demonſtration, that Heat is propagated in water *in conſequence* of its internal motions;—or that it is tranſported or *carried* by the particles of that liquid, and that it does not ſpread and expand in it as has generally been imagined.

I have ſhewn in another place, and I believe I may venture to ſay I have proved *, that Heat is actually propagated in *air*, in the ſame manner I here ſuppoſe it to be propagated in water; and if the conducting powers of both theſe fluids are found to be impaired by the *ſame means*, it affords very ſtrong grounds to conclude that they both conduct Heat in the *ſame manner:* but this has been found to be actually the caſe.

Eider-down, which cannot affect the ſpecifical qualities of either of thoſe fluids, and which certainly does no more when mixed with them, than merely to obſtruct and embarraſs their internal motions, has been found to retard very much the Propagation of Heat in both of them: on comparing theſe Experiments with thoſe I formerly made on the conducting power of air, it will even be found, that the conducting power of water is nearly, if not quite, as much impaired by a mixture of *eider-down* as that of air.

In the courſe of my Experiments on the various ſubſtances uſed in forming artificial clothing for confining Heat, I found that the thickneſs of a ſtratum of air, which ſerved as a barrier to Heat, remaining the ſame, the paſſage of Heat through it was ſometimes rendered more difficult by in-

* See Philoſophical Tranſactions, 1792.

creaſing

creafing the quantity of the light fubftance which was mixed with it to obftruct its internal motion.

To fee if fimilar effects would be produced by the fame means when Heat is made to pafs through water, I repeated the Experiments with *eider-down*, reducing the quantity of it mixed with the water to 48 grains, or *one quarter* of the quantity ufed in the Experiments No. 11 and No. 12.

The refults of thefe Experiments, and a comparifon of them with thofe before mentioned, may be feen in the following Tables:

	Time the Heat was in paffing INTO the Thermometer,		
	Through Water with 48 grs. or 1/16 of its bulk of EIDER-DOWN.	Through Water with 192 grs. or 1/16 of its bulk of EIDER-DOWN.	Through pure WATER
	Experiment No. 13.	Experiment No. 11.	Mean of two Exp. N° 5 & N° 7.
	Seconds.	Seconds.	Seconds.
In heating the Thermometer from 32° to 40°	51	83	45
from 40° to 60°	47	55	35½
60° to 80°	39	49	32½
100°	40	52	30
120°	45	57	36½
140°	56	67	44
160°	74	93	61½
180°	118	133	91½
200°	293	360	220½
Total times in heating from 32° to 200°	763	949	597
Times employed in heating the inftrument 80 degrees, or from 80° to 160°	215″	269″	172″

	Time the Heat was paſſing OUT OF the Thermometer,		
	Through Water with 48 grs. or $\frac{1}{30}$ of its bulk of EIDER-DOWN.	Through Water with 192 grs. or $\frac{4}{30}$ of its bulk of EIDER-DOWN.	Through pure WATER.
	Experiment No. 14.	Experiment No. 12.	Mean of two Exp. No 6 & No 8.
	Seconds.	Seconds.	Seconds.
In cooling the Thermometer from 200° to 180°	49	68	$41\frac{1}{2}$
from 180° to 160°	50	61	$39\frac{1}{2}$
160° to 140°	56	72	43
120°	70	91	$52\frac{1}{2}$
100°	96	120	73
80°	151	177	$108\frac{1}{2}$
60°	262	279	202
40°	661	673	472
Total times in cooling from 200° to 40°	1395	1541	1032
Times employed in cooling the inſtrument 80 degrees, viz. from 160° to 80°	373″	460″	277″

The reſults of theſe Experiments are extremely intereſting. They not only make us acquainted with a new and very curious fact,—namely, that feathers, and other like ſubſtances, which, in air, are known to form very warm covering for confining Heat, not only ſerve the ſame purpoſe in water, but that their effects in preventing the paſſage of Heat is even greater in water than in air.

This

This discovery, if I do not deceive myself, throws a very broad light over some of the most interesting parts of the economy of Nature, and give us much satisfactory information respecting the final causes of many phænomena which have hitherto been little understood.

As *liquid water* is the vehicle of Heat and nourishment, and consequently of life, in every living thing; and as water, left to itself, freezes with a degree of cold much less than that which frequently prevails in cold climates, it is agreeable to the ideas we have of the wisdom of the Creator of the world, to expect that effectual measures would be taken to preserve a sufficient quantity of that liquid in its fluid state, to maintain life during the cold season: and this we find has actually been done; for both plants and animals are found to survive the longest and most severe winters; but the means which have been employed to produce this miraculous effect have not been investigated;—at least not, in as far as they relate to vegetables.

But as animal and vegetable bodies are essentially different in many respects, it is very natural to suppose, that the means would be different which are employed to preserve them against the fatal effects which would be produced in each by the congelation of their fluids.

Among organized bodies, which live on the surface of the earth, and which of course are exposed to the vicissitudes of the seasons, we find that as the proportion of fluids to solids is greater,

 the

the greater is the Heat which is required for the ſupport of life and health; and the leſs are they able to endure any conſiderable change of their temperature.

The proportion of Fluids to Solids is much greater in *animals* than in vegetables; and in order to preſerve in them the great quantity of Heat which is neceſſary to the preſervation of life, they are furniſhed with lungs, and are warmed by a proceſs ſimilar to that by which Heat is generated in the combuſtion of inflammable bodies.

Among *vegetables*, thoſe which are the moſt ſucculent are *annual*. Not being furniſhed with lungs to keep the great maſs of liquids warm, which fill their large and ſlender veſſels, they live only while the genial influence of the ſun warms them, and animates their feeble powers; and they droop and die as ſoon as they are deprived of his ſupport.

There are many tender plants to be found in cold countries, which die in the autumn, the roots of which remain alive during the winter, and ſend off freſh ſhoots in the enſuing ſpring. In theſe we ſhall conſtantly find the roots more compact and denſe than the ſtalk, or with ſmaller veſſels, and a ſmaller proportion of Fluids.

Among the trees of the foreſt, we ſhall conſtantly find, that thoſe which contain a great proportion of *thin watery liquids*, not only ſhed their leaves every autumn, but are ſometimes frozen, and actually killed, in ſevere froſts. Many thouſands of the largeſt walnut-trees were killed

killed by the froſt in the Palatinate, during the very cold winter in the year 1788; and it is well known that few, if any, of the deciduous plants of our temperate climate would be able to ſupport the exceſſive cold of the frigid zone.

The trees which grow in thoſe inhoſpitable climates, and which brave the colds of the ſevereſt winters, contain very little watery liquids. The ſap which circulates in their veſſels is thick and viſcous, and can hardly be ſaid to be fluid. Is there not the ſtrongeſt reaſon to think, that this was ſo contrived for the expreſs purpoſe of preventing their being deprived of all their Heat, and killed by the cold during the winter?

We have ſeen by the foregoing Experiments, how much the Propagation of Heat in a liquid is retarded by diminiſhing its fluidity; and who knows but this may continue to be the caſe, as long as any degree of fluidity remains?

As the bodies and branches of trees are not covered in winter by the ſnow which protects their roots from the cold atmoſphere, it is evident that extraordinary meaſures were neceſſary to prevent their being frozen. The bark of all ſuch trees as are deſigned by nature to ſupport great degrees of cold, forms a very warm covering; but this precaution alone would certainly not have been ſufficient for their protection. The ſap, in all trees which are capable of ſupporting a long continuance of froſt, grows thick and viſcous on the approach of winter. What more important purpoſe could this change anſwer, than that here indicated?—

And

And it would be more than folly to pretend that it anſwers no uſeful purpoſe at all.

We have ſeen, by the reſults of the foregoing Experiments, how much the ſimple embarraſſment of liquids in their internal motions tends to retard the Propagation of Heat in them, and conſequently its paſſage out of them;—and when we conſider the extreme ſmallneſs of the veſſels in which the ſap moves in vegetables, and particularly in large trees;—when we recollect that the ſubſtance of which theſe ſmall tubes are formed, is one of the beſt non-conductors of Heat known*;—and when we advert to the additional embarraſſments to the paſſage of the Heat, which ariſe from the increaſed viſcoſity of the ſap in winter, and to the almoſt impenetrable covering for confining Heat, which is formed by the bark, we ſhall no longer be at a loſs to account for the preſervation of trees during the winter, notwithſtanding the long continuation of the hard froſts to which they are annually expoſed.

* I lately, by accident, had occaſion to obſerve a very ſtriking proof of the extreme difficulty with which Heat paſſes in wood. Being preſent at the foundery at Munich, when cannons were caſting, I obſerved that the founder uſed a wooden inſtrument for ſtirring the melted metal. It was a piece of oak plank, green, or unſeaſoned, about ten inches ſquare and two inches thick, with a long wooden handle, which was fitted into a hole in the middle of it. As this inſtrument was frequently uſed, and ſometimes remained a conſiderable time in the furnace, in which the Heat was moſt intenſe, I was ſurpriſed to find that it was not conſumed; but I was ſtill more ſurpriſed, on examining the part of the plank which had been immerſed in the melted metal, to find that the Heat had penetrated it to ſo inconſiderable a depth, that, at the diſtance of one-twentieth of an inch below its ſurface, the wood did not ſeem to have been in the leaſt affected by it. The colour of the wood remained unchanged, and it did not appear to have loſt even its moiſture.

On the ſame principles we may, I think, account, in a ſatisfactory manner, for the preſervation of ſeveral kinds of fruit,—ſuch as apples and pears for inſtance,—which are known to ſupport, without freezing, a degree of cold, which would ſoon reduce an equal volume of *pure water* to a ſolid maſs of ice.

At the ſame time that the compact ſkin of the fruit effectually prevents the evaporation of its fluid parts, which, as is well known, could not take place without occaſioning a very great loſs of Heat, the internal motions of thoſe fluids are ſo much obſtructed by the thin partitions of the innumerable ſmall cells in which they are confined, that the communication of their Heat to the air ought, according to our hypotheſis, to be extremely ſlow and difficult. Theſe fruits do, however, freeze at laſt, when the cold is very intenſe; but it muſt be remembered, that they are compoſed almoſt entirely of liquids, and of ſuch liquids as do not grow viſcous with cold; and moreover, that they were evidently not deſigned to ſupport, for a long time, very ſevere froſts.

Parſnips and carrots, and ſeveral other kinds of roots, ſupport cold without freezing, ſtill longer than apples and pears, but theſe are leſs watery, and I believe the veſſels in which their fluids are contained, are ſmaller; and both theſe circumſtances ought, according to our aſſumed principles, to render the paſſage of their Heat out of them more difficult, and conſequently to retard their congelation.

But

But there is ſtill another circumſtance, and a very remarkable one indeed, which, if our conjectures reſpecting the manner in which Heat is propagated in liquids be true, muſt act a moſt important part in the preſervation of Heat, and conſequently of animal and vegetable life, in cold climates. But as the probability of all theſe deductions muſt depend, very much, on the evidence which is brought to prove the great fundamental fact on which they are eſtabliſhed;—that reſpecting the internal motions among the particles of liquids, which *neceſſarily* take place when they are heated or cooled;—before I proceed any farther in theſe ſpeculations, I ſhall endeavour to throw ſome more light on that curious and intereſting ſubject.

CHAP. II.

Farther Investigations of the internal Motions among the Particles of Liquids, which necessarily take place when they are heated or cooled.—Description of a mechanical Contrivance, by which these Motions in Water were rendered visible.—An Account of various amusing Experiments, which were made with this new-invented Instrument.—They lead to an important Discovery.—Heat cannot be propagated DOWNWARDS *in Liquids, as long as they continue to be condensed by Cold.—Ice found, by Experiment, to melt more than eighty times slower, when boiling-hot Water stood on its Surface, than when the Ice was suffered to swim on the Surface of the hot Water.—The melting of Ice by Water standing on its Surface can be accounted for, even on the Supposition that Water is a perfect Non-conductor of Heat.—According to the assumed Hypothesis, Water only eight Degrees of Fahrenheit's Scale above the freezing Point, or at the Temperature of 40°, ought to melt as much Ice, in any given Time, when standing on its Surface, as an equal Volume of that Fluid, at any higher Temperature, even were it boiling hot.—This remarkable Fact is proved by a great Variety of decisive Experiments.—Water at the Temperature of 41° is found to melt even* MORE *Ice, when standing on its Surface, than boiling-hot Water.—The*

 Results

Results of all these Experiments tend to prove that Water is, in fact, a perfect Non-conductor of Heat; or that Heat is propagated in it, merely in consequence of the Motions it occasions among the insulated or solitary Particles of that Fluid, which, among themselves, have no Communication or Intercourse whatever in this Operation.—The Discovery of this Fact opens to our View one of the grandest and most interesting Scenes in the Economy of Nature.

As the particles of water, as also of all other Fluids, are infinitely too small to be seen by human eyes, their motions must of course be imperceptible by us; but we are frequently enabled to judge, with the utmost certainty, of the motions of invisible Fluids, by the motions they occasion in visible bodies. Air is an invisible Fluid, but we acquire very just notions of the motions in air, by the dust, and other light bodies which are carried along with it in its motions. Nobody who has ever seen a whirlwind sweep over the surface of a ploughed field in dry weather, can have any doubt respecting the nature of the motions, into which the air is thrown on those occasions; notwithstanding that they are extremely complicated, and would be very difficult to describe.

It was by the motions of the very fine particles of dust, which by accident had been mixed with the spirits of wine in my large thermometer, and which, when strongly illuminated by the direct beams of the sun, became visible, that I first discovered the internal motions in that Fluid, which take place when it is cooling; and, availing myſelf of this kind hint, I contrived to render the internal motions of water equally viſible. This, I immediately ſaw, could be done with the utmoſt facility, if I could but find any ſolid body of the ſame ſpecific gravity as water, which would be proper to mix with it;—that is to ſay, that would not be liable to be diſſolved by it, or to be reduced to ſuch ſmall particles as to become itſelf inviſible; but ſuch a ſubſtance was not to be found. On reflection it occurred to me, that it is very fortunate that ſuch ſubſtances do not abound; for otherwiſe we ſhould find great difficulty in procuring water in a pure ſtate.

Not being able to find any ſolid ſubſtance fit for my purpoſe, of the ſame ſpecific gravity as pure water, I was obliged to have recourſe to the following ſtratagem.

Looking over the tables of ſpecific gravities, I found that the ſpecific gravity of tranſparent yellow amber was but a little greater than that of water, being 1.078, while that of water is 1.000; and it occurred to me, that by diſſolving a certain quantity of pure alkaline ſalt, I might augment its ſpecific gravity, or rather bring the ſpecific gravity of the ſolution to be preciſely equal to that of the amber, without impairing the tranſparency of the liquid, or changing any of its properties, by which the manner of its receiving and tranſporting Heat could be ſenſibly affected.

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This contrivance was put in execution in the following manner, with complete ſucceſs. Having provided myſelf with a number of glaſs globes of various ſizes, with long cylindrical necks, I choſe one which was about two inches in diameter, with a cylindrical neck $\frac{3}{4}$ of an inch in diameter, and twelve inches long; and putting into it about half a tea-ſpoonful of yellow amber, in the form of a coarſe powder, (the pieces, which were irregular in their forms, and tranſparent, being about the ſize of muſtard-ſeeds,) I poured upon it a certain quantity of diſtilled water, which was at the temperature of the air in my room,—(about 60° F.)

Finding, as I expected, that the amber remained at the bottom of the globe, I now added to the water as much of a ſaturated ſolution of pure vegetable alkali, as was ſufficient to increaſe the ſpecific gravity of the water, (or rather of the diluted ſaline ſolution,) till the pieces of amber began to float, and remained apparently motionleſs, in any part of the liquid where they happened to reſt.

As the glaſs body was not yet as full as I wiſhed, I continued to add more of the alkaline ſolution, and of water, in due proportions, till the globe was full; and alſo till its cylindrical tube was filled to within about three inches of its end; and then cloſed it well with a clean cork.

Having ſhaken the contents of this glaſs body well together, I placed it, with its cylindrical tube in a vertical poſition, on a wooden ſtand, and left it to repoſe in quiet, in order to ſee how long the ſolid particles of amber—(which appeared to be very equally diſperſed about in the whole maſs of the liquid)—would remain ſuſpended.

Though the greater number of theſe particles ſeemed at firſt to have no tendency either to aſcend or to deſcend, yet ſome of them ſoon began to move very ſlowly upwards, and others to move as ſlowly downwards; and as theſe particles were moving at the ſame time promiſcuouſly in all parts of the ſame liquid, and even in the ſame part of it in both directions at the ſame time, the aſcending and deſcending particles frequently paſſing each other ſo near as to touch, I ſaw that theſe motions were independent of any internal motion of the liquid, and aroſe merely from the difference of the ſpecific gravity of the different ſmall pieces of the amber, and of that of the liquid. Some of the pieces of amber, being evidently heavier than the liquid, moved downward, while others which were lighter aſcended to its ſurface.

Finding that there was ſo much difference in the ſpecific gravities of the different pieces of amber, I now added more of this ſubſtance to the liquid, and ſuffering it to ſubſide after I had ſhaken it well together, I gently poured off what had riſen to the top of the liquid, and retaining only that which had ſettled at the bottom of it, I increaſed the ſpecific gravity of the liquid by adding a little of the alkaline ſolution, till the ſmall pieces of amber which remained in the glaſs were juſt buoyed up and ſuſpended in the different parts of the Fluid, where they ſeemed to have taken their permanent ſtations.

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I had now an inſtrument which appeared to me to be well calculated for the very intereſting Experiments I had projected, and it will eaſily be imagined that I loſt no time in making uſe of it.

The firſt Experiment I made with this inſtrument was to plunge it into a tall glaſs jar, nearly filled with water almoſt boiling hot. The reſult was juſt what I expected. Two currents, in oppoſite directions, began at the ſame inſtant to move with great celerity in the liquid in the cylindrical tube, the aſcending current occupying the ſides of the tube, while that which moved downwards occupied its axis.

As the ſaline liquor grew warm, the velocity of theſe currents gradually diminiſhed; and at length, when the liquor had acquired the temperature of the ſurrounding water in the jar, theſe motions ceaſed entirely.

On taking the glaſs body out of the hot water, the internal motions of the liquor recommenced; but the currents had changed their directions, that which occupied the axis of the tube being now the aſcending current.

When the cylindrical tube, inſtead of being held in a vertical poſition, was inclined a little, the aſcending current occupied that ſide of it which happened to be uppermoſt, while the under ſide of it was occupied by the current which moved (with equal velocity) downwards.

When the contents of the glaſs body had acquired the temperature of the air of the room, theſe

these motions ceafed, but they immediately recommenced on expofing the inftrument to any change of temperature.

In all cafes where the inftrument *received Heat*, the current in the axis of its cylindrical tube, when it was placed in a vertical pofition, (and that which occupied its *upper fide* when it was inclined,) moved downwards.—When it parted with Heat its motion was in an oppofite direction, that is to fay, *upwards*.

A change of temperature amounting only to a few degrees of Fahrenheit's fcale, was fufficient to fet the contents of the inftrument in motion; and the motion was more or lefs rapid as the velocity was greater or lefs with which it acquired or parted with Heat, and the motion was moft rapid in thofe parts of the inftrument where the communication was not rapid.

A partial motion might at any time be produced in any part of the inftrument, by applying to that part of it any body either hotter or colder than the inftrument. If the body fo applied were hotter than the inftrument, the motion of the faline liquor in it in that part of it immediately in contact with the hot body, was *upwards*,—if colder, downwards; and whenever a hot or cold body produced a current upwards or downwards, this current immediately produced another in fome other part of the liquid which flowed in an oppofite direction.

On inclining the cylindrical tube of the inftrument to an angle of about 45 degrees with the plane of the horizon, and holding the middle of it over the

flame

flame of a candle, at the diſtance of three or four inches above the point of the flame; the motion of the Fluid in the upper part of the tube became exceſſively rapid, while that in the lower end of it where it was united to the globe, as well as that in the globe itſelf, remained almoſt perfectly at reſt.

I even found that I could make the Fluid in the upper part of the tube *actually boil*, without that in the lower part of it appearing to the hand to be ſenſibly warmed. But when the flame was directed againſt the lower part of the tube, all the upper parts of it in contact with the liquid, and eſpecially that ſide of it which was uppermoſt as it lay in an inclined poſition, where the aſcending current was moſt rapid, where it impinged againſt the glaſs, were very ſoon heated very hot.

The motions in oppoſite directions, in the liquid in the tube, were exceedingly rapid on this ſudden application of a ſtrong Heat, and afforded a very entertaining ſight:—but to a ſcientific obſerver they were much more than amuſing. They detected Nature, as it were, in the very act, in one of her moſt hidden operations, and rendered motions viſible in the midſt of an inviſible medium which never had been ſeen before, and which moſt probably had never been ſuſpected.

Encouraged by this ſucceſs, and confirmed in my opinions reſpecting the intereſting fact I had undertaken to inveſtigate; I now proceeded with confidence to ſtill more direct and deciſive Experiments.

It is an opinion which, I believe, is generally received among philofophers, that water cannot be heated in contact with ice: reflecting on the fubject, I immediately perceived that either this muft be a miftake, or all my ideas refpecting the manner in which Heat is propagated in that Fluid muft be erroneous. I faw that as long as the ice floats at the furface of water which is attempted to be warmed over a fire, (or in any other way,) the ice-cold water which refults from the melting of the ice, muft, according to my own hypothefis, defcend, and fpreading over the bottom of the containing veffel, and, before it has time to be much heated, being in its turn forced to give place to the ice-cold water, which, as long as any ice remains, continues to defcend in an uninterrupted ftream as long as this operation is going on, the mafs of the water cannot be much heated; but on the fuppofition that water is not a conductor of Heat, according to the common acceptation of that term, or that Heat cannot pafs in that Fluid except when it is *carried* by its particles, which, being put in motion by the change it occafions in their fpecific gravity, *tranfports* it from place to place, it does not appear how ice, if inftead of being permitted to fwim on water, were confined at the bottom of it, or at any given diftance below its furface, could in any way affect the temperature of the fuperincumbent water, or prevent its receiving Heat from other bodies.

Were water a conductor of Heat, there is no doubt but that the influence of the prefence of the ice would be propagated in the water in all directions.

The metals are all conductors of Heat, and Professor PICTET found, by an ingenious and decisive Experiment *, that in a bar of copper 33 inches in length, placed in a vertical position, Heat passed downwards as well as upwards, and nearly with the same facility in both these directions; and if it can be shown that Heat cannot descend in water, that alone will, I imagine, be thought quite sufficient to prove that water is not a conductor of Heat.

When we meditate profoundly on the nature of Fluidity, it seems to me that we can perceive some faint lights which might lead us to suspect that the *cause*, and I may say the very *essence of fluidity*, is that property which the particles of bodies acquire when they become fluid, by which all farther interchange or communication of Heat among them is prevented. But however this may be, the result of the following Experiments will certainly be considered as affording indisputable evidence of one important fact, respecting the manner in which Heat is propagated in water.

Experiment, No. 15.

Into a cylindrical glass jar 4.7 inches in diameter, and 14 inches high, I fitted a circular cake of ice nearly as large as the internal diameter of the jar, and $3\frac{1}{2}$ inches thick, weighing $10\frac{1}{8}$ oz.

* Essais de Physique, tome 1. Genève 1790.

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This cake of ice being ready, I now poured into the jar 6 lb. 1¼ oz. Troy, of boiling-hot water, and putting the ice gently into it, I found that it was entirely melted in 2 minutes and 58 feconds.

Having found by this Experiment how long the ice was in melting at the furface of the hot water, I now endeavoured to find out whether it would not require a longer time to melt at the bottom of the water.

Experiment, No. 16.

Into the fame jar which was ufed in the foregoing Experiment, I now put a cake of ice of the fame form and dimenfions as that above defcribed, but inftead of letting it fwim at the furface of the hot water, I faftened it down on the bottom of the jar, and poured the water upon it.

This cake of ice was faftened down in the jar by means of two flender and elaftic pieces of deal about ⅛ of an inch thick, and ¼ of an inch wide, which being a trifle longer than the internal diameter of the jar, were of courfe a little bent when they were introduced into it in an horizontal pofition, and on being put down upon the ice, at right angles to each other, ferved to confine the ice, and prevent its rifing up to the furface when the water was put into the jar upon it.

To protect the ice while the boiling-hot water was pouring into the jar, its furface was covered with a circular piece of ftrong writing paper, which was afterwards removed as gently as poffible by means

means of a ſtring which was faſtened to one ſide of it; and to prevent the glaſs jar from being cracked by the ſudden application of the boiling-hot water, I began by pouring a ſmall quantity of cold water into the jar,—juſt enough to fill up the interſtices between the ice and the glaſs, and to cover the ice to the height of about $\frac{1}{4}$ of an inch; and in pouring the hot water into the jar, out of a large tea-kettle in which it had been boiled, I took care to direct the ſtream againſt the middle of the circular piece of paper which covered the ice.

The jar with the ice and the hot water in it being placed on a table near a window, I drew away as gently as poſſible the paper which covered the ſurface of the ice, and prepared myſelf to obſerve at my eaſe the reſult of this moſt intereſting Experiment.

A very few moments were ſufficient to ſhow me that my expectation with regard to it would not be diſappointed. In the former Experiment a ſimilar cake of ice had been entirely melted in leſs than three minutes; but in this, after more than twice that time had elapſed, the ice did not ſhow any apparent ſigns of even *beginning to melt*. Its ſurface remained ſmooth and ſhining, and the water immediately in contact with it appeared to be perfectly at reſt, though the internal motions of the hot water above it, which was giving off its heat to the ſides of the jar and to the air, were very rapid, as I could diſtinctly perceive by means of ſome earthy particles

or

or other impurities which this water happened to contain.

I examined the ice with a very good lens, but it was a long time before I could perceive any figns of its melting. The edges of the cake remained fharp, and the minute particles of duft, which by degrees were precipitated by the hot water as it grew colder, remained motionlefs as foon as they touched the furface of the ice.

As the hot water had been brought from the kitchen in a tea-kettle, it was not quite boiling hot when it was poured into the jar. After it had been in the jar one minute, I plunged a thermometer into it, and found its temperature to be at 180°.

After 12 minutes had elapfed, its temperature at the depth of one inch under the furface was 170°. At the depth of feven inches, or one inch above the furface of the ice, it was at $169\frac{1}{10}$; while at only $\frac{3}{4}$ of an inch lower, or $\frac{1}{4}$ above the furface of the ice, its temperature was 40°.

When 20 minutes had elapfed, the Heat in the water at different depths was found to be as follows:

Immediately above the furface of the ice	40°
At the diftance of $\frac{1}{2}$ an inch above it	46°
At 1 inch - - -	130°
At 3 inches - - -	159°
At 7 inches - - -	160°

When 35 minutes had elapfed, the Heat was as follows:

At the furface of the ice -	40°
$\frac{1}{2}$ an inch above it - -	76°

1 inch

1 inch above it - - -	110°
2 inches - - -	144°
3 inches - - -	148°
5 inches - - -	148½°
7 inches - - -	149°

At the end of one hour the Heat was as follows:

At the ſurface of the ice -	40°
1 inch above it - -	80°
2 inches - - -	118°
3 inches - - -	128°
4 inches - - -	130°
7 inches - - -	131°

After 1 hour and 15 minutes had elapſed, the Heat was found to be as follows:

At the ſurface of the ice -	40°
1 inch above it - -	82°
2 inches - - -	106°
3 inches - - -	123°

The Heat of the water had hitherto been taken near the ſide of the jar;—in the two following trials it was meaſured in the middle or axis of the jar.

When 1 hour and 30 minutes (reckoning always from the time when the boiling-hot water was poured into the jar) had elapſed, the Heat of the water in the middle of the jar was found to be as follows:

At the ſurface of the ice -	40°
1 inch above it - -	84°
2 inches - - -	115°
3 inches - - -	116°
7 inches - - -	117°

 When

When 2 hours had elapſed, the Heat in the middle of the jar was found to be as follows:

At the ſurface of the ice -	40°
1 inch above it - -	76°
2 inches - - -	94°
3 inches - - -	106°
4 inches - - -	108°
6 inches - - -	$108\frac{1}{4}$°
7 inches - - -	$108\frac{1}{2}$°

An end being now put to the Experiment, the hot water was poured off from the ice, and on weighing that which remained, it was found that 5 oz. 6 grains Troy (=2406 grains) of ice had been melted.

Taking the mean temperature of the water at the end of the Experiment at 106°, it appears that the maſs of hot water (which weighed $73\frac{1}{4}$ ounces) was cooled 78 degrees, or from the temperature of 184° to that of 106°, during the Experiment. Now, as it is known that one ounce of ice abſorbs juſt as much Heat in being changed to water as one ounce of water loſes in being cooled 140 degrees, it is evident that one ounce of water which is cooled 78 degrees, gives off as much Heat as would be ſufficient to melt $\frac{78}{140}$ of an ounce of ice; conſequently the $73\frac{1}{4}$ ounces of hot water, which in this Experiment were cooled 78 degrees, actually gave off as much Heat as would have been ſufficient to have melted $\frac{73\frac{1}{4} \times 78}{140} = 40\frac{8}{10}$ ounces of ice.

But, the quantity of ice actually melted was only about five ounces; and hence it appears that

leſs

less than one-eighth part of the Heat lost by the water was communicated to the ice; the rest being carried off by the air.

As the same quantity of hot water was used in this Experiment, and in that, No. 15, which immediately preceded it, and as this water was contained by the same vessel,—(the glass jar above described,)—it appears that ice melts more than *eighty times slower* at the bottom of a mass of boiling-hot water, than when it is suffered to swim on its surface: For, as in the Experiment, No. 15, $10\frac{1}{8}$ oz. of ice were melted in 2 minutes and 58 seconds, 5 ounces at least must have been melted in 1 minute and 29 seconds; but in the Experiment No. 16, 2 hours or 120 minutes were employed in melting 5 ounces.

The ice however *was melted*, though very slowly, at the bottom of the hot water; and that circumstance alone would have been sufficient to have overturned my hypothesis respecting the manner in which Heat is propagated in liquids, had I not found means to account in a satisfactory manner for that fact, without being obliged to abandon my former opinions.

In about half an hour after the hot water had been poured into the jar, in the last Experiment, examining the surface of the ice I discovered an appearance which fixed my attention and excited all my curiosity; I perceived that the ice had been melted and diminished at its surface, excepting only where it had been covered, or as it were *shadowed*, by the flat flips of deal by which the cake of ice was faftened down in its place.

Had the ice been protected and prevented from being melted by that piece of the wood *only*, which, being undermoft of the two, repofed immediately on the furface of the ice, I fhould not perhaps have been much furprifed; but that part of the furface of the ice being likewife protected which was fituated immediately under the other piece of wood,—that which, lying acrofs the under piece, and refting on it, *did not touch the ice any where except juft at its edge*,—that circumftance attracted my attention; and I could at firft fee no way of accounting for thefe appearances but by fuppofing that the ice had been melted by the *calorific rays* which had been emitted by the hot water; and that thofe parts of the ice which had been *fhadowed* by the pieces of deal, receiving none of thefe rays, had of courfe not been melted.

I was fo much ftruck with thefe appearances, that I immediately made the following Experiments, with a view merely to the elucidation of this matter.

Experiment, No. 17.

Into a cylindrical glafs jar, 6½ inches in diameter, and 8 inches high, I put a circular cake of ice, as large as could be made to enter the jar, and about 3½ inches thick; and on the flat and even furface of the ice I placed a circular plate of the thinneft tin I could procure, near 6½ inches in diameter,

meter, or ſufficiently large juſt to cover the ice. This plate of tin (which, to preſerve its form, or keep it quite flat, was ſtrengthened by a ſtrong wire, which went round it at its circumference) had a circular hole in its centre, juſt two inches in diameter, and it was firmly fixed down on the upper ſurface of the cake of ice, by means of ſeveral thin wooden wedges which paſſed between its circumference and the ſides of the jar.

A ſecond circular plate of tin, with a circular hole in its centre two inches in diameter, and in all other reſpects exactly like that already deſcribed, was now placed over the firſt, and parallel to it, at the diſtance of juſt one inch, and like the firſt was firmly fixed in its place by wooden wedges.

Theſe perforated circular plates being fixed in their places, the jar was placed in a room where Fahrenheit's thermometer ſtood at 34°; and ice-cold water was poured into it till the water juſt covered the upper plate; and then the jar was filled to within half an inch of its brim with boiling water: and being covered over with a board, was ſuffered to remain quiet two hours.

At the end of this time, the water, which was ſtill warm, was poured off, and the circular plate being removed, the ice was examined.

A circular excavation, juſt as large as the hole in the tin-plate which covered the ice, (namely two inches in diameter,) and correſponding with it,—perfectly well defined, and about $\frac{1}{10}$ of an inch deep in the centre, had been made in the ice.

 This

This was what I expected to find; but there was something more, which I did not expect, and which, for some time, I was quite at a loss to account for. Every part of the surface of the ice which had been covered by the tin plate, appeared to be perfect, level, and smooth, and showed no signs of its having been melted or diminished, excepting only in one place, where a channel, about an inch wide, and a little more than $\frac{2}{10}$ of an inch deep, which showed evident marks of having been formed by a stream of warm water, led from the excavation just mentioned, in the centre of the upper part of the cake of ice, to its circumference. As the edge, or vertical side, of the cake of ice was evidently worn away where this stream passed, there could be no doubt with respect to its direction. It certainly ran *out of* the circular excavation in the middle of the ice; and though it might at first appear difficult to explain the fact, and to show how this hot water could arrive at that place, yet it was quite evident that the immediate cause of the motion of this stream of water could be no other than its specific gravity being greater than that of the rest of the water at the same depth: and that this greater specific gravity was at the same time accompanied by a higher degree of Heat, is evident from the deep channel which this stream had melted in the ice, while other parts of the surface of the ice, at the same level, were not melted by the water which rested on it. To elucidate this point, I made the following Experiment:

Expe-

Experiment, No. 18.

Thinking it probable, that if the circular excavation in the ice, which anſwered to the circular hole in the middle of the tin-plate which covered the ice, and alſo to that in the ſecond plate which was placed an inch higher, had been melted by *radiant Heat*, (as it has improperly been called,) or by the calorific rays from the hot water; then, in that caſe, as ſome of theſe rays muſt probably have been reflected downwards at the ſurface of the water, in attempting to paſs upwards into the air, I thought that by preventing this part of them from reaching the ice, which I endeavoured to do by cauſing them to be abſorbed by a light black body, (a circular piece of deal board, covered over with black ſilk,) which I cauſed to ſwim on the ſurface of the water, their effects in melting the ice might perhaps be ſenſibly diminiſhed. Had this really been the caſe, it would certainly have afforded ſtrong grounds to ſuſpect that theſe rays were in fact the cauſe of the appearances in queſtion; but on making the Experiment with the greateſt care, I could not perceive that the covering of the ſurface of the hot water with a black body produced any difference whatever in the reſult of the Experiment as it was firſt made, (Experiment No. 17,) or when this black covering was not uſed.

After ſome meditation on the ſubject, it occurred to me that this melting of the ice at its upper ſurface could be accounted for, in a manner which appeared to me to be perfectly ſatisfactory; without ſuppoſing either that water is a conductor of Heat, or that the effect in queſtion was produced by calorific rays.

Though it is one of the moſt general laws of nature with which we are acquainted, that all bodies, ſolids as well as fluids, are condenſed by cold; yet, in regard to *water*, there appears to be a very remarkable exception to this law. Water, like all other known bodies, is indeed condenſed by cold at every degree of temperature which is conſiderably higher than that of freezing, but its condenſation, on parting with Heat, does not go on till it is changed to ice; but when in cooling its temperature has reached to the 40th degree of Fahrenheit's ſcale, or eight degrees above freezing, it ceaſes to be farther condenſed; and on being cooled ſtill farther, *it actually expands*, and continues to expand, as it goes on to loſe more of its Heat, till at laſt it freezes; and at the moment when it becomes ſolid, and even after it has become ſolid, it expands ſtill more, on growing colder. This fact, which is noticed by M. DE LUC, in his excellent treatiſe on the modifications of the atmoſphere, has ſince been farther inveſtigated and put beyond all doubt, by SIR CHARLES BLAGDEN. See Philoſophical Tranſactions, vol. lxxviii.

Now, as water in contact with melting ice is always at the temperature of 32°, it is evident that water at that temperature muſt be ſpecifically lighter than water which is eight degrees warmer, or or at the temperature of 40°; consequently, if two parcels of water at these two temperatures be contained in the same vessel, that which is the coldest and lightest must necessarily give place to that which is warmer and heavier, and currents of the warmer water will *descend* in that which is colder.

In the two last Experiments, as the circular tin-plate which covered the surface of the ice served to confine the thin sheet of water which was between the plate and the ice, as this water could not rise upwards, being hindered by the plate, and as it had no tendency to descend, it is probable that it remained in its place; and as it was *ice-cold*, it was not capable of melting the ice on which it reposed.

But as the tin-plate had a circular hole in its centre, the surface of the ice *in that part* was of course naked, and the ice-cold water in contact with it being displaced by the warmer and heavier water from above, an excavation, in the form of a shallow basin, was formed in the ice by this descending warm current.

The warm water contained in this basin overflowed its banks as soon as the basin began to be formed; and issuing out on that side which happened to be the lowest, opened itself a passage under the tin-plate to the edge of the ice, over which it was precipitated, and fell down to the bottom of the jar. The water of this rivulet being warm, it soon formed for itself a deep channel in the ice; and at the end of the Experiment it was found

found to be every where *deeper* than the bottom of the basin where it took its rise.

This manner of accounting for the appearances in question seemed to me to be quite satisfactory; and the more I meditated on the subject, the more I was confirmed in my suspicions that *all liquids* must necessarily be perfect *non-conductors of Heat*.

On these principles I was now enabled to account for the melting of the ice at the bottom of the hot water in the Experiment No. 16; as also for the slowness with which that process went on; —and encouraged by this success, I now proceeded with confidence to plan and to execute still more decisive Experiments; from the results of which, I may venture to say it,—the important facts in question have been put beyond all possibility of doubt.

If water be in fact a perfect *non-conductor* of Heat,—that is to say, if there be *no communication whatever of Heat* between neighbouring particles or *molécules* of that fluid, (which is what I suppose,) then,—as Heat cannot be propagated in it but *only* in consequence of the motions occasioned in the fluid by the changes in the specific gravity of those particles which are occasioned by the changes of their temperature, it follows that Heat cannot be propagated *downwards* in water, as long as that fluid continues to be condensed with cold; and that it *is only in that direction,* (downwards,) that it *can be propagated* after the water has arrived at that temperature, where it begins to be expanded by cold;—which has been found to be at about the 40th degree of Fahrenheit's scale.

Reasoning

Reafoning on thefe principles, we are led to this remarkable conclufion; namely, that *water which is only eight degrees above the freezing point, or at the temperature of 40°, muft be able to melt as much ice in any given time*, WHEN STANDING ON ITS SURFACE, *as an equal volume of water at any higher temperature*, EVEN THOUGH IT WERE BOILING HOT.

My philofophical reader will doubtlefs think that I muft have had no fmall degree of confidence in the opinion I had formed on this interefting fubject, to have had the courage to make, *even in private*, the Experiments which were neceffary to afcertain that fact.

Experiment, No. 19.

Into a cylindrical glafs jar, 4.7 inches in diameter, and 13.8 inches high, I put 43.87 cubic inches, or 1 lb. 11$\frac{1}{7}$ oz. Troy, in weight, of water, and placing the jar in a freezing mixture, compofed of pounded ice and common fea-falt, I caufed the water to freeze into one compact mafs; which adhered firmly to the bottom and fides of the jar, and which formed a cylinder of ice juft three inches high.

Had the bottom of the jar been quite flat, inftead of being raifed or vaulted, the cylinder of ice would have been no more than 2.67 inches high.

As foon as the water in the jar was completely frozen, the jar was removed from the freezing mixture, and placed in a mixture of pounded ice and pure water, where it was fuffered to remain four

four hours, in order that the cake of ice in the jar might be brought to the temperature of 32°.

The jar ſtill ſtanding in a ſhallow diſh in the pounded ice and water, the ſurface of which cold mixture was juſt on a level with the ſurface of the ice in the jar, I covered the top of the cake of ice with a circular piece of ſtrong paper, and poured gently into the jar 73½ oz. Troy of boiling-hot water, which filled it to the height of eight inches above the ſurface of the ice. (See Plate II.)

I then removed very gently the circular piece of paper which covered the ſurface of the ice, and after leaving the hot water in contact with the ice a certain number of minutes, I poured it off, and —weighing immediately the jar, and the unmelted ice which remained in it,—I aſcertained the *quantity of ice which had been melted* by the hot water during the time it had been ſuffered to remain in the jar.

This Experiment I repeated four times the ſame day, (16th March 1797,) varying at each repetition of it the time the water was permitted to remain on the ice. The reſults of theſe Experiments were as follows:

Number of the Experiment.	Time the hot water remained on the ice.	Temperature of the hot water when it was poured on the ice.	Temperature of the water 1 inch below its ſurface at the end of the Experiment.	Quantity of ice melted.
	Minutes.			Grains.
No. 19	1	186°	Not obſerved	1632
No. 20	3½	185°	Not obſerved	1824
No. 21	15	184°	170°	1757
No. 22	60	186°	140°	2573

From

From the refults of thefe Experiments, it was plain that a very confiderable portion of the ice which was melted, was melted in the very beginning of the Experiments, or while the hot water was actually *pouring* into the jar; which operation commonly lafted about one minute: and the irregularities in the refults of the Experiments, and particularly of the three firft, fhowed evidently that the quantity of ice melted in that operation was different in different Experiments. I had indeed forefeen that this would be the cafe; and on that account it was that I covered the furface of the ice with a circular piece of ftrong paper, and always took care to pour the water very gently into the jar: but I found that all thefe precautions were not fufficient to prevent very confiderable anomalies in the refults of the Experiments; and as I found reafon to fufpect that the motion in the mafs of the hot water, which was unavoidably occafioned by removing the circular piece of paper which covered the ice, was the principal caufe of thefe inaccuracies, I had recourfe to another, and a better contrivance.

Having procured a flat, fhallow difh, of light wood, half an inch deep, 4½ inches in diameter, (or fomething lefs than the internal diameter of the jar,)—and about ¼ of an inch thick at its bottom, I bored a great number of very fmall holes through its bottom, which gave it the appearance of a fieve. This perforated wooden difh having been previoufly made *ice-cold*, was placed on the furface of the ice in the jar; and the hot water being gently

gently poured into the diſh through a long wooden tube; as this perforated diſh floated and remained conſtantly at the ſurface of the water, and as the water paſſing through ſuch a great number (many hundreds) of ſmall holes, was not projected downwards with force, it is evident that by this ſimple contrivance thoſe violent motions in the maſs of water in the jar, which before took place when the hot water was poured into the ice, and when the paper which covered the ice was removed, were, in a great meaſure, prevented.

In order that the water which was poured through the wooden tube (the bore of which was about half an inch in diameter) might not impinge perpendicularly and with force againſt the bottom of the diſh, the lower end of the tube was cloſed by a fit cork-ſtopple, and the water was made to iſſue horizontally through a number of ſmall holes in the ſides of this tube, at its lower end.

As ſoon as the operation of pouring the hot water into the jar was finiſhed, the perforated diſh was carefully removed, and the jar was covered with a circular wooden cover, from the centre of which a ſmall mercurial thermometer was ſuſpended.

The effects produced by this new arrangement of the machinery will appear by comparing the reſults of the two following Experiments with thoſe juſt mentioned.

Number of the Experiment.	Time the hot water remained on the ice.	Temperature of the hot water,		Quantity of ice melted.
		At the beginning.	At the end.	
	Minutes.			Grains.
No. 23	1	196°	196°	423
No. 24	3	190°	188°	703

In order ſtill more effectually to prevent the inaccuracies ariſing from the internal motions in the maſs of hot water which were occaſioned in pouring the water into the jar, (and which could not fail to affect, more or leſs, the reſults of the Experiment,) I had recourſe to the following contrivance.

I filled a ſmall phial containing 8¾ cubic inches with ice-cold water, and then emptying the phial in the jar, I covered the ſurface of the ice with this ice-cold water to the height of 0.478 of an inch.

On the ſurface of this ice-cold water, inſtead of that of the ice, I now placed the perforated wooden diſh previouſly made ice-cold, and poured the hot water upon it.

The reſults of the following Experiments ſhow that this contrivance tended much to diminiſh the apparent irregularities of the Experiments.

The air of the room in which theſe Experiments were made was of the temperature of 41°

No. of the Experiment.	Time the hot water was on the ice.	Temperature of the hot water one inch below its surface, At the beginning	At the end.	Quantity of ice melted.
	Minutes.			Grains.
No. 25	10	192°	182°	580
No. 26	30	190°	165°	914
No. 27	180	190°	95°	3200

From the reſults of theſe laſt three Experiments, we can now determine with a very conſiderable degree of certainty how much ice was melted *in the act of pouring the water into the jar*, and conſequently the rate at which it was melted in the ordinary courſe of the Experiment ;—ſuppoſing equal quantities to be melted in equal times.

As in the 27th Experiment 3200 grains were melted in 180 minutes, and in the 25th Experiment 580 grains were melted in 10 minutes, we may ſafely conclude that the ſame quantity muſt have been melted in the ſame time (10 minutes) in the 27th Experiment ; if, therefore, from 3200 grains,—the quantity melted in 180 minutes in this laſt Experiment,—we deduct 580 grains for the quantity melted during the firſt 10 minutes, there will remain 2620 grains for the quantity melted in the ſucceeding 170 minutes, when the motions occaſioned in the water on its being poured into the jar having ſubſided, we may ſuppoſe the proceſs of melting the ice to have gone on regularly.

But if in the regular courſe of the Experiment, no more than 2620 grains were melted in 170 minutes, it is evident that not more than 154 grains could have been melted in the ordinary courſe of the proceſs in 10 minutes; for 170 minutes : 2620 grains : : 10 minutes : 154 grains.—If, therefore, from 580 grains, the quantity of ice actually melted in 10 minutes in the 25th Experiment, we deduct 154 grains, there remains 426 for the quantity melted in pouring the water into the jar.

Let us ſee now how far this agrees with the reſult of the 26th Experiment. In this Experiment 914 grains of ice were melted in 30 minutes. If from this quantity we deduct 426 grains, the quantity which according to the foregoing computation muſt have been melted *in pouring the hot water into the jar*, there will remain 478 grains for the quantity melted in the ordinary courſe of the proceſs in 30 minutes; which gives 159 grains for the quantity melted in 10 minutes; which differs very little from the reſult of the foregoing computation, by which it appeared to be=154 grains. This difference however, ſmall as it is, is ſufficient to prove an important fact, namely, that the effects produced by the motion into which the hot water had been thrown in being poured into the jar had not ceaſed entirely in 10 minutes, or when an end was put to the 5th Experiment. We ſhall therefore come nearer the truth, if, in our endeavours to diſcover the quantity of ice melted in any given time in the ordinary courſe of the Experiments, we found our computation on the reſults of the two Experiments No. 26 and No. 27.

In the latter of these Experiments 3200 grains of ice were melted in 180 minutes, and in the former 914 grains were melted in 30 minutes. If, therefore, from 3200 grains, the quantity melted in 180 minutes, we take the quantity melted in the first 30 minutes, = 914 grains, there will remain 2286 grains for the quantity melted in the succeeding 150 minutes, and this gives 152 grains for the quantity melted in 10 minutes. By the former computation it turned out to be 154 grains.

But if 152 grains of ice is the quantity melted in 10 minutes, in the ordinary course of the process, three times that quantity, or 456 grains only, could have been melted *in this manner* in the 30 minutes during which the 26th Experiment lasted; and deducting this quantity from 914 grains, the quantity actually melted in that Experiment, the remainder, 458 grains, shows how much must have been melted in the pouring the hot water on the ice, or in consequence of the motions into which the water was thrown in the performance of that operation. By the preceding computation this quantity turned out to be 426 grains.

From the result of these computations I think we may safely conclude, that in the ordinary course of the Experiments not more than 152 grains of ice were melted by the hot water in 10 minutes.

I shall now proceed to give an account of several Experiments in which the water employed to melt the ice was at a *much lower temperature*.

Having

Having removed a ſmall quantity of ice which remained unmelted in the bottom of the jar, I put a freſh quantity of water into it, and placing the jar in a freezing mixture, cauſed this water, which filled the jar to the height of four inches, to freeze into one ſolid maſs of ice. I then placed the jar in a ſhallow earthen diſh, and ſurrounded it to the height of the level of the top of the ice with a mixture of ſnow and water (ſee Plate II.); and placing it in a room in which there had been no fire made for many months, and in which the temperature of the air was at 41°, I let it remain quiet two hours, in order that the ice might acquire the temperature of 32°.

This being done, I took the jar out of the earthen diſh, and wiping the outſide of it dry with a cold napkin, I weighed the jar with the ice in it very exactly, and then replaced it in the earthen diſh, and ſurrounded it as before with ſnow and water, to the height of the level of the ſurface of the ice.

I then poured 73¾ ounces Troy (= 15,160 grains) of water, at the temperature of 41°, into the jar, which covered the ice to the ſame height to which it had been covered in the former Experiments,—namely, to about 8 inches; and ſuffering it to ſtand on the ice a certain number of minutes, I then poured it off, and wiping the outſide of the jar, weighed it, in order to aſcertain how much ice had been melted.

In putting this cold water into the jar, the ſame precautions were uſed (by pouring it through the wooden tube into the perforated wooden diſh, &c.) as were uſed when the Experiment was made with boiling the water.

The following Table ſhows the reſults of ſix Experiments made the ſame day, (the 19th March 1797) in the order in which they are numbered, and which were all made with the utmoſt care:

Number of the Experiment.	Temperature of the water in the jar 1 inch below its ſurface,		Temperature of the air.	Time the water remained on the ice.	Quantity of ice melted.
	At the beginning of the Exp.	At the end of the Exp.			
				Minutes.	Grains.
No. 28	41°	40°	41°	10	203
No. 29	41°	40°	41°	10	220
No. 30	41°	40°	41°	10	237
No. 31	41°	40°	41°	10	228
No. 32	41°	38°	41°	30	617
No. 33	41°	38°	41°	30	585

The agreement in the reſults of theſe Experiments is not much leſs extraordinary than the ſurpriſing fact which is proved by them,—namely, that boiling-hot water does not thaw more ice in any given time *when ſtanding quietly on its ſurface* than water at the temperature of 41°—or nine degrees only above the point of freezing!

There is reaſon to conclude that it does not even thaw ſo much;—and this ſtill more remarkable circumſtance may, I think, be accounted for in a ſatisfactory manner, on the ſuppoſition (which, however, I imagine will no longer be conſidered as

6 a bare

a bare fuppofition) that water is a non-conductor of Heat.

It appeared from the refults of the Experiments made with hot water, that the quantity of ice melted in 10 minutes in the ordinary courfe of that procefs amounted to no more than 152 grains;—but in thefe Experiments with cold water, the quantity melted in that time was never lefs than 203 grains, and taking the mean of four Experiments, it amounted to 222 grains.

There is one circumftance, however, refpecting thefe Experiments with cold water, which it is neceffary to inveftigate before their refults can be admitted as complete proof in the important cafe in queftion.

In the Experiments which were made with hot water, it was found that a confiderable part of the ice which was melted, was melted in confequence of the motions into which the water was thrown upon being poured into the jar, and that the effect of thefe motions continued to be fenfible for a longer time than moft of thefe Experiments with cold water lafted. Is it not poffible that the refults of thefe Experiments with cold water may alfo have been affected by the fame caufe? This is what I fhall endeavour to find out.

In the 32d Experiment 617 grains of ice were melted in 30 minutes, and in the 33d Experiment 585 grains were melted in the fame time; and taking the mean of thefe two Experiments it appears that 601 grains were melted in 30 minutes. If now from this quantity we deduct that which, according to the mean reſult of the four preceding Experiments, muſt have been melted in 10 minutes, namely, 222 grains, there will remain 379 grains for the quantity melted in the laſt 20 minutes in theſe two Experiments; conſequently, half this quantity, or 189½ grains, is what muſt have been melted in 10 minutes in the ordinary courſe of the proceſs.

But this quantity, (189½ grains,) though leſs than what was actually melted in the Experiments which laſted only 10 minutes, is ſtill conſiderably greater than 152 grains, the quantity which was found to have been melted in the ſame time in the ordinary courſe of the proceſs in thoſe Experiments in which hot water was uſed; conſequently the great queſtion, for the deciſion of which theſe Experiments were contrived, is,—I believe I may venture to ſay,—decided.

But, however concluſive the reſult of theſe Experiments appeared to me to be, I felt myſelf too much intereſted in the ſubject to reſt my inquiries here.

Having found, as well from the reſults of the Experiments made with cold water as from thoſe made with hot water, that a conſiderable quantity of ice was melted in the act of pouring the water into the jar, and in conſequence of thoſe undulatory motions into which the water was thrown in that operation, notwithſtanding all the pains I had taken to diminiſh thoſe motions and prevent their effects, I now doubled my precautions in guarding againſt thoſe ſources of error and uncertainty.

Before

Before I poured the water into the jar I covered the ſurface of the ice to the height 0.936 of an inch with ice-cold water, and this I did when water at the temperature of 41° was uſed, as well as in thoſe Experiments in which boiling-hot water was employed. In the former Experiments I had covered the ſurface of the ice with ice-cold water only in thoſe Experiments in which hot water was uſed, and even in thoſe I uſed only half as much ice-cold water as I now employed for that purpoſe.

I alſo now poured the water into the jar in a ſmaller ſtream, employing no leſs than three minutes in filling it up to the height of eight inches above the ſurface of the ice; and I endeavoured to aſcertain how far the reſults of the Experiments were influenced by the temperature of the air, and alſo by wrapping up the jar in warm covering.

The ſame jar was uſed in all the Experiments, and it was always placed in the ſame earthen diſh, and ſurrounded, to the level of the top of the ice, with melting ſnow. This jar is very regular in its form, being very nearly a perfect cylinder, and is on that account peculiarly well calculated for the uſe for which I ſelected it.

In each of the three firſt Experiments, which are entered in the following Table, the jar was well covered up with a very warm covering of cotton wool. This covering (which was above an inch thick) reached from the ſurface of the melting ſnow in which the jar ſtood, quite to the top of the jar. The mouth of the jar was firſt covered with a round wooden cover, (from the centre of which a thermo-

thermometer, the bulb of which reached one inch below the furface of the water, was fufpended) and on the top of this wooden cover there was put a thick covering of cotton.

In all the Experiments in the following Table, except the three firft, the jar was expofed naked to the air, except the lower part of it, which, as I have already more than once obferved, was always covered, as high as the ice in the jar reached, with melting fnow, or with pounded ice and water.

In the two Experiments No. 37 and No. 38, which are marked with afterifks, the furface of the ice was covered with ice-cold water to the depth of 0.478 of an inch only;—in all the other Experiments it was covered to the depth of 0.956 of an inch.

Number of the Experiment.	Temperature of the water in the jar 1 inch below its surface,		Temperature of the air.	Time the water remained on the ice.	Quantity of ice melted.
	In the beginning of the Exp.	At the end of the Exp.			
				Minutes.	Grains.
No. 34	188°	179°	41°	30	634
No. 35	189°	180°	41°	30	747
No. 36	190°	147°	41°	180	3963
No. 37	41°	38°	41°	30	592 *
No. 38	41°	43°	61°	30	676 *
No. 39	186°	157°	61°	30	559
No. 40	188°	156°	61°	30	575
No. 41	190°	156°	61°	30	542
No. 42	41°	43°	61°	30	573
No. 43	42°	44°	61°	30	575
No. 44	42°	35°	61°	120	2151

The refults of thefe Experiments afford matter for much curious fpeculation, but I fhall content myfelf for the prefent with making only two or three obfervations refpecting them. And in the firft place, it is remarkable, that although in the Experiments No. 34 and No. 35, of 30 minutes each, confiderably lefs ice was melted than in that No. 26, which lafted the fame time; yet, in that No. 36, of 180 minutes, more was melted than in that No. 27, of the fame duration. This difference in the two laft-mentioned Experiments will be accounted for hereafter.

With regard to the difference in the refults of the Experiments of 30 minutes, there is no doubt but that it arofe from the precautions which had been

been taken in this laſt ſet of Experiments to prevent the effect of the violent motions into which the hot water was thrown in being poured into the jar, that leſs ice was melted in the Experiments No. 34 and No. 35, than in that No. 26.

Secondly, It appears that more ice was melted in the ſame time, in the Experiments in which the jar was covered up with warm covering, than in thoſe in which it was left naked and expoſed to the air of the room.

The difference is even conſiderable. The quantity melted in 30 minutes when the jar was covered, at a mean of two Experiments, (No. 34 and No. 35,) was 690½ grains;—but when the jar was naked, the quantity at a mean of three Experiments (No. 39, No. 40, and No. 41) was only 558⅔ grains.

Thirdly, The quantity of ice melted under ſimilar circumſtances,—that is to ſay, when the jar was naked,—was ſenſibly greater when the water was at the temperature of about 41°, than when it was nearly boiling hot. In the Experiment No. 41, when the water which was poured on the ice was at the temperature of 190 deg. 542 grains only of ice were melted in 30 minutes; whereas, in the next Experiment, (No. 42,) when the water was at 41°, or 149 degrees colder, 573 grains were melted in the ſame time.

Finding that covering up the jar with a thick and warm covering of cotton cauſed more ice to be melted by the hot water, I was curious to ſee what effects would be produced by keeping the jar plunged

plunged *quite up to its brim* in a mixture of ſnow and water, inſtead of merely ſurrounding that part of it which was occupied by the cake of ice by this cold mixture.

I was likewiſe deſirous of finding out,—and if poſſible at the ſame time,—whether water, at a temperature ſomething above that at which that Fluid ceaſes to be condenſed with cold, would not melt more ice in any given time than an equal quantity of that Fluid, either colder or much hotter. The reſult of the 43d Experiment had ſhewn me,—what indeed a very ſimple computation would have pointed out,—namely, that when the temperature of the water is but a few degrees above the point of freezing, if its quantity or depth is not very conſiderable, it will ſoon be ſo much cooled as very ſenſibly to retard the proceſs of melting the ice: and with reſpect to hot water, the increaſed quantity of ice which was melted by it when the jar was covered up with a warm covering, convinced me that the real cauſe which prevented the hot water from melting as much ice as the cold water in my Experiments, was the embarraſſments in the proceſs of melting the ice, which were occaſioned by the deſcending currents formed in the hot water on its being cooled by the air at its ſurface, and by the ſides of the jar.

Theſe deſcending currents meeting in the region of the conſtant temperature of 40° with thoſe cold currents which aſcended from the ſurface of the ice, it ſeems very probable that the aſcending currents,

currents, on the motion of which the melting of ice depends, were checked by this collision.

By retarding the cooling of the hot water above, by wrapping up the jar in a warm covering, the velocity of the descending currents was of course diminished; but when this was done, the results of the Experiment showed that the melting of the ice was accelerated.

When the jar being naked, the cooling of the hot water, and consequently the motions of the descending currents, were rapid, no more than about 542 grains, or at most 575 grains, were melted in 30 minutes; but when the jar was covered with a warm covering, 634 grains, and in one Experiment (that No. 35) 747 grains, were melted in the same time.

As plunging the jar into a cold mixture of snow and water could not fail to accelerate the cooling of the hot water in the jar, and consequently to increase the rapidity of the descending currents in it, ought not this to embarrass, in an extraordinary degree, the ascending currents of ice-cold water from the surface of the ice, and to diminish the quantity of ice melted?—This is what the following Experiments, compared with the results of those No. 39, No. 40, and No. 41, will show.

Number of the Experiment.	Temperature of the water in the jar 1 inch below its surface, In the beginning of the Exp.	At the end of the Exp.	Temperature of the cold mixture in which the jar was kept plunged to its brim.	Time the water remained on the ice.	Quantity of ice melted.
				Minutes.	Grains.
No. 45	188°	68°	32°	30	406
No. 46	186°	67°	32°	30	440
No. 47	189°	68°	32°	30	432
No. 48	187°	67°	32°	30	355
No. 49	188°	68°	32°	30	364
Quantity of ice melted in these 5 Experiments,					1997

	Grains.
Mean quantity melted by hot water when the jar was kept plunged to its brim in melting ice and water,	$399\frac{2}{5}$
Mean quantity melted by hot water in 30 minutes, in the two Experiments, No. 26 and No. 27, when the part of the jar occupied by the water was surrounded by air, at the temperature of 41°, - -	456
Mean quantity melted by hot water in 30 minutes, in the three Experiments, No. 39, No. 40, and No. 41, when the part of the jar occupied by the water was surrounded by air, at the temperature of 61°, -	$558\frac{1}{3}$
Mean quantity melted by hot water in 30 minutes, in the two Experiments, No. 34 and No. 35, when the part of the jar occupied by the water was kept covered up by a thick and warm covering of cotton, -	$690\frac{1}{2}$

As all the Experiments were made in the same manner, and with equal care, and differed only in respect to the manner in which the outside of the jar, above the surface of the ice in it, was covered, their results show the effects produced by those differences.

I should

I should perhaps have fuſpected that the greater quantity of ice which was melted when the heat of the water in the jar was confined for the longeſt time had been occaſioned, at leaſt in part, by the Heat communicated downwards by the medium of the glaſs; but that this could not have been the caſe was evident, not only from the manner in which the ice was always found to have been melted, but alſo from the reſults of ſimilar Experiments made with much colder water.

Had it been melted by Heat communicated by the glaſs, it would undoubtedly have been moſt melted in thoſe parts of its ſurface where it was in contact with the glaſs, but this I never once found to be the caſe.

The reſults of the following Experiments will ſhow,—what indeed might eaſily have been foreſeen,—that the temperature of the medium by which the upper part of the jar was ſurrounded does not always affect the reſult of the Experiment in the ſame degree,—nor even always in *the ſame manner*,—in different Experiments in which the temperature of the water in the jar is very different.

To facilitate the compariſon of theſe Experiments, and that of the foregoing, which are ſimilar to them, I ſhall here place them together.

Number of the Experiment.	Temperature of the water in the jar 1 inch below its surface,		Temperature of the medium by which the upper part of the jar was surrounded.	Time the water remained on the ice.	Quantity of ice melted.
	In the beginning of the Exp.	At the end of the Exp.			
				Grains.	Minutes.
No. 50	41°	36°	32°	30	542
No. 37	41°	38°	41°	30	592
No. 42	41°	43°	61°	30	576

It is certainly very remarkable indeed that ſo much more ice ſhould be melted by water at the temperature of 41°, when the jar which contained it was ſurrounded by a cold mixture of pounded ice and water, than by an equal quantity of boiling-hot water in the ſame circumſtances. In the Experiment No. 50, the quantity melted by the cold water was 542 grains, while that melted by the boiling-hot water, taking the mean of five Experiments, (thoſe No. 45, 46, 47, 48, and 49,) was no more than $399\frac{2}{5}$ grains. But the reſults of the four following Experiments are, if poſſible, ſtill more ſurpriſing.

Theſe Experiments were made with water at the temperature of 61°, the temperature of the air of the room being at the ſame time 61°; in the two firſt of theſe Experiments the jar was kept plunged to its brim in a mixture of ſnow and water,—in the two laſt its lower part only, namely, as high as the level of the ſurface of the ice, was ſurrounded

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by this cold mixture, its upper part being naked, and ſurrounded by the air of the room.

In each of the Experiments, (as in thoſe which preceded them,) before the water was poured into the jar the ſurface of the ice was covered to the height of 0.956 of an inch with ice-cold water, in order more effectually to defend it againſt the effects of the temporary motions into which the water employed to melt the ice was unavoidably thrown in the performance of this operation; and the ſame quantity of water was always uſed, namely, 73¼ ounces Troy, or as much as was ſufficient to fill the jar to the height of 8 inches.

Number of the Experiment.	Temperature of the water in the jar 1 inch below its ſurface,		Temperature of the medium by which the upper part of the jar was ſurrounded.	Time the water remained on the ice.	Quantity of ice melted.
	In the beginning of the Exp.	At the end of the Exp.		Minutes.	Grains.
No. 51	61°	49°	32°	30	660
No. 52	61°	50°	32°	30	662
No. 53	61°	60°	61°	30	642
No. 54	61°	60°	61°	30	650

Theſe Experiments are remarkable, not only on account of the very ſmall difference in the quantities of ice melted which reſulted from the cooling of the ſides of the jar, but alſo, and more eſpecially, as that difference was directly contrary to the effects produced by the ſame means in the Experiments with hot water. More ice was melted when the

the outſide of the jar was kept ice-cold than when it was ſurrounded by air at the temperature of 61°.

All theſe appearances might, I think, be accounted for in a ſatisfactory manner on the principles we have aſſumed reſpecting the manner in which Heat is propagated in liquids; but without engaging ourſelves at preſent too far in theſe abſtruſe ſpeculations, let us take a retroſpective view of all our Experiments, and ſee what general reſults may with certainty be drawn from them.

One of the Experiments in which the greateſt quantity of ice was melted by *hot water* is that No. 36, in which 3963 grains were melted in three hours, or 180 minutes. If now from this quantity we deduct that which, according to the reſults of the two preceding Experiments, muſt have been melted in the firſt 30 minutes, namely, 690½ grains, there will remain 3272½ grains for the quantity melted in the laſt 150 minutes, which gives 654½ grains for the quantity melted in 30 minutes *in the ordinary courſe of the Experiment.*

This quantity, 654½ grains, deducted from that which at a mean of two Experiments (thoſe No. 34 and No. 35) was found to be actually melted in 30 minutes, namely, 690½ grains, leaves 36 grains for the quantity which in theſe two Experiments was melted in conſequence of the temporary motions into which the hot water was thrown in the operation of pouring it into the jar. The difference between theſe two quantities (=36 grains) is very

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inconſiderable, and ſhows that the means uſed for diminiſhing the effects produced by thoſe motions had been very efficacious.

As the reſults of the three Experiments No. 34, No. 35, and No. 36, were exceedingly regular and ſatisfactory,—as the Heat of the water appears to have been ſo completely confined by the warm covering which ſurrounded the jar, and as the proceſs of melting the ice went on regularly or equally for ſo great a length of time (three hours) in the 36th Experiment, we may venture to conclude that more ice could not poſſibly have been melted by boiling-hot water—*ſtanding on it*—than was melted in theſe Experiments.

This quantity was found to be at the rate of 654½ grains in 30 minutes.

But as in theſe three Experiments extraordinary means were uſed, by which an uncommonly large quantity of ice was melted, they cannot be conſidered as ſimilar to thoſe which were made with cold water, and conſequently cannot with propriety be compared with them.

When the Experiments were ſimilar, the mean reſults of thoſe which were made with water at different temperatures were as follows:

		Ice melted in 30 minutes
		Grains.
In the Experiments in which the part of the jar which was occupied by the water was exposed uncovered to the air at the temperature of 61°	With boiling-hot water (*Experiments* No. 39, 40, *and* 41)	$558\frac{2}{3}$
	With water at the temperature of 61° (*Experiments* No. 53 *and* No. 54)	646
	With water at the temperature of 41° (*Experiments* No. 42 *and* No. 43)	574
In the Experiments in which the part of the jar which was occupied by the water was surrounded by pounded ice and water, and consequently was at the temperature of 32°	With boiling-hot water, (*Experiments* No. 45, 46, 47, 48, *and* 49)	$399\frac{2}{3}$
	With water at the temperature of 61° (*Experiments* No. 51 *and* No. 52)	661
	With water at the temperature of 41° (*Experiment* No. 50)	542

From the results of all these Experiments we may certainly venture to conclude, that boiling-hot water is not capable of melting more ice *when standing on its surface*, than an equal quantity of water at the temperature of 41°, or when it is only *nine degrees* above the temperature of freezing!

This fact will, I flatter myself, be considered as affording the most unquestionable proof that could well be imagined, that water is a perfect *non-conductor of Heat*, and that Heat is propagated in it *only* in consequence of the motions which the

 Heat

Heat occasions in the insulated and solitary particles of that fluid*.

The discovery of this fact opens to our view one of the most interesting scenes in the economy of Nature:—but in order to prepare our minds for the contemplation of it, it will be not amiss to refresh our memory by recapitulating what has already been said on the Propagation of Heat in Fluids, and particularly in water; and adding such occasional observations as may tend to elucidate that abstruse subject.

Those who enter into the spirit of these investigations will not consider these repetitions and illustrations as either superfluous or tiresome.

* The insight which this discovery gives us in regard to the nature of the mechanical process which takes place in chemical solutions is too evident to require illustration;—and it appears to me that it will enable us to account in a satisfactory manner for all the various phænomena of chemical affinities and vegetation. Perhaps all the motions among inanimate bodies on the surface of the globe may be traced to the same cause,—namely, to the non-conducting power of Fluids with regard to Heat.

CHAP. III.

Recapitulation, and farther Investigation of the Subject.—All Bodies are condensed by Cold without Limitation, WATER ONLY EXCEPTED.—*Wonderful Effects produced in the World in consequence of the particular Law which obtains in the Condensation of Water.—This Exception to one of the most general Laws of Nature, a striking Proof of Contrivance in the Arrangement of the Universe;—a Proof which comes home to the Feelings of every ingenuous and grateful Mind.—This particular Law does not obtain in the Condensation of* SALT-WATER.—*Final Cause of the Saltness of the Sea—The Ocean probably designed by the Creator to serve as an Equalizer of Heat—Could not have answered that Purpose had its Waters been fresh.—Final Cause of the Freshness of Lakes and inland Seas in high Latitudes.—Usefulness of these Speculations.*

As the immediate cause of the motions in a liquid, which take place on its undergoing a change of temperature, is evidently the change in the specific gravity of those particles of the liquid which become either hotter or colder than the rest of the mass, and as the specific gravities of some liquids are much more changed by any given change of temperature than that of others, ought not this circumſtance (independent of the more or leſs perfect fluidity of the liquid) to make a ſenſible difference in the conducting power of liquids?

The more a liquid is expanded by any given change of temperature, the more rapid will be the aſcent of the particles which firſt receive the Heat; and as theſe are immediately replaced by other colder particles, which, in their turns, come to be heated, this muſt of courſe produce a rapid communication of Heat from the hot body of the liquid.

But when, on the other hand, the ſpecific gravity of a liquid is but little changed by any given change of temperature, the motions among the particles of the liquid occaſioned by this change muſt be very ſluggiſh, and the communication of Heat of courſe very ſlow.

Let us ſtop here for one moment juſt to aſk ourſelves a very intereſting queſtion. Suppoſe that in the general arrangement of things it had been neceſſary to contrive matters ſo that water ſhould not freeze in winter,—or that it ſhould not freeze *but with the greateſt difficulty*;—very ſlowly; *and in the ſmalleſt quantity poſſible*;—How could this have been moſt readily effected?—

Thoſe who are acquainted with the law of the condenſation of Water on parting with its Heat have already anticipated me in theſe ſpeculations; and it does not appear to me that there is any thing which human ſagacity can fathom, within the wide-extended bounds of the viſible creation, which affords a more ſtriking or more palpable

proof

proof of the wisdom of the Creator, and of the special care he has taken in the general arrangement of the universe to preserve animal life, than this wonderful contrivance:—for though the extensiveness and immutability of the general laws of Nature impress our minds with awe and reverence for the Creator of the universe, yet *exceptions to those laws*, or particular modifications of them, from which we are able to trace effects evidently *salutary* or advantageous to ourselves and our fellow-creatures, afford still more striking proofs of contrivance, and ought certainly to awaken in us the most lively sentiments of admiration, love, and gratitude.

Though in temperatures above blood-heat the expansion of water with Heat is very considerable, yet in the neighbourhood of the freezing point it is almost nothing. And what is still more remarkable, as it is an exception to one of the most general laws of Nature with which we are acquainted, when in cooling it comes within eight or nine degrees of Fahrenheit's scale of the freezing point, instead of going on to be farther condensed as it loses more of its Heat, it *actually expands* as it grows colder, and continues to expand more and more as it is more cooled.

If the whole amount of the condensation of any given quantity of boiling-hot water, on being cooled to the point of freezing, be divided into any given number of equal parts, the condensations corresponding to equal changes of temperature will be very unequal in different temperatures.

In cooling $22\frac{1}{2}$ degrees of Fahrenheit's ſcale, (or one-eighth part of the interval between the boiling and the freezing points) the condenſation will be,

				Condenſation.
In cooling $22\frac{1}{2}°$ viz. from	$212°$	to $189\frac{1}{2}°$	-	18 parts
	$189\frac{1}{2}°$	— $167°$	-	16.2
	$167°$	— $144\frac{1}{2}°$	-	13.8
	$144\frac{1}{2}°$	— $122°$	-	11.5
	$122°$	— $99\frac{1}{2}°$	-	9.3
	$99\frac{1}{2}°$	— $77°$	-	7.1
	$77°$	— $54\frac{1}{2}°$	-	3.9
	$54\frac{1}{2}°$	— $32°$	-	0.2

Hence it appears that the condenſation of water, or increaſe of its ſpecific gravity in being cooled $22\frac{1}{2}$ degrees of Fahrenheit's ſcale, is at leaſt *ninety times greater* when the water is boiling-hot, than when it is at the mean temperature of the atmoſphere in England ($54\frac{1}{2}°$), or within $22\frac{1}{2}$ degrees of freezing—(for 18 is to 0.2 as 90 to 1).

All liquids, it is true, in cooling, are more condenſed by any given change of temperature when they are very hot, than when they are colder; but theſe differences are nothing compared to thoſe we obſerve in water.

The ratio of the condenſation in cooling from $212°$ to $189\frac{1}{2}°$ to that in cooling from $54\frac{1}{2}°$ to $32°$ in each of the under-mentioned fluids, has been ſhown, by the Experiments of M. DE LUC, to be as follows:

Olive oil - -	as $1\frac{14}{100}$ to 1
Strong ſpirits of wine -	as $1\frac{29}{100}$ to 1
A ſaturated ſolution of ſea-ſalt in water	as $1\frac{38}{100}$ to 1
Pure water -	as 90 to 1

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The difference between the laws of the condensation of pure water, and of the same fluid when it holds in solution a portion of salt, is striking; but when we trace *the effects* which are produced in the world by that arrangement, we shall be lost in wonder and admiration.

Let me beg the attention of my reader while I endeavour to investigate this most interesting subject, and let me at the same time bespeak his candour and indulgence. I feel the danger to which a mortal exposes himself who has the temerity to undertake to explain the designs of Infinite Wisdom.—The enterprise is adventurous, but it cannot surely be improper.

The wonderful simplicity of the means employed by the Creator of the world to produce the changes of the seasons, with all the innumerable advantages to the inhabitants of the earth, which flow from them, cannot fail to make a very deep and a lasting impression on every human being whose mind is not degraded, and quite callous to every ingenuous and noble sentiment: but the farther we pursue our inquiries respecting the constitution of the universe, and the more attentively we examine the effects produced by the various modifications of the active powers which we perceive, the more we shall be disposed to admire, adore, and love that great First Cause which brought all things into existence.

Though winter and summer, spring and autumn, and all the variety of the seasons, are produced in a manner at the same time the most simple fimple and the moft ftupendous (by the inclination of the axis of the earth to the plane of the ecliptic); yet this mechanical contrivance alone would not have been fufficient (as I fhall endeavour to fhow) to produce that gradual change of temperature in the various climates which we find to exift, and which doubtlefs is indifpenfably neceffary to the prefervation of animal and vegetable life.

Though change of temperature feems neceffary to the growth and perfection of moft vegetables, yet thefe changes muft be within certain limits. Some plants can fupport greater changes of temperature than others, but the extremes of Heat and of Cold are alike fatal to all.

As the rays of the fun are the immediate caufe of the Heat on the furface of the globe, and as the length of the days in high latitudes is fo very different in fummer and in winter, it is evident that, in order to render thofe regions habitable, fome contrivance was neceffary to prevent the confequences which this great inequality of the Heat generated by the fun in fummer and in winter would naturally tend to produce; or, in other words, to equalize the Heat, and moderate its extremes in thefe two feafons.

Let us fee how far *Water* is concerned in this operation, and then let us examine how far the remarkable law which has been found to obtain in its condenfation by cold tends to render it well adapted to anfwer that moft important purpofe.

The

The vaſt extent of the ocean, and its great depth,—but ſtill more its numerous currents, and the power of water to abſorb a vaſt quantity of Heat, render it peculiarly well adapted to ſerve as an equalizer of Heat.

On the retreat of the ſun after the ſolſtice, it is cloſely followed by the cold winds from the regions of eternal froſt, which are continually endeavouring to preſs in towards the equator. As the power of the ſun to warm the ſurface of the earth and the air diminiſhes very faſt in high latitudes on the days growing ſhorter, it ſoon becomes too weak to keep back the denſe atmoſphere which preſſes on from the polar regions, and the cold increaſes very faſt.

There is, however, a circumſtance by which theſe rapid advances of winter are in ſome meaſure moderated. The earth, but more eſpecially the *water*, having imbibed a vaſt quantity of Heat during the long ſummer days, while they received the influence of the ſun's vivifying beams; this Heat, being given off to the cold air which ruſhes in from the polar region, ſerves to warm it and ſoften it, and conſequently to diminiſh the impetuoſity of its motion, and take off the keenneſs of its blaſt. But as the cold air ſtill continues to flow in as the ſun retires, the accumulated Heat of ſummer is ſoon exhauſted; and all ſolid and fluid bodies are reduced to the temperature of freezing water. In this ſtage the cold in the atmoſphere increaſes very faſt, and would probably increaſe ſtill faſter, were it not for the vaſt quantity of Heat which is communicated

municated to the air by the watery vapours which are firſt condenſed, and then congealed, in the atmoſphere, and which afterwards fall upon the earth in the form of ſnow; and by that ſtill larger quantity which is given off by the water in the rivers and lakes, and in the ground upon its being frozen.

But in very cold countries, the ground is frozen and covered with ſnow, and all the lakes and rivers are frozen over in the very beginning of winter. The cold then firſt begins to be extreme, and there appears to be no ſource of Heat left, which is ſufficient to moderate it in any ſenſible degree.

Let us ſee what muſt have happened if things had been left to what might be called their natural courſe;—if the condenſation of water on being deprived of its Heat had followed the law which we find obtains in other fluids, and even in water itſelf in ſome caſes, namely, when it is mixed with certain bodies.

Had not Providence interfered on this occaſion in a manner which may well be conſidered as *miraculous*,—all the freſh water within the polar circle muſt inevitably have been frozen to a very great depth in one winter, and every plant and tree deſtroyed; and it is more than probable that the regions of eternal froſt would have ſpread on every ſide from the poles, and, advancing towards the equator, would have extended its dreary and ſolitary reign over a great part of what are now the moſt fertile and moſt inhabited climates of the world!

In latitudes where now the return of ſpring is hailed by the voice of gladneſs,—where the earth decks

decks herſelf in her gayeſt attire; and millions of living beings pour forth their ſongs of joy and gladneſs, nothing would have been heard but the whiſtling of the rude winds,—and nothing ſeen but ice and ſnow, and flying clouds charged with wintry tempeſts.

Let us, with becoming diffidence and awe, endeavour to ſee what the means are which have been employed by an almighty and benevolent God to protect his fair creation.

As nouriſhment and life are conveyed to all living creatures through the medium of water; —*liquid,—living* water;—to preſerve life, it was abſolutely neceſſary to preſerve a great quantity of water in a fluid ſtate, in winter as well as in ſummer.

But in cold climates the temperature of the atmoſphere, during many months in the year, is ſo much below the freezing point, that, had not meaſures been taken to prevent ſo fatal an accident, all the water muſt inevitably have been changed to ice, which would infallibly have cauſed the deſtruction of every living thing.

Extraordinary meaſures were therefore neceſſary for preſerving in a liquid ſtate as much of the water exiſting in thoſe climates as is indiſpenſably neceſſary for the preſervation of vegetable and animal life; and this could only be done by contriving matters ſo as to prevent this water from parting with its Heat to the cold atmoſphere.

It has been ſhown,—I believe I may venture to ſay proved,—in the moſt ſatisfactory manner,— that

that liquids part with their Heat ONLY in consequence of their internal motions;—and that the more rapid theſe motions are, the more rapid is the communication of the Heat;—that theſe motions are produced by the change in the ſpecific gravity of the liquid, occaſioned by the change of temperature,—and of courſe that they are more rapid, as the ſpecific gravity of the liquid is the more changed by any given change of temperature.

But it has been ſhown that the change in the ſpecific gravity of water is extremely ſmall, which takes place in any given change of temperature, *below the mean temperature of the atmoſphere;* and particularly when the temperature of the water is very near the freezing point; and hence it follows, that water muſt give off its Heat very ſlowly when it is near freezing.

But this is not all. There is a ſtill more extraordinary, and in its conſequences more wonderful, circumſtance which remains to be noticed. When water is cooled to within eight or nine degrees of the freezing point, it not only ceaſes to be farther condenſed, but is actually expanded by farther diminutions of its Heat; and this expanſion goes on as the Heat is diminiſhed, as long as the water can be kept fluid; and when it is changed to ice, it expands even ſtill more, and the ice floats on the ſurface of the uncongealed part of the Fluid.

Let us ſee how very powerfully this wonderful contrivance tends to retard the cooling of water when it is expoſed in a cold atmoſphere.

It

It is well known that there is no communication of Heat between two bodies as long as they are both at the ſame temperature; and it is likewiſe known that the *tendency* of Heat to paſs from a hot body into one which is colder, with which it is in contact, is greater, as the difference is greater in the temperatures of the two bodies.

Suppoſe now that a maſs of very cold air repoſes on the quiet ſurface of a large lake of freſh water, at the temperature of 55° of Fahrenheit's thermometer. The particles of water at the ſurface, on giving off a part of their Heat to the cold air with which they are in contact, and in conſequence of this loſs of Heat becoming ſpecifically heavier than thoſe hotter particles on which they repoſe, muſt of courſe deſcend. This deſcent of the particles which have been cooled neceſſarily forces other hotter particles to the ſurface, and theſe being cooled in their turns bend their courſe downwards; and the whole maſs of water is put into motion, and continues in motion as long as the proceſs of cooling goes on.

Before I proceed to trace this operation through all its various ſtages, I muſt endeavour to remove an objection which may perhaps be made to my explanation of this phænomenon. As I have ſuppoſed the maſs of air which reſts on the ſurface of the water to be *very cold*, and as I have taken it for granted that there is no communication whatever of Heat between the particles of water in contact with this very cold air, and the neighbouring warmer particles of water, it may be aſked, how

how it happens that these particles at the surface are not so much cooled as to be immediately changed to ice? To this I answer, that there are two causes which conspire to prevent the *immediate* formation of ice at the surface of the water: *First*, the specific gravity of the particle of water at the surface being increased at the same moment when it parts with Heat, it begins to descend as soon as it begins to be cooled; and before the air has had time to rob it of all its Heat, it escapes and gets out of its reach: And *secondly*, air being a bad conductor of Heat, it cannot receive and transmit or *transport it* with sufficient celerity to cool the surface of water so suddenly as to embarrass the motions of the particles of that liquid in the operation of giving it off.

But to return to our lake: As soon as the water in cooling has arrived at the temperature of about 40°, as at that temperature it ceases to be farther condensed, its internal motion ceases, and those of its particles which happen to be at its surface remain there; and after being cooled down to the freezing point, they give off their latent Heat, and ice begins to be formed.

As soon as the surface of the water is covered with ice, the communication of Heat from the water to the atmosphere is rendered extremely slow and difficult; for ice being *a bad conductor of Heat* forms a very warm covering to the water,—and moreover it prevents the water from being agitated by the wind. Farther, as the temperature of the ice at its lower surface is always very nearly the same as

that