III. *An Account of a curious Phenomenon observed on the Glaciers of Chamouny; together with some occasional Observations concerning the Propagation of Heat in Fluids. By* Benjamin *Count of* Rumford, *V. P. R. S. Foreign Associate of the National Institute of France, &c. &c.*

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In an excursion which I made the last summer, in the month of August, to the Glaciers of Chamouny, in company with Professor Pictet of Geneva, I had an opportunity of observing, on what is called the Sea of Ice, (*Mer de Glace,*) a phenomenon very common, as I was told, in those high and cold regions, but which was perfectly new to me, and engaged all my attention. At the surface of a solid mass of ice, of vast thickness and extent, we discovered a pit, perfectly cylindrical, about seven inches in diameter, and more than four feet deep; quite full of water. On examining it on the inside, with a pole, I found that its sides were polished; and that its bottom was hemispherical, and well defined.

This pit was not quite perpendicular to the plane of the horizon, but inclined a little towards the south, as it descended; and, in consequence of this inclination, its mouth or opening, at the surface of the ice, was not circular, but elliptical.

From our guides I learnt, that these cylindrical holes are frequently found on the level parts of the ice; that they are formed during the summer, increasing gradually in depth, as long as the hot weather continues; but that they are frozen up, and disappear, on the return of winter.

I would ask those who maintain that water is a conductor of heat, how these pits are formed? On a supposition that there is no direct communication of heat between neighbouring particles of that fluid, which happen to be at different degrees of temperature, the phenomenon may easily be explained; but it appears to me to be inexplicable on any other supposition.

The quiescent mass of water, by which the pit remains constantly filled, must necessarily be at the temperature of freezing; for it is surrounded on every side by ice: but the pit goes on to increase in depth, during the whole summer. From whence comes the heat that melts the ice continually at the bottom of the pit? and how does it happen, that this heat acts on the *bottom* of the pit only, and not on its sides?

These curious phenomena may, I think, be explained in the following manner. The warm winds which, in summer, blow over the surface of this column of ice-cold water, must undoubtedly communicate some small degree of heat to those particles of the fluid with which this warm air comes into immediate contact; and the particles of the water at the surface so heated, being rendered specifically heavier than they were before, by this small increase of temperature, sink slowly to the bottom of the pit; where they come into contact with the ice, and communicate to it the heat by which the depth of the pit is continually increased.

This operation is exactly similar to that which took place in one of my experiments, (See my Essay on the Propagation of heat in Fluids, *Experiment* 17,) the results of which, no person, to my knowledge, has yet explained.

There is another very curious natural phenomenon, which I could wish to see explained in a satisfactory manner, by those who still refuse their assent to the opinions I have been led to adopt, respecting the manner in which heat is propagated in fluids. The water at the bottoms of all deep lakes is constantly at the same temperature, (that of 41° FAHRENHEIT,) summer and winter, without any sensible variation. This fact alone appears to me to be quite sufficient to prove, that if there be any immediate communication of heat between neighbouring particles or molecules of water, *de proche en proche*, or from one of them to the other, that communication must be so extremely slow, that we may with safety consider it as having no existence; and it is with this limitation that I beg to be understood, when I speak of fluids as being non-conductors of heat.

In treating of the propagation of heat in fluids, I have hitherto confined myself to the investigation of the simple matter of fact, without venturing to offer any conjectures relative to the causes of the phenomena observed. But the results of recent experiments on the calorific and frigorific radiations of hot and of cold bodies, (an account of which I shall have the honour of laying before the Royal Society in a short time,) have given me some new light respecting the nature of heat, and the mode of its communication; and I have hopes of being able to show *why* all changes of temperature, in *transparent* liquids, must necessarily take place at their surfaces.

I have seen with real pleasure, that several ingenious gentlemen, in London, and in Edinburgh, have undertaken the investigation of the phenomena of the propagation of heat in fluids; and that they have made a number of new and ingenious experiments, with a view to the farther elucidation of that most interesting subject. If I have hitherto abstained from taking public notice of their observations on the opinion I have advanced on that subject, in my different publications, it was not from any want of respect for those gentlemen that I remained silent, but because I still found it to be quite impossible to explain the results of my own experiments, on any other principles than those which, on the most mature and dispassionate deliberation, I had been induced to adopt; and because my own experiments appeared to me to be quite as conclusive (to say no more of them) as those which were opposed to them; and, lastly, because I considered the principal point in dispute, relative to the passage of heat in fluids, as being so clearly established by the circumstances attending several great operations of nature, that this evidence did not appear to me to be in danger of being invalidated by conclusions drawn from partial and imperfect experiments, and particularly from such as are allowed on all hands to be extremely delicate.

In all our attempts to cause heat to descend in liquids, the heat unavoidably communicated to the sides of the containing vessel, must occasion great uncertainty with respect to the results of the experiment; and, when that vessel is constructed of ice, the flowing down of the water resulting from the thawing of that ice, will cause motions in the liquid, and consequently inaccuracies of still greater moment, as I have found from my own experience; and, when thermometers immersed in a liquid, at a small distance below its surface, acquire heat, in consequence of a hot body being applied to the surface of the liquid, that event is no decisive proof that the heat acquired by the thermometer is communicated by the fluid, from above, downwards, from molecule to molecule, *de proche en proche;* so far from being so, it is not even a proof that it is from the fluid that the thermometer receives the heat which it acquires; for it is possible, for aught we know to the contrary, that it may be occasioned by the radiation of the hot body placed at the surface of the fluid.

In the experiments of which I have given an account, in my Essay on the Propagation of Heat in Fluids, great masses, many pounds in weight, of boiling hot water, were made to repose for a long time (three hours) on a cake of ice, without melting but a very small portion of it; and, on repeating the experiment with an equal quantity of very cold water, (namely, at the temperature of 41° FAHRENHEIT,) nearly twice as much ice was melted in the same time. In these experiments, the causes of uncertainty above mentioned did not exist: and the results of them were certainly most striking.

The conclusions which naturally flow from those results, have always appeared to me to be so perfectly evident and indisputable, as to stand in no need, either of elucidation, or of farther proof.

If water be a conductor of heat, how did it happen that the heat in the boiling water did not, in three hours, find its way downwards to the cake of ice, on which it reposed, and from which it was separated only by a stratum of cold water, half an inch in thickness?

I wish that gentlemen who refuse their assent to the opinions I have advanced respecting the causes of this curious phenomenon, would give a better explanation of it than that which I have ventured to offer. I could likewise wish that they would inform us how it happens, that the water at the bottoms of all deep lakes remains constantly at the same temperature: and, above all, how the cylindrical pits, above described, are formed in the immense masses of solid and compact ice which compose the Glaciers of Chamouny?

A remark, which surprised me not a little, has been made by a gentleman of Edinburgh, (Dr. THOMSON,) on the experiments I contrived, to render visible the currents into which liquids are thrown on a sudden application of heat, or of cold. He conceives, that the motions observed in my experiments, among the small pieces of amber which were suspended in a weak solution of potash in water, were no proof of currents existing in that liquid; as they might, in his opinion, have been occasioned by a change of specific gravity in the amber, or by air attached to it. I am sorry that so mean an opinion of my accuracy as an observer should have been entertained, as to imagine that I could have been so easily deceived. For nothing surely is easier, than to distinguish the motion of a solid suspended in a liquid of the same specific gravity, which is carried along by a current in the liquid, from that of a body which descends, or ascends, in the liquid, in consequence of its relative weight, or levity. In the one case, the motion is uniform; in the other, it is accelerated. In a current, the body may be carried forward in all directions, and even in curved lines; but, when it falls in a quiescent fluid, by the action of gravity, or rises, in consequence of its being specifically lighter than the fluid, it must necessarily move in a vertical direction.

The fact is, that I very often observed, in the course of my numerous experiments, the motions of small particles of matter, of different kinds, in water, which Dr. THOMSON describes; but, so far from inferring *from them* the existence of currents in that fluid, their cause was so perfectly evident, that I did not even think it necessary to make any mention of them.

I cannot conclude this Paper, without requesting that the Royal Society would excuse the liberty I have taken in troubling them with these remarks. Very desirous of avoiding every species of altercation, I have hitherto cautiously abstained from engaging in literary disputes; and I shall most certainly endeavour to avoid them in future.

I am responsible to the public for the accuracy of the accounts which I have published of my experiments; but it cannot reasonably be expected, that I should answer all the objections that may be made to the conclusions which I have drawn from them. It will however, at all times, afford me real satisfaction to see my opinions examined, and my mistakes corrected; for my first and most earnest wish is, to contribute to the advancement of useful knowledge.