects which are produced in the world by the operations of that fimple law which has been found to obtain

in

in the condenſation of water on its being deprived of Heat, we ſhall find more and more reaſon to admire the wiſdom of the contrivance.

That high latitudes might be habitable, it was neceſſary that vegetables ſhould be protected from the effects of the chilling froſts of a long and ſevere winter: but if it be true that watery liquids do not part with their Heat but in conſequence of their internal motions; and if theſe motions are occaſioned merely by the change produced in the ſpecific gravity of thoſe particles of the liquid which receive Heat, or which part with it, who does not ſee how very powerfully the ſudden diminution and final ceſſation of the condenſation of water in cooling, as ſoon as its temperature approaches to the freezing point, operates to prevent the ſap in vegetables from being frozen?

But if, for the purpoſes of life and vegetation, it be neceſſary that the ground, the rivers, the lakes, and the trees be defended from the cold winds from the poles, it may be aſked how this innundation of cold air is to be warmed?—I anſwer by the waters of the ocean, which there is the greateſt reaſon to think were not only deſigned principally for that uſe, but particularly *prepared* for it.

Sea water contains a large proportion of ſalt in ſolution; and we have ſeen that the condenſation of a ſaline ſolution, on its being cooled, follows a law which is extremely different from that obſerved in regard to pure water; and which (as may eaſily be ſhown) renders it peculiarly well adapted for commu-

communicating Heat to the cold winds which blow over its ſurface.

As ſea water continues to be condenſed as it goes on to cool, even after it has paſſed the point at which freſh water freezes, the particles at the ſurface, inſtead of remaining there after the maſs of the water had been cooled to about 40°, and preventing the other warmer particles below from coming in their turns and giving off their Heat to the cold air, (as we have ſeen always happens when freſh or pure water is ſo cooled,) theſe cooled particles of *ſalt water* deſcend as ſoon as they have parted with their Heat, and in moving downward force other warmer particles to move upwards; and in conſequence of this continual ſucceſſion of warm particles which come to the ſurface of the ſea, a vaſt deal of Heat is communicated to the air;—incomparably more than could poſſibly be communicated to it by an equal quantity of freſh water at the ſame temperature, as will appear by the following computation.

Without taking into the account that very great advantage which ſea water poſſeſſes over freſh water, conſidered as an equalizer of the temperature of the atmoſphere, which ariſes from the comparative *lowneſs of the point of its congelation*;—ſuppoſing even ſea water to freeze at as high a temperature as freſh water, namely, at 32°; and ſuppoſing (what is ſtrictly true) that as ſoon as either ſea water or freſh water is frozen at its ſurface, and this ice covered with ſnow, the communication of Heat from the water to the atmoſphere ceaſes

ceaſes almoſt entirely;—we will endeavour to determine how much more Heat would, even on this ſuppoſition, be communicated to the air by ſalt water than by freſh water, after both have arrived at the temperature of 40°.

When freſh water, in cooling, has arrived at this temperature, it ceaſes to be farther condenſed with cold, and its internal motions (which, as we have already more than once obſerved, are cauſed *ſolely by the changes produced in the ſpecific gravity of its* particles) ceaſe, of courſe, and ice immediately begins to be formed on its ſurface; but as the condenſation of ſalt water goes on as its Heat goes on to be diminiſhed, its internal motions will continue; and it is evidently impoſſible for ice to be formed at its ſurface till the whole maſs of the water has become ice-cold, or till its temperature is brought down to 32°. It would therefore give off a quantity of Heat equal to 8 degrees, at leaſt, of Fahrenheit's thermometer, *more than the freſh water* would part with before ice could be formed on its ſurface.

To be able to form an idea of this enormous quantity of Heat, we have only to recollect what has already been ſaid, and we ſhall find reaſon to conclude that it would be ſufficient to melt a covering of ice equal in thickneſs to $\frac{2}{36}$ of the depth of the ſea.—It would therefore be ſufficient in that part of the North Sea (lat. 67°) where Lord Mulgrave ſounded at the depth of 4680 feet, to melt a cake of ice 265 feet thick!

But

But the Heat evolved in the formation of each superficial foot of ice would be sufficient to raise the temperature of a stratum of incumbent air 2220 times as thick as the ice, (consequently in the case in question 265×2220 feet, or 869 miles thick,) 28 degrees, or from the temperature of freezing water, to that of 50° of Fahrenheit's thermometer, or to the mean annual temperature of the northern parts of Germany!

The Heat given off to the air by each superficial foot of water in cooling *one degree* is sufficient to heat an incumbent stratum of air 44 times as thick as the depth of the water, 10 degrees. Hence we see how very powerfully the water of the ocean, which is never frozen over, except in very high latitudes, must contribute to warm the cold air which flows in from the polar regions.

But the ocean is not more useful in moderating the extreme cold of the polar regions, than it is in tempering the excessive heats of the torrid zone;—and what is very remarkable, the fitness of the sea water to serve this last important purpose is owing to the very same cause which renders it so peculiarly well adapted for communicating Heat to the cold atmosphere in high latitudes, namely, *to the salt which it holds in solution.*

As the condensation of salt water with cold continues to go on even long after it has been cooled to the temperature at which fresh water freezes, those particles at the surface which are cooled by an immediate contact with the cold winds must descend, and

and take their places at the bottom of the ſea, where they muſt remain, till, by acquiring an additional quantity of Heat, their ſpecific gravity is again diminiſhed. But this Heat *they never can regain in the polar regions;* for innumerable experiments have proved, beyond all poſſibility of doubt, that there is no *principle of Heat* in the *interior parts of the globe*, which, by exhaling through the bottom of the ocean, could communicate Heat to the water which reſts upon it.

It has been found that the temperature of the earth at great depth under the ſurface is different in different latitudes, and there is no doubt but this is alſo the caſe with reſpect to the temperature at the bottom of the ſea, in as far as it is not influenced by the currents which flow over it; and this proves to a demonſtration that the Heat which we find to exiſt, without any ſenſible change during ſummer and winter, at great depths, is owing to the action of the ſun, and not to *central fires*, as ſome have too haſtily concluded.

But if the water of the ocean, which, on being deprived of a great part of its Heat by cold winds, deſcends to the bottom of the ſea, cannot be warmed *where it deſcends*, as its ſpecific gravity is greater than that of water at the ſame depth in warmer latitudes, it will immediately begin to ſpread on the bottom of the ſea, and to flow towards the equator, and this muſt neceſſarily produce a current at the ſurface in an oppoſite direction; and there are the moſt indubitable proofs of the exiſtence of both theſe currents.

The

The proof of the exiſtence of one of them would indeed have been quite ſufficient to have proved the exiſtence of both, for one of them could not poſſibly exiſt without the other; but there are ſeveral direct proofs of the exiſtence of each of them.

What has been called the gulph ſtream, in the Atlantic Ocean, is no other than one of theſe currents, that at the ſurface, which moves from the equator towards the north pole, modified by the trade winds and by the form of the continent of North America; and the progreſs of the lower current may be conſidered as proved directly by the cold which has been found to exiſt in the ſea at great depths in warm latitudes;—a degree of temperature much below the mean annual temperature of the earth in the latitudes where it has been found, and which of courſe muſt have been *brought from colder latitudes.*

The mean annual temperature in the latitude of 67° has been determined by Mr. KIRWAN, in his excellent treatiſe on the temperature of different latitudes, to be 39°; but Lord Mulgrave found on the 20th of June, when the temperature of the air was 48½°, that the temperature of the ſea at the depth of 4680 feet was 6 degrees below freezing, or 26° of Fahrenheit's thermometer.

On the 31ſt of Auguſt, in the latitude of 69°, where the annual temperature is about 38°, the temperature of the ſea at the depth of 4038 feet was 32°; the temperature of the atmoſphere (and probably

probably that of the water at the ſurface of the ſea) being at the ſame time at 59½°.

But a ſtill more ſtriking, and I might, I believe, ſay an incontrovertible proof of the exiſtence of currents of cold water at the bottom of the ſea, ſetting from the poles towards the equator, is the very remarkable difference that has been found to ſubſiſt between the temperature of the ſea at the ſurface and at great depth, at the tropic,— though the temperature of the atmoſphere there is ſo conſtant that the greateſt changes produced in it by the ſeaſons ſeldom amounts to more than five or ſix degrees; yet the difference between the Heat of the water at the ſurface of the ſea, and that at the depth of 3600 feet, has been found to amount to no leſs than 31 degrees; the temperature above or at the ſurface being 84°, and at the given depth below no more than 53°*.

It appears to me to be extremely difficult, if not quite impoſſible, to account for this degree of cold at the bottom of the ſea in the torrid zone, on any other ſuppoſition than that of cold currents from the poles; and the utility of theſe currents in tempering the exceſſive heats of thoſe climates is too evident to require any illuſtration.

Theſe currents are produced, as we have already ſeen, in conſequence of the difference in the ſpecific gravity of the ſea water at different temperatures; their velocities muſt therefore be in proportion to the change produced in the ſpecific gra-

* Phil. Tranſactions, 1752.

vity

vity of water by any given change of temperature; and hence we see how much greater they must be in salt water than they could possibly have been had the ocean been composed of fresh water.

It is not a little remarkable that the water of all great lakes is fresh, and nearly so in all inland seas (like the Baltic) in cold climates, and which communicate with the ocean by narrow channels. We shall find reason to conclude that this did not happen without design, when we consider what consequences would probably ensue should the waters of a large lake in an inland situation, in a cold country, (such as the lake Superior, for instance, in North America,) become as salt as the sea.

Though the cold winds which blow over the lake in the beginning of winter would be more warmed, and the temperature of the air on the side of the lake opposite to the quarter from whence these winds arrive, would be rendered somewhat milder than it now is; yet, as the water of the lake would give off an immense quantity of Heat before a covering of ice could be formed on its surface for its protection, it would, on the return of spring, be found to be *extremely cold*; and as it would require a long time to regain from the influence of the returning sun the enormous quantity of Heat lost during the winter, it would remain very cold during the spring, and probably during the greatest part of the summer; and this could not fail to chill the atmosphere, and check vegetation in the surrounding country to a very considerable distance. And though

though a large lake of salt water in a cold country would tend to render the winter *somewhat milder* on one side of it, namely, on the side opposite to the quarter from whence the cold winds came; yet this advantage would not only be confined to a small tract of country, but would not any where be very important, and would by no means counterbalance the extensive and fatal consequences which would be produced in summer by so large a collection of very cold water.

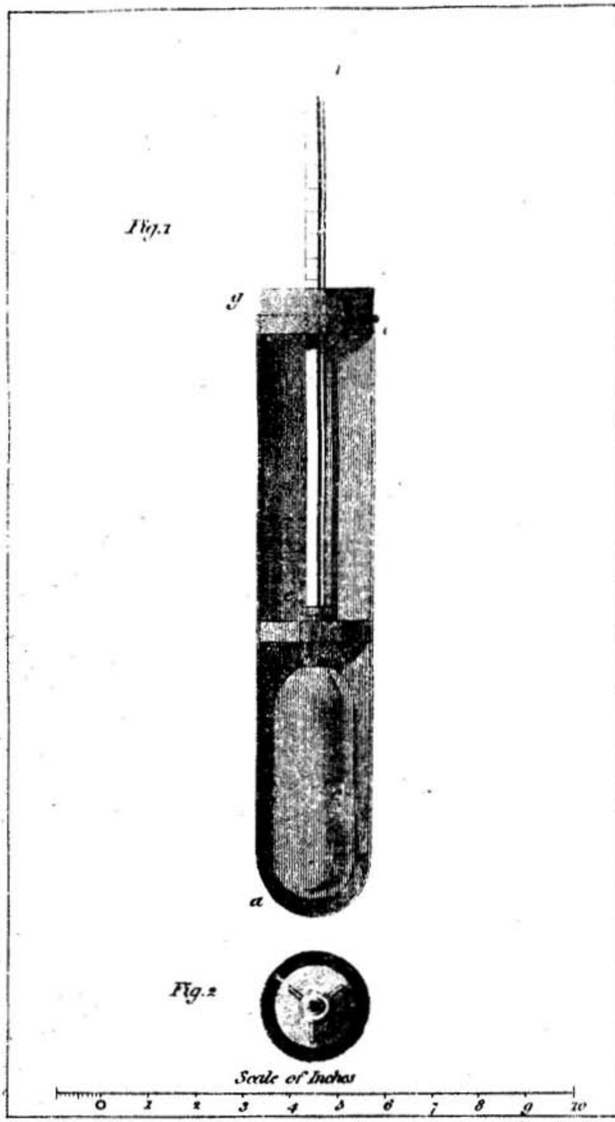
When the winter is once fairly set in,—when the earth is well covered with snow, and the rivers and lakes with ice, and more especially when the ice as well as the land is covered with that warm winter garment, a few degrees more of cold in the air cannot produce any lasting bad consequences. It may oblige the inhabitants to use additional precautions to guard themselves, their domestic animals, and their provisions, from the uncommon severity of the weather; but it can have very little influence in the temperature of the ensuing summer; and even it is probable, if it influences it at all, that it tends rather to make it *warmer* than *colder*. Lakes of salt water could therefore be of no real use *in winter* in cold countries, and in summer they could not fail to be very hurtful; while fresh lakes, as they are frozen over almost as soon as the winter sets in, and long before the whole mass of their water is cooled down to the temperature of freezing, preserve the greater part of their Heat through the winter. and if they are of no uſe during the cold ſeaſon, they probably do little or no harm in ſummer.

But I muſt take care not to tire my reader by purſuing theſe ſpeculations too far. If I have perſiſted in them,—if I have dwelt on them with peculiar ſatisfaction and complacency,—it is becauſe I think them uncommonly intereſting,—and alſo becauſe I conceived that they might be of real uſe in this age of *refinement* and *ſcepticiſm*.

If, among barbarous nations, the *fear of a God*, and the practice of religious duties, tend to ſoften ſavage diſpoſitions, and to prepare the mind for all thoſe ſweet enjoyments which reſult from peace, order, induſtry, and friendly intercourſe,—a *belief in the exiſtence of a Supreme Intelligence*, who rules and governs the univerſe with wiſdom and goodneſs, is not leſs eſſential to the happineſs of thoſe who, by cultivating their mental powers, HAVE LEARNED TO KNOW HOW LITTLE CAN BE KNOWN.

DESCRIP-

Plate 1.



Neele sc. Strand

London Pub.d June 20th 1797 by Cadell & Davies Strand

DESCRIPTION OF THE PLATES.

PLATE I.

THIS Plate repreſents the cylindrical Paſſage Thermometer uſed in the Experiments, on the conducting power of liquids with regard to Heat.

Fig. 1. *a*, *b*, is a ſection of the braſs tube in which the Thermometer *c*, with an oblong copper bulb, is placed.

e, *f*, is the glaſs tube of the Thermometer, which, for want of room in the Plate, is repreſented as broken off at *f*.

g, is a ſtopple of cork by which the end of the braſs tube, *a*, *b*, is cloſed; and

h, is a circular diſk of the ſame ſubſtance.

The ſpace in the braſs tube below this diſk *h*, ſurrounding the bulb of the Thermometer, was occupied by the liquid whoſe conducting power was determined. The ſpace between the diſk and the cork-ſtopper *g*, was filled with eider-down.

Between the inſide of the braſs tube and the lower part of the bulb of the Thermometer are ſeen the wooden pins which ſerved to confine the Thermometer in its place.

Fig. 2. This is an horizontal ſection of the braſs tube, and a bird's-eye view of the Thermometer in its place.

PLATE II.

Fig. 3. This Figure fhows the manner in which the Experiments were made; in which a cake of ice at the bottom of a tall glafs jar was thawed by hot water ftanding on its furface.

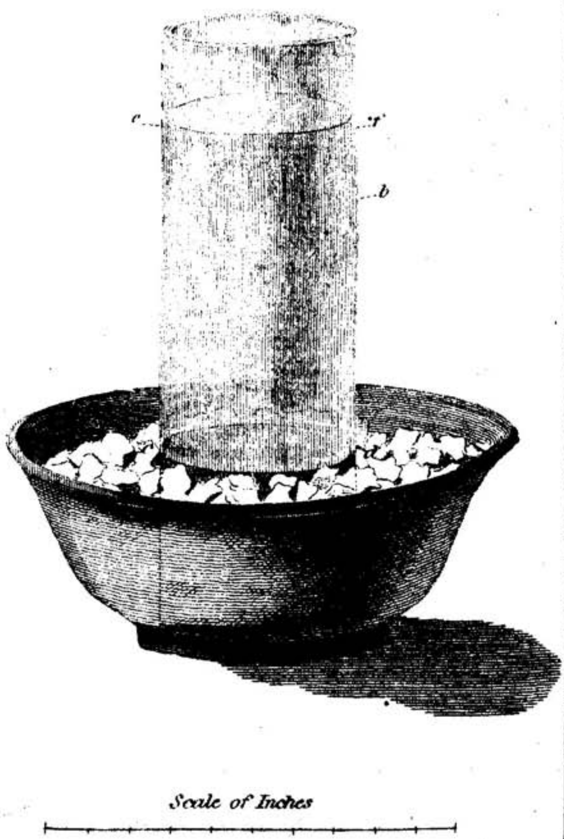
a, is an earthen bowl filled with pounded ice and water, in which the glafs jar, *b*, was placed.

c, *d*, is the level of the upper furface of the ice in the jar.

e, *f*, is the level of the furface of the water ftanding on the ice in the jar.

END OF PART I. OF ESSAY VII.

Fig. 3



Neele sc. Strand.

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CONTENTS

OF

PART II. of ESSAY VII.

PART II.

An Account of ſeveral NEW EXPERIMENTS, with occaſional Remarks and Obſervations, and CONJECTURES reſpecting Chemical Affinity and Solution, and the mechanical Principle of Animal Life.

CHAP. I.

Feeling

Feeling nor the Thermometer can detect.—The Evaporation of Ice during the severest Frost explained on that Supposition.—Probability that the Metals would evaporate when exposed to the Action of the Sun's Rays were they not good Conductors of Heat.—Mercury is actually found to evaporate under the mean Temperature of the Atmosphere.—This Fact is a striking Proof that FLUID MERCURY *is a Non-conductor of Heat.—Probability that the Heat generated by the Rays of Light is always the same in Intensity; and that those Effects which have been attributed to Light ought perhaps in all Cases to be ascribed to the Action of the Heat generated by them—A striking Proof that the most intense Heat does sometimes exist where we should not expect to find it.—Gold actually melted by the Heat which exists in the Air of the Atmosphere, where there is no Appearance of Fire, or of any Thing red-hot.—We ought to be cautious in attributing to the Action of unknown Powers, Effects similar to those produced by the Agency of Heat.—The most intense Heat may exist without leaving any visible Traces of its Existence behind it.—This important Fact illustrated by the necessary Result of an imaginary Experiment.* Page 345

CHAP. IV.

An Account of a Variety of Miscellaneous Experiments.—Thermometers with cylindrical Bulbs may be used to show that Liquids are Non-conductors of Heat.—Ice-cold Water may be heated and made to boil standing on Ice.—Remarkable Appearances attending

ESSAY VII.

PART II.

An Account of ſeveral NEW EXPERIMENTS, with occaſional Remarks and Obſervations, and CONJECTURES reſpecting Chemical Affinity and Solution, and the mechanical Principle of Animal Life.

CHAP. I

*Account of a Circumſtance of a private Nature, by which the Author has been induced to add this and the following Chapters to the Second Edition of this Eſſay.—Experimental Inveſtigation of the Subject continued.—*OIL *found by Experiment to be a Non-conductor of Heat.—*MERCURY *is likewiſe a Non-conductor.—Probability that all* FLUIDS *are* NON-CONDUCTORS, *and that this Property is* ESSENTIAL TO FLUIDITY.—*The Knowledge of that Fact may be of great Uſe in enabling us to form more juſt Ideas with regard to the Nature of thoſe mechanical Operations which take place in chemical Solutions and Combinations; in the Proceſs of Vegetation; and in the various Changes effected by the Powers of Life in the Animal Economy.—Rapidity of Solution no Proof of the Exiſtence of an At-*

traction of Affinity.—Strata of fresh Water and of salt Water may be made to repose on each other in actual Contact without mixing.—Probability that the Water at the Bottom of fresh Lakes, that are very deep, may be actually salt.

At the end of a French translation of the First Edition of this Essay, published at Geneva, Professor Pictet (the translator) has added the following extract of one of my private letters to him, (of the 9th June 1797,) written in answer to one from him to me, acknowledging the receipt of a manuscript copy of the Essay which I had sent him.

" I should have been much surprised if my Se- " venth Essay had not interested you; for in my " life I never felt pleasure equal to that I enjoyed " in making the experiments of which I have given " an account in that performance. You will per- " haps be surprised when I tell you, that I have " suppressed a whole Chapter of interesting specu- " lation, merely with a view of leaving to others a " tempting field of curious investigation *untouched*, " and to give more effect to my concluding reflec- " tion, which I consider as being by far the most " important of any I have ever published."

As these assertions, (which were not originally intended for the public eye,)—are liable to several interpretations, I think it my duty, not only to explain them, but also to let the Public know precisely how far I have pushed my inquiries in the investigation of the subject under consideration: This is an act of justice which I owe to those who may be engaged

engaged in the fame purfuits; for it would be very unfair, by *obfcure hints of important information kept back*, to keep others in doubt with refpect to the originality of the difcoveries they may make in the profecution of *their* inveftigations. This would tend to *damp* the fpirit of inquiry, inftead of *exciting* it; and throwing out fuch hints looks fo much like lying in wait to feize on the fair fruits of the labours of others, that I cannot reft till I have fhewn that I do not deferve to be fufpected of fuch pitiful views.

My worthy friend, Profeffor Pictet, certainly did not fufpect any unhandfome defign in any thing I faid to him in my (private) letter; but thofe who are lefs acquainted with my character, may not be difpofed to give me credit for candour and difintereftednefs without proofs.

With regard to the affertion in my letter, "that "I had fuppreffed a whole Chapter of interefting "fpeculation, with a view to leaving to others a "tempting field, *untouched*, for curious inveftiga-"tion;"—this is perfectly true in fact, as will, I flatter myfelf, appear, by what I fhall now lay before the Public; and I am confident that thofe who will take the trouble to confider with attention the reafons which induced me to do this, will find them fuch as will deferve their approbation.

Having, as I flattered myfelf, laid open a new and moft enticing profpect to thofe who are fond of philofophical purfuits, I was afraid, if I advanced too far, that others, inftead of ftriking out roads for themfelves, might perhaps content themfelves with

 following

following in my footsteps; and consequently that many, and probably the most interesting parts of the new field of enquiry, would remain a long time unexplored: And with regard to the reputation of being a *discoverer*, though I rejoice, I might say, exult and triumph—in the progress of human knowledge,—and enjoy the sweetest delight in contemplating the advantages to mankind which are derived from the introduction of useful improvements; yet I can truly say, that I set no very high value on the honour of being the first to stumble on those treasures which every where lie so slightly covered.

In respect to the "concluding reflection" of the First Edition of this Essay;—though some may smile in pity, and others frown at it, I am neither ashamed nor afraid to own, that I consider the subject as being of the *utmost importance* to the peace, order, and happiness of mankind, *in our present advanced state of society*. But to return from these digressions—

Though it appeared to me that the important fact I undertook to investigate, relative to the *manner* in which Heat is propagated in Fluids, is fully established by the Experiments, of which an account has been given in the preceding Chapters of this Essay; yet, as a thorough examination of the subject is a matter of much importance, in many respects, I did not rest my enquiries here, but made a number of Experiments with a view to throwing still more light upon it, and enabling us to form more clear and distinct ideas respecting those curious

mechanical

mechanical operations which appear to take place in Fluids, when Heat is propagated in them.

Having frequently obſerved when a quantity of water in one of my glaſs jars was frozen to a cake of ice, by placing the jar in a freezing mixture, that, as the ice firſt began to be formed at the ſides of the jar, and increaſed gradually in thickneſs, the portion of water in the axis of the jar (which laſt retained its fluidity) being compreſſed by the expanſion of the ice, was always forced upwards towards the end of the proceſs, and formed a pointed projection of ice in the form of a nipple, (*papilla*,) which was ſometimes above half an inch high in the middle of the upper ſide of the cake; I was led by that circumſtance to make the following intereſting Experiments.

Experiment, No. 55.

A cake of ice, 3 inches thick, which had a pointed projection, ½ an inch high, which aroſe from the centre of its upper ſurface, being frozen faſt in the bottom of a tall cylindrical glaſs jar, 4¾ inches in diameter; this jar, ſtanding in an earthen pan, and being ſurrounded by pounded ice and water, to the height of an inch above the level of the upper ſurface of the cake of ice, was placed on a table, near a window, in a room where the air was at the temperature of 31° of Fahrenheit's thermometer; and fine *olive oil*, which had previouſly been cooled down to the temperature of 32°, was poured into the jar till it ſtood at the height of 2 inches above the ſurface of the cake of ice.

Having ready a ſolid cylinder of wrought iron, $1\frac{1}{4}$ inch in diameter, and 12 inches long, with a ſmall hook at one end of it, by means of which it could occaſionally be ſuſpended in a vertical poſition, and furniſhed with a fit hollow cylindrical ſheath of thick paper, into which it juſt paſſed,— open at both ends, and about $\frac{1}{10}$ of an inch longer than the ſolid cylinder of iron, to which it ſerved as a covering for keeping it warm; this iron cylinder, being heated to the temperature of 210° in boiling water, and being ſuddenly introduced into its ſheath, was ſuſpended by an iron wire which deſcended from the ceiling of the room, in ſuch a manner, that its lower end entering the jar, (in the direction of its axis,) was immerſed in the oil to ſuch a depth, that the middle of the flat ſurface of this end of the hot iron, which was directly above the point of the conical projection of ice, was diſtant from it only $\frac{2}{10}$ of an inch. The end of the ſheath deſcended $\frac{1}{10}$ of an inch lower than the end of the hot metallic cylinder.

As the oil was very tranſparent, and the jar placed in a favourable light, the conical projection of ice was perfectly viſible, even after the hot cylinder was introduced into the jar; and had *any Heat* DESCENDED through the thin ſtratum of fluid oil which remained interpoſed between the hot ſurface of the iron and the pointed projection of ice, which was under it, there is no doubt but this Heat muſt have been apparent, by the melting of

the

the ice; which event would have been difcovered, either by the diminution of the height of this projection, or by an alteration of its form. But this was not the cafe: the ice did not appear to be in the fmalleft degree diminifhed, or otherwife affected by the vicinity of the hot iron.

My reader will naturally fuppofe, without my mentioning the circumftance, that due care was taken in introducing the cylinder into the jar, to do it in the moft gentle manner poffible, to prevent the oil from being thrown into undulatory motions; and that proper means were ufed for confining the cylinder, motionlefs, in its place, when it had arrived there.

As this experiment appears to me to be unexceptionable, and its refult unequivocal and decifive; in order that a perfect idea may the more eafily be formed of it, I have added the Figure 4, where a fection of the whole of the apparatus ufed in making it may be feen, expreffed in a clear and diftinct manner.

If the general refult of the Experiments, of which an account has been given in the two firft Chapters of this Effay, afforded reafon to conclude that *water* is a *non-conductor* of Heat, the refult of that here defcribed certainly proves, in a manner quite as fatisfactory, that *oil* is alfo a *non-conductor*; and ferves to give an additional degree of probability to the conjecture, that all Fluids are *neceffarily* non-conductors of Heat.

As *mercury*, which is a metal in fufion, is different in many refpects from all other Fluids, I was very impatient to know if it agreed with them in that essential property, from which they have been denominated non-conductors of Heat, and this I found to be actually the case, by the result of the following decisive Experiment.

Experiment, No. 56.

Having emptied and cleaned out the cylindrical glass jar used in the last mentioned Experiment, and replenished it with a fresh cake of ice, with a conical projection in the middle of its upper side, I placed the jar, surrounded by pounded ice and water, on the table, in the cold room, where the foregoing Experiment had been made; and poured over the cake of ice as much ice-cold *mercury*, as covered it to the height of about an inch. Having cleaned the surface of the mercury in the jar with blotting paper, I suffered the whole to remain quiet, about an hour; and then very gently introduced the end of the hot cylinder of iron (inclosed in its paper sheath) into the mercury, and fixed it immoveably in such a position, that its flat end, which was naked, was immediately over the point of the conical projection of ice, and distant from it about $\frac{1}{4}$ of an inch; where I suffered it to remain several minutes.

It is necessary that I should mention, that, in order to prevent the internal motions in the mass of mercury, which would otherwise have been occasioned by the rising and spreading out on its surface of those particles of that fluid, which, having touched

touched the flat end of the hot iron, became specifically lighter in consequence of their increase of temperature, the end of the hollow cylindrical sheath, in which the solid cylinder of iron was placed, was made to project about $\frac{1}{10}$ of an inch below the flat end of the iron. This precaution was likewise used, and for a similar reason, in the preceding Experiment; when oil was used in the place of the mercury; as was mentioned, though without being explained, in giving an account of that Experiment.

As the cake of ice, on which the mercury reposed, was at that temperature precisely at which ice is disposed to melt with the smallest additional quantity of Heat, if *any Heat* had found its way *downwards* through the mercury to the ice in this Experiment, water would most undoubtedly have been formed; and this water would as undoubtedly have appeared on the surface of the mercury on taking away the iron: but there was not the smallest appearance of any ice having been melted.

To find out whether the cake of ice was *really* at that temperature at which it was disposed to melt with any additional Heat, I thrust down the end of my finger through the mercury, and touched the ice; and this Experiment removed all my doubts; for I found that, however expeditiously I performed that operation, it was hardly possible for me to touch the ice without evident signs of water having been produced being left behind, on the clean and bright surface of the mercury, on taking away my finger.

From

From the refults of all thefe experimental inveftigations it appears to me, that we may fafely conclude that *water*, *oil*, and *mercury* are perfect *non-conductors* of Heat; or, that when either of thofe fubftances takes the form of a Fluid, all interchange and communication of Heat *among its particles*, or from one of them to the other, directly, becomes from that moment *abfolutely impoffible*.

That this is alfo the cafe with refpect to the particles of *air*, has been rendered extremely probable, —I believe I might fay proved,—by the Experiments of which I gave an account in one of my Papers on Heat, publifhed in the Tranfactions of the Royal Society;—and I have fhewn elfewhere— (in my Sixth Effay) how much reafon there is to conclude, that the particles of *Steam* and of *Flame* are in the fame predicament.

But if all interchange and communication of Heat, from particle to particle—*immediately*, or *de proche en proche*, be abfolutely impoffible in fo many *elaftic* and *unelaftic Fluids*,—and in Fluids fo effentially different in many other refpects,—is there not fufficient grounds to conclude from hence, that this property is common to all Fluids—and that it is even *effential to fluidity?*

It is eafy to conceive, that the difcovery of fo important a circumftance muft neceffarily occafion a confiderable change in the ideas we have formed in refpect to the mechanical operations which take place in many of the great phenomena of Nature; as well as in many of thofe ftill more interefting chemical

chemical operations, which we are able to direct, but which we find, alas! very difficult to explain.

In my Paper on Heat, above mentioned, published in the Philosophical Transactions for the year 1792, I endeavoured to apply the discovery of the non-conducting power of *air* in accounting for the warmth of the hair of beasts;—of the feathers of birds;—of artificial clothing;—and of snow, the winter garment of the earth;—and also, in explaining the causes of the cold winds from the polar regions, and of their different directions in different countries, which prevail at the end of winter, and early in the spring.

In my Sixth Essay—(on the Management of Heat and the Economy of Fuel)—I availed myself of the knowledge of the non-conducting power of *steam* and of *flame*, in explaining the effects of a blow-pipe in increasing the action of pure flame; and in investigating the most advantageous forms for boilers: and in the third Chapter of this Essay I have endeavoured to apply the discoveries which had been made, respecting the manner in which Heat is propagated in *water*, in explaining the means which appear to have been used by the Creator of the world for equalizing the temperatures of the different climates, and preventing the fatal effects of the extremes of heat and of cold on the surface of the globe. But a most interesting application remains to be made of these discoveries to *chemistry*;—*vegetation*;—and the *animal economy*;—and to the learned in those branches of science I beg leave most earnestly to recommend them. If I am not much

much mistaken they will throw a new light on many of those mysterious operations of Nature, in which *inanimate bodies* are put in motion—their forms changed—their component parts separated, and new combinations formed; and it is possible that they may even enable us to account, on mechanical principles, for those surprising appearances of preference and predilection among bodies, which, without ever having been attempted to be explained, have been distinguished by the appellation of *chemical affinity*.

Perhaps it will be found that every change of form, in every kind of substance, is owing to Heat; and to Heat alone:—that every concretion is a true *congelation*, effected by cold, or a diminution of Heat;—and that every change from a solid to a fluid form is a real *fusion*. That the difference between calcination in the *wet* and in the *dry* way, is, in fact, much less than has hitherto been generally imagined; and that no metal is ever dissolved till it has *first been melted*.

Perhaps it will be found, that the apparent violence with which solid bodies of some kinds are attacked by their liquid solvents,—and which has, I believe, been considered as a proof of a strong chemical affinity—is not owing to any particular attraction, or election, but to the considerable degree of heat, or of cold, which is produced in their union with their menstrua; or to a great difference in the specific gravity of the menstruum in its natural state, and that of the same fluid after it has been changed to a saturated solution.

If Fluids are non-conductors of Heat, it is evident that, if any change of temperature takes place in chemical ſolution, it muſt neceſſarily produce *currents* in the ſolvent; and that theſe currents muſt be the more rapid, as the change of temperature is greater; and as they neceſſarily cauſe a ſucceſſion of freſh particles of the ſolvent to come into contact with the ſolid, it is evident,—all other things being equal,—that the rapidity of the proceſs of ſolution will be as the rapidity of theſe currents,—or as the change of temperature.

But the currents produced by the difference in the ſpecific gravities of the fluid menſtruum, and of the ſaturated ſolution, have perhaps, in general, a ſtill greater effect in bringing a rapid ſucceſſion of freſh particles of the menſtruum into contact with the ſolid body that is diſſolved in it, than thoſe produced by the change of temperature.

When theſe two cauſes conſpire to accelerate the motion of the ſame current, or when their tendencies are *in the ſame direction*, as is the caſe in the ſolution of common ſea-ſalt in water,—the ſolution ought to be moſt rapid.

When common ſalt is diſſolved in water, the ſpecific gravity of the ſaturated ſolution is greater than that of pure water, and will therefore deſcend in it; and cold being produced in the proceſs, and water being a non-conductor of Heat, the ſpecific gravity of the ſaturated ſolution will be *ſtill farther increaſed*, in conſequence of its condenſation with this cold, by which its deſcent in the water will be ſtill farther accelerated.

A curious queſtion here preſents itſelf, which, could it be reſolved, might greatly tend to elucidate this abſtruſe ſubject of philoſophical inveſtigation. Suppoſing that, in a caſe where Heat is generated in the ſolution of a ſolid in a fluid menſtruum, the *addition* to the ſpecific gravity of the menſtruum, ariſing from its chemical union with the ſolid, ſhould ſo preciſely counter-balance the *diminution* of the ſpecific gravity of the Fluid, by the Heat generated in the proceſs, that the *hot* ſaturated ſolution ſhould be preciſely of the ſame ſpecific gravity as the *cold* menſtruum;—would, or would not the proceſs of ſolution be poſſible under ſuch circumſtances?

If the *apparent* tendency to approach each other, which we ſometimes perceive in ſolids and their fluid menſtrua, were real;—if that peculiar kind of attraction of predilection which has been called chemical affinity, has a real exiſtence, and if its influence reaches *beyond the point of actual contact*, (as has, I believe, been generally ſuppoſed,) as there is no appearance of any attraction whatever, or affinity, between any ſolid body, and a ſaturated ſolution of the ſame body in its proper menſtruum, it ſeems probable that the ſolution would take place, —under the circumſtances deſcribed: but ſhould the attraction of affinity, according to the definition of it here given, have no exiſtence in fact, (which is what I very much ſuſpect,) in that caſe it is evident that the ſolution, though it would not be abſolutely impoſſible, would be ſo very ſlow as hardly to be perceptible.

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It would not be *impossible*, because the particles of the menstruum in immediate contact with the solid, though, in the moment of their saturation, they would have no tendency to move out of their places, yet, as they would by degrees necessarily give off, to the undissolved part of the solid, a part of the Heat acquired in the chemical process by which they were saturated, being condensed by this loss of Heat, they would, at length, begin to descend, and give place to other particles of the menstruum; which, in their turns, would follow them, but with velocities, however, continually decreasing;—on account of the gradual augmentation of temperature of the undissolved part of the solid, and of the Heat communicated by that solid substance to the whole mass of the liquid menstruum.

Though it would, probably, be extremely difficult to contrive any single experiment, from the result of which a satisfactory decision of this question could be obtained, yet it does not appear to be impossible to discover by *indirect means*, the principal fact on which its decision must depend.

It is a well known fact, that, when water which holds sea-salt in solution is mixed, in any vessel, with fresh water, the salt will, after a short time, be found to be very equally distributed in every part of the whole mass; and I believe that it has been generally considered, that this equal distribution of the salt is owing to the affinity which is supposed to exist between sea-salt and water.

Having doubts with respect to the existence of this supposed attraction; and suspecting that the equal diſtribution of the ſalt was owing to a very different cauſe—the internal motions among the particles of the water, occaſioned by accidental changes of temperature—I made the following Experiment, which, I fancy, will be conſidered as deciſive.

Experiment, No. 57.

I took a cylindrical glaſs jar, $4\frac{1}{4}$ inches in diameter, and $7\frac{3}{4}$ inches high, and placing it in the middle of another cylindrical glaſs jar, $7\frac{1}{2}$ inches in diameter, and 8 inches high, which ſtood in a very ſhallow earthen diſh, nearly filled with pounded ice and water, I placed the diſh, with its contents, on a ſtrong table, in an uninhabited room, in a retired part of the houſe, where the temperature of the air, which was the ſame, with very little variation, day and night, was at about 36° F. Having prepared, and at hand, a quantity of the ſtrongeſt *brine* I could make with ſea-ſalt, which was very clear, tranſparent, perfectly colourleſs, and ice-cold; and alſo, a quantity of freſh, or pure water,—ice-cold, lightly tinged of a red colour with turnſol; and ſome ice-cold *olive-oil;* I firſt poured as much of the freſh water into the ſmall cylindrical jar as was neceſſary to fill it up to the height of above 2 inches; and then, by means of a glaſs funnel, which ended in a long and narrow tube, by introducing this tube into the freſh water, and reſting it on the bottom of the jar, I poured a quantity

quantity of the brine, equal to that of the fresh water, into the jar; and in performing this operation I took so much care to do it gently, and without disturbing the fresh water already in the jar, that, when it was finished, the fresh water, which, as it was coloured red, could easily be distinguished from the brine, remained perfectly separated from this heavier saline liquor; on which it reposed quietly, without the smallest appearance of any tendency to mix with it.

I now filled, to the height of about 5 inches, the void space between the outside of the small jar and the inside of the large jar in which it was placed, with ice-cold water, mixed with a quantity of ice, in pieces as large as walnuts,—(pounded ice would have obstructed the view in observing, through the sides of the large jar, what passed in the smaller)—and when this was done, I very carefully poured ice-cold *olive oil** into the smaller jar till it covered the surface of the (tinged) fresh water to the height of about an inch (see Fig. 5. Plate IV.); and placing myself near the table, in a situation where I had a distinct view of the contents of the small jar, I set myself to observe the result of the Experiment.

After waiting above an hour without being able to perceive the smallest appearance of any motion, either in the brine, or in the fresh water, (the one continuing to repose on the other with the most

* This oil served not only to keep the water on which it reposed, quiet; but also to prevent any communication of heat between it and the air of the atmosphere.

perfect tranquillity, and without the ſmalleſt diſpoſition to mix together) I left the room.

When I returned to it the next day, I found things preciſely in the ſtate in which I had left them; and they continued in this ſtate, without the ſmalleſt appearance of any change, or of any diſpoſition to change, during *four days*.

At the end of that time, thinking that any farther prolongation of the Experiment would be quite uſeleſs, I removed the ſmall jar, taking care not to agitate its contents, and placed it in the window of a room heated by a German ſtove.

In leſs than an hour I perceived that the brine and the (tinged) freſh water began to mix, and at the end of 24 hours they were intimately mixed throughout, as was evident by the colour of the aqueous fluid on which the oil repoſed; which now appeared to the eye to form one uniform maſs of a light red tint.

I ſhall leave it to philoſophers to draw their own concluſions from the reſults of this Experiment: In the mean time, there is one fact which it ſeems to point out that I ſhall juſt mention, which is not only curious in itſelf, but may lead to very important diſcoveries. It appears to me to afford ſtrong reaſons to conclude that, were a lake but *very deep*, its waters, near the ſurface, would neceſſarily be freſh, even though its bottom ſhould be one ſolid maſs of rock ſalt!

Would it be ridiculous to make Experiments to determine whether the water at the bottom of ſome very deep lakes is not impregnated with ſalt?

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Should it be found to be actually the case, it might prove an inexhaustible treasure in an inland country, where salt is scarce.

As mines of rock-salt are often found in the neighbourhood of fresh lakes, it seems reasonable to suppose that the waters of such lakes should *sometimes* be in contact with *strata* of these mines; and when I first began to meditate on this subject, I was much surprised,—not that the salt water which may lie at the bottom of fresh lakes should not already have been discovered,—for from the first I plainly perceived that nothing could happen in the ordinary course of things that could bring it to the light, or even afford any grounds to suspect its existence;—but, as *strata* of salt mines frequently lie higher than the mean level of the country, I was surprised that lakes *of salt water* should not more frequently be found; and as these reflections occurred to me *after* I had discovered what appeared to me to be an evident proof of the wisdom and goodness of the Creator in *making* all lakes in cold countries *fresh*, I began to be alarmed for the fatal consequences that might ensue, if, by chance, the side of a lake should come into contact with a mountain of salt; as I saw might easily happen.

Shall I,—or shall I not attempt to give my reader an idea of what I felt, when, meditating on the subject, and almost beginning to repent of what many, no doubt, have already condemned as the foolish dream of an enthusiastic imagination, I saw, all at once, that the most effectual care had been taken to prevent the evils I apprehended;—that from the very constitution of things, and the ordinary and uniform operation of the known laws of Nature, the permanent *existence of a lake*, SALT AT THE SURFACE, is *absolutely impossible*; even though it should be surrounded on every side by mountains of salt *?

Though the explosion of a volcano, an earthquake, or any other great convulsion, by which the shores of a lake might be brought into contact with a vast mine of salt, might cause the whole mass of its water to be salt for a time; yet, the evil would soon effect its own remedy: The falling in of the crust of earth and stones by which mines of salt are every where found to be covered, (and without which they could not exist) would very soon cover the naked salt, and the water *at the surface of the lake* would again become perfectly fresh. Should, however, the lake be so deep that the temperature at the bottom should remain the same summer and winter, without any sensible variation, it is most certain that its waters *there*—(at the bottom of the lake)—would remain perfectly saturated with salt for ever.

But are there not some reasons to conclude that the water at the bottoms of *all very deep lakes* ought necessarily to be salt, even in situations where there are no mines of salt near?

The sea-shells that are frequently found in high inland situations, as well as many other appear-

* By the word *Lake* I mean, as is easy to perceive, a collection of water, in a high inland situation, from which there is a constant efflux.

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ances noticed by naturalifts, ftrongly indicate that moft of our continents have been covered by the waters of the ocean. Now if that event ever happened—however remote the period may be at which it took place—it feems highly probable that the falt water left at the bottoms of all deep lakes, by the fea, on its retiring, *muft be there now.*

I cannot take my leave of this fubject without juft obferving, that the difcovery of the *impoffibility* of the permanent exiftence of what we can plainly perceive would be an evil, certainly ought not to *diminifh* our admiration of the wifdom of the great Architect of the Univerfe.

CHAP. II.

Water made to congeal at its under Surface.—Observation respecting the Formation of Ice at the Bottoms of Rivers.—Reasons for concluding that Heat can never be equally distributed in any Fluid.—Perpetual Motions occasioned in Fluids by the unequal Distribution of Heat.—An inconceivably rapid Succession of Collisions among the integrant Particles of Fluids is occasioned by the internal Motions into which Fluids are thrown in the Propagation of Heat.—An Attempt to estimate the Number of those Collisions which take place in a given Time.—These Investigations will greatly change our Ideas respecting the real State of Fluids apparently at rest.—FLUIDITY *may be called the* LIFE OF INANIMATE BODIES.—*Conjectures respecting the* VITAL PRINCIPLE *in Living Animals; and the Nature of Physical* STIMULATION.

WHATEVER the mechanical operation may in fact be, by which those effects are produced that have given rise to the idea of the existence of an attraction of affinity—(a power different from gravitation)—between solid bodies and their liquid menstrua, and between different portions of the same menstruum differently saturated; the result of the foregoing Experiment (No. 57) proves that two particles of water in combination with very different

different quantities of sea-salt; or a particle of water *saturated* with salt, and another perfectly free from salt; *may be* in contact with each other for any length of time without showing any appearance of a disposition to equalize the salt between them.

But should we even admit as a fact, what this Experiment seems to indicate, namely, that there is no such thing as an *attraction of predilection* between solids and their solvents; and that all those motions which have been attributed to the action of that supposed power,—(as well as all other motions which take place in Fluids,)—are the immediate effects of *gravitation* acting according to immutable laws, and *changes of specific gravity by Heat;* yet, there would still remain one great difficulty in explaining chemical solution. As all mechanical operations require *a certain time* for their performance; and as the motion which is occasioned in a Fluid by a change of specific gravity in any individual particles of it, *begins* as soon as the change begins to take place, if there be no attraction between the particles of solid bodies and the particles of their menstrua;—as Heat is supposed to be generated or absorbed, or, to speak more properly,—both generated and absorbed,—in the *contact* of those particles, and previous to the completion of their chemical union;—how does it happen that the particle of the menstruum whose specific gravity is necessarily changed by this change of temperature, does not *immediately* quit the solid, in consequence of this change; and before the process of solution has *had time to be completed?*

A confideration of the effects of the *vis inertiæ* of the particle of the menftruum whofe fpecific gravity is thus changed, and alfo of the *vis inertiæ* of the reft of the Fluid, and the refiftance it muft oppofe to the motion of its individual folitary particles, would furnifh us with arguments that might be employed with advantage in removing this difficulty; but I fancy that the refult of the Experiment of which I fhall prefently give an account will be more fatisfactory than any reafoning, unfupported by facts, that I could offer on the fubject.

When a doubt arifes with regard to the *poffibility* of any operation of a peculiar kind, which is *fuppofed* to take place, in any procefs of nature among thofe infinitely fmall integrant particles of bodies which efcape, and muft ever efcape, the cognizance of our grofs organs, however they may be affifted by art, the fhorteft way of deciding the queftion is to put the known powers of nature in action under fuch circumftances that the effects produced by them muft fhow, unequivocally, whether the fuppofed operation be poffible, or not: and if it be found to be poffible in one cafe, we may then argue with lefs diffidence on the probability of its actually taking place in the fpecific cafe in queftion.

It has been abundantly proved by the Experiments of M. de Luc, and by thofe of my friend Sir Charles Blagden, that, when water, in cooling, has arrived at the temperature of about 41°F. its condenfation with cold ceafes; and it begins to expand;

expand; and continues to expand gradually as its temperature goes on to be farther diminished, till it is changed to ice. Availing myſelf of that moſt important diſcovery, I made the following Experiment.

Experiment, No. 58.

Having poured *mercury*, at the temperature of 60°, into a common glaſs-tumbler, till this Fluid ſtood at the height of about an inch; I then poured about twice as much water (at the ſame temperature) upon it; and placing the tumbler in a ſhallow earthen diſh, ſurrounded it to the height of the level of the ſurface of the mercury with a freezing mixture of ſnow and common ſalt. Having done this, I was very curious indeed to ſee in what part of the water ice would firſt make its appearance. Could it be at the upper ſurface of it? That appeared to me to be impoſſible; for the Experiment being made in a room warmed by a German ſtove, the temperature of the air which repoſed on that ſurface was conſiderably above the point at which water freezes.

Could it be at its lower ſurface, where it reſted on the upper ſurface of the mercury?—If that ſhould happen, it would ſhow, that, notwithſtanding the diminution of the ſpecific gravity of the water in paſſing from the temperature of 41° to that of 32°; and the tendency which this diminution gave it to quit the ſurface of the mercury from the inſtant when, in being cooled by a contact with it, it had paſſed the point of 41°; yet, there

there was time sufficient for the congelation to be completed *before the particle of water ſo cooled could make its eſcape.*

The reader will naturally conclude from what was ſaid in the preceding page, that it was merely with a view to the determination of that ſingle fact, that this Experiment was contrived; and he will perceive by the reſult of it that my expectations with regard to it were fully anſwered.

Ice was not only formed *at the bottom of the water*, at its under ſurface, where it was in contact with the cold mercury; but, I found on repeating the experiment, and varying it, by previouſly cooling the mercury in the tumbler to about 10°, that *boiling hot water*, poured gently upon it, was inſtantly frozen, and gradually formed a thick cake of ice, covering the mercury; though almoſt the whole of the maſs of the unfrozen water, which reſted on this ice, remained nearly boiling hot.

This Experiment not only determines the point for the deciſion of which it was undertaken; but alſo enables us to form a juſt opinion reſpecting a matter of fact which has been the ſubject of a good deal of diſpute.

Though many accounts have been publiſhed of ice found at the bottom of rivers, yet doubts have been entertained of the poſſibility of its being *formed* in that ſituation. From the reſult of the foregoing Experiment it appears to me that we may ſafely conclude, that, if after a very long and a very ſevere froſt, by which the ſurface of the ground has not only been frozen to a conſiderable depth,

but

but alſo cooled ſeveral degrees below the freezing point, a river ſhould overflow its banks, and cover the ſurface of ground *previouſly ſo cooled*, ice would be formed at the bottom of the water: but all the Experiments that have been made on the congelation of water ſhow the abſolute impoſſibility of ice being ever formed, in any country, at the bottom of a river which conſtantly fills its banks, or which never leaves its bed expoſed, dry, to the cold air of the atmoſphere.

By reflecting on the various conſequences that ought to follow from the peculiar manner in which Heat appears to be propagated in Fluids, we are led to conclude, that it is almoſt impoſſible that any Fluid expoſed to the action of light ſhould ever be throughout of the ſame temperature, though its maſs be ever ſo ſmall; and that the difference in the Heat of its different particles muſt occaſion perpetual motions among them.

Suppoſe any open veſſel,—as a common glaſs tumbler for inſtance,—containing a piece of money, a ſmall pebble, or any other ſmall ſolid opaque body, to be filled with water, and expoſed in a window, or elſewhere, to the action of the ſun's rays. As a ray of light cannot fail to generate Heat when and where it is ſtopped or abſorbed, the rays, which, entering the water, and paſſing through it, impinge againſt the ſmall ſolid opaque body at the bottom of the veſſel, and are *there abſorbed*, muſt neceſſarily generate a certain quantity of Heat; a part of which will penetrate into the interior parts of the ſolid, and a part of it will be commu-

communicated to thofe colder particles of the water which repofe on its furface.

Let us fuppofe the quantity of Heat fo communicated to one of the integrant particles of the water to be fo fmall, that its effect in diminifhing the fpecific gravity of the particle is but juft fufficient to caufe it to move upwards in the mafs of the liquid with the very fmalleft degree of velocity that would be perceptible by our organs of fight, were the particle in motion large enough to be vifible. This would be at the rate of about *one hundredth part of an inch* in a fecond.

This velocity, though it appears to us to be flow in the extreme, when we compare it with thofe motions that we perceive among the various bodies by which we are furrounded, yet, we fhall be furprifed when we find what a rapid fucceffion of events it is capable of producing.

If we fuppofe the diameter of the integrant particles, or *molécules* of water, to be *one millionth part of an inch*—(and it is highly probable that they are even lefs—*)—in that cafe, it is moft certain that an individual particle, moving on in a quiefcent mafs of that Fluid with the velocity in queftion, namely, at the rate of $\frac{1}{100}$ part of an inch in 1 fecond, would run through a fpace equal to

* Leaf gold, fuch as is prepared and fold by the gold-beaters, is not *four times* as thick as the diameter here affumed for the integrant particles of water. Thefe leaves of folid metal have been found by computation to be no more than $\frac{1}{282000}$ of an inch in thicknefs. How much lefs muft be the diameter of the integrant particles of gold?

ten

ten thousand times the length of its diameter in *one second*, and consequently, would come into contact with at least *six hundred thousand* different particles of water in that time.

Hence it appears how inconceivably short the time must be that an individual particle, in motion, of any Fluid, can remain in contact with any other individual particle, not in motion, against which it strikes in its progress, (however slow that progress may appear to us to be) through the quiescent mass of the Fluid!

Supposing the contact to last as long as the moving particle employs in passing through a space equal to the length of its diameter—which is evidently all that is possible; and more than is probable;—then, in the case just stated, the contact could not possibly last longer than $\frac{1}{10000}$ part of a second! This is the time which a cannon bullet, flying with its greatest velocity, (that of 1600 feet in a second) would employ in advancing 2 inches.

If the cannon bullet be a *nine pounder*, its diameter will be four inches; and if it move with a velocity of 1600 feet (=19200 inches) in a second, it will pass through a space just equal to 4800 times the length of its diameter in 1 second. But we have seen that a particle of water moving $\frac{1}{10}$ of an inch in a second actually passes through a space equal to 10000 times the length of its diameter in that time: Hence it appears that *the velocity with which the moving body quits the spaces it occupies* is more than twice as great in the particle of water, as in the cannon bullet!

There

There is one more computation which may be of use in enabling us to form more just ideas of the subject under consideration,—and surely too much cannot be done to enlighten the mind, and assist the imagination, in our attempts to contemplate those invisible operations of nature which nothing but the sharpest ken of the intellectual eye will ever be able to detect and seize.

As succeeding events which fall under the cognizance of our senses cannot be distinguished if they happen oftener than about *ten times in a second* *, it appears that when a particle of water moves in a quiescent mass of that fluid at the rate of $\frac{1}{100}$ part of an inch only, in one second, its succeeding collisions with the different particles, at rest, of that fluid, against which it strikes as it moves on, must be so inconceivably rapid that no less than *one thousand* of them must actually take place, *one after the other*, in the shortest space of time that is perceptible by the human mind †.

After

* This assertion, in as far, at least, as it relates to objects of sight, may be proved by the following easy experiment: Let a wheel, with any known number of spokes, be turned round its axis with such a velocity as shall be found necessary, in order that the spokes may disappear or become invisible.—From the velocity of the wheel, and the number of spokes in it, the fact will be decided.

† It probably will not escape the observation of my learned readers, that the velocity which I have here assigned to the single particle of water, moving upwards in that fluid in consequence of a change of its specific gravity by Heat, though apparently very small,—($\frac{1}{100}$ part of an inch in a second)—is, however, most probably considerably greater, in fact, than any individual *solitary* particle of that fluid could possibly acquire, in the supposed circumstances, by any change of temperature, however great, owing to the resistance which

would

After we have patiently examined the reſult of theſe inveſtigations, and the imagination has become *familiarized* with the contemplation of the intereſting facts they preſent to it, how much will our ideas be changed with regard to the real ſtate of fluids apparently at reſt! They will then appear to us to be, what no doubt they really are in fact, an aſſemblage of an infinite number of infinitely ſmall particles of matter moving continually, or without ceaſing, and with inconceivable velocities.

We ſhall then conſider fluidity as the *life of inanimate bodies*, and congelation as the *ſleep of death*;—and we ſhall ceaſe to aſcribe active powers, or exertions of any kind, to dead *motionleſs* matter.

But what ſhall we think of the *vital principle* in living animals?—Does not their life alſo depend on the internal motions in *their* fluids, occaſioned by an *unequal* diſtribution of heat?—And is not

would neceſſarily be oppoſed to its motion by the quieſcent particles of the fluid. Aware of this objection, and being deſirous of being prepared to meet it, I took ſome pains to compute, by the rules laid down by Sir ISAAC NEWTON in his *Principia*, book ii. ſect. vii., what the greateſt velocity is that a ſolitary particle of water (ſuppoſed to be $\frac{1}{1000000}$ of an inch in diameter) could poſſibly acquire by a given change of its ſpecific gravity: And I found that if the ſpecific gravity of water at the temperature of 32° F. be taken at 1.00082, and its ſpecific gravity at 80°, at 0.99759, as lately determined by accurate experiments, then, a ſingle particle of water at the temperature of 80°, ſituated in a quieſcent maſs of that fluid at 32°, the greateſt velocity this hot particle could acquire in moving upwards in conſequence of its comparative levity would be that of $\frac{1}{2638}$ part of an inch in 1 ſecond. This is at the rate of about one inch and an half in 1 hour.—But it is evident, that when great numbers of particles unite and form currents, they will make their way through the quieſcent fluid with greater facility, and conſequently will move faſter.

ſtimulation,

ſtimulation, in all caſes, the mere mechanical effect of the communication of Heat?

It is an opinion which we know to be as old as the days of MOSES, that *the life of an animal reſides in its blood*; and it is highly probable that it dates from a period ſtill more remote. It was lately revived by an anatomiſt and phyſiologiſt, (now no more,) who was eminently diſtinguiſhed for ſagacity; and it appears to me that the late diſcoveries reſpecting the manner in which Heat is propagated in Fluids tend greatly to elucidate the ſubject, and to give to the hypotheſis a high degree of probability.

According to this hypotheſis—(as it may now be explained)—every thing that increaſes the *inequality of the diſtribution* of the Heat in the maſs of the blood—(even though it ſhould not immediately augment its quantity)—ought to increaſe the intenſity of thoſe *actions* in which life conſiſts. But are there not many ſtriking proofs that this is the caſe in fact?

Do not *reſpiration*,—*digeſtion*,—and *inſenſible perſpiration* all tend evidently—(that is to ſay, according to our aſſumed principles, with regard to the manner in which Heat is propagated in Fluids)—to *produce*, and to *perpetuate* this inequality of heat in the animal fluids? And do we not ſee what an immediate and powerful effect they have in increaſing the intenſity of the action of the powers of life?

If animal life depends eſſentially on thoſe *internal* motions in the animal fluids,—which, as has

has been ſhown,—are occaſioned by the differ-ence of the *ſpecific gravities* of their integrant par-ticles, or *molécules*, ariſing from their different tem-peratures;—in that caſe, it is evident that the *vital powers* would be ſtrengthened, or their action increaſed, either by *heat* or by *cold* properly applied. But is not this found to be the caſe in fact? Does not the *dram of brandy* at St. Peterſburgh produce the ſame effects as the *draught of iced le-monade* at Naples, and by the ſame mechanical ope-ration, but acting in oppoſite directions? And does not the *loſs of Heat*, by inſenſible perſpiration, contribute as efficaciouſly to the preſervation of that *inequality of temperature* which is eſſential to life, as the *introduction of Heat* into the ſyſtem in reſpiration?

Is not the ſudden coagulation of blood, when drawn from a living animal, and are not all the other rapid changes that take place in it, evident proofs of an unequal diſtribution of Heat? And does not the *viſcoſity* of blood, as well as its perpe-tual motions in the vaſcular ſyſtem, contribute very powerfully to the preſervation of that ine-quality?

Are not the livid ſpots on the ſurface of the body, which indicate a beginning of mortification, produced in conſequence of a ſeparation, or *pre-cipitation* of the heterogeneous particles of the nal Fluids, according to their ſpecific gravities individual temperatures, occaſioned by reſt, in interruption of circulation? And may we emphatically pronounce ſuch Fluids to be l?

 Would

Would not any liquid in which Heat were *equally diſtributed* be a *fatal poiſon* if injected into the veins of a living animal? And would not this be the caſe even were the liquid ſo injected a portion of the animal's own blood, or of the lymph or any other of its component parts, and were it at the mean temperature preciſely of the healthy Fluids circulating in the veins and arteries of the animal?

Is not glandular ſecretion a true precipitation? and is it not poſſible that the formation of the ſolids, and the growth of an animal body, may be effected by a proceſs exactly ſimilar to congelation? And are there not even circumſtances from which we might conclude, with a conſiderable degree of probability, that moſt of theſe congelations are formed at or about the temperature of boiling water?

But I forbear to enlarge on this ſubject. I find I have unawares entered a province, where, if I advance farther, I ſhall certainly be expoſed to the danger of being conſidered and treated as an intruder; and I muſt haſten to make my retreat, which I ſhall endeavour to effect by abruptly putting an end to this Chapter.

CHAP. III.

Probability that intenſe Heat frequently exiſts in the ſolitary Particles of Fluids, which neither the Feeling nor the Thermometer can detect.—The Evaporation of Ice during the ſevereſt Froſt explained on that Suppoſition.—Probability that the Metals would evaporate when expoſed to the Action of the Sun's Rays were they not good Conductors of Heat.—Mercury is actually found to evaporate under the mean Temperature of the Atmoſphere.—This Fact is a ſtriking Proof that FLUID MERCURY *is a Non-conductor of Heat.—Probability that the Heat generated by the Rays of Light is always the ſame in Intenſity; and that thoſe Effects which have been attributed to Light ought perhaps in all Caſes to be aſcribed to the Action of the Heat generated by them—A ſtriking Proof that the moſt intenſe Heat does ſometimes exiſt where we ſhould not expect to find it.—Gold actually melted by the Heat which exiſts in the Air of the Atmoſphere, where there is no Appearance of Fire, or of any Thing red-hot.—We ought to be cautious in attributing to the Action of unknown Powers, Effects ſimilar to thoſe produced by the Agency of Heat.—The moſt intenſe Heat may exiſt without leaving any viſible Traces of its Exiſtence behind it.—This important Fact illuſtrated by the neceſſary Reſult of an imaginary Experiment.*

HOW far the poſſibility of the communication of Heat between the integrant particles of a Fluid may or may not be owing to the extreme mobility of thofe particles, and to the infinitely fhort time that two of them, of different fpecific gravities, (owing to a difference of temperature) can remain in contact, I leave others to determine; in the mean time, it is moft certain that the exiftence of this impoffibility of any immediate communication of Heat among the particles of a Fluid renders the diftribution of Heat very unequal; and it feems highly probable that many appearances which have been attributed to very different caufes, are in fact owing to *intenfe Heat* exifting and producing the effects proper to it in fituations where its exiftence has not even been fufpected.

If Fluids are non-conductors of Heat, no fituation can poffibly be more favourable to its prefervation than when it exifts in them; and it is not only evident *a priori* that the moft intenfe Heat *may exift* in a few folitary particles of fome Fluids, without its being poffible for us to detect it, or to difcover the fact, either by our feeling or by the thermometer; but there are many appearances that ftrongly indicate,—and others that prove, that intenfe Heat actually does exift in that concealed or imperceptible ftate very often.

There is no reafon to fuppofe that it is poffible for ice to be reduced to fteam without being previoufly melted; and it is well known that ice cannot be melted with a lower degree of Heat than that of 32° of Fahrenheit's fcale: but in the midft of winter, in the coldeft climates, and when the temperature of air of the atmofphere, as fhown by the thermometer, has been much below 32°, ice, expoſed to the air, has been found to evaporate.

How can we account for this event, except it be by ſuppoſing that ſome of the particles of air, which accidentally (as we expreſs it) come into contact with the ice, are ſo hot, as not only to melt the ſmall particles of ice which they happen to touch, but alſo to reduce a part of the generated water to ſteam, before it has time to freeze again; or by ſuppoſing that this is effected by intenſe Heat generated by light abſorbed by ſmall projecting points of the ice? As ice is a very bad conductor of Heat, that circumſtance renders it more likely that the event in queſtion ſhould actually take place, in either of theſe ways.

If the metals were very bad conductors of Heat, inſtead of being very good conductors of it, I think it more than probable that even they would be found to evaporate, when expoſed to the action of the direct rays of the ſun; and perhaps alſo in ſituations in which ſuch an event would appear ſtill more extraordinary.

Mercury has been actually found to *evaporate* under the mean temperature of the atmoſphere!—What a ſtriking proof is this that *fluid mercury* is a non-conductor of Heat;—and alſo, that very intenſe Heat may be generated, or exiſt, where it would not naturally be expected to be found. And does not the evaporation of water under the mean tem-

 perature

perature of the atmoſphere afford another proof of this laſt fact?

That the moſt intenſe Heat is often excited in very ſmall particles of ſolid bodies diſperſed about in the midſt of maſſes of cold liquids is not to be doubted. It is well known what an intenſe Heat the rays of the ſun are capable of exciting; and it ſeems to be highly probable that Heat actually excited by them is always the ſame—that is to ſay—*intenſe in the extreme:* but when the rays are few, and when circumſtances are not favourable to the *accumulation* of the Heat they generate, it is often ſo ſoon diſperſed, that it eſcapes the cognizance of our ſenſes, and of our inſtruments; and ſometimes leaves no viſible traces of its exiſtence behind it.

Why ſhould we not ſuppoſe that the Heat generated by a ray of light, which, entering a maſs of cold water, accidentally meets with an infinitely ſmall particle of any ſolid and opaque ſubſtance which happens to be floating in the liquid, and is abſorbed by it, is not juſt as intenſe as that generated in the focus of the moſt powerful burning mirror, or lens?

Mr. Senebier has given us an account of a great number of intereſting Experiments on the effects produced on different bodies by expoſure to the direct rays of the ſun; but why may we not attribute all thoſe effects to the intenſe *local* Heat, generated by the light abſorbed by the infinitely ſmall—and, if I may uſe the expreſſion—*inſulated* particles

of

of the bodies which were found to be affected by it?

The ſurface of wood of various kinds was turned brown. The ſame appearances might be produced in a ſhorter time by the rays which proceed from a red-hot iron, which change the ſurface of the wood to an imperfect coal. But were not the ſurfaces of the woods which were turned brown by the light of the ſun in Mr. Senebier's experiments changed to an imperfect coal?—And is it poſſible for a Heat leſs intenſe than that of *incandeſcence* to produce that effect?

Among the many facts that might be adduced to prove that the moſt intenſe Heat *may*, and frequently *does exiſt* where we ſhould not expect to find it, the following appears to me to be very ſtriking and convincing. It is, I believe, generally imagined that the intenſity of the Heat generated in the combuſtion of fuel is much leſs in a ſmall fire, than in a great one; but there is reaſon to think that this is an erroneous opinion, founded on appearances that are not concluſive; at leaſt it is certain that the intenſe Heat of a large ſmelting furnace, ſuch as is neceſſary for melting the moſt refractory metals, actually exiſts in the feeble flame of the ſmalleſt candle:—and what may appear ſtill more extraordinary,—this intenſe degree of Heat often exiſts in the air of the atmoſphère, *where no viſible ſigns of Heat appear*, as I ſhall preſently ſhow.

Iron is fully *red-hot* by day light at the temperature of about 1000° of Fahrenheit's ſcale; braſs melts at 3807°,—copper at 4587°,—silver at 4717°,—and gold at 5237°; and nothing is more certain than that the Heat must be at that intensity which corresponds to the 5237th degree of Fahrenheit's scale, *where gold is found to melt.* But very fine gold, silver, or copper wire, flatted, (such as is used to cover thread to make lace) melts instantaneously on being held in the flame of a candle. It will even be melted if it be held a few seconds *over* the flame of a candle, *at the distance of more than an inch from the top of the flame*, in a place where there is no appearance of fire, or of any thing red-hot.

From the important information which we acquire from the result of these Experiments, we see how much we ought to be on our guard in forming an opinion with respect to the *intensity* of the Heat which *may exist* in the invisible insulated particles of matter of any kind that may be scattered about in a given space,—or which may float in any Fluid, where neither our feeling nor our thermometers can possibly be sensibly affected by it.

A thermometer can do no more than indicate the *mean of the different temperatures of all those bodies or particles of matter which happen to come into contact with it.* If it be suspended in air, it will indicate the mean of the temperatures of those particles of air *which happen to touch it;* but it can never give us any information respecting the *relative* temperatures of those particles of air.

If, during the most intense frost, a thermometer were suspended in the neighbourhood of a burning candle,—

candle,—in the ſame room for inſtance,—if it were placed over the candle, or nearly ſo, though it ſhould be diſtant from it ſeveral feet, as air is a non-conductor of Heat, there is not the ſmalleſt doubt, but that ſome ſolitary particles of air heated by the candle to the intenſe Heat of melting gold, would reach the thermometer; but neither the thermometer, nor the hand held in the ſame place, could give any indication of ſuch an event.

As it appears from all that has been ſaid that intenſe Heat *may exiſt* even under the form of *ſenſible Heat*, where its preſence cannot be diſcovered or detected by us; and as it ſeems highly probable that in many caſes, where its exiſtence may eſcape our obſervation, it may nevertheleſs be capable of producing very viſible effects, I think we ought always to be much on our guard in accounting for effects ſimilar to thoſe which are known to be produced by Heat; and never, without very ſufficient reaſons, attribute them to the agency of any other, *unknown* power: and this caution appears to me to be peculiarly neceſſary in accounting for thoſe effects which have been found to be produced in various bodies when they are expoſed to the action of the ſun's rays.

If the ſolar rays concentrated in the focus of a lens, when they are made to fall on a piece of wood, inſtantly change its ſurface to a black colour, and reduce it to charcoal, why may we not conclude that the change of colour which is gradually or more ſlowly produced in the ſame kind of wood, when

when it is ſimply expoſed in the ſun-beams, is produced in the ſame manner?

"The difference in the *times* neceſſary to produce ſimilar effects in theſe two caſes is no proof that they are not produced *in the ſame manner*; for if they are effected merely by the agency of Heat, (which I ſuppoſe) then the effects produced in any given time will not be as the denſity of the light, or as the number of rays, but as that part of the Heat generated, which, not being immediately diſperſed or carried off by the air, has time to produce the action proper to it in the wood; and conſequently muſt be incomparably greater, in proportion, when the rays are concentrated, than when they are not.

Luna cornea expoſed to the action of light changes colour;—but why ſhould we not attribute this change to the expulſion of the oxygen united with the metal, by the agency of the Heat generated by the light? To remove every poſſible objection to this explanation of the phenomenon nothing more appears to be neceſſary than to admit, what is well known, that this metallic oxyd may be reduced, without addition, *with ſome degree of Heat*,—and that this ſubſtance is a bad conductor of Heat.

Will not the admiſſion of our hypotheſis reſpecting the *intenſity* of the Heat which is ſuppoſed to be generated where light is ſtopped, and of that reſpecting the non-conducting power of Fluids with regard to Heat, enable us to account, in a manner

manner more satisfactory than has hitherto been done, for the effects of the sun's light in bleaching linen, when it is exposed wet to the action of his direct rays? as also for the reduction of those metallic oxyds which have been found to be revived by exposure to light?—And will it not also assist us in accounting for the production of pure air in the beautiful Experiment of Doctor Ingenhouz, in which the green leaves of living vegetables are exposed, immersed in water, to the sun's rays?

Mr. Senebier has shown that the colouring matter of healthy green leaves of vegetables, which is extracted from them by spirits of wine, and which tinges the spirits of a beautiful green colour, is destroyed, or rather changed to a dirty brown colour, in a few minutes, on exposing this tincture in a transparent phial, and *in contact with pure air*, to the direct rays of a bright sun:—but why should we not consider this process as a real combustion?

The Heat acquired by the liquid,—which, as I have often perceived in repeating the Experiment, is very considerable,—and the necessity there is for the presence of *pure air*, that the Experiment may succeed, seem to indicate that something very like combustion must take place in it.

If liquids are non-conductors of Heat, they ought certainly, *on that account*, to be peculiarly well calculated for confining, and consequently furthering the operations of that Heat which is generated by light, or by any other means, in their integrant particles, or in the infinitely small and insulated particles when it is fimply expofed in the fun-beams, is produced in the fame manner?

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