hand, the right ventricle can be dissected only in connexion with the left.

For these reasons the Lecturer is inclined to regard the left ventricle as the typical one, and the right as a mere segment thereof; and in further corroboration of this opinion, he referred to the shape of the right and left ventricular cavities, as shown by casts of their interior. The left always yields a beautifully finished and perfect right-handed conical screw, while the cast of the right ventricle, although it has the same twist, represents only an incomplete portion. This statement was illustrated by a wax-cast of the ventricles of the heart of a deer.

In conclusion, the Lecturer remarked that the arrangement of the fibres composing the ventricles of the mammalian heart, as he had endeavoured to expose it, is characterized by comparative simplicity, and harmonizes perfectly with what is known of the heart's movements.

[The matters touched on by the Lecturer are more fully treated of, and the descriptions copiously illustrated by figures, in his Paper entitled "On the Arrangement of the Muscular Fibres of the Ventricular Portion of the Vertebrate Heart." By JAMES PETTIGREW, Esq. Communicated by JOHN GOODSIR, Esq., Professor of Anatomy in the University of Edinburgh. Received Nov. 22, 1859.]

April 26, 1860.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The following communications were read:—

I. "Note on Regelation." By MICHAEL FARADAY, D.C.L., F.R.S. &c. Received March 13, 1860.

The philosophy of the phenomenon now understood by the word Regelation is exceedingly interesting, not only because of its relation to glacial action under natural circumstances, as shown by Tyndall and others, but also, and as I think especially, in its bearings upon molecular action; and this is shown, not merely by the desire of different philosophers to assign the true physical principle of action, but also by the great differences between the views which they have taken.

Two pieces of thawing ice, if put together, adhere and become one; at a place where liquefaction was proceeding, congelation suddenly occurs. The effect will take place in air, or in water, or in vacuo. It will occur at every point where the two pieces of ice touch; but not with ice below the freezing-point, *i. e.* with dry ice, or ice so cold as to be everywhere in the solid state.

Three different views are taken of the nature of this phenomenon. When first observed in 1850, I explained it by supposing that a particle of water, which could retain the liquid state whilst touching ice only on one side, could not retain the liquid state if it were touched by ice on both sides; but became solid, the general temperature remaining the same*. Professor J. Thomson, who discovered that pressure lowered the freezing-point of water†, attributed the regelation to the fact that two pieces of ice could not be made to bear on each other without pressure; and that the pressure, however slight, would cause fusion at the place where the particles touched, accompanied by relief of the pressure and resolidification of the water at the place of contact, in the manner that he has fully explained in a recent communication to the Royal Society‡. Professor Forbes assents to neither of these views; but admitting Person's idea of the gradual liquefaction of ice, and assuming that ice is essentially colder than ice-cold water, *i. e.* the water in contact with it, he concludes that two wet pieces of ice will have the water between them frozen at the place where they come into contact§.

Though some might think that Professor Thomson, in his last communication, was trusting to changes of pressure and temperature so inappreciably small as to be not merely imperceptible, but also ineffectual, still he carried his conditions with him into all the cases he referred to, even though some of his assumed pressures were due to capillary attraction, or to the consequent pressure of the

* Researches in Chemistry and Physics, 8vo. pp. 373, 378.

† Mousson says that a pressure of 13,000 atmospheres lowers the temperature of freezing from 0° to −18° Cent.

‡ Royal Society Proceedings, vol. x. p. 152.

§ Proceedings of the Royal Society of Edinburgh, April 19, 1858.

atmosphere, only. It seemed to me that experiment might be so applied as to advance the investigation of this beautiful point in molecular philosophy to a further degree than has yet been done; even to the extent of exhausting the power of some of the principles assumed in one or more of the three views adopted, and so render our knowledge a little more defined and exact than it is at present.

In order to exclude all pressure of the particles of ice on each other due to capillary attraction or the atmosphere, I prepared to experiment altogether under water; and for this purpose arranged a bath of that fluid at 32° F. A pail, surrounded by dry flannel, was placed in a box; a glass jar, 10 inches deep and 7 inches wide, was placed on a low tripod in the pail; broken ice was packed between the jar and the pail; the jar was filled with ice-cold water to within an inch of the top; a glass dish filled with ice was employed as a cover to it, and the whole enveloped with dry flannel. In this way the central jar, with its contents, could be retained at the unchanging temperature of 32° F. for a week or more; for a small piece of ice floating in it for that time was not entirely melted away. All that was required to keep the arrangement at the fixed temperature, was to renew the packing ice in the pail from time to time, and also that in the basin cover. A very slow thawing process was going on in the jar the whole time, as was evident by the state of the indicating piece of ice there present.

Pieces of good Wenham-lake ice were prepared, some being blocks three inches square, and nearly an inch thick, others square prisms four or five inches long: the blocks had each a hole made through them with a hot wire near one corner; woollen thread passed through these holes formed loops, which being attached to pieces of lead, enabled me to sink the ice entirely under the surface of the ice-cold water. Each piece was thus moored to a particular place, and, because of its buoyancy, assumed a position of stability. The threads were about $1\frac{1}{2}$ inch long, so that a piece of ice, when depressed sideways and then left to itself, rose in the water as far as it could, and into its stable position, with considerable force. When, also, a piece was turned round on its loop as a vertical axis, the torsion force tended to make it return in the reverse direction.

Two similar blocks of ice were placed in the water with their opposed faces about two inches apart; they could be moved into any desired position by the use of slender rods of wood, without any change of temperature in the water. If brought near to each other and then left unrestrained, they separated, returning to their first position with considerable force. If brought into the slightest contact, regelation ensued, the blocks adhered, and remained adherent notwithstanding the force tending to pull them apart. They would continue thus, even for twenty-four hours or more, until they were purposely separated, and would appear (by many trials) to have the adhesion increased at the points where they first touched, though at other parts of the contiguous surfaces a feeble thawing and dissecting action went on. In this case, except for the first moment and in a very minute degree, there was no pressure either from capillary action or any other cause. On the contrary, a tensile force of considerable amount was tending all the time to separate the pieces of ice at their points of adhesion; where still, I believe, the adhesion went on increasing—a belief that will be fully confirmed hereafter.

Being desirous of knowing whether anything like soft adhesion occurred, such as would allow slow change of position without separation during the action of the tensile force, I made the following arrangements. The blocks of ice being moored by the threads fastened to the lowest corners, stood in the water with one of the diagonals of the large surfaces vertical; before the faces were brought into contact, each block was rotated 45° about a horizontal axis, in opposite directions, so that when put together, they made a compound block, with horizontal upper edges, each half of which tended to be twisted upon, and torn from the other. Yet by placing indicators in holes previously made in the edges of the ice, I could not find that there was the slightest motion of the blocks in relation to each other in the thirty-six hours during which the experiment was continued. This result, as far as it goes, is against the necessity of pressure to regelation, or the existence of any condition like that of softness or a shifting contact; and yet I shall be able to show that there is either soft adhesion or an equivalent for it, and from that state draw still further cause against the necessity of pressure to regelation.

Torsion force was then employed as an antagonist to regelation. The ice-blocks, being separate, were adjusted in the water so as to be parallel to each other, and about $1\frac{1}{2}$ inch apart. If made to approach each other on one side, by revolution in opposite directions on vertical axes, a piece of paper being between to prevent ice contact, the torsion force set up caused them to separate when left to themselves; but if the paper were away and the ice pieces were brought into contact, by however slight a force, they became one, forming a rigid piece of ice, though the strength was, of course, very small, the point of adhesion and solidification being simply the contact of two convex surfaces of small radius. By giving a little motion to the pail, or by moving either piece of ice gently in the water with a slip of wood, it was easy to see that the two pieces were rigidly attached to each other; and it was also found that, allowing time, there was no more tendency to a changing shape here than in the case quoted above. If now the slip of wood were introduced between the adhering pieces of ice, and applied so as to aid the torsion force of one of the loops, *i. e.* to increase the separating force, but unequally as respects the two pieces, then the congelation at the point of contact would give way, and the pieces of ice would move in relation to each other. Yet they would not separate; the piece unrestrained by the stick would not move off by the torsion of its own thread, though, if the stick were withdrawn, it would move back into its first attached position, pulling the second piece with it; and the two would resume their first associated form, though all the while the torsion of both loops was tending to make the pieces separate.

If when the wood was applied to change the mutual position of the two pieces of ice, without separating them, it were retained for a second undisturbed, then the two pieces of ice became fixed rigidly to each other in their new position, and maintained it when the wood was removed, but under a state of restraint; and when sufficient force was applied, by a slight tap of the wood on the ice to break up the rigidity, the two pieces of ice would rearrange themselves under the torsion force of their respective threads, yet remain united; and, assuming a new position, would, in a second or less, again become rigid, and remain inflexibly conjoined as before.

By managing the continuous motion of one piece of ice, it could be kept associated with the other by a flexible point of attachment for any length of time, could be placed in various angular positions to it, could be made (by retaining it quiescent for a moment) to assume and hold permanently any of these positions when the external force was removed, could be changed from that position into a new one, and, within certain limits, could be made to possess at pleasure, and for any length of time, either a flexible or a rigid attachment to its associated block of ice.

So, regelation includes a flexible adhesion of the particles of ice, and also a rigid adhesion. The transition between these two states takes place when there is no external force like pressure tending to bring the particles of ice together, but, on the contrary, a force of torsion is tending to separate them; and, if respect be had to the mere point of contact on the two rounded surfaces where the flexible adhesion is exercised, the force which tends to separate them may be esteemed very great. The act of regelation cannot be considered as complete until the junction has become rigid; and therefore I think that the necessity of pressure for it is altogether excluded. No external pressure can remain (under the circumstances) after the first rigid contact is broken. All the forces which remain tend to separate the pieces of ice; yet the first flexible adhesions and all the successive rigid adhesions which are made to occur, are as much effects of regelation as those which occur under the greatest pressure.

The phenomenon of flexible adhesion under tension looks very much like sticking and tenacity; and I think it probable that Professor Forbes will see in it evidence of the truth of his view. I cannot, however, consider the fact as bearing such an interpretation; because I think it impossible to keep a mixture of snow and water for hours and days together without the temperature of the mixed mass becoming uniform; which uniformity would be fatal to the explanation. My idea of the flexible and rigid adhesion is this:—Two convex surfaces of ice come together; the particles of water nearest to the place of contact, and therefore within the efficient sphere of action of those particles of ice which are on both sides of them, solidify; if the condition of things be left for a moment, that the heat evolved by the solidification may be conducted away and dispersed, more particles will solidify, and ultimately enough to form a fixed and rigid junction, which will remain until a force sufficiently great to break through it is applied. But if the direction of the force resorted to can be relieved by any hinge-like motion at the point of contact, then I think that the union is broken up among the particles on the opening side of the angle, whilst the particles on the closing side come within the effectual regelation distance ; regelation ensues there and the adhesion is maintained, though in an apparently flexible state. The flexibility appears to me to be due to a series of ruptures on one side of the centre of contact, and of adhesion on the other,—the regelation, which is dependent on the vicinity of the ice surfaces, being transferred as the place of efficient vicinity is changed. That the substance we are considering is as brittle as ice, does not make any difficulty to me in respect of the flexible adhesion ; for if we suppose that the point of contact exists only at one particle, still the angular motion at that point must bring a second particle into contact (to suffer regelation) before separation could occur at the first ; or if, as seems proved by the supervention of the rigid adhesion upon the flexible state, many particles are concerned at once, it is not possible that all these should be broken through by a force applied on one side of the place of adhesion, before particles on the opposite side should have the opportunity of regelation, and so of continuing the adhesion.

It is not necessary for the observation of these phenomena that a carefully-arranged water-vessel should be employed. The difference between the flexible and rigid adhesion may be examined very well in air. For this purpose, two of the bars of ice before spoken of, may be hung up horizontally by threads, which may be adjusted to give by torsion any separating force desired ; and when the ends of these bars are brought together, the adhesion of the ice, and the ability of placing these bars at any angle, and causing them to preserve that angle by the rigid adhesion due to regelation, will be rendered evident ; and though the flexible adhesion of the ice cannot in this way be examined alone, because of the capillary attraction due to the film of water on the ice, yet that is easily obviated by plunging the pieces into a dish of water at common temperatures, so that they are entirely under the surface, and repeating the observations there. All the important points regarding the flexible and rigid junction of ice due to regelation, can in this way be readily investigated.

It will be understood that, in observing the flexible and rigid state of union, convex surfaces of contact are necessary, so that the contact may be only at one point. If there be several places of contact, apparent rigidity is given to the united mass, though each of the places of contact might be in a flexible and, so to say, adhesive condition. It is not at all difficult to arrange a convex surface so that, bearing at two places only on the sides of a depression, it should form a flexible joint in one direction, and a rigid attachment in a direction transverse to the former.

It might seem at first sight as if the flexible adhesion of the ice gave us a point to start from in the further investigation of the principle of pressure. If the application of pressure causes ice to freeze together, the application of tension might be expected to produce the contrary effect, and so cause liquidity and separation at the flexible joint. This, however, does not necessarily follow; nor do I intend to consider what might be supposed to take place whilst theoretically contemplating that case. I think the changes of temperature and pressure are too infinitesimal to go for anything; and in illustration of this, will describe the following experiment. Wool is known to adhere to ice in the manner, as I believe, of regelation. Some woollen thread was boiled in distilled water, so as thoroughly to wet it. Some clean ice was broken up small and mixed with water, so as to produce a soft mass, and, being put into a glass jar clothed in flannel that it might keep for some hours, had a linear depression made in the surface, so as to form a little ice-ditch filled with water; in this depression some filaments of the wetted wool were placed, which, sinking to the bottom, rested on the ice only with the weight which they would have being immersed in water; yet in the course of two hours these filaments were frozen to the ice. In another case, a small loose ball of the same boiled wool, about half an inch in diameter, was put on to a clean piece of ice; that into a glass basin; and the whole wrapped up in flannel and left for twelve hours. At the end of that time it was found that thawing had been going on, and that the wool had melted a hole in the ice, by the heat conducted through it to the ice from the air. The hole was filled with the water and wool, but at the bottom some fibres of the wool were frozen to the ice.

Is this remarkable property peculiar to water, or is it general to all bodies? In respect of water it certainly seems to offer us a glimpse into the joint physical action of many particles, and into the nature of cohesion in that body when it is changing between the solid and liquid state. I made some experiments on this point. Bismuth was melted and kept at a temperature at which both solid and liquid metal could be present; then rods of bismuth were introduced, but when they had acquired the temperature of the mixed mass, no adhesion could be observed between them. By stirring the metal with wood, it was easy to break up the solid part into small crystalline granules; but when these were pressed together by wood under the surface, there was not the slightest tendency to cohere, as hail or snow would cohere in water. The same negative result was obtained with the metals tin and lead. Melted nitre appeared at times to show traces of the power; but, on the whole, I incline to think the effects observed resulted from the circumstance that the solid rods experimented with had not acquired throughout the fusing temperature. Nitre is a body which, like water, expands in solidifying; and it may possess a certain degree of this peculiar power.

Glacial acetic acid is not merely without regelating force, but actually presents a contrast to it. A bottle containing five or six ounces, which had remained liquid for many months, was at such a temperature that being stirred briskly with a glass rod crystals began to form in it; these went on increasing in size and quantity for eight or ten hours. Yet all that time there was not the slightest trace of adhesion amongst them, even when they were pressed together; and as they came to the surface, the liquid portion tended to withdraw from the faces of the crystals; as if there were a disinclination of the liquid and solid parts to adhere together.

Many salts were tried (without much or any expectation),—crystals of them being brought to bear against each other by torsion force, in their saturated solutions at common temperatures. In this way the following bodies were experimented with:—Nitrates of lead, potassa, soda; sulphates of soda, magnesia, copper, zinc; alum; borax; chloride of ammonium; ferro-prussiate of potassa; carbonate of soda; acetate of lead; and tartrate of potassa and soda; but the results with all were negative.

My present conclusion therefore is that the property is special for water; and that the view I have taken of its physical cause does not appear to be less likely now than at the beginning of this short investigation, and therefore has not sunk in value among the three explanations given.

Dr. Tyndall added to one of his papers*, a note of mine "On ice of irregular fusibility" indicating a cause for the difference observed in this respect in different parts of the same piece of ice. The view there taken was strongly confirmed by the effects which occurred in the jar of water at constant temperature described in the beginning of the preceding pages, where, though a thawing process was set up, it was so slow as not to dissolve a cubic inch of ice in six or seven days. The blocks retained entirely under water for several days, became so dissected at the surfaces as to develope the mechanical composition of the masses, and to show that they were composed of parallel layers about the tenth of an inch thick, of greater and lesser fusibility, which layers appear, from other modes of examination, to have been horizontal in the ice whilst in the act of formation. They had no relation to the position of the blocks in the water of my experiments, or to the direction of gravity, but had a fixed position in relation to each piece of ice.

ADDENDUM, received April 28.

The following method of examining the regelation phenomena above described may be acceptable. Take a rather large dish of water at common temperatures. Prepare some flat cakes or bars of ice, from half an inch to an inch thick; render the edges round, and the upper surface of each piece convex, by holding it against the inside of a warm saucepan cover, or in any other way. When two of these pieces are put into the water they will float, having perfect freedom of motion, and yet only the central part of the upper surface will be above the fluid; when, therefore, the pieces touch at their edges, the width of the water-surface above the place of contact may be two, three, or four inches, and thus the effect of capillary action be entirely removed. By placing a plate of clean dry wax or spermaceti upon the top of a plate of ice, the latter may be entirely submerged, and the tendency to approximation from capillary action converted into a force of separation. When two or more of such floating pieces of ice are brought together by contact at some point under the water, they adhere; first with an apparently flexible, and then with a rigid adhesion. When five or six pieces are grouped in a contorted shape, as

* Philosophical Transactions, 1858, p. 228.

an S, and one end piece be moved carefully, all will move with it rigidly; or, if the force be enough to break through the joint, the rupture will be with a crackling noise, but the pieces will still adhere, and in an instant become rigid again. As the adhesion is only by points, the force applied should not be either too powerful or in the manner of a blow. I find a piece of paper, a small feather, or a camel-hair brush applied under the water very convenient for the purpose. When the point of a floating, wedge-shaped piece of ice is brought under water against the corner or side of another floating piece, it sticks to it like a leech; if, after a moment, a paper edge be brought down upon the place, a very sensible resistance to the rupture at that place is felt. If the ice be replaced by like rounded pieces of wood or glass, touching under water, nothing of this kind occurs, nor any signs of an effect that could by possibility be referred to capillary action; and finally, if two floating pieces of ice have separating forces attached to them, as by threads connecting them and two light pendulums, pulled more or less in opposite directions, then it will be seen with what power the ice is held together at the place of regelation, when the contact there is either in the flexible or rigid condition, by the velocity and force with which the two pieces will separate when the adhesion is properly and entirely overcome.

II. "Notes on the apparent Universality of a Principle analogous to Regelation, on the Physical Nature of Glass, and on the probable existence of Water in a state corresponding to that of Glass." By Edward W. Brayley, Esq., F.R.S. &c. Received April 26, 1860.

1. Recent experimental investigations, and the reasoning founded upon them, have elevated the designation of an observed property of ice to the character of a principle in physics. The growth of crystals of camphor and of iodide of cyanogen, by the deposition of solid matter upon them from an atmosphere unable to deposit like solid matter upon the surrounding glass, except at a lower temperature; and that of crystals in solution, by the deposition of solid matter upon them which is not deposited elsewhere in the solution, have been adduced by Mr. Faraday to illustrate the extension of the principle